The Effect of Debt Collection Laws on Access to Credit

Charles Romeo

Ryan Sandler

2018-01

The Effect of Debt Collection Laws on Access to Credit

Charles Romeo and Ryan Sandler[*]

February 12, 2018

Abstract

Debt collection by third party collection agencies is an important part of the market for consumer credit, but has been little studied in the economics literature. Regulations on debt collection practices can protect consumers, but may also lead to unintended consequences as the costs of better practices are passed on to creditors, who in turn restrict consumers' access to credit or raise prices. Using detailed administrative data on new credit card accounts, this paper studies the effects of four recent state laws and regulations that placed restrictions on the conduct of debt collectors. We find that such restrictions reduce access to credit card accounts and raise prices for credit cards, but that this effect is very small—of a similar magnitude to a minor change in the average consumers' credit score.

1 Introduction

Not all credit is paid back on-time or in-full. When consumers default on unsecured debts, creditors frequently turn to debt collectors, whether by hiring a third-party collection agency, or selling delinquent debts entirely to a debt buyer that collects on its own behalf. Debt collectors are an important part of the ecosystem of consumer credit, but also a potential source of concern. Debt collectors are routinely the most common source of consumer complaints filed with the U.S. Federal Trade Commission and the Consumer Financial Protection Bureau. The U.S. federal government and many states regulate the conduct of debt collectors to reduce the likelihood of consumer harm, but this may have unintended consequences. Creditors' expected returns for the provision of credit take late payments and debt collection costs into account. Changes in debt collection rules that increase the cost of collections or reduce expected collections returns will reduce expected returns to making credit available in the first place. Profit maximizing firms may respond to such restrictions by recalibrating the provision of credit to consumers along at least one of three dimensions of credit provision: credit access, quantity and price.[1]

Several states and local governments have tightened their debt collection laws or conducted rulemakings since 2009 to address sources of consumer harm in the debt collection market. The new requirements were aimed at preventing certain problems in the debt collection markets, such as collectors pursuing the wrong consumer, seeking the wrong amount or deceiving consumers regarding any statute of limitations. The provisions include additional documentation to substantiate debts, or additional disclosures prior to filing lawsuits and informing consumers that the state's statute of limitations has run out. In this paper, we study the impact of these debt collection restrictions on access to credit in the market for credit cards. We bring to bear two large administrative datasets to estimate a difference-in-difference model of the effects of the new state-level restrictions. We use a panel of consumer credit records to link credit inquiries to new open credit card accounts, allowing us to directly estimate the effect of debt collection restrictions on access to credit cards on the extensive margin. We also estimate the effect of the restrictions on initial credit limits. Separately, we employ credit card account records from a set of 35 large banks to examine effects on credit limits and interest rates.

We find that tightening of debt collection laws reduces access to credit and credit limits on average, but that the effect is very small in magnitude. For interest rates we find a tightly estimated zero effect. The richness of our data allows us to dig further

---

[1]Our empirical setting focuses on revolving credit with no fixed term. For installment loans with a fixed number of payments, firms may also restrict the term of the loan.

into this average effect, estimating separate effects by consumers' credit scores, separate effects for each treated state, and separate effects by credit card issuer. Splitting our results on credit access by prime and subprime consumers, we find a somewhat larger effect on credit access for sub-prime borrowers. Even broken out, our estimated effects are still small in magnitude–the effect of these debt collection restrictions is equivalent to an error that lowers consumers' credit scores by 8 points or less. We observe some variation in effects across treated states, although largely we find consistent effects. We do not observe a consistent effect on interest rates, although we do observe that subprime borrowers are less able to get cards with an introductory APR of zero percent after a debt collection restriction is imposed. Allowing for different effects by bank, we find significant heterogeneity—our near-zero average effect is made up of moderate effects from some banks, zero effect on others, and effects with the opposite sign from still others.

In addition to our main results on access to credit and credit pricing, we present suggestive results illustrating the mechanism underlying our results. Using our credit record data, we examine outcomes relating to collections on consumers' credit records. We find a tightly estimated zero effect of debt collection restrictions on payments to collectors, although formal disputes may increase somewhat, perhaps due to the additional information that collectors are required to disclose. This suggests that debt collectors' costs are raised somewhat by dealing with additional disputes, but that this is not significant enough to be consistently passed on to upstream creditors.

Despite the importance of third-party collections to the overall market for credit, the collections industry and regulation of its practices have received little attention in the economics literature. The two exceptions are Fedaseyeu (2015) and Fonseca, Strair and Zafar (2017).[2] Fedaseyeu (2015) examines the effect of various changes in state debt collection laws on credit access between 1999 and 2012, using aggregate data capturing the total number of new open accounts. Our work differs both in its use of rich microdata, rather than aggregates, and its focus on consumer protections. Fedaseyeu's measure of the strength of state debt collection laws counts the number of new debt collection restrictions, ignoring differences between the practical effects of different types of restrictions. In particular, adjustments in the licensing fees for debt collectors, which only affect fixed costs and which are more than half of Fedaseyeu's sample, are given the same weight as major requirements that impact marginal costs or collections effectiveness. However, even with this weakness, he finds results similar to ours: a one unit increase in the number of collector restrictions in Fedaseyeu's model, has approximately the same effect on access

---

[2]Beyond these works, Hynes and Posner (2002) and Livshits (2015) review the literature on the law and economics of creditor remedies for consumers in default.

to credit as a four point decrease in the average credit score.

Fonseca et al. (2017) use administrative microdata for the period 1999–2014 to examine the impact of licensing requirements, surety bonds and rights of private action on access to credit. The use of rich microdata enables Fonseca et al. (2017) to avoid Fedeseyu's approach of counting debt collection restrictions and take a more standard treatment effects approach: the treatment effect is set equal 0 pre-treatment and 1 afterwards. However, as with Fedaseyeu (2015), it is not clear what mechanism is driving their treatment effect estimates—licensing requirements and surety bonds only impact fixed costs of collections, and a private right of action has always been included in the federal debt collection statute. Perhaps including these items in a state collections act may signal that state authorities intend to police debt collection practices more strictly. Their results suggest as much; on credit card limits their results are consistent with our own, and their results on credit card balances suggest that interest rates are responsive to these policies.

Although there is little past literature on the affect of debt collection laws, other works have examined the interaction of laws affecting consumer defaults and access to credit. The most closely related empirical work on the impact of state and national law differences on credit provision appears in the bankruptcy literature.[3] States allow for different levels of bankruptcy exemptions that may affect the provision of credit. Gropp, Scholz and White (1997) and Cerqueiro et al. (2016) find strong credit supply effects of bankruptcy exemptions. Gropp et al. (1997) find that households in the lowest quartile of the asset distribution are more likely to be denied credit completely or receive smaller loans, while Cerqueiro et al. (2016) finds that higher bankruptcy exemptions reduce innovation by small firms; patent quality increases but small firms shelve some of their lower quality projects. Pattison (2016) compares borrower welfare during and after bankruptcy. He finds that higher exemptions reduce the amount that borrowers pay in default, but that this payment reduction is financed through an increase in the interest rate that more than compensates for the reduced payments during bankruptcy, thereby leaving borrowers worse off. White (2007) presents evidence showing that credit card companies responded to a 2005 federal law tightening bankruptcy requirements by expanding the supply of credit.

There is also a strand of literature studying judicial foreclosure laws that is related

---

[3]In addition, there is a link between formal bankruptcy and the creditors' demand for debt collection. Dawsey and Ausubel (2004) report that in one large bank's credit card portfolio, about half of defaults occurred without a formal bankruptcy filing. Dawsey et al. (2013) show that this "informal bankruptcy" behavior is more common, and formal bankruptcy less common, in states with anti-harassment laws restricting debt collectors conduct.

to our work. Some states require the foreclosure process to be handled by the court. Involving the court raises the cost and extends the timeline of foreclosure proceedings. Pence (2006) finds that loan sizes are four to six percent smaller in states with defaulter-friendly foreclosure laws. Dagher and Sun (2014) find a significant increase in rejection rates of jumbo mortgage loans in states with judicial foreclosure laws.

This paper proceeds with a discussion of recent changes to debt collection laws in Section 2. We lay out a simple model of lender behavior with collections in section 3, and we describe our data in Section 4. Section 5 presents our difference-in-difference results on changes in credit access, credit limits, interest rates, disputes and payments resulting from the law changes. Section 6 concludes.

2   Recent changes to state debt collection laws

The basic conduct requirements for debt collectors in the United States are set by the federal Fair Debt Collection Practices Act (FDCPA), passed in 1977. The FDCPA prohibits a set of "abusive and deceptive" practices, but also allows states to set more stringent requirements. In recent years, North Carolina extended a series of restrictions to firms that buy debts through its Consumer Economic Protection Act (NC CEP Act) which took effect in October 2009. California passed the Fair Debt Buying Practices Act (CA Debt Buyer Law), also affecting debt buyers, in January 2014. Arkansas enacted a state Fair Debt Collection Practices Act in July 2009 which includes enhanced penalties for illegal practices but otherwise mirrors the Federal FDCPA. Both New York City and New York State have introduced new debt collection restrictions in recent years via administrative regulations. The New York City regulation (NYC Regulation) was implemented at the end of April 2010, while New York State implemented its regulation (NY Debt Collection Reg.) in December 2014. We refer to the these measures collectively as "debt collection restrictions." In each case, the updates concentrated on establishing standards for what constitutes unfair or deceptive acts or practices, although the exact details of the new restrictions vary.

With the exception of Arkansas, these updated restrictions generally require collectors to take additional steps before collecting, including requiring additional documents to substantiate debts before collections can begin, requiring disclosures or additional documentation before lawsuits can be filed to enforce a debt, and requiring disclosures once the state's statute of limitations has run out. These updates build upon the federal FDCPA. The FDCPA requires that "Within five days after initial communication with a consumer in connection with the collection of any debt, a debt collector shall...send the

consumer a written notice containing—(1) the amount of the debt; (2) the name of the creditor to whom the debt is owed."[4] The FDCPA does not require disclosures or additional substantiation before suits can be filed, nor does it require a statute of limitations disclosure.

The new restrictions we study add substantiation requirements, requiring collectors to obtain and provide:

1. the name of the original creditor,

2. the name and address of the debtor as appearing on the original creditor's records,

3. the original consumer account number,

4. a copy of the contract or other document evidencing the consumer debt, and

5. an itemized accounting of the amount claimed to be owed, including all fees and charges.

In addition, the New York State and North Carolina restrictions both require "that the collector has a complete chain-of-title proving that the collector has the right to collect on the debt."[5] Items 1-5 are generally available to third party collectors and to a lesser extent to debt buyers[6] though they are not typically passed to the consumer unless the consumer disputes the debt. Chain-of-title, however, generally is not transferred as part of debt sales. As such, this requirement may add a substantial cost burden and, in North Carolina, it may impact market structure by shifting collections activity away from debt buyers, or at a minimum reducing the number of sales of collections portfolios. In North Carolina, moreover, the short time from adoption of the law, September 9. 2009, until it became effective, October 1, 2009, and the fact that it applies to "debt collection activities undertaken, and actions filed on or after that date."[7] suggests that this may have constituted a substantial negative shock to the value of portfolios of North Carolina debt held by debt buyers. In contrast, the New York State law provided 180 days after it was published in the state Register before the substantiation requirements became effective.[8]

---

[4]15 U.S.C. §§1692-1692p, 1692f.

