The Effect of Debt Collection Laws on Access to Credit

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Abstract

Debt collection is an important part of the market for consumer credit, but has received little attention in the finance literature. Regulations on 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' credit access or raise prices. Using detailed data on new credit card accounts, we study the effects of four recent state laws and regulations that instituted conduct restrictions. We find that such restrictions reduce access to credit card accounts and raise prices, but that this effect is very small.

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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 Bureau of Consumer Financial Protection. 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

[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.

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 29 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 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 sub-prime 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 3 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 sub-prime 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. Our results are noisy but our point estimates suggest that payments may decline somewhat and formal disputes may increase somewhat, perhaps due to the additional information that collectors were required to disclose under the new restrictions.

Another important potential mechanism is through effects on consumer demand for credit, leading to both selection and moral hazard. The literature thus far has attempted to assess whether consumers respond to laws governing default and bankruptcy by estimating the impact of law changes on debt balances (Gropp et al., 1997; Fonseca et al., 2017; Severino and Brown, 2017). In practice, such law changes may not be salient to consumers. Moreover, changes in debt balances may be confounded by supply effects. For instance, an increase in interest rates will increase debt balances even without a change in the demand for credit. We can test directly whether restrictions on debt collection affect consumers' demand for credit by estimating the effect of the laws on the frequency of credit inquiries. We find that conditional on making at least one inquiry for a credit card, debt collection restrictions cause consumers to make a larger number of inquiries at a time, but the overall demand for credit does not increase. This is consistent with the small changes in inquiry success that we find combined with the heterogeneity across firms, which together suggest that consumers may need to search more to find a credit card, but usually are ultimately able to obtain an account. However, we find no evidence that the probability a consumer seeks a credit card in the first place increases as a result of these restrictions.

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 main exception is Fedaseyeu (2015). 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. Beyond allowing for richer covariates, the use of microdata is important in this context to accurately measure access to credit. Our data allows us to measure the success rate of particular credit applications, rather changes in the overall number of new accounts, which are confounded with demand effects. In addition, Fedaseyeu's measure of the strength of state debt collection laws counts the number of new debt collection restrictions, regardless of the intended function of the restrictions. In particular, adjustments in the licensing fees for debt collectors, which only affect collectors' 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. In addition, because of the use of aggregate data, Fedaseyeu is unable to address the demand side effects of debt collections laws. [2]

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.

[2]Beyond Fedaseyeu (2015), Hynes and Posner (2002) and Livshits (2015) review the literature on the law and economics of creditor remedies for consumers in default. We are also aware of a parallel working paper by Fonseca et al. (2017), made public around the same time as our work, that extends Fedaseyeu (2015) with microdata. However, this work still uses a set of policies that predominately include changes to fixed costs, and as with much recent literature assumes without evidence that debt collection laws are salient enough to effect demand for credit. The fact that Fonseca et al. (2017) find significant effects on automotive debts, which are secured debt and thus rarely collected on by third party debt collectors, supports our argument that changes to licensing requirements are a problematic measure of debt collection restrictions.

[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.

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

[4] 15 U.S.C. §§1692-1692p, 1692f.
[5] 2009 N.C. Sess. Laws p. 573.

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]

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]

[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).

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

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 the consumer reaffirms or makes 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, because based on our discussions with industry experts, this 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 attempts.[11] The Massachusetts regulation also extended FDCPA protections to collection activities by creditors themselves, who are usually exempt.

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

[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.

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.[12]

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, $\bar{b} \equiv \bar{b}(e)$. Consistent with our results below, which indicate that changes in collection laws are not salient to consumer credit demand decisions, we do not model b as a function of collection effort.

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

[12] We do not extend the theory to include changes to consumer demand for credit in response to new collections restrictions. This topic is the focus of Durbin and Romeo (2018). We note that if the law changes are not salient to consumers when applying for credit there will be no change in demand. However, even if law changes are not salient, consumers may still respond to reductions in credit access by making additional credit inquiries. We report findings on both changes in demand for credit and changes in credit inquiries in our empirical analysis.

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 $b(r^*(e))$ 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,

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

This yields,

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

$\underline{\rho}^*(e) = 0$ 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

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

where $m(R^*;\rho,\ell,e)$ is the lender's profit margin and ε^{-1}_{b,r^*} 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 received by the creditor, 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 new restrictions on debt collection impact $\underline{\rho}^*(e)$ and $R^*(e)$. The mechanism is likely to operate through an 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 $\overline{b}^*(e)$ 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 $\overline{b}^*(e)$ in order to have less capital at risk of default.

In this sense, decreasing $\overline{b}^*(e)$ 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 $\overline{b}^*(e)$ 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 firm-level responses to new debt collection restrictions. Overall, effective new collections restrictions will likely affect the provision of credit, but each lender may make its own assessment of consumer and competitor responses to changes in $\underline{\rho}^*(e)$, $R^*(e)$ and $\overline{b}^*(e)$. 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 $\underline{\rho}^*(e)$ or $\overline{b}^*(e)$. 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. Bureau of Consumer Financial Protection: 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,[13] and the most common type of debt referred to debt collectors.[14] We discuss each of our datasets in turn and present basic summary statistics.[15]

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 2017, the latest available at the time of our analysis. We limit our data to CCP waves beginning in June 2010 because geographic identifiers are not available in earlier instances of the data. The credit records

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

[14]Bureau of Consumer Financial Protection (2017)

[15]Consistent with the BCFP'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.

available in the CCP include records of accounts, referred to as tradelines, and so-called "hard" inquiries, where a creditor has pulled the consumer's credit record in response to a consumer-initiated inquiry. Such inquiries are visible to other prospective creditors and are considered in the calculation of credit scores.[16] 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 2017.

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. Our results are robust to including Massachusetts.

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

[16]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 or by creditors seeking to market unsolicited "pre-approved" credit offers to consumers. The CCP does not have information on soft inquiries.

inquiry if within 14 days of the inquiry, we observe a new credit card account being opened.[17] For new 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 a score of 660, the cut-off between prime and sub-prime 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. The plot for credit limit is flatter for consumers with sub-prime credit scores, a result we will see reflected in lower coefficients on credit score for these consumers in our regression results.

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 bureau receives 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

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

of business of the original creditor. Example codes are "medical" and "finance." Although survey evidence indicates that consumers are contacted by debt collectors about credit cards about as often as they are about medical debts (Bureau of Consumer Financial Protection, 2017), 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 is largely because the CCDB includes private-label retail cards, while our CCP sample only general-purpose cards. Average credit limit for general-purpose cards in the CCDB is roughly the same as the average in the CCP. We also note 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.

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 data 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:

$$y_{istb} = \beta treat_{st} + \gamma X_{ist} + \delta_s + \alpha_t + \theta_l + f_s(t) + \varepsilon_{istb}, \tag{5}$$

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] 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 two 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 (3) report results using only county and time fixed effects. The coefficient on the treatment indicator for inquiry success has the right sign but is quite noisy, while the coefficient for initial credit limit has the wrong sign relative to the theoretical prediction—credit limits increase following a debt collection law or regulation. Columns (2) and (4) 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 with substantially smaller standard errors. These results indicate that the debt collection restrictions reduce access to credit, although for both outcomes the coefficients are small, statistically insignificant and precise enough to reject a moderate sized effect. Using the coefficient on credit score to scale our results, our point

[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.

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 scores on average 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. For credit limits, we can reject a change in credit limits on the order of $76, equivalent to a 3 point change in credit score.

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 significant positive coefficient, although the magnitude is still quite small. Our estimated effect on sub-prime borrowers is negative, but still not statistically different from zero. Our point estimate is equivalent to a 2 point decrease in credit score, and we can reject a change equivalent to a 6 point decrease in credit score at 5%. 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 $23. Compared to a change in credit score, our effect is of an equivalent magnitude to a 3 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 for prime borrowers. Our point estimate for the effect on credit limits for prime borrowers is about the same in dollar terms, but much smaller compared to the stronger effect of credit score on credit limits for prime borrowers, and the much larger average credit limit obtained by prime borrowers.

Our results are robust to alternate specifications. We obtain substantively similar results if we add controls for other features of consumers' credit report such as balances on existing credit cards, mortgages, auto loans and student loans. We prefer to use only credit score because this allows us to easily scale our treatment effect by the equivalent change in credit score. Individual fixed effects have little effect on our results, particularly after controlling for other credit score covariates. This is to be expected—our analysis is of creditor choices, and creditors only observe the information in the consumers' credit report, which we fully observe.[20] Results of these robustness checks are available in Appendix table A1. We also obtain similar results using linear or cubic state-specific trends.

Thus far we have been treating the five 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. The New York State Regulation seems to be an outlier, with positive coefficients on both outcomes (albeit small and statistically insignificant for the credit limit model), but otherwise most of the results are consistent with theory. The remaining law changes have statistically significant negative effects on inquiry success for sub-prime borrowers, although the magnitudes are still small, equivalent to a change in credit score of between 3 and 7 points. We note that our estimates are sufficiently noisy that we cannot reject the null hypothesis that the negative effects are equal to each other, and to the global effects in tables 3 and 4. Separate results by law for credit limit generally suffer from a lack of power, and we cannot reject the pooled coefficients from Table 3, except for the New York City Regulation. In Appendix Table A2 we report results using one treatment state at a time, excluding observations

[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.

from the other treatment states, and find similar results to table 5.

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

[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.

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 collection 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:

$$y_{ist} = \sum_{d=-24}^{24} \beta^d D_{st}^d + \gamma X_{ist} + \delta_s + \alpha_t + f_s(t) + \varepsilon_{ist} \tag{6}$$

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. For inquiry success by sub-prime borrowers, the plot is noisy, but shows no evidence of a confounding pre-trend. We observe a slight drop in inquiry success after collection laws are passed, relative to the pre-period, consistent with our difference-in-differences results. There is some evidence of a secular trend in credit limits, albeit again with substantial noise, but this is roughly linear and in the wrong direction to bias our results—credit limits appear to be growing over time, then have a small, discrete drop after a debt collection restriction is passed, and then continue growing at about the same rate.

5.2 Difference-in-Difference Results on Credit Pricing

Using our CCDB data, we estimate the model from equation (5) above, with some slight modifications. We employ ZIP code fixed effects rather than county fixed effects, and add

[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.

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. Our outcome variables are now 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 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.

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 subprime 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

[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.

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 Mechanisms for Debt Collection Restrictions to Affect Access to Credit

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. We next examine the mechanism through which this occurs. 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 first test whether the state restrictions on debt collection increase costs or reduce effectiveness of collection firms. Then, we examine whether these restrictions led to an increase in demand for credit by sub-prime borrowers, which could change the mix of borrowers applying for credit.

5.3.1 Collectors' Costs and Effectiveness

We test the effect of state debt collection restrictions on collection activity using 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 party collector or a debt buyer. This is important, because the California and North Carolina restrictions 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 and Arkansas 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. Moreover, because consumers with collections tradelines frequently have multiple such credit records for the same debt, we have less of a problem with implied selection.

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 0.04 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 cannot reject that consumers were slightly 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. Our coefficient is negative and noisy, and while we cannot reject zero effect, neither can we reject a decrease of about 1 percentage point. Thus, it would appear that the effect on access to credit could stem from increased compliance costs by debt collectors, and 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.

5.3.2 Difference-in-Difference Results on Demand for Credit

Another possible mechanism for our results on access to credit may be through changes in demand for credit. This could take two forms. First, given the heterogeneity we observe by firm, it could be that consumers seeking credit have to shop more and make more inquiries—recall that we do not observe the success of individual inquiries, only whether consumers ultimately were able to obtain a credit card account. It may be that consumers are being denied access to credit on the intensive or extensive margins by particular firms, but there is sufficient competition among firms that many are able to obtain credit eventually, leading to the small effects we find. Second, if consumers are aware of changes in debt collection restrictions, they may be more likely to apply for credit, since the consequences of default are less severe. This in turn could affect our measures of access to credit through selection—marginal consumers who enter the credit market because of a lower cost of default are probably less credit worthy than inframarginal consumers, and may be less likely to have a successful credit inquiry or will have a lower credit limit conditional on success. If this explanation holds, our results on credit access will in part reflect creditor response to moral hazard.