[5]2009 N.C. Sess. Laws p. 573.

[6]The initial debt buyer usually has a contractual right to request substantiation information from the original creditor for some percentage of consumer records in portfolios it purchases. This right, however, does not generally transfer to subsequent debt buyers.

[7]Ibid.

[8]Vol XXXVI N.Y. Reg. p. 12 (July 16, 2014).

In addition, prior to filing suit to obtain judgment on a debt, the NC CEP Act introduces a disclosure and additional substantiation requirements for debt buyers. The disclosure requires the debt buyer to give "the debtor written notice of intent to file a legal action at least 30 days in advance of filing." Substantiation introduces four more pieces of information the debt buyer must have:

1. The original charge-off balance, or, if the balance has not been charged off, an explanation of how the balance was calculated.

2. An itemization of post charge-off additions, where applicable.

3. The date of last payment.

4. The amount of interest claimed and the basis for the interest charged.[9]

Again with the exception of Arkansas, all the new restrictions also contain time-barred debt disclosures. Each state sets a Statute of Limitations beyond which a consumer can no longer be sued to collect a debt.[10] The clocks on these statutes, however, can be reset if a consumer affirms the debt or makes a payment on the debt. The disclosures, as such, require third party collectors and/or debt buyers to inform consumers that they can no longer be sued to collect the debt, but if they reaffirm or make any payments on the debt the clock on the Statute of Limitations will restart. The cost to collectors of these disclosures is more indirect than direct; they eliminate an asymmetric information problem that may induce consumers to choose not to pay a debt, but they have little effect on collector costs.

We include the Arkansas Act, the main effect of which was the creation of licensing standards for debt collectors and a state board to regulate licenses, but otherwise mirrored the Federal FDCPA, because the state statute provides a new venue for filing suits for debt collection practice violations and includes enhanced statutory penalties of up to $10,000 for individual violations of the state FDCPA. In addition, the passage of this Act may signal a heightened level of policing of collections tactics at the state level.

We exclude one recent set of debt collection restrictions from consideration that, based on our discussions with industry experts, did not place a binding constraint on collector practices. In Massachusetts, a March 2012 regulation limits conversations between collectors and consumers to no more than two per week. This, however, appears to be a fairly minor restriction on collector behavior, as it only affects conversations not contact

---

[9]2009 N.C. Sess. Laws at p. 573-5

[10]Statutes of Limitations range from a minimum of 2 years in California to a maximum of 15 years in Rhode Island.

attempts.[11] The Massachusetts regulation also extended FDCPA protections to collection activities by creditors themselves, who are usually exempt.

3 Theory: lender credit decisions with debt collection

We model lender decisions whether to extend credit to consumers, how much credit to extend and the price of the credit in a simple two-period framework. We show that lender decisions will be affected by collection restrictions, and that heterogeneous responses by lenders are possible in moving to a new equilibrium.

In period 0, consumers seek to borrow an amount b, and are required to repay $(1+r)b$ out of income in period 1. Consumers' period 1 income is uncertain, such that with probability ρ they will earn enough income to repay the loan. Lenders observe ρ, which we assume varies among consumers, and set a minimum value $\underline{\rho}$ such that they only lend to consumers for whom $\rho \geq \underline{\rho}$. If a lender extends credit to a particular consumer, the lender further decides whether to lend b or some amount $\bar{b} < b$ and chooses the price of the loan, r.

For consumers who do not repay in full, lenders have access to a collections technology, e, that they use to recover a fraction $\ell(e) \in [0,1)$ of the amount due, or $\ell(e)(1+r)b$. We treat e as exogenous, having been set by policy. Collections technology can alternatively be thought of as collection effort, and we assume that lenders commit to the maximum level of effort allowed by law. Lending decisions are all a function of e, as recoveries influence profit maximizing decisions about whether to lend, $\underline{\rho} \equiv \underline{\rho}(e)$, the optimal price, $r \equiv r(e)$, and where to cap borrowing, $\bar{b} \equiv \bar{b}(e)$.

Lenders make lending decisions in period 0 and earn profits in period 1 that depend on their period 0 decisions. Let $R(e) = 1 + r(e)$ and specify lender profits from a consumer with repayment probability ρ as

$$\pi(\rho, r; e) = \rho(R(e) - \delta)b(r(e)) + (1 - \rho)(\ell(e)R(e) - \delta)b(r(e)) - F(e), \tag{1}$$

where δ denotes marginal cost of funds per dollar lent and $F(e)$ denotes fixed costs which may also be a function of collections technology.

Lenders maximize total profits across all borrowers by choosing $\underline{\rho}^*(e)$ and $r^*(e)$. Knowing $r^*(e)$ implies borrowing demand $b(r^*(e))$ and lenders decide whether to lend $b(r^*(e))$

---

[11]It is our understanding, from conversations with industry representatives, that debt collectors rarely if ever need more than two live conversations per week to collect a debt.

or to cap lending at $\overline{b}^*(e)$.

To choose $\underline{\rho}^*(e)$, lenders find the smallest value of ρ that yields non-negative expected variable profits. Specifically,

$$\text{choose } \underline{\rho}^*(e) : \rho(R(e) - \delta)b(r(e)) + (1 - \rho)(\ell(e)R(e) - \delta)b(r(e)) = 0. \qquad (2)$$

This yields,

$$\underline{\rho}^*(e) = \frac{-(\ell(e)R(e) - \delta)}{(R(e) - \delta) - (\ell(e)R(e) - \delta)} \ if \ \ell(e)R(e) - \delta \leq 0, \qquad (3)$$

$\underline{\rho}^*(e) = 0$ otherwise. To gain intuition, consider how $\underline{\rho}^*$ changes with recovery rates ℓ. As $\ell(e) \to 0 \Rightarrow \underline{\rho}^*(e) \to \frac{\delta}{R(e)}$, which implies that the minimum repayment probability that a lender will consider in making a loan moves inversely to markup, defined as $(R(e) - \delta)$, with larger markups allowing lenders to consider making riskier loans, while low markups will yield a more a more constrained credit policy. In the extreme, as $(R(e) - \delta) \Rightarrow 0, \underline{\rho}^*(e) \Rightarrow 1$, implying that the only loans that get made are ones for which repayment is a certainty. Conversely, as recovery rates increase such that $\ell(e) \to \frac{\delta}{R(e)} \Rightarrow \underline{\rho}^*(e) \to 0$. In this case, recovery rates are high enough that all loans get made.

To choose $R^*(e)$, firms maximize profits yielding the Bertrand profit margin condition

$$m(R^*; \rho, \ell, e) = \frac{(\rho + (1 - \rho)\ell(e))R^*(e) - \delta}{(\rho + (1 - \rho)\ell(e))R^*(e)} = -\left(\frac{\partial b(r(e))}{\partial r(e)}\bigg|_{r^*}\right)^{-1} \frac{b(r^*(e))}{r^*(e)} = -\varepsilon_{b,r^*}^{-1}, \quad (4)$$

where $m(R^*; \rho, \ell, e)$ is the lender's profit margin and ε_{b,r^*}^{-1} is the consumer's inverse elasticity of demand for borrowing. The term $(\rho + (1-\rho)\ell(e))R^*(e)$ is the effective interest rate on loans accounting for the fact that $(1-\rho)$ loans are not paid back in full. Substituting $\underline{\rho}^*(e)$ from (3) into ρ in (4) yields $m(R^*; \rho, \ell, e) = 0$; as lenders make zero expected profits from the riskiest consumers. $m(R^*; \rho, \ell, e)$ increases in $\rho \geq \underline{\rho}^*(e)$, equaling $(R^*(e) - \delta)/R^*(e)$ for consumers who do not present any default risk.

With the lender's optimization problem now in place, we can consider how the new restrictions impact $\underline{\rho}^*(e)$ and $R^*(e)$. Direct effects are likely to come through impact on recovery rates, $\ell(e)$, and fixed costs, $F(e)$. Specifically, increased substantiation requirements are likely to induce lenders to make technology upgrades which will increase $F(e)$. These upgrades will only impact $\underline{\rho}^*(e)$ and/or $R^*(e)$ if the substantiation requirements cannot be satisfied for all consumers, or if dispute rates increase, in which case these requirements will reduce the recovery rate. Alternatively, disclosure requirements are likely to directly increase collector's marginal costs as disclosures will have to be mailed to all

consumers who have either reached the statute of limitations for their state or before suits are filed. Disclosures are also likely to directly reduce $\ell(e)$ through their effect in reducing asymmetric information.

Suppose that $\ell(e)$ decreases as a result of a new restriction. Equations (3) and (4) indicate that there will be more than one possible equilibrium response by lenders. Both equations suggest that a variety of combinations of increases in $\underline{\rho}^*(e)$ and/or $R^*(e)$ could be used to re-equilibrate both sides of each equation. Response differences are likely to be driven at least in part by market position. For example, the distribution of ρ, or the range of interest rates, in the population of consumers served by each lender may differ from that of consumers served by other lenders.

Neither (3) or (4) can be used to provide insights into how $\bar{b}^*(e)$ might be adjusted in response to a decrease in $\ell(e)$. To intuit why lenders might reduce lending caps in response to a reduction in the effectiveness of collections, we introduce elements of the model of period 1 income and consumption from Dávila (2016). Suppose period 1 income is distributed according to cdf $G(y_1) \in [\underline{y}_1, \overline{y}_1]$, where $\underline{y}_1 \geq 0$ and $\overline{y}_1$ could be infinite and period 1 consumption is given by $y_1 - \ell(e)R(e)b$. Further suppose that consumers have a desired level of period 1 consumption $y_1^D : y_1^D \in [\underline{y}_1, \overline{y}_1]$. Distribution $G(\cdot)$ is assumed to be common information to both lenders and consumers, but each consumer's y_1 draw is private information, and lenders use collection efforts that imposes a utility cost on consumers to improve recovery rates.

With this setup we can characterize three groups of consumers based on their y_1 draw. Group 1 have $y_1 - R(e)b < 0$. These consumers are forced to default, though assuming that for some $\ell(e)$, $y_1 - \ell(e)R(e)b \geq 0$ effective collection efforts can recover some portion of the debt. Group 2 consumers are assumed to have $0 \leq y_1 - R(e)b < y_1^D$. These consumers will default strategically, paying only $\ell(e)$: $y_1 - \ell(e)R(e)b \geq y_1^D$. Effective collections that impose a utility cost on consumers will push this relationship toward equality. Finally, for Group 3 consumers, $y_1^D \leq y_1 - R(e)b$. These consumers are assumed to pay in full to avoid bearing the costs of collections. For consumers in Groups 1 and 2 a decrease in $\ell(e)$ resulting from a new restriction will decrease collections. This may induce some lenders to decrease $\bar{b}^*(e)$ in order to have less capital at risk of default.

In this sense, decreasing $\bar{b}^*(e)$ can be thought of as a substitute for an increase in $\underline{\rho}^*(e)$. Which one a lender might use will likely depend on their information; if, for example, lenders cannot accurately assess who is more or less likely to default among broad groups of consumers, then a decrease in $\bar{b}^*(e)$ within one or more groups of consumers may be a more effective strategy for reducing default losses.

This simple model of lender decisions then suggests that we will likely observe many

possible responses to new debt collection restrictions. Overall, effective new collections restrictions will likely effect the provision of credit, but each lender will make its own assessment of consumer and competitor responses to changes in $\underline{p}^*(e)$, $R^*(e)$ and $\bar{b}^*(e)$. If a lender assesses that its competitors are not likely to increase $R^*(e)$, it might fear a highly elastic consumer response to an interest rate increase and instead alter $\underline{p}^*(e)$ or $\bar{b}^*(e)$. Alternatively, if each lender expects its competitors to increase rates, or if the interest rate is only one element in a consumer's decision when applying for a credit card, then interest rate increases may be an important part of the response to debt collection restrictions.

4   Data

We use two main datasets for our empirical work, both maintained by the U.S. Consumer Financial Protection Bureau: the Consumer Credit Panel (CCP) and the Credit Card Database (CCDB). We focus our empirical work on new credit card accounts. Credit cards are one of the most common unsecured debts defaulted on,[12] and the most common type of debt referred to debt collectors.[13] We discuss each of our datasets in turn and present basic summary statistics.[14]

4.1   Consumer Credit Panel (CCP) Data

The CCP is a 1-in-48 sample of de-identified consumer credit records from one of the three nationwide credit reporting agencies. The CCP provides annual snapshots of consumers' credit records beginning in 2001, with quarterly data available starting in 2004 and monthly updates beginning in 2013. For our analysis, we use quarterly data only, starting in June 2010, running through December 2016, the latest available at the time of our analysis. We limit our data to CCP waves beginning in June 2010 because certain variables are not available in earlier instances of the data. The credit records available in the CCP include records of accounts, referred to as tradelines, and so-called "hard" inquiries, where a creditor has pulled the consumers credit record in response to a consumer initiated inquiry. Such inquiries are visible to other prospective creditors and are

---

[12]Federal Reserve Bank of New York (2017)

[13]Consumer Financial Protection Bureau (2017)

[14]Consistent with the CFPB's confidentiality rules, this paper only presents results that are aggregated and do not identify any specific institutions. Additionally, the data used contain no direct consumer identifiers.

considered in the calculation of credit scores.[15] Hard inquiries are reported on a credit report for two years and then removed, and so in practice our data cover the period from June 2008 to December 2016.

To draw the sample for our main analysis on access to credit, we select all inquiries and tradelines whose account type and kind-of-business code indicate that the record is for a credit card or similar revolving credit. Because we are interested in newly opened accounts, we drop accounts that are reported as having opened before June 2008. In addition to the direct credit record information, the CCP contains separate files providing each consumers' credit score, birth year, and the Census tract of their residence. We link these files to our inquiry and tradeline data, and additionally link Census tract demographic information from the 2009–2014 5-year sample of the American Community Survey.

We make two exclusions from our sample. First, we drop consumers who are reported as living in more than one state during the sample period. This allows us to abstract from movers who might be affected by the laws of more than one state during our sample period. Only about 5% of our sample is dropped in this manner. Second, we exclude consumers residing in Massachusetts—as noted in section 2, Massachusetts implemented a debt collection law during our sample period, where the main binding constraint was on original creditors, rather than third-party collectors. As such, it is not clear whether Massachusetts should properly be considered treated, or in the control group. Rather than risk misclassifying, we drop all consumers residing in the state.

To prepare our analysis dataset we keep only one instance of each tradeline and inquiry, from the latest CCP wave in which it appears. We create an indicator for a successful inquiry if within 14 days of the inquiry, we observe a new credit card account being opened.[16] For accounts, we record the credit limit associated with the first appearance of that account, and consider that the initial credit limit offered to the consumer. Figure 1 plots the means of our two outcome variables from the CCP by 5 point bins of credit score, with a vertical line denoting the cut-off between prime and subprime borrowers. We observe that both measures rise essentially monotonically with credit score, with a small amount of noise around that basic trend. Particularly for our measure of inquiry success, we take this as a sign that our outcome measure is capturing what it is supposed to capture. Both plots are flatter for consumers with subprime credit scores, a result we will see reflected in lower coefficients on credit score in our regression results.

---

[15]This is distinct from "soft" inquiries, which are only visible to the consumer and are not used in credit scores. Such inquiries are often used by current creditors to monitor the accounts of their customers. The CCP does not have information on soft inquiries.

[16]Where we observe multiple successful inquiries within 14 days of each other, we keep only one such inquiry.

We note that we do not observe the universe of inquiries. While mortgage originators generally pull credit reports from all three major credit bureaus, for credit cards typically only one or two bureaus receive an inquiry from the issuer. However, we should observe the universe of open accounts, such that for any account inquiry, we should observe whether an account is eventually opened. As long as there is no systematic difference in which credit bureaus credit card providers pull inquiries from, this should not bias our results.

Table 1 shows basic summary statistics on our sample of credit card inquiries and accounts.

In addition to credit card records, we also provide ancillary results on collections tradelines, that is, collections by third party collectors or debt buyers that have been reported to the credit reporting agency. We note that while credit card accounts and inquiries are generally recorded consistently in the CCP, there are many instances of inconsistent reporting with collection accounts. Moreover, not all collectors report to the credit bureaus. Some collectors are required by the original creditor not to report, as the creditor continues to do so itself. The relative shares of different types of debt illustrate this inconsistency. The CCP collections tradelines have a code indicating the broad type of business of the original creditor. Example codes are "medical" and "finance." Although a CFPB survey found that consumers were contacted by debt collectors about credit cards about as often as medical debts, some 57% of the collections tradelines in the CCP are for medical debt, while less than 5% have a code for "financial" or "credit union", much less for credit cards specifically. Given these limitations, we view our results using the collections tradelines as merely suggestive of the mechanism underlying our main results on access to credit.

We form our sample of collections tradelines in much the same way as our main sample, selecting tradeline information quarterly from the June 2010 through December 2016 waves of the CCP, and we link these data to credit score and geographic information. Here, since collections records can remain on a credit record for up to seven years, some of our data goes back as far as June 2003. We select tradelines with a kind-of-business code indicating the creditor is a debt collector or debt buyer. Our unit of observation is a tradeline, which is associated with a particular account opening date. That is, as with our credit card data discussed above, we keep only one observation per tradeline, using the most recent information available, even though the tradeline may be reported in every CCP wave. We note, however, that many of these tradelines are duplicates of the same debt, but held by a different collector. Thus, our unit of observation for the analysis of collection outcomes can more precisely be thought of as a collector-debt pair.

4.2 Credit Card Database (CCDB) Data

The CCDB is a compilation of de-identified loan-level information from large banks' credit card portfolios. Overall these data cover between 85% and 90% of credit card industry balances. The data are updated monthly, running from January of 2008 through February of 2017. Although 29 large banks are represented in the dataset, not every bank is represented in every month, as the sourcing of the data has changed over time, which has changed the set of banks available. The full dataset contains a monthly panel for each account, documenting payments, fees and balances in each billing cycle, but for purposes of this paper we only use the first period the account is opened, and only use accounts where we observe the opening during our sample. For computational reasons, we use only a 10% random sample of the full CCDB. Even limiting our sample to initial observations of accounts opened during our sample period, a 10% sample still gives us more than 60 million new accounts. Unlike the CCP, the CCDB does not link accounts owned by the same individuals.

The CCDB data report the initial credit limit, the consumer's credit score when the account was opened, the annual percentage rate (APR) for the first month the account was opened, and the state and ZIP code of the consumers' residence. In addition, there is some limited information about the type of card, including the purpose of the card and whether the card was branded (e.g., a card for a specific retail store, or a gas card). We can identify business and corporate cards, and we drop these from our analysis dataset. We link consumer ZIP codes to Census tract level data from the 2009–2014 ACS using the Census Bureau's crosswalk between tracts and Zip-Code Tabulation Areas (ZCTAs). Where a ZCTA overlaps more than one tract, we average the demographic characteristics of the component tracts, weighting by the share of the ZCTA population in each tract.

Table 2 shows summary statistics for our CCDB sample. We observe that about 18 percent of all new accounts have a zero APR reported for the first month. Presumably these correspond to introductory rates. We observe a somewhat lower average credit limit for new accounts in the CCDB compared to the CCP. This may be a compositional effect, due to the different mix of banks involved.[17] We note, however, that the ZIP code demographics match almost exactly between the two datasets, so we are not looking at fundamentally different sets of consumers. The average non-zero APR is 21.6%, with a standard deviation of about 5 percentage points. Likely this includes some non-zero introductory rates as well.

---

[17]It is also possible that some of what appear to be new accounts in the CCP are actually duplicates of older accounts, created when a lost or stolen card is re-issued with a new account number.

5 Results

Restrictions on debt collection may raise costs or reduce revenues for firms engaged in collection activities, which in turn may reduce lender returns to extending credit and thereby induce lenders to restrict credit access or raise interest rates. We now test this proposition with a difference-in-difference analysis of the recent state laws and regulations requiring more stringent consumer protections in the debt collection industry. In this section, we first present our main results on access to credit, using credit card inquiries and account information from the CCP. Next, we present results on credit pricing from the CCDB. Finally, we provide suggestive results on the mechanism underlying these results, using collections records from the CCP.

5.1 Difference-in-Difference Results on Access to Credit

We estimate a fixed effects difference-in-difference model for the effect of implementing a debt collection law on the probability of opening a credit card account following a hard inquiry, and the initial credit limit conditional on opening an account. Our baseline model for the effect on outcome y_{ist} is as follows:

$$ y_{istb} = \beta treat_{st} + \gamma X_{ist} + \delta_s + \alpha_t + \theta_l + f_s(t) + \varepsilon_{istb}, $$

where i indexes either accounts or inquiries, s states, t time and l lenders, with $treat_{st}$ an indicator equal to one if a debt collection law or regulation is implemented in state s and time t, X_{ist} is a vector of covariates, δ_s, α_t and θ_l are state, time and lender fixed effects respectively, and $f_s(t)$ is a flexible, state-specific function of time. The state and time fixed effects subsume the indicators for treated/control and before/after treatment, making β the difference-in-difference estimator of interest. In practice, we specify $f_s(t)$ as a nationwide set of monthly time dummies, plus a quadratic function of time, in days, interacted with a dummy for each state. We allow additional flexibility in our geographic controls by including at least county fixed effects in place of δ_s.[18] In all specifications, X_{ist} includes credit score, year of birth, and Census tract-level controls for percent black, percent Hispanic, percent with less than high school education and median income. We do not control for bankruptcy exemption levels but our results are robust to their inclusion.[19]

---

[18]This also makes $treat_{st}$ a proper estimator of the difference-in-difference estimator of the treatment effect given the existence of the New York City debt collection regulations. New York City occupies the entirety of five counties in New York state.

[19]We thank Neil Pattison for providing us with bankruptcy exemption data for 2008-2015. We included a variable based on these data in test regressions, but the coefficient was statistically insignificant and it had no impact on our treatment effect estimates so we removed it from our final results.

There are a several thousand unique, anonymous bank identifiers in the CCP, and it would not be feasible to include fixed effects for all of them in addition to the county fixed effects, as the fixed effects estimator can only efficiently difference out one set of fixed effects. Instead, we assume that only the 29 banks with the largest number of open accounts in our sample have unique fixed effects, with a separate effect for all other banks. We allow for 29 unique bank fixed effects to match our results in the next subsection using the CCDB, where we have exactly 29 large issuers, and include fixed effects for all of them.