We test each of these explanations using data from the CCP. We want to estimate the shift in demand for new credit resulting from the new debt collection restrictions, and the increased demand for credit inquiries resulting from reductions in credit access. We specify two outcome variables: a 0-1 variable for whether the consumer made any hard inquires at time t, and the count of hard inquiries made by each consumer at time t conditional on having made at least one inquiry. Rather than using account level information conditional on making an inquiry or opening and account, we construct a consumer-quarter panel using all consumers in the CCP, with variables summarizing the consumers' entire credit profile.

We use these data to estimate two versions of the model from equation (5). To test for increased demand for credit, we use as our outcome variable a binary indicator for whether consumer i makes any credit card inquiries at time t. To test for increased demand for inquiries, independent of credit, we limit our sample to consumer-quarters with at least one credit card inquiry, and use as our outcome variable the count of credit card inquiries in the current quarter.[24] As with our results on collections activity, we are focused here on consumer rather than firm decisions, and so individual fixed effects may be relevant and important. We show results with and without individual fixed effects.

It is important to note that because of the nature of our inquiry data, our estimates of the effect on demand for credit will be biased if there is any effect on the count of inquiries. As discussed in section 4, we do not observe the universe of inquiries, but instead only observe inquiries that were submitted to the national credit reporting agency behind the CCP. Credit card issuers appear to generally submit inquiries to only one of the three nationwide credit reporting agencies, and so the chance of a hard inquiry appearing in the CCP is approximately 1/3.[25] As a result, if the debt collection restrictions cause consumers to submit more inquiries at a time, this will cause us to be more likely to see at least one inquiry in the CCP, even if there is no change in the number of consumers seeking credit. We can quantify the size of the bias and correct for it using our estimates of the effect on demand for inquiries—see appendix B for details.

Table 11 shows results on demand for inquiries, with the count of inquiries as the outcome variable. as noted above, we focus on the subsample of consumer-quarters with at least one credit card inquiry. The first two columns show results for all consumers, with the first column using the specification from equation 5, and the second column using individual fixed effects instead of county fixed effects. The third and fourth columns

[24]In principle, both outcomes could be estimated simultaneously using the two-step model of Heckman (1976). In practice this would be computationally infeasible: even using OLS the models in this section take several days to run on a fast multi-processor Linux server. Moreover, we do not have an instrument for the first stage that would allow us to identify the parameters separate from the functional form.

[25]In fact, from table 1, the count of inquiries multiplied by the average success rate is almost exactly one third of the count of new accounts.

show results for prime borrowers with and without individual fixed effects, and the fifth and sixth columns show results for sub-prime borrowers: The effects are generally small, showing an increase of about a 0.005 to 0.008 additional inquiries in response to increased restrictions on debt collection. Although small, these effects are statistically significant in all three categories when individual fixed effects are employed. Thus, our results show evidence of a small increase in credit inquiries consistent with a small decrease in credit availability at individual issuers.

Table 12 shows our results on demand for credit. The column format matches that of Table 11, and columns (1), (3) and (5) use the specification from equation 5, while columns (2), (4) and (6) use individual fixed effects instead of county fixed effects. We find that on average a debt collection restrictions causes the probability of observing at least one credit card inquiry to increase. Although the magnitude is small, our the estimates of the treatment effect for all and sub-prime consumers are statistically significant, and the magnitude of the point estimate for prime borrowers is similar to that for all and sub-prime. As discussed above, a positive treatment effect coefficient here could come about in two ways: moral hazard and bias due to the increase in the count of inquiries making it more likely that we observe at least one in the CCP. Using our coefficients from table 11 and assuming that the probability of observing an inquiry is exactly one third, we quantify the bias in the second row of Table 12 and present corrected treatment effects are in the third row. With the bias eliminated, the treatment effect is small in magnitude and statistically insignificant when we control for individual fixed effects. Note that theory would predict that moral hazard effects should be larger in magnitude for sub-prime relative to prime consumers; the fact that this pattern appears in the corrected treatment effects and not the raw effects points to the need for bias correction.

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 3 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.

We also find suggestive evidence on the mechanisms for the small effects we find. The data suggest that consumer disputes relating to debt collection increase slightly and payments to debt collectors decrease slightly, although these estimates have a great deal of noise. If true, this would imply a possibility for decreased returns to creditors that would lead to reductions in access to credit. We also find that consumer shopping may mitigate any effects on credit access. In addition to the cross-firm heterogeneity in treatment effects, we find that the number of credit inquiries generated by consumers seeking credit increases as a result of debt collection restrictions, suggesting that consumers have to search slightly more in order to obtain credit. We do not find evidence that consumers are more likely to seek credit in the first place after a restriction on debt collection, however.

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

	Mean	SD	Min	Max	N
Inquiry Success Rate	0.45	0.50	0	1.00	8,537,064
Credit Limit of Opened Accounts	5335	6133	100	92000	11,504,125
Credit Score	687.9	97.57	300	839	18,734,007
Year of Birth	1967	16	1888	1999	19,006,371
Census Trace % Black	11.68	19.01	0	100.00	19,478,754
Census Tract % Hispanic	17.82	22.07	0	100.00	19,478,754
Census Tract % HS Dropout	13.22	10.90	0	100.00	19,478,654
Census Tract Median Income ($)	31060	12230	2501	131362	19,476,655

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)
Treatment Indicator	-0.00608	-0.000167	343.1***	-26.70
	(0.0105)	(0.00338)	(114.7)	(25.10)
Credit Score	0.00231***	0.00231***	27.41***	27.40***
	(0.0000142)	(0.0000142)	(0.285)	(0.283)
Year of Birth	0.000947***	0.000949***	-11.57***	-11.65***
	(0.0000470)	(0.0000471)	(0.805)	(0.812)
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	No	Yes	No	Yes
N	7495569	7495569	10111128	10111128

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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	0.00650**	-0.00456	-29.62	-22.81**
	(0.00320)	(0.00417)	(35.20)	(10.68)
Credit Score	0.00169***	0.00215***	33.71***	7.139***
	(0.0000399)	(0.0000129)	(0.341)	(0.194)
Year of Birth	0.00132***	0.000141	-5.403***	-13.23***
	(0.0000517)	(0.0000888)	(0.854)	(0.619)
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	3559101	3936468	7316211	2794917