We begin by presenting results on consumers at all credit score levels. These results are shown in table 3. The first three columns show results on inquiry success while the second three show results for the initial credit limit conditional on opening an account. Columns (1) and (4) report results using only county and time fixed effects. The coefficients on the treatment indicator have the wrong sign relative to the theoretical prediction—success rates and credit limits increase following a debt collection law or regulation. Columns (2) and (5) add state-specific quadratic time trends to account for any raw differences in the underlying trends in credit access between the treated and control states. This is our preferred specification, and produces the expected sign for the treatment indicator for both outcome variables, indicating that the debt collection restrictions reduce access to credit, although for both outcomes the coefficients are small, statistically insignificant and precisely enough estimated to reject a moderate sized effect. Using the coefficient on credit score to scale our results, our point estimates of the average effect of the state debt collection restrictions on the probability of a successful inquiry and the initial credit limit conditional on success is equivalent to lowering consumers' credit score by less than 1 point. For inquiry success, our estimate is precise enough that we can reject a change in success rates equivalent to a 3 point change in credit scores, although our credit limit estimate is noisier, such that we cannot reject a change in credit limits on the order of $100, equivalent to a 5-6 point change in credit score.

Finally, as a robustness check, columns (3) and (6) of table 3 report results adding in individual fixed effects in addition to the state-specific trends. If our specification is correct, these should not dramatically affect our results—creditors make decisions to extend credit or not based entirely on information contained in the credit report, which we observe. As such, there should not be substantial unobserved heterogeneity for individual fixed effects to control for.[20] Indeed our results show that adding individual fixed effects

---

[20]In addition, we note that in our particular setting, the sample selection forced by using individual fixed effects is not desirable. By construction, a model with individual fixed effects will only use variation from consumers who happened to seek or open more than one credit card during the sample period, a relatively unusual occurrence. In addition, the treatment indicator will be identified only by consumers who happen to seek or open at least one card both before and after a debt collection law was implemented.

has relatively little effect on our results above and beyond state-specific quadratic trends. Our difference-in-difference estimate of the effect of the debt collection restrictions on inquiry switches sign and the effect on initial credit limits decreases, but we cannot reject that they are equal to our estimates in the models with state-specific trends and without individual fixed effects. We omit individual fixed effects in what follows.

Since debt collection restrictions are only relevant to creditors to the extent that accounts default and require collection activity, we expect that we may find different results for consumers who are considered more or less likely to default. To examine this, we re-estimate our preferred specification separately for consumers with credit scores at or above 660 (prime borrowers) and those below this threshold (sub-prime borrowers). These results are shown in table 4. Columns (1) and (2) show results on the probability that an inquiry results in an open account, for prime and sub-prime borrowers respectively. For prime borrowers we observe a statistically insignificant positive coefficient, although the magnitude is still quite small and our standard errors are fairly tight. Our estimated effect on sub-prime borrowers is negative, but still not statistically different from zero. Columns (3) and (4), show results on initial credit limits. Here we find a statistically significant decrease in credit limits for sub-prime borrowers, although still small at $46. Compared to a change in credit score, our effect is of an equivalent magnitude to a 8 point reduction in credit scores for sub-prime borrowers, although consistent with figure 1, a one point change in credit score affects credit limits for sub-prime borrowers much less than prime borrowers. Our point estimate for the effect on credit limits for prime borrowers is larger in dollar terms, but much smaller compared to the stronger effect of credit score on credit limits for prime borrowers.

Thus far we have been treating the four debt collection restrictions imposed during our sample period as a homogeneous treatment with a single treatment effect. In table 5 we relax this assumption, allowing a separate treatment indicator for each of the four laws. The first three columns show results for inquiry success, for all consumers, prime borrowers and sub-prime borrowers, respectively, while the second three columns show results for initial credit limits for the same sets of consumers. Generally these split results are consistent with our results in tables 3 and 4. For California and North Carolina's laws we find statistically significant negative effects on both inquiry success and credit limit for sub-prime borrowers, with a magnitude equivalent to a 5-10 point change in credit score. We note that our estimates are sufficiently noisy that we cannot reject the null hypothesis that the California and North Carolina effects are equal to each other, and to the global effects in tables 3 and 4. In contrast, the New York state regulation seems to have primarily affected prime borrowers, with an increase in inquiry success accompanied

by a decrease in credit limits, but effects that are statistically and economically zero for sub-prime borrowers. The New York City Regulation seems to be an outlier, with inquiry success and credit limits increasing for both prime and sub-prime borrowers. It is not clear whether this unusual result due to the New York City regulation being local rather than state-wide, or due to the sample for New York City being relatively small.

We note that the differences across states may reflect differences in lender behavior, rather than true differences in the effects of the various debt collection restrictions. To test this, we next allow our treatment effect to vary by bank. Many of the issuers in our CCP sample have only a small number of open accounts and inquiries. To focus on cases where we have enough statistical power to make reliable estimates, we estimate our model with interactions between our treatment indicator and issuer dummies. As with our bank fixed effects, we allow separate effects for the 29 largest banks and a pooled effect for all other banks. We plot the coefficients for each issuer in figure 2, with the effect on inquiry success on the Y-axis, and the effect on initial credit limit on the X-axis.[21] The CCP's terms of use do not allow us to provide information by issuer in a way that could be used to identify individual banks, but we can use a separate symbol to distinguish the 10 largest banks from the remaining large issuers in our sample. Panel (a) shows coefficients from regressions on all borrowers, panel (b) shows coefficients from regressions on prime borrowers only, and panel (c) shows results for sub-prime borrowers. The figure shows a significant amount of heterogeneity across banks, with each bank seemingly choosing a different approach to dealing with restrictions on debt collection in the treated states; this is consistent with our theory, which indicates that lenders may have different equilibrium responses to these restrictions. Although our point estimates suggest that the majority of issuers reduce both inquiry success and initial credit limits, several increase one variable while reducing another, and a small number actually increase access to credit along both dimensions. We find more banks, and particularly more of the larger banks, reducing credit along both dimensions for sub-prime borrowers, compared with prime borrowers, consistent with our pooled regressions results. Altogether, these findings suggest that the small average effects we find reflect somewhat larger opposing responses by firms that end up offsetting each other.

Finally, we estimate our model as a distributed-lags event study, allowing the treatment effect to vary with time before and after the implementation of the debt collec-

---

[21]Some coefficients are omitted from the figure—we do not report the coefficient for all other banks, and 4 issuers' interactions could not be estimated in the inquiry success model, because they apparently did not report inquiries to the particular nationwide credit reporting agency that supplies the CCP. We also omit points for two relatively small issuers that were outliers on the inquiry success dimension, in order to make the plot readable. Although large, neither coefficient was statistically different from zero.

tion laws. This serves both as a test of the parallel trends assumption underlying our difference-in-difference model, as well as shedding light on whether our estimated effects are persistent. Specifically, we modify our estimating equation above to:

$$y_{ist} = \sum_{d=-24}^{24} \beta^d D_{st}^d + \gamma X_{ist} + \delta_s + \alpha_t + f_s(t) + \varepsilon_{ist},$$

where D^d is an indicator equal to one if a debt collection law or regulation was implemented d months ago.[22] We plot the β^d coefficients in figure 3, focusing on the sub-prime borrower, where we find statistically significant average treatment effects. Panel (a) shows event study results for inquiry success, and panel (b) shows results on initial credit limit. In both plots we see no evidence of a confounding pre-trend for a year or more prior to treatment, although there is possibly a trend before that, and we observe substantial noise in the pre-period. For inquiry success by sub-prime borrowers, we observe no change around the imposition of debt collection restrictions, consistent with our small, statistically insignificant coefficients in table 5. For credit limits, we observe a small, discrete drop in credit limits, which seems to persist for at least a year and half post-treatment, after which there may be some recovery. However, there is considerable noise that far post event, when we are identifying primarily off of the earlier restrictions in North Carolina and New York City, and so we are reluctant to interpret this as strong evidence of a dynamic response over time.

5.2 Difference-in-Difference Results on Credit Pricing

Using our CCDB data, we estimate a very similar model as in our credit access results above. We employ ZIP code fixed effects rather than county fixed effects, and add controls for the type of card. Because we have a limited set of banks in these data, it is feasible to include a bank fixed effect for each bank, rather than having an aggregate fixed effect for smaller issuers. We estimate the effect of recent debt collection restrictions on initial credit limits and initial APRs. Because almost a fifth of new accounts in our data have an initial APR of zero, we estimate interest rate effects in three ways. We first estimate effects on all interest rates, including zeros. We then estimate effects on the level of interest rates

---

[22]We include all observations in these regressions—the indicators for 24 months before and after treatment include observations outside our event window. Note also that we allow more than one indicator to be equal to one at a time for residents of New York City, as these consumers are treated twice, once by the NYC Regulation, and later by the New York State regulation. Despite the distance between these events, ignoring one or the other event can create spurious trends in the event study (Sandler and Sandler, 2014). Allowing more than one event dummy to be equal to one makes the set of indicators not mutually exclusive, and as such an omitted category is not needed, or desirable.

excluding all accounts with a zero rate. Finally, we estimate a linear probability model with an indicator for a zero rate as the outcome. We expect that we may see different results across these formulations—creditors could respond to debt collection restrictions and the potential reduction in recovery by changing rates, or by changing the availability of low introductory rates.

We show results for all consumers in table 6. The first column shows results on credit limits. We find that credit limits are reduced by an average of $123 following a restriction on debt collection practices. Although this is somewhat larger than the average effect we find in the CCP, we cannot reject equality. In any event, a change of this size is of a similar magnitude as a 5 point change in credit score, and thus is quite small. The next three columns of table 6 show results for all interest rates, non-zero interest rates, and the indicator for a zero percent rate. For the full sample we find essentially zero effect on APRs. Our coefficients are the wrong sign, showing a decrease in APRs, but the magnitudes are miniscule, and our standard errors are sufficiently tight to reject a 0.05 percentage point increase in APR. Unlike for inquiry success and credit limits, the relationship between initial credit card interest rates and credit scores is not monotonic across the distribution of credit scores (see appendix Figure A1). To account for this we control for 20-point bins of credit score in these regressions. Unfortunately, this means we lose the convenient interpretation of our treatment effect as equivalent to a change in credit score.

Tables 7 and 8 show results for sub-prime and prime borrowers, respectively, in the same layout as table 6. For sub-prime borrowers, as in the CCP we get a small, precisely estimated effect of debt collection restrictions on credit limits. We find a moderate increase in APRs, although all of this comes through a reduction in introductory zero APR offers. Our estimate for the effect on non-zero APR's is wrong-signed and very small in magnitude. Likely the negative coefficient here is a composition effect due to a shift from zero introductory rates to small positive introductory rates. For prime borrowers we observe a small effect on credit limits, again larger than our estimate using the CCP but again sufficiently noisy that we cannot reject equality. We do not observe any reliable evidence of an increase in interest rates for prime borrowers. If anything rates may decline slightly, although there is significant noise in these estimates.

Returning to all borrowers, we next present results with separate effects for the five restrictions we study, shown in table 9. As in the CCP we observe a degree of heterogeneity. We observe a decreased rate of zero-APR cards in California, while the New York City Regulation seems to have caused a substantial increase in non-zero rates, accompanied by an increase in zero-APR offers. The increase in APRs may explain our result in table

5 where we observe creditors increasing access and credit limits in response to the New York City restriction. It may be that creditors serving New York City chose to respond primarily through interest rates, but compensate with higher limits to avoid losing too much market share.

Finally, we break out results by bank in figure 4, which shows a scatter plot of by-issuer effects on probability zero APR and credit limits, similar to figure 2 but with a different Y-axis. Again we are limited in the information we can provide about each bank, to avoid identifying any particular issuer, but again we are able to separately identify the 10 largest issuers in the sample from the smaller issuers.[23] We note that these may not be the same 29 banks as in our CCP analysis, as the CCDB includes 29 particular issuers, which may not be the same as the 29 issuers with the most new accounts in the CCDB. As in figure 2, the three panels of figure 4 show results for all borrowers, prime borrowers, and sub-prime borrowers, respectively. We observe a significant amount of heterogeneity across banks in response to debt collection restrictions. Similar to our results from the CCP in figure 2, we observe that while many banks reduce credit limits and zero-APR offers, some banks reduce one while increasing the other. A small number of banks increase both, although most of these are smaller issuers, such that this result might simply be the result of statistical noise stemming from the relatively small samples for these issuers. Interestingly, our results on sub-prime borrowers in panel (c) show a variety of effects on credit limits, centered around zero, while almost all banks reduced the rate of initial zero APR cards to sub-prime consumers.

5.3 Effects of Debt Collection Restrictions on Collection Outcomes

Our results in the previous two subsections indicate that creditors respond to restrictions on debt collection by reducing, slightly, the availability of credit to consumers. Theory suggests that restrictions on debt collection should have this effect to the extent that collection becomes more difficult or more expensive. This would reduce the expected value of defaulted debt, and thus the expected value of the initial extension of credit. We next examine whether there is evidence that the state restrictions on debt collection operate through this mechanism, both as an extension of our previous results and a test of their validity.

We test the effect of state debt collection restrictions on collection activity using

---

[23]Again we omit one or two banks from each plot, depending on the sub-sample, to avoid having noisy outliers distort the scale. Unlike in the CCP, we were able to estimate both coefficients for all 29 banks, and so no other issuers are excluded.

collections tradelines in the CCP. As noted in Section 4, we stress that these data are an imperfect measure of collection activity, as not all collectors furnish information to the credit bureau, and they likely do so inconsistently. At the same time, although the data is less representative, we are able to use additional variation to help identify the causal impact of the state laws and regulations. The collection tradeline information in the CCP identifies whether the firm in question is a third part collector or a debt buyer. This is important, because the California and North Carolina laws only pertain to debt buyers, not third party collectors. Thus, we can exploit within-state variation in those two states, comparing outcomes for accounts held by third party collectors to those owned by debt buyers. Specifically, we estimate:

$$y_{ist} = \beta treat_{stb} + \gamma X_{ist} + \delta_s + \phi_{sb} + \alpha_t + \mu_{tb} + f_s(t) + \varepsilon_{ist},$$

where where b indexes firm type, collector or debt buyer, and $treat_{stb}$ is equal to one if a collections tradeline opened in time t, and state s by a firm of type b was subject to a debt collection restrictions. This means that we mark an observation as treated if it is held by a debt buyer in California or North Carolina after the laws were passed in those states, or any collection in Arkansas, New York state or New York City after those regulations were passed. By including interactions between firm and state and time fixed effects, we essentially create a triple difference model, modified only in that all post-treatment New York observations have the three-way interaction $treat_{stf}$ turned on.

We examine two collections outcomes: disputes and payments. The FDCPA requires debt collectors to respond to consumer disputes and cease collections until they have resolved the dispute. Disputes are costly for collectors to handle, requiring staff time to process, and delaying collection cases where disputes are ultimately deemed not valid. Some collectors abandon collections entirely when a dispute has been filed. The CCP includes a flag for disputes, and as our outcome we examine whether any dispute flag occurs on a collections account.

For payments, we create a single indicator for any evidence of payment to the collector while they reported a given tradeline. The CCP has several variables that could indicate payment, including a field for date of last payment, a field indicating a payment amount for the current period, and flags indicating an account was paid in full. Each of these fields individually has numerous inconsistencies in our sample of collection accounts. For our outcome variable, we create an indicator equal to one if there is any indication of payment on a tradeline.

Our results on disputes and payments are reported in table 10. Each cell reports

the coefficient on the treatment indicator from a separate regression. Results in the first column use the same controls as in our preferred specification from our analysis of access to credit, with county and month fixed effects, Census tract demographics and state-specific quadratic trends. Results in the second column add in consumer fixed effects. Unlike in our access to credit results, here we are interested in consumer choices rather than creditor choices, and so unobserved heterogeneity at the individual level is relevant and possibly important.

Focusing on our results with individual consumer fixed effects we find that the state debt collection restrictions caused a small uptick in consumer disputes filed with the credit bureau, with a point estimate of 0.18 percentage points relative to a mean of about 3 percentage points. Our estimate is not statistically different from zero, but we cannot reject a moderate effect here—a 95% confidence interval includes changes of more than 10% of the mean. We speculate that consumers are either more likely to dispute due to the disclosures informing them of their rights, or perhaps the disclosures provide consumers enough information that they respond to a debt rather than ignore it. To the extent that our point estimate captures a real effect, this small uptick in disputes would very likely raise costs for affected debt buyers and debt collectors, which is consistent with our results on access to credit.

Turning to the final row of table 10, we do not find any evidence that consumers were less likely to pay their debts in collection as a result of the debt collection restrictions. Approximately 11% of all collection tradelines have some evidence of payment, while we can reject a decrease of even 0.4 percentage points. Thus, it would appear that the effect on access to credit stems from increased compliance costs by debt collectors, rather than a decline in recoveries. However, in addition to the caveats about the quality of the collections data in the CCP, we note that credit record disputes are not necessarily the same as disputes with a collector—disputes made directly to a collector may not be furnished to the credit bureau, and the dispute itself may be due to an error in the credit report, rather than a problem with the underlying collection account.

6   Conclusions

We study the impact of tightening state debt collection laws and rules in four states and the City of New York on the availability, price and quantity of credit. With the exception of Arkansas, these restrictions contained new requirements that concentrated on establishing standards for what constitutes unfair or deceptive acts or practices; some of the acts require additional documentation to substantiate debts before collections can begin,

while others require disclosures before filing suit or to inform consumers that the statutes of limitations for filing suit has passed. The Arkansas law maintained the restrictions contained in Federal law, but imposed stiff penalties for violations.

We find that these requirements reduced access to credit and credit limits on average and increased interest rates or reduced the prevalence of zero APR introductory rates, but that the effects are very small in magnitude; the effect on consumers of these additional requirements is equivalent to a reduction in consumer credit scores of 5 points or less. Running separate regressions for consumers with sub-prime and prime credit scores we find that effects on credit access, credit limits and zero APR introductory rates are concentrated on sub-prime consumers. Since the statutes differ in approach and reach, the California and North Carolina laws limit attention to debt buyers, we allow for separate treatment effects for each state. In addition, as our theory indicates, different lenders will likely have different equilibrium responses to the new requirements, so we also allow for lender specific treatment effects in a different set of regressions. Our results show response heterogeneity in both the state and lender treatment effects though the effects remain small in magnitude.

References

Cerqueiro, Geraldo, Deepak Hegde, Maria Fabiana Penas, and Robert Seamans, "Debtor rights, credit supply, and innovation," *Management Science*, 2016.

Consumer Financial Protection Bureau, "Consumer Experiences with Debt Collection: Findings from the CFPB's Survey of Consumer Views on Debt," Report January 2017.

Dagher, Jihad and Yangfan Sun, "Borrower protection and the supply of credit: Evidence from foreclosure laws," 2014. IMF Working Paper WP/14/212.

Dávila, Eduardo, "Using elasticities to derive optimal bankruptcy exemptions," 2016. European Systemic Risk Board Working Paper No 26.

Dawsey, Amanda E and Lawrence M Ausubel, "Informal bankruptcy," 2004. Working Paper.

_ , Richard M Hynes, and Lawrence M Ausubel, "Non-judicial debt collection and the consumer's choice among repayment, bankruptcy and informal bankruptcy," *American Bankruptcy Law Journal*, 2013, *87*, 1–26.

Fedaseyeu, Viktar, "Debt collection agencies and the supply of consumer credit," 2015. Federal Reserve Bank of Philadelphia Working Paper NO. 15-23.

Federal Reserve Bank of New York, "Quarterly Report on Household Debt and Credit," Research and Statistics Group May 2017.

Fonseca, Julia, Katherine Strair, and Basit Zafar, "Access to credit and financial health: Evaluating the impact of debt collection," 2017. Federal Reserve Bank of New York Staff Report No.814.

Gropp, Reint, John Karl Scholz, and Michelle J White, "Personal bankruptcy and credit supply and demand," *The Quarterly Journal of Economics*, 1997, *112* (1), 217–251.

Hynes, Richard and Eric A Posner, "The law and economics of consumer finance," *American Law and Economics Review*, 2002, *4* (1), 168–207.

Livshits, Igor, "Recent developments in consumer credit and default literature," *Journal of Economic Surveys*, 2015, *29* (4), 594–613.

Pattison, Nathaniel, "Consumption smoothing and debtor protections," 2016. *mimeo* Department of Economics, University of Virginia.

Pence, Karen M., "Foreclosing on opportunity: State laws and mortgage credit," *Review of Economics and Statistics*, 2006, *88* (1), 177–182.

Sandler, Danielle H. and Ryan Sandler, "Multiple event studies in public finance and labor economics: A simulation study with applications," *Journal of Economic and Social Measurement*, 2014, *39* (1), 31–57.

White, Michelle J, "Bankruptcy and credit cards," *Journal of Economic Perspectives*, 2007, *21* (4), 175–200.