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 credit 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	-0.00372***	0.00325**	-0.00888***	-50.62**	-64.01**	-16.67
	(0.00124)	(0.00156)	(0.00115)	(21.32)	(26.89)	(12.26)
NY Debt Collection Reg.	0.0238***	0.0364***	0.0139***	-70.08***	-110.1***	0.539
	(0.00227)	(0.00319)	(0.00160)	(20.19)	(24.23)	(10.03)
NC CEP ACT	-0.00346	0.00923	-0.0110**	-43.21	-41.92	-16.56
	(0.00745)	(0.00953)	(0.00520)	(38.90)	(49.00)	(18.90)
AR FDCPA	-0.0107	0.00215	-0.0191***	272.0***	303.9***	-27.93
	(0.00692)	(0.00974)	(0.00436)	(46.39)	(57.33)	(21.06)
NYC Regulation	-0.0104*	-0.0177**	0.00292	36.99	82.26**	-137.9***
	(0.00553)	(0.00723)	(0.00383)	(24.48)	(32.24)	(18.53)
Credit Score	0.00231***	0.00169***	0.00215***	27.40***	33.71***	7.139***
	(0.0000141)	(0.0000399)	(0.0000129)	(0.283)	(0.341)	(0.194)
Year of Birth	0.000949***	0.00132***	0.000141	-11.65***	-5.403***	-13.23***
	(0.0000471)	(0.0000516)	(0.0000888)	(0.812)	(0.854)	(0.619)
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	7495569	3559101	3936468	10111128	7316211	2794917

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

		Initial APR		
	Credit Limit	All	Non-Zero	P(Zero APR)
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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

		Initial APR		
	Credit Limit	All	Non-Zero	P(Zero APR)
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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

		Initial APR		
	Credit Limit	All	Non-Zero	P(Zero APR)
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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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	-0.00341**	0.000396
	(0.00147)	(0.00154)
Dep. Variable: Some Payment Made to Collector		
Debt Buyer × Reg Passed	-0.00312	-0.00297
	(0.00230)	(0.00389)
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	18813149	18813149

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 11: Effect of Debt Collection Restrictions on Demand for Credit Inquiries: Consumer-Level Differences-in-Differences

	All Consumers		Prime		Sub-Prime	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Indicator	0.000551	0.00639***	0.00530	0.00850***	-0.0000732	0.00550***
	(0.00340)	(0.00150)	(0.00397)	(0.000885)	(0.00328)	(0.00156)
Credit Score (00s)	-0.0520***	0.0580***	-0.126***	-0.0226***	0.0146***	0.111***
	(0.00191)	(0.00177)	(0.00237)	(0.00201)	(0.00121)	(0.00276)
Total CC Limit ($100,000s)	0.0544***	0.0791***	0.116***	0.116***	0.185***	0.283***
	(0.00542)	(0.00365)	(0.00350)	(0.00356)	(0.0105)	(0.0325)
Year of Birth	0.00181***		0.00102***		0.00230***	
	(0.0000746)		(0.0000548)		(0.000118)	
Census Tract Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Credit Report Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	No	Yes	No	Yes	No
Individual Fixed Effects	No	Yes	No	Yes	No	Yes
N	7897307	7897307	3771914	3771914	4125393	4125393

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

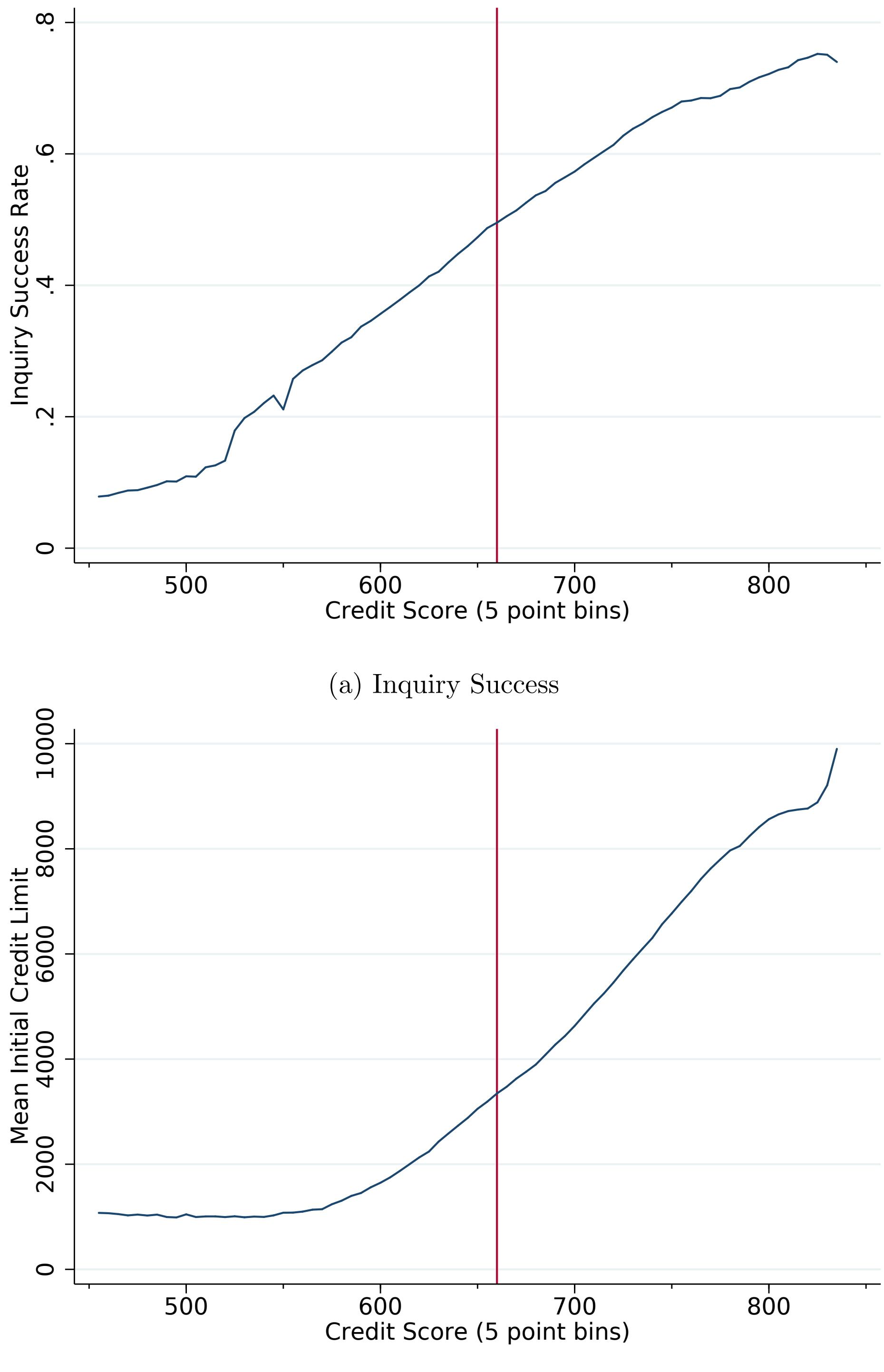
Standard errors clustered at the state level reported in parentheses. The unit of observation in these regressions is a consumer-quarter. Outcome variable is the count of hard inquiries for general purpose credit card accounts. The sample is limited to consumer-quarters with at least one of the relevant type of inquiry. "Total CC Limit" is the total of the credit limits on all open credit cards, in $100,000s. Credit report covariates include current credit card balances on all cards, current student loan balances on all cards, current mortgage balance, current total auto loan balances, and counts of derogatory reports for credit card, auto loan, student loan and mortgage debts.