Table 1: Summary Statistics on Credit Card Inquiries and New Accounts in the CCP

	Mean	SD	Min	Max	N
Inquiry Success Rate	0.44	0.50	0	1.00	7,291,797
Credit Limit of Opened Accounts	5309	5919	100	75000	10,364,743
Credit Score	683.7	105.8	300	839	17,377,531
Year of Birth	1967	16	1888	1998	17,465,118
Census Trace % Black	11.41	18.73	0	100.00	17,571,467
Census Tract % Hispanic	17.65	21.93	0	100.00	17,571,467
Census Tract % HS Dropout	13.07	10.81	0	100.00	17,571,396
Census Tract Median Income ($)	31246	12227	2501	131362	17,569,841

Table 2: Summary Statistics on New Credit Card Accounts in the CCDB

	Mean	SD	Min	Max	N
Initial Credit Limit	4415	5031	200	75000	57,727,132
Initial APR	17.67	9.63	-99	31.24	51,044,718
Proportion with Zero Initial APR	0.183	0.387	0	1	51,044,718
APR (non-zero only)	21.64	5.27	0	31.24	41,689,449
Self-Reported Borrower Income	71299	78778	0	580000	55,323,303
Credit Score	724.4	74.5	250	900.0	55,272,764
Proportion Secured Cards	0.0415	0.199	0	1	59,368,659
Census Tract % Black	11.08	16.11	0	98.03	57,967,754
Census Tract % Hispanic	17.11	20.20	0	99.82	57,967,754
Census Tract % HS Dropout	12.92	9.26	0	71.81	57,967,754
Census Tract Median Income ($)	31223	10559	2520	113480	57,966,970

Table 3: Effect of Debt Collection Restrictions on Access To Credit: Differences-in-Differences with Common Treatment Effect

	Inquiry Success			Initial Credit Limit		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Indicator	-0.00640	-0.00121	0.00152	246.3*	-67.77	-50.73*
	(0.00731)	(0.00236)	(0.00107)	(130.1)	(60.70)	(30.14)
Credit Score	0.00178***	0.00178***	0.000262***	17.17***	17.17***	0.543***
	(0.0000194)	(0.0000195)	(0.0000183)	(0.207)	(0.205)	(0.108)
Year of Birth	-0.000295***	-0.000292***		-31.04***	-31.06***	
	(0.0000706)	(0.0000707)		(1.280)	(1.282)	
Census Tract Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	No	Yes	Yes	No
Lender Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic Trends by State	No	Yes	Yes	No	Yes	Yes
Individual Fixed Effects	No	No	Yes	No	No	Yes
N	6687037	6687037	6703672	9619554	9619554	9762132

Standard errors clustered at the state level reported in parentheses. Month fixed effects are based on the month when the inquiry was made for the first three columns, and the month the account was opened for the second three columns. An inquiry is successful if a credit card account is opened within 14 days of the date of the inquiry.

Table 4: Effect of Debt Collection Restrictions on Access To Credit: Differences-in-Differences by Credit Score Group

	Inquiry Success		Initial Credit Limit	
	(1) Prime	(2) Sub-Prime	(3) Prime	(4) Sub-Prime
Treatment Indicator	0.00393 (0.00241)	-0.00384 (0.00248)	-89.02 (80.32)	-46.76*** (17.04)
Credit Score	0.00205*** (0.0000600)	0.00121*** (0.00000966)	26.64*** (0.390)	5.577*** (0.194)
Year of Birth	0.000646*** (0.0000442)	-0.000793*** (0.000137)	-26.88*** (1.324)	-22.33*** (0.840)
Census Tract Demographics	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Lender Fixed Effects	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes
N	3046450	3640587	6779422	2840132

Standard errors clustered at the state level reported in parentheses. Month fixed effects are based on the month when the inquiry was made for the first three columns, and the month the account was opened for the second three columns. An inquiry is successful if a credit card account is opened within 14 days of the date of the inquiry. A consumer is sub-prime if their vantage score is below 660 at the time of the inquiry or account opening.

Table 5: Effect of Debt Collection Restrictions on Access To Credit: Differences-in-Differences with Separate Treatment Effects

	Inquiry Success			Initial Credit Limit		
	(1) All	(2) Prime	(3) Sub-Prime	(4) All	(5) Prime	(6) Sub-Prime
CA Debt Buyer Law	-0.00323**	0.00298	-0.00719***	-165.7***	-212.7***	-31.66***
	(0.00135)	(0.00220)	(0.00138)	(20.55)	(29.34)	(11.69)
NY Debt Collection Reg.	0.0103***	0.0208***	-0.00153	-48.15	-82.12*	12.78
	(0.00290)	(0.00466)	(0.00195)	(35.60)	(45.44)	(17.15)
NC CEP ACT	-0.00417	0.0141	-0.0108*	-168.5***	-159.7***	-177.3***
	(0.00718)	(0.00898)	(0.00567)	(46.74)	(57.51)	(20.40)
AR FDCPA	-0.00341	-0.00294	-0.00164	119.4**	70.49	43.54*
	(0.00616)	(0.00848)	(0.00410)	(50.74)	(62.15)	(23.30)
NYC Regulation	0.0155***	0.0296***	0.00194	-1.134	30.14	58.09**
	(0.00349)	(0.00610)	(0.00225)	(132.8)	(149.3)	(26.64)
Credit Score	0.00178***	0.00205***	0.00121***	17.17***	26.64***	5.577***
	(0.0000195)	(0.0000600)	(0.0000000966)	(0.205)	(0.389)	(0.194)
Year of Birth	-0.000292***	0.000646***	-0.000793***	-31.06***	-26.88***	-22.33***
	(0.0000707)	(0.0000442)	(0.000137)	(1.281)	(1.323)	(0.840)
Census Tract Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Lender Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes	Yes	Yes
N	6687037	3046450	3640587	9619554	6779422	2840132

Standard errors clustered at the state level reported in parentheses. Month fixed effects are based on the month when the inquiry was made for the first three columns, and the month the account was opened for the second three columns. An inquiry is successful if a credit card account is opened within 14 days of the date of the inquiry.

Table 6: All Consumers: Effect of Debt Collection Restrictions on Credit Limits and Pricing in the CCDB

| | Credit Limit | Initial APR | | P(Zero APR) |
		All	Non-Zero	
Treatment Indicator	-122.6**	-0.0480	-0.0417	-0.0000308
	(58.64)	(0.0518)	(0.0634)	(0.00452)
Credit Score	27.70***			
	(0.343)			
Annual Income (000s)	12.39***	0.00175***	-0.000382***	-0.000105***
	(0.416)	(0.000194)	(0.0000914)	(0.00000985)
Secured Account	-624.1***	5.852***	0.186	-0.256***
	(94.62)	(0.371)	(0.240)	(0.0190)
Card Type Fixed Effects	Yes	Yes	Yes	Yes
Census Tract Demographics	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
ZIP Code Fixed Effects	Yes	Yes	Yes	Yes
Lender Fixed Effects	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes
Credit Score Bin Fixed Effects	No	Yes	Yes	Yes
N	50341486	43623339	35045878	43623339

Standard errors clustered at the state level reported in parentheses. The unit of observation is a new credit card account.

Table 7: Sub-Prime Borrowers: Effect of Debt Collection Restrictions on Credit Limits and Pricing in the CCDB

| | Credit Limit | Initial APR | | P(Zero APR) |
		All	Non-Zero	
Treatment Indicator	-14.83	0.195***	-0.103**	-0.0135***
	(9.086)	(0.0397)	(0.0409)	(0.00279)
Credit Score	5.722***			
	(0.139)			
Annual Income (000s)	2.508***	0.00293***	0.00104***	-0.0000845***
	(0.0704)	(0.000203)	(0.0000775)	(0.00000699)
Secured Account	-833.3***	2.807***	-1.895***	-0.205***
	(36.88)	(0.290)	(0.147)	(0.0132)
Card Type Fixed Effects	Yes	Yes	Yes	Yes
Census Tract Demographics	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
ZIP Code Fixed Effects	Yes	Yes	Yes	Yes
Lender Fixed Effects	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes
Credit Score Bin Fixed Effects	No	Yes	Yes	Yes
N	10715955	7859214	6397453	7859214

Standard errors clustered at the state level reported in parentheses. The unit of observation is a new credit card account.

Table 8: Prime Borrowers: Effect of Debt Collection Restrictions on Credit Limits and Pricing in the CCDB

| | | Initial APR | | |
	Credit Limit	All	Non-Zero	P(Zero APR)
Treatment Indicator	-136.5**	-0.129*	-0.0245	0.00412
	(67.09)	(0.0679)	(0.0641)	(0.00519)
Credit Score	32.30***			
	(0.417)			
Annual Income (000s)	13.97***	0.00147***	-0.000572***	-0.000101***
	(0.417)	(0.000177)	(0.0000849)	(0.0000102)
Secured Account	-2032.4***	3.936***	0.196	-0.156***
	(270.0)	(0.381)	(0.279)	(0.0239)
Card Type Fixed Effects	Yes	Yes	Yes	Yes
Census Tract Demographics	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
ZIP Code Fixed Effects	Yes	Yes	Yes	Yes
Lender Fixed Effects	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes
Credit Score Bin Fixed Effects	No	Yes	Yes	Yes
N	39625531	35764125	28648425	35764125

Standard errors clustered at the state level reported in parentheses. The unit of observation is a new credit card account.

Table 9: All Consumers: Effect of Debt Collection Restrictions on Credit Limits and Pricing in the CCDB

| | Credit Limit | Initial APR | | P(Zero APR) |
		All	Non-Zero	
CA Debt Buyer Law	-195.4***	0.0505	-0.109***	-0.00714***
	(23.19)	(0.0393)	(0.00961)	(0.00171)
NY Debt Collection Reg.	-161.3***	-0.598***	-0.156***	0.0191***
	(28.34)	(0.0746)	(0.0240)	(0.00233)
NC CEP ACT	93.61**	-0.152**	-0.147***	0.00333
	(36.79)	(0.0685)	(0.0366)	(0.00243)
AR FDCPA	85.21*	0.101**	0.195***	-0.00144
	(43.19)	(0.0499)	(0.0240)	(0.00195)
NYC Regulation	144.8***	0.0777	0.868***	0.0328***
	(27.02)	(0.0882)	(0.0322)	(0.00536)
Credit Score	27.70***			
	(0.343)			
Annual Income (000s)	12.39***	0.00175***	-0.000383***	-0.000105***
	(0.416)	(0.000194)	(0.0000914)	(0.00000986)
Secured Account	-624.1***	5.853***	0.187	-0.256***
	(94.62)	(0.371)	(0.240)	(0.0190)
Card Type Fixed Effects	Yes	Yes	Yes	Yes
Census Tract Demographics	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes
ZIP Code Fixed Effects	Yes	Yes	Yes	Yes
Lender Fixed Effects	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes
Credit Score Bin Fixed Effects	No	Yes	Yes	Yes
N	50341486	43623339	35045878	43623339

Standard errors clustered at the state level reported in parentheses. The unit of observation is a new credit card account.