Table 12: Effect of Debt Collection Restrictions on Demand for Credit: Consumer-Level Differences-in-Differences

	All Consumers		Prime		Sub-Prime	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Indicator	0.00230***	0.00285***	0.00249*	0.00321	0.00293**	0.00307**
	(0.000480)	(0.000665)	(0.00113)	(0.00178)	(0.00105)	(0.00104)
Treatment Bias	0.00015	0.00178**	0.00158	0.00252***	-0.00002	0.00146*
	(0.00017)	(0.00088)	(0.00169)	(0.00060)	(0.00002)	(0.00087)
Corrected Treatment Effect	0.00215***	0.00107	0.00091	0.00069	0.00295***	0.00161
	(0.00065)	(0.00154)	(0.00282)	(0.00238)	(0.00105)	(0.00191)
Credit Score (00s)	-0.0236***	0.0173***	-0.0458***	-0.0135***	0.00837***	0.0469***
	(0.000511)	(0.000544)	(0.00233)	(0.000856)	(0.000995)	(0.00116)
Census Tract Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Credit Report Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	No	Yes	No	Yes	No
Individual Fixed Effects	No	Yes	No	Yes	No	Yes
N	121770673	121770673	79812151	79812151	41958522	41958522

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

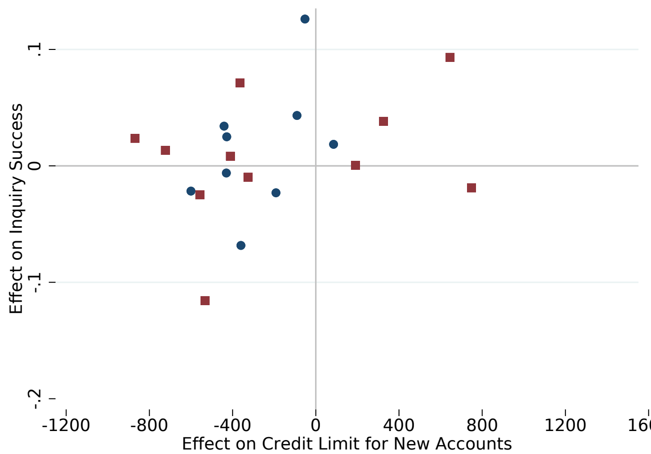
Standard errors clustered at the state level reported in parentheses. The unit of observation in these regressions is a consumer-quarter. Outcome variable is an indicator for at least one hard inquiry for general purpose credit card accounts. Credit report covariates include current credit card balances on all cards, current student loan balances on all cards, total of the credit limits on all open credit cards, current mortgage balance, current total auto loan balances, and counts of derogatory reports for credit card, auto loan, student loan and mortgage debts.



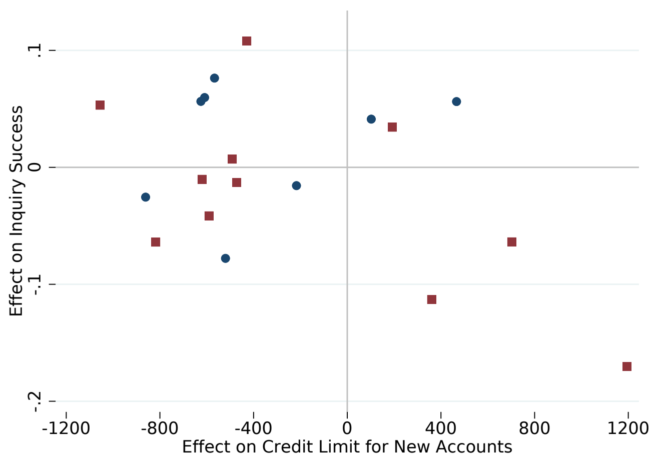
(a) Inquiry Success

(b) Initial Credit Limit

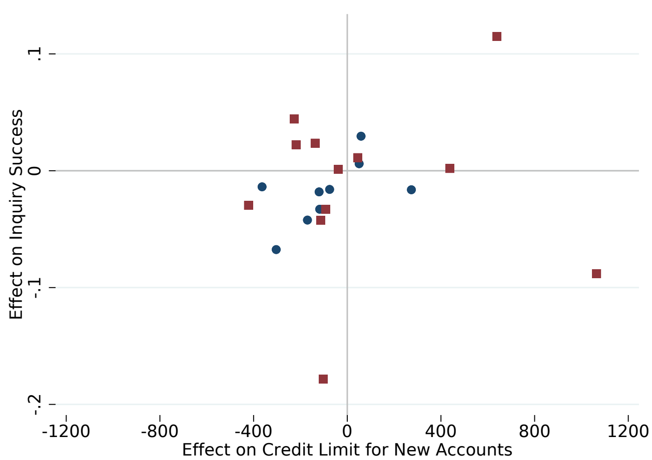
Figure 1: Inquiry Success Rate and Initial Credit Limit by 5-point Credit Score Bins