Table 10: Triple Difference Effect of Debt Collection Restrictions on Disputes and Payments

	(1)	(2)
Dep. Variable: Any Dispute Record		
Debt Buyer × Reg Passed	0.000390	0.00180
	(0.00139)	(0.00126)
Dep. Variable: Some Payment Made to Collector		
Debt Buyer × Reg Passed	-0.00121	0.00112
	(0.00268)	(0.00227)
County Fixed Effects	Yes	No
Consumer Fixed Effects	No	Yes
Month Opened Fixed Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Census Tract Demographics	Yes	Yes
N	16174276	16387532

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$
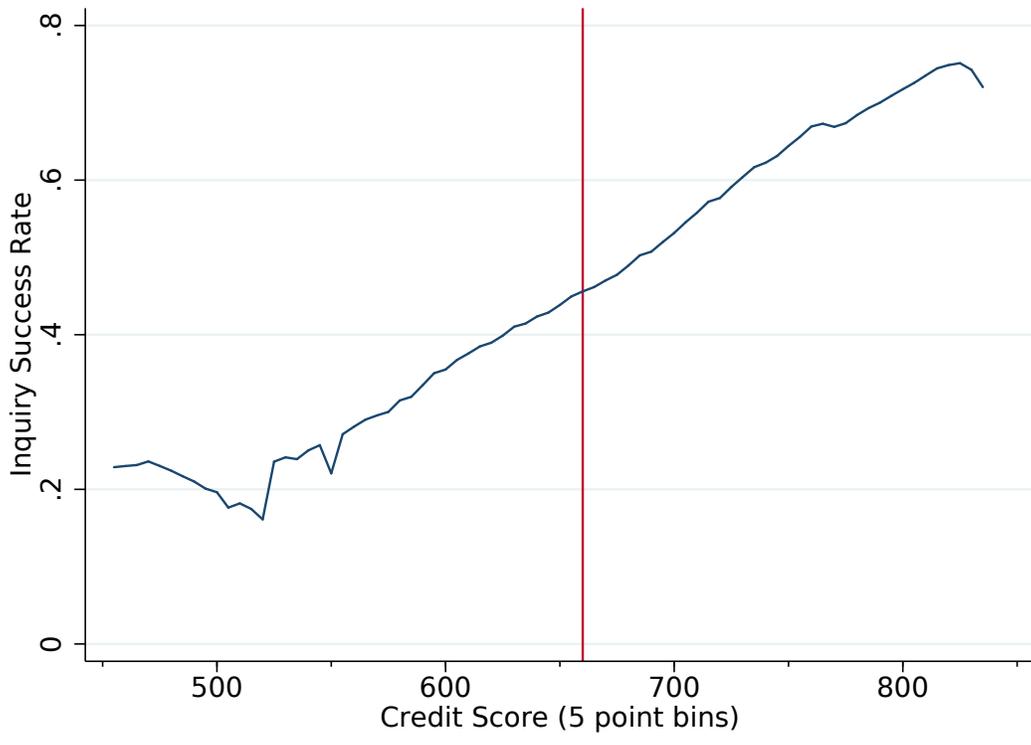
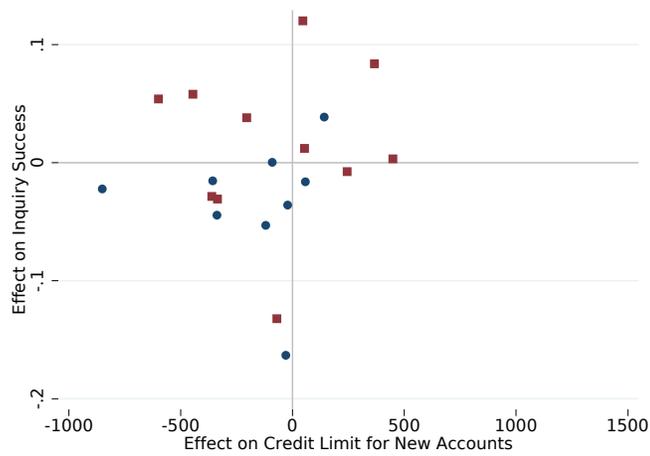(a) Inquiry Success
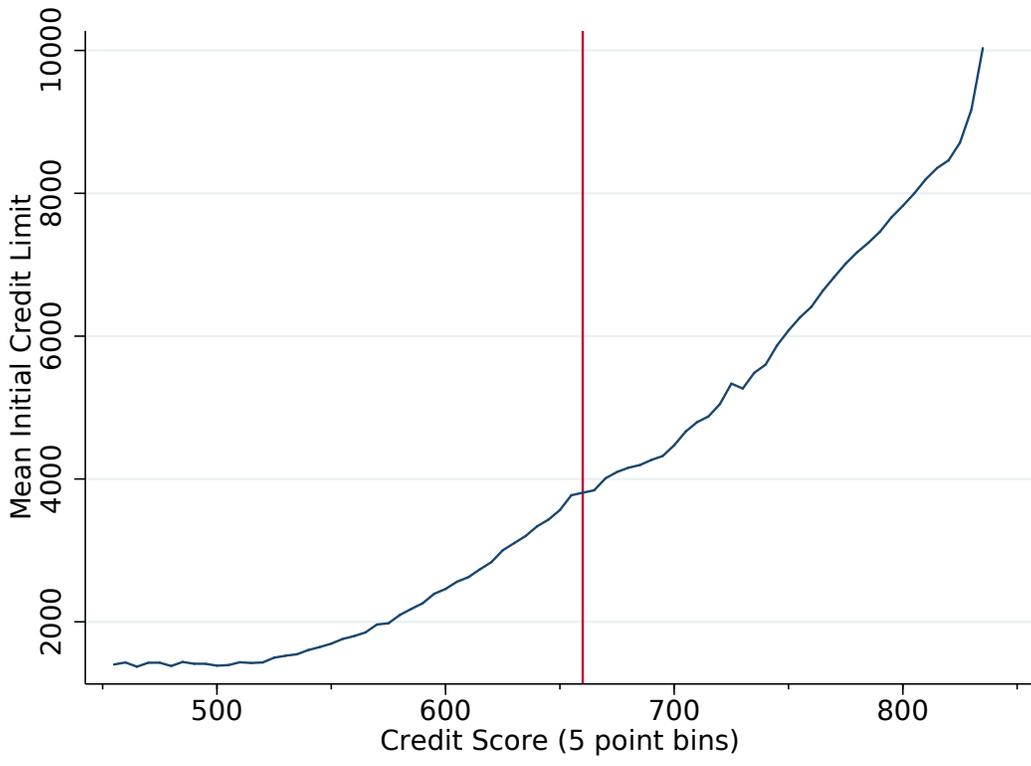
(b) Initial Credit Limit

Figure 1: Inquiry Success Rate and Initial Credit Limit by 5-point Credit Score Bins
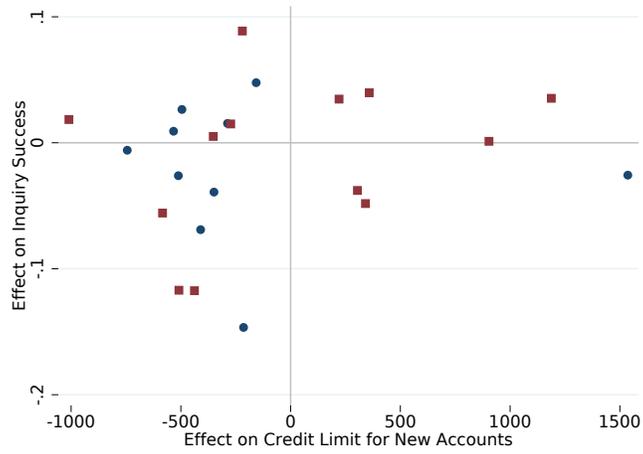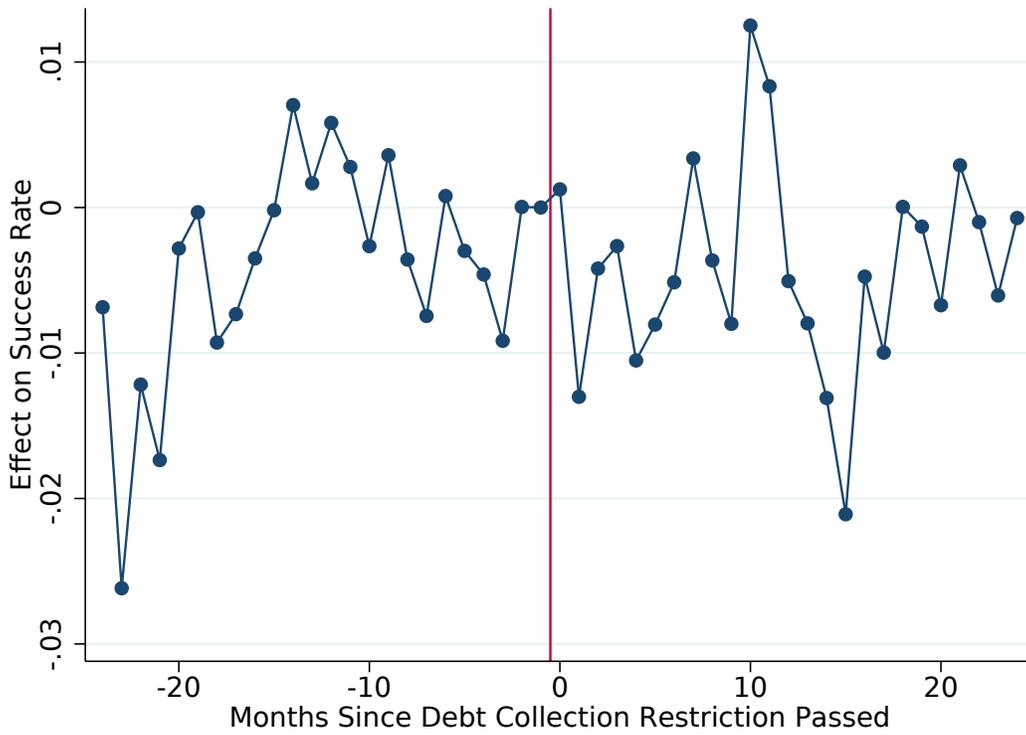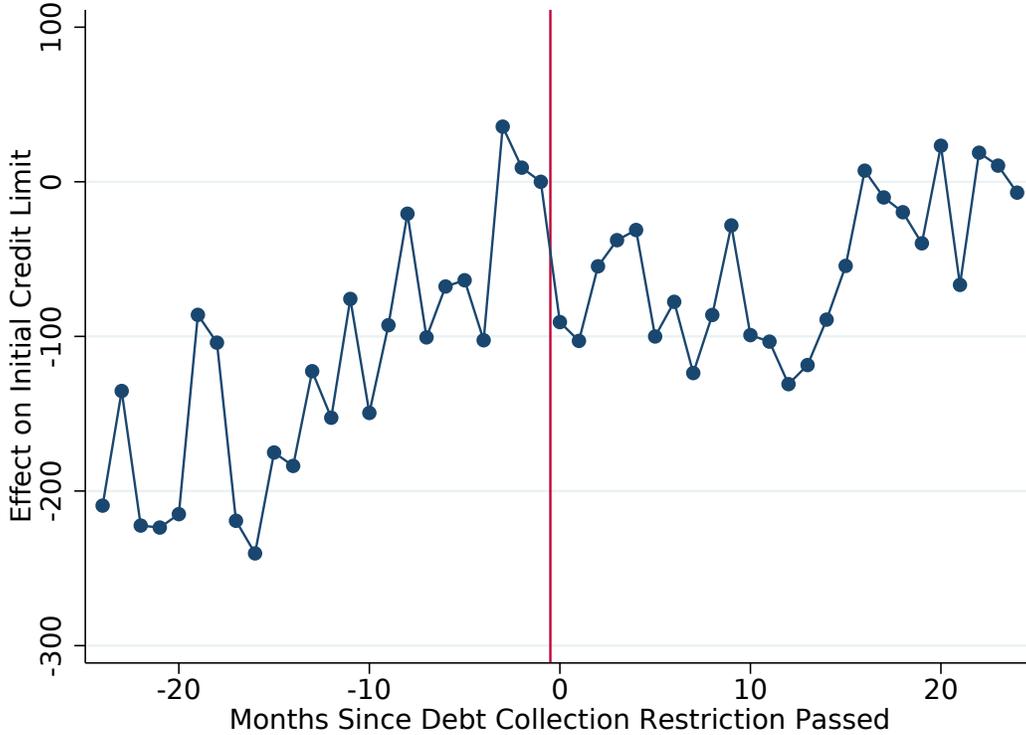
(a) All Borrowers
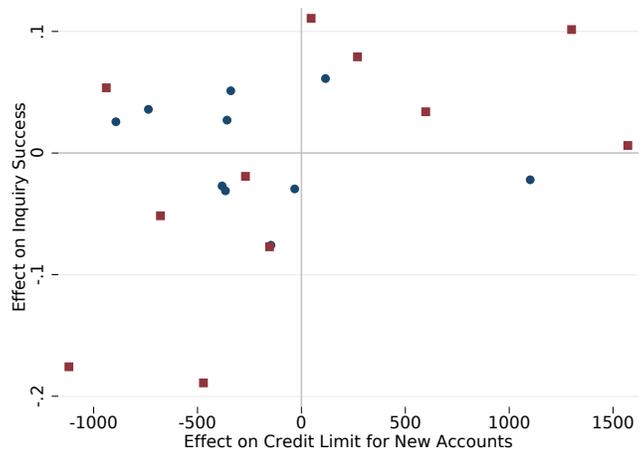
(b) Prime Borrowers