(a) All Borrowers

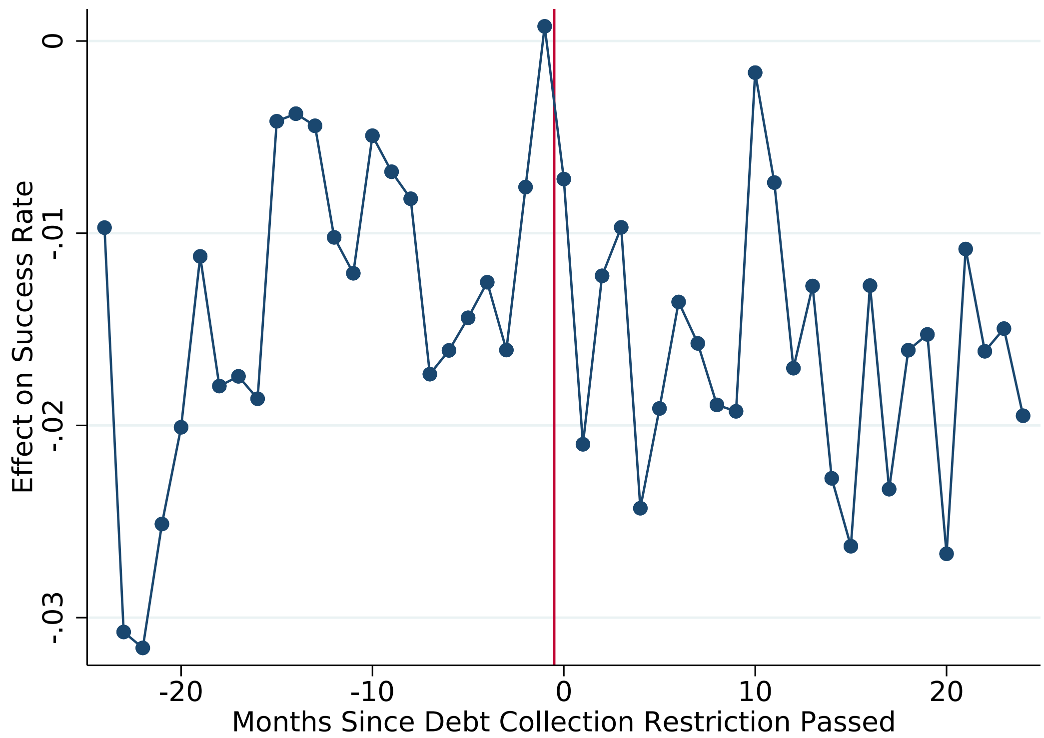


(b) Prime Borrowers

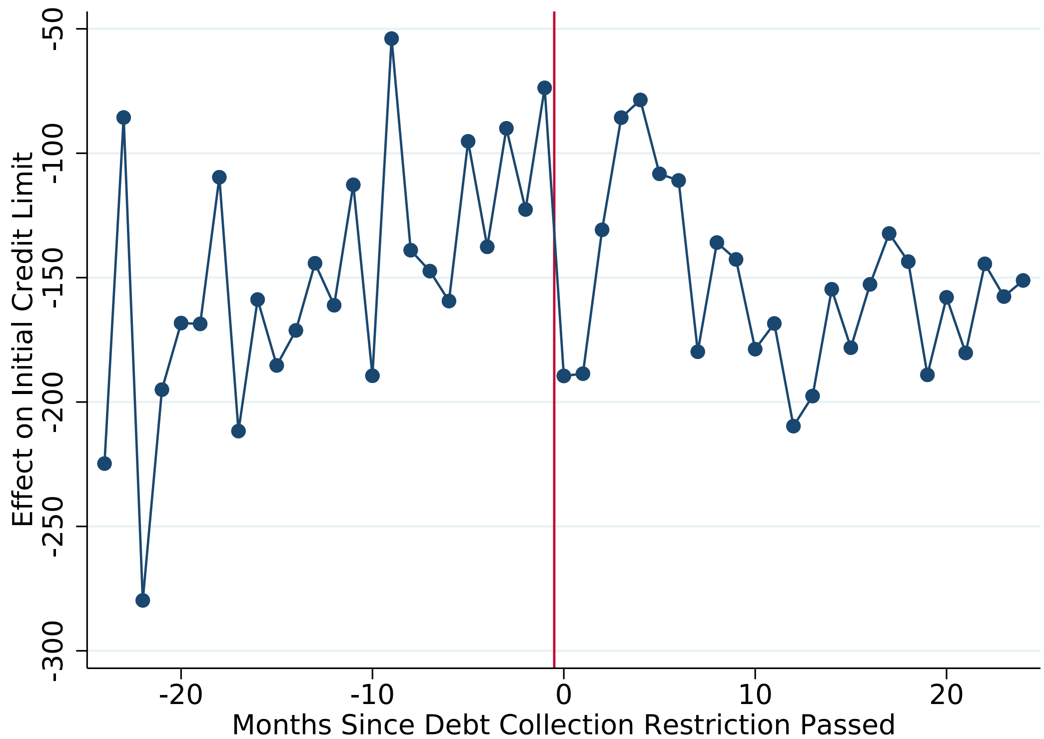


(c) Subprime Borrowers

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

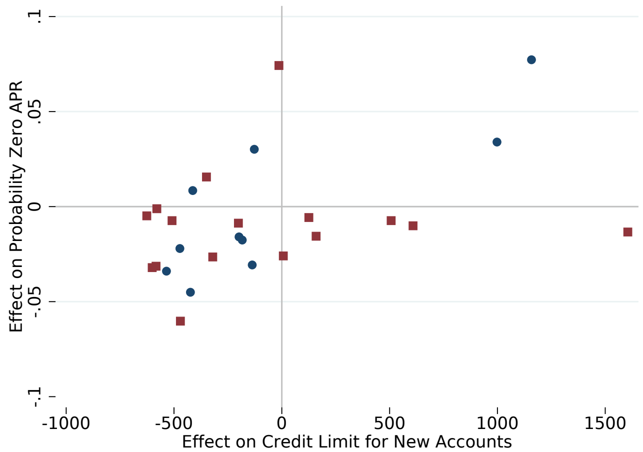


(a) Inquiry Success for Sub-prime Borrowers

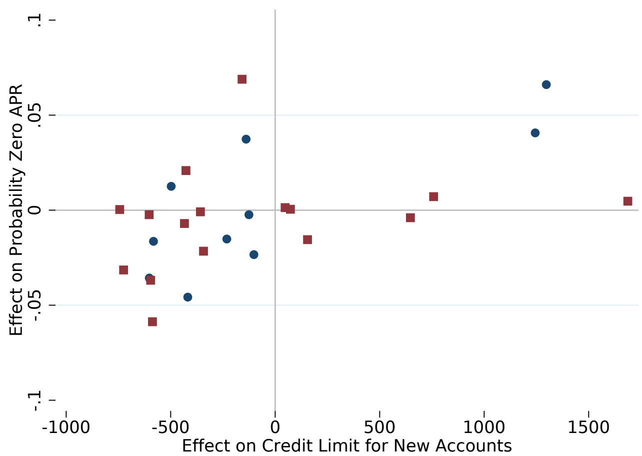


(b) Initial Credit Limit for Prime Borrowers

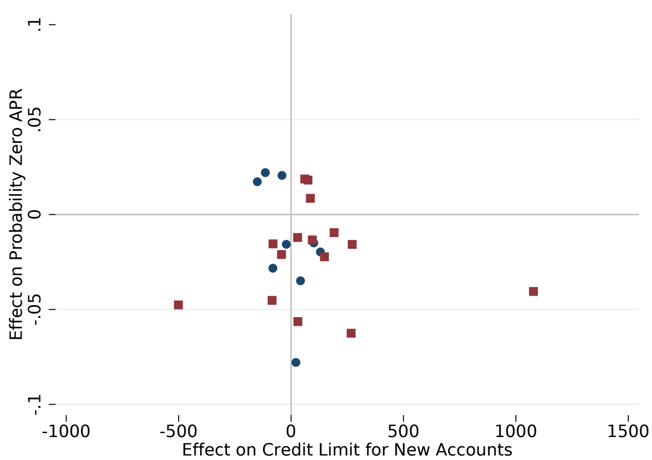
Figure 3: Event Studies of the Effect of Debt Collection Restrictions on Access to Credit



(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: Differences-in-Differences Robustness Checks

	Panel A: Outcome is Inquiry Success					
	Additional Credit Controls			Individial Fixed Effects		
	All	Prime	Sub-Prime	All	Prime	Sub-Prime
Treatment Indicator	0.000360	0.00583*	-0.00433	0.00329*	0.0114***	-0.00237
	(0.00273)	(0.00303)	(0.00370)	(0.00180)	(0.00333)	(0.00222)
Credit Score	0.00234***	0.00175***	0.00208***	0.00170***	0.00120***	0.00146***
	(0.0000154)	(0.0000452)	(0.0000118)	(0.0000106)	(0.0000214)	(0.0000114)
N	6676621	3232558	3444063	6676621	3232558	3444063

	Panel B: Outcome is Initial Credit Limit					
	Additional Credit Controls			Individial Fixed Effects		
	All	Prime	Sub-Prime	All	Prime	Sub-Prime
Treatment Indicator	-70.58**	-80.08	-55.03***	-23.93	-37.16	-37.94*
	(34.36)	(50.87)	(5.830)	(20.94)	(23.69)	(22.13)
Credit Score	24.17***	32.05***	5.252***	12.05***	15.85***	2.443***
	(0.311)	(0.280)	(0.168)	(0.147)	(0.147)	(0.0946)
Census Tract Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Credit Report Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Month Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	No	No	No
Lender Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic Trends by State	Yes	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	No	No	No	Yes	Yes	Yes
N	9221863	6733283	2488580	9221863	6733283	2488580

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

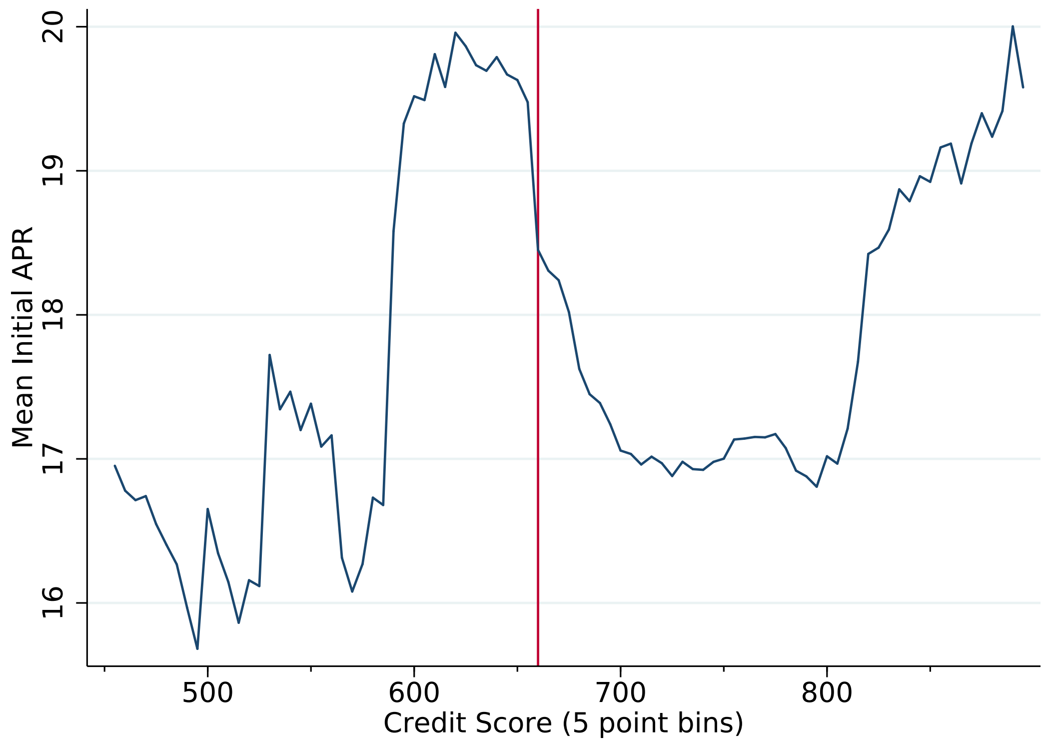
Standard errors clustered at the state level reported in parentheses. Month fixed effects are based on the month when the inquiry was made for the top panel, and the month the account was opened for the bottom panel. An inquiry is successful if a credit card account is opened within 14 days of the date of the inquiry. Credit Report Covariates include total credit limit on all credit card accounts in the current quarter, separate variables for the total balances and total derogatory flags for credit card, auto loans,