(c) Subprime Borrowers

Figure 2: Effects on Inquiry Success and Initial Credit Limit by Issuer.
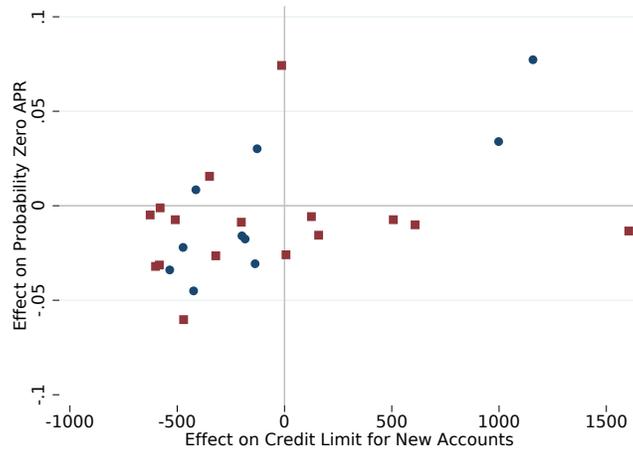Note: blue circles denote the 10 largest issuers in sample.

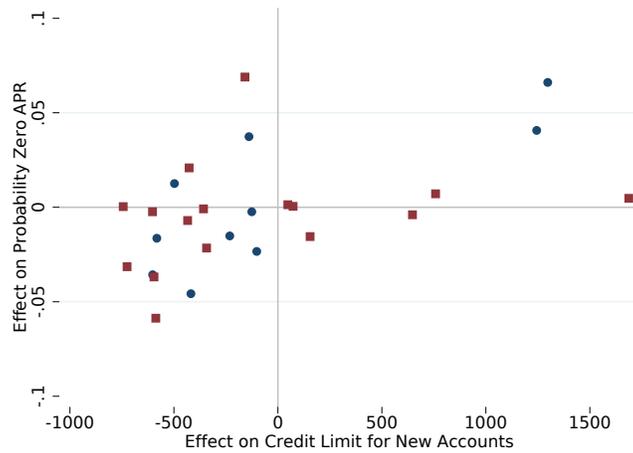(a) Inquiry Success for Sub-prime Borrowers

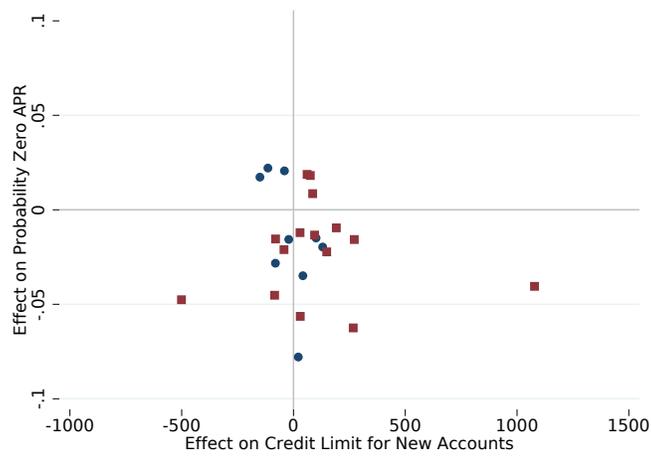(b) Initial Credit Limit for Prime Borrowers

Figure 3: Event Studies of the Effect of Debt Collection Restrictions on Access to Credit

(a) All Borrowers

(b) Prime Borrowers

(c) Subprime Borrowers

Figure 4: Effects on Probability Zero APR and Initial Credit Limit by Issuer.
Note: blue circles denote the 10 largest issuers in sample.

Appendix

## A   Additional Tables and Figures

Table A1: Effect of Debt Collection Restrictions on Access To Credit: Separate Regressions for Each Treamtent State, Sub-Prime Only

	Inquiry Success				Initial Credit Limit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CA Debt Buyer Law	-0.00725***				-27.95**			
	(0.00154)				(12.53)			
NY Debt Collection Reg.		-0.00261				3.694		
		(0.00225)				(15.14)		
NC CEP ACT			-0.0109				-174.8***	
			(0.00728)				(25.06)	
AR FDCPA				-0.000898				38.63
				(0.00515)				(26.71)
Credit Score	0.00121***	0.00120***	0.00121***	0.00120***	5.561***	5.718***	5.709***	5.714***
	(0.00000985)	(0.00000994)	(0.0000101)	(0.0000104)	(0.214)	(0.193)	(0.204)	(0.212)
Year of Birth	-0.000793***	-0.000923***	-0.000956***	-0.000950***	-22.70***	-21.65***	-21.92***	-21.97***
	(0.000153)	(0.000105)	(0.000113)	(0.000117)	(0.850)	(0.654)	(0.633)	(0.657)
Census Tract Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3254540	2964389	2779852	2679097	2552656	2364678	2231328	2154265

Standard errors clustered at the state level reported in parentheses. Month fixed effects are based on the month when the inquiry was made for the first three columns, and the month the account was opened for the second three columns. An inquiry is successful if a credit card account is opened within 14 days of the date of the inquiry. California observations are excluded from columns 2, 3, 5 and 6; New York observations are excluded from columns 1, 3, 4 and 6; and North Carolina Observations are excluded from columns 1, 2, 4, and 5.
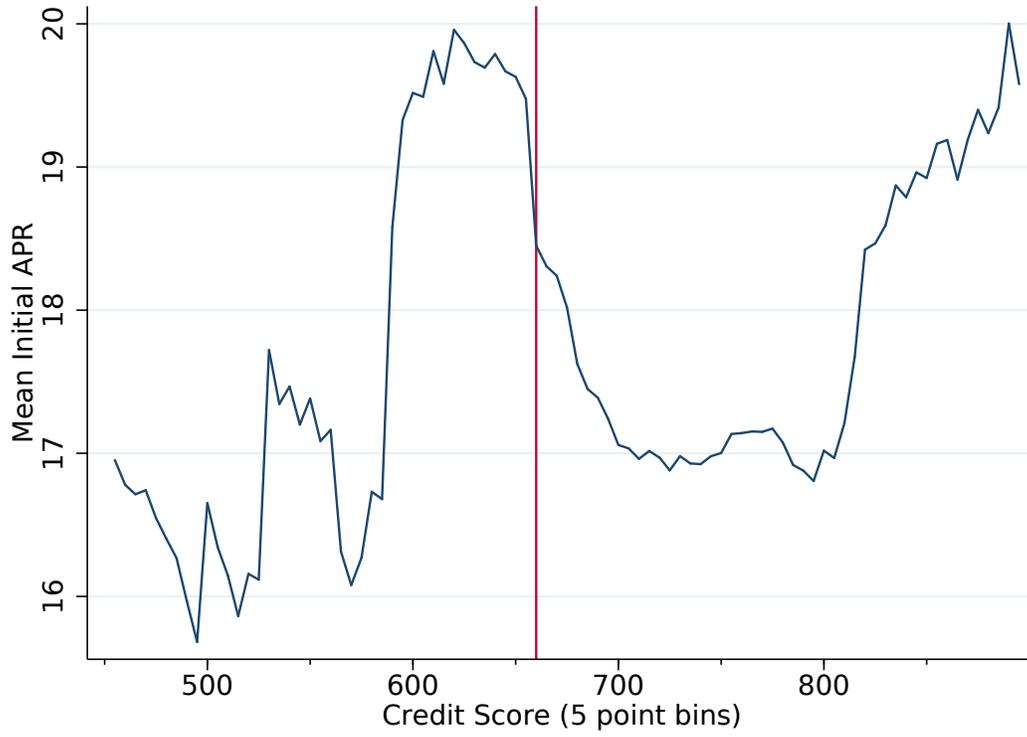
(a) Initial APR
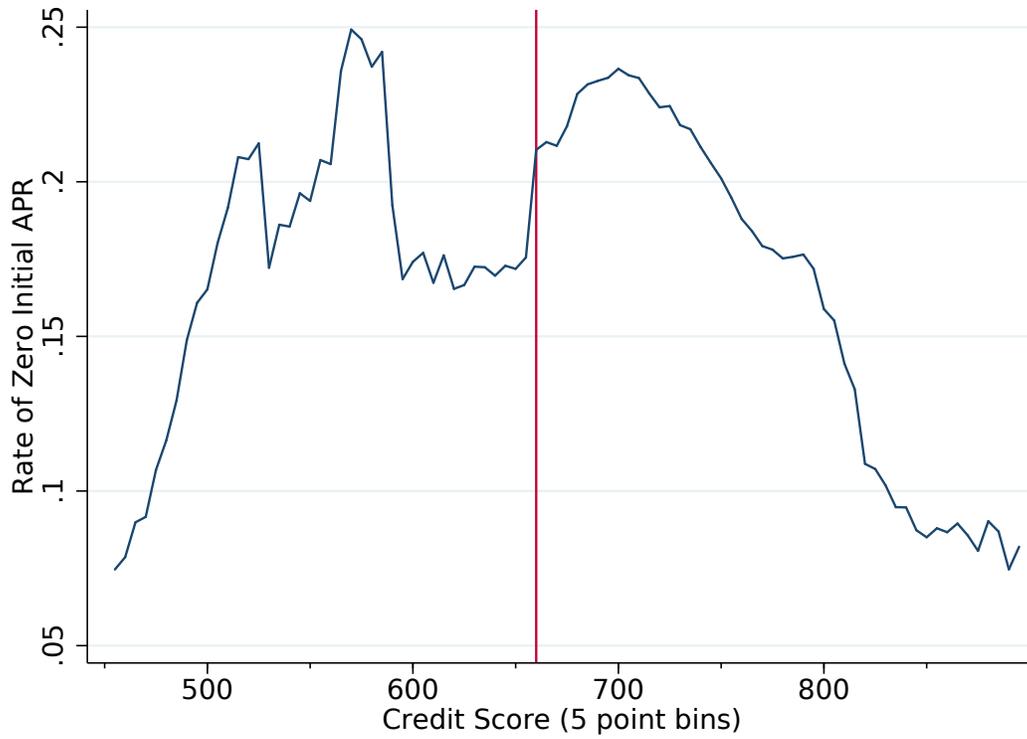
(b) Percent Zero APR

Figure A1: Initial APR and Percent Zero APR by 5-point Credit Score Bins