Table A2: Effect of Debt Collection Restrictions on Access To Credit: Separate Regressions for Each Treamtent State, Sub-Prime Only

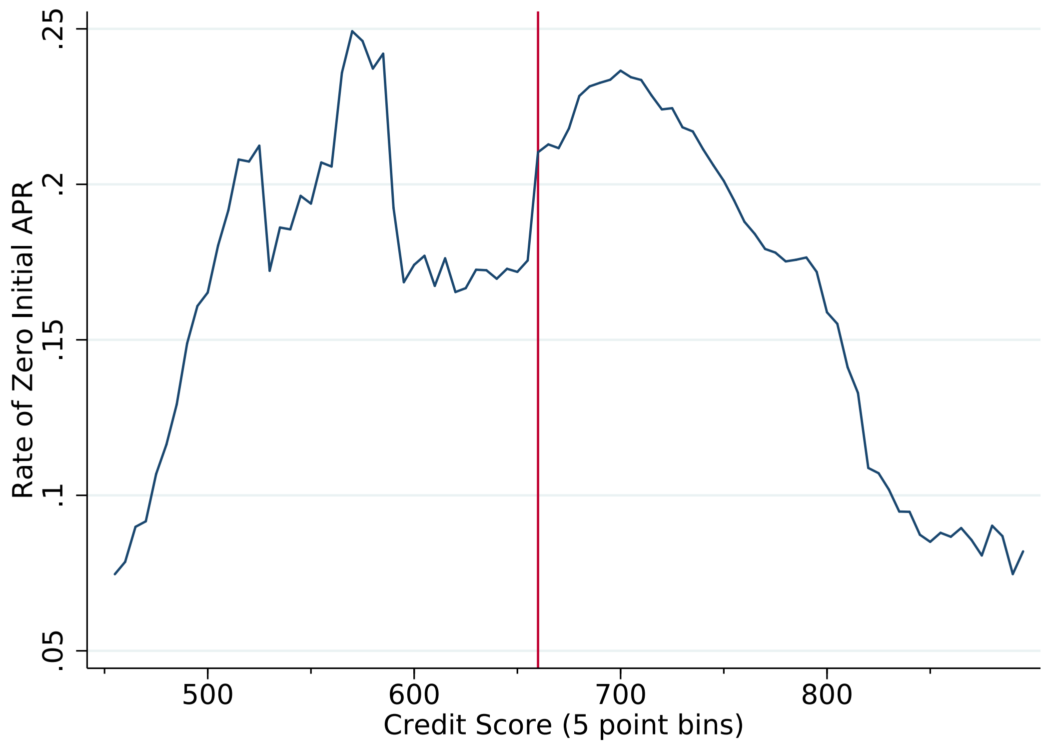
	Inquiry Success			Initial Credit Limit				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CA Debt Buyer Law	-0.00835***				-10.07			
	(0.00107)				(11.61)			
NY Debt Collection Reg.		0.0142***				-7.329		
		(0.00189)				(8.037)		
NC CEP ACT			-0.00944				-28.44	
			(0.00636)				(20.66)	
AR FDCPA				-0.0168***				-42.48*
				(0.00512)				(23.37)
Credit Score	0.00215***	0.00215***	0.00215***	0.00215***	7.182***	7.146***	7.201***	7.200***
	(0.0000136)	(0.0000139)	(0.0000147)	(0.0000149)	(0.213)	(0.233)	(0.243)	(0.251)
Year of Birth	0.000135	0.0000759	0.0000405	0.0000494	-13.47***	-12.87***	-12.97***	-13.05***
	(0.0000983)	(0.0000832)	(0.0000894)	(0.0000911)	(0.654)	(0.568)	(0.584)	(0.609)
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	3526260	3219150	3029242	2919092	2522811	2315790	2197108	2120396

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

B Estimating bias in demand for inquires

Suppose the typical consumer seeking credit in a given quarter makes $3k$ credit inquires prior to a debt collection restriction and $3(k+\delta)$ inquires after, with $k \in \mathcal{R}^+$ and $\delta \in \mathcal{R}$. Assume further that the probability that a given inquiry is observed in the CCP is exactly $1/3$. Then the probability at least one inquiry is observed in the CCP for a given consumer seeking credit can be expressed as

$$P(CCP) = \left(1 - \left(\frac{2}{3}\right)^{3x}\right) \tag{B-1}$$

where $x = k$ prior to the rule change and $x = k + \delta$ once the rule change takes effect. The change in the probability of observing at least one inquiry in the CCP post-rule change, can then be written

$$\Delta P(CCP) = \left(\frac{2}{3}\right)^{3k}\left(1 - \left(\frac{2}{3}\right)^{3\delta}\right) \in \mathcal{R}. \tag{B-2}$$

This is our bias estimate. The bias has the same sign as δ, is decreasing in k and increasing in the absolute value of δ. We can approximate k with the mean positive credit inquires per quarter observed in the data: $k = 1.20947$, 1.15498, and 1.25099 for all, prime and sub-prime consumers respectively. δ is the estimated treatment effect the corresponding demand for credit inquires model in Table 11. Using the delta method, the standard deviation of $\Delta P(CCP)$ can be expressed as

$$StdDev[\Delta P(CCP)] = StdDev(\delta)\left(\frac{2}{3}\right)^{3k} 3\delta\left(\frac{2}{3}\right)^{(3\delta-1)}, \tag{B-3}$$

which allows for inference on the estimated bias and the bias corrected coefficients.