No Credit For Time Served?
Incarceration and Credit-Driven Crime Cycles*

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Abstract

We document that incarceration significantly reduces access to credit, and that in turn leads to substantial increases in recidivism, creating a perverse feedback loop. In the first part of the paper, we use random assignment of criminal cases across judges to document significant post-release reductions in credit outcomes, including credit scores, mortgages, autoloans, and lender assessment of income. In the second part, we use sharp discontinuities in lending based on credit scores to show that this loss of financial access feeds back into future crime. Consequently, the financial distortions that imprisonment creates undermine the crime-reduction goal of incarceration.

Keywords: Incarceration, Financial Inclusion, Criminal Justice, Equitable Finance, Household Finance

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1 Introduction

The United States has the largest incarcerated population in the world. Around 3.4 percent of the U.S. adult population has been incarcerated.[1] While the U.S. has only about 4 percent of the world's people, it contains about 20 percent of its carceral population.

Incarceration imposes substantial direct and indirect costs to society. Total correction spending is the second-fastest growing federal budget item in the U.S. behind Medicaid (Henrichson and Delany, 2012). Estimates of the fiscal costs of the combined federal, state, and local expenditures on all justice-related programs, which include policing and judicial services, exceeded $228 billion in 2007. There are also indirect costs stemming from loss of human capital and lost income. An ex-incarcerated person is significantly more likely to remain jobless (Visher et al., 2011), have lower lifetime earnings (Sabol, 2007), and develop criminal skills while incarcerated (Bayer et al., 2009). Furthermore, more periods of unemployment and lower incomes will tend to limit a person's access to credit and the benefits associated with smoothing consumption and potentially undertaking such productive investments as starting a business. Despite the importance of credit markets for welfare, there are yet no assessments of the effects of incarceration on access to credit.

In this paper, we evaluate the impact of incarceration on access to credit and the implications of access to credit on recidivism. Specifically, there are four interrelated components to our analysis. First, we analyze the impact of incarceration on an individual's post-release credit scores and likelihood of obtaining auto or home loans. Second, we examine the mechanisms linking incarceration with access to credit by examining (a) the inability to service debts while incarcerated, (b) the reduction in income following incarceration, and (c) discrimination by lenders toward convicts. Third, we demonstrate how changes in an individual's observable credit traits following incarceration—credit scores and income—obscure the ability of lenders to draw accurate inferences about unobservable features when making credit allocation decisions. This informational friction shapes credit allocation decisions, the average performance of loans given to ex-convicts, and the degree of adverse or advantageous selection into the pool of convicts seeking credit. Finally, we evaluate how access to credit influences recidivism.

The first component of our analysis is to causally assess the impact of incarceration on ex-convicts' access to credit. Our identification strategy exploits institutional features of the court system. After criminal charges are filed against a defendant, cases are randomly assigned to judges

[1] Shannon, Uggen, Thompson, Schnittker, and Massoglia (2010).

with the intent of facilitating an equal workload. Judges, however, are heterogeneous; they have different propensities to incarcerate. Since the judge the defendant is assigned to is strongly predictive of ultimate incarceration status, we can use judge fixed effects as instruments for incarceration. Exploiting this exogenous variation in the likelihood of being incarcerated arising from quasi-random assignment of cases recovers the causal effects of incarceration for individuals at the margin of release. This instrumental variables (IV) research strategy is similar to that used by Kling (2006), Aizer and Doyle (2015), and Mueller-Smith (2015), to estimate the impact of incarceration and bail in the United States. Using this design we show that ex-convicts face a drop of between 42 to 69 points in their credit scores, reductions in their auto loan financing of between 24 and 45 percent, and declines in mortgages of 14–16%.

The second component of our analysis is to examine the mechanisms that link incarceration with access to credit. First, the most direct mechanism through which incarceration affects the post-release credit opportunities of an ex-convict is by incapacitating her to service her debt while confined. Although this incapacitation is temporary, it affects the credit history of the borrower, which leads to harsher terms of credit in the future. To assess incapacitation we exploit exogenous variation in the intensive margin of incarceration, sentence length. Each year in carceral confinement leads to a drop of 47 points in credit score, meaning that the intensive margin accounts for approximately half of the total deterioration of credit scores due to incarceration.[2] This deterioration of credit scores due to longer sentences is consequential, as lower credit scores increase the cost of borrowing due to higher interest rates (Furletti, 2003) and decreased likelihood of being approved for a loan. A back-of-the-envelope calculation shows that a drop of 50 points in credit scores can lead to an increase of up to 4 percentage points in interest rates.

Second, incarceration affects access to credit through changes in income. Sending individuals to jail/prison dampens their labor market prospects in many ways: (i) incarceration generates human capital depreciation indirectly by forcing the individual out of the labor force, and, potentially, as a direct effect of incarceration itself (e.g., Bayer et al., 2009); (ii) removal from the labor force—i.e. falling off the job ladder—weakens a former convict's negotiation benchmark, which takes time and effort to build; and (iii) labor market discrimination (either because of taste or statistical discrimination) reduces their ability to bargain and obtain outside offers. We find support for each of these labor market mechanisms. We first show that ex-convicts' income is reduced by 18 to 39

[2] In Section (5.1) we discuss alternative channels that may also explain the connection between sentence length and lower credit scores.

percent. As we distinguish between sources of labor income loss, we find that approximately 29.4 percent is due to depreciation of human capital, 23.5 percent of the loss of income is due to removal from the labor force, and 47.1 percent is due to labor market discrimination.

Third, ex-inmates may face discrimination when applying for credit in the same way that they can be discriminated against in the labor market. Discrimination can arise for many reasons; it can be the result of stigma, statistical discrimination, or it can inadvertently occur due to, for example, the use of algorithms and computerized systems by the lender. We use two approaches to assess discrimination in lending. First, we look at the performance of borrowers on probation, accounting for pre-trial detention, since these borrowers would have a criminal record but would not be affected by human capital loss or labor force removal. This yields an estimate of the effects of credit market discrimination *and* the downstream effects of labor market discrimination on credit and, hence, provides an upper bound on the amount of discrimination in lending. Second, following the literature on adverse selection and positive correlation tests (Chiappori and Salanié, 2000), we test for the presence of advantageous selection in the allocation of credit— which would arise if ex-convicts are stigmatized by lenders. Using both methods, we find that there is very little evidence that the reduction in access to credit is due to discrimination in lending.

We next turn to the third component of our analysis: How do changes in an individual's observable credit traits following incarceration, such as credit scores and income, create informational asymmetries between lenders and ex-convicts that adversely distort the allocation of credit? Because of imperfect information, when lenders use observable credit traits to make credit allocations they must also make inferences about unobservable credit traits. When an individual's characteristics that lead to her conviction are related to credit traits unobservable to the lender, changes in an individual's observable credit traits following incarceration create a mismatch between the ex-convict's *real* and the *inferred* unobservable credit traits. Using our random judge assignment design, we decompose the effects of being incarcerated into an exogenous component and a residual component summarizing the unobservable characteristics that lead to a conviction. Because of the legal underpinnings of American criminal law, this summary measure of unobservables can be interpreted as a measure of criminal intent which we will henceforth refer to as the *criminal type* of the individual.[3] We then show that this criminal type is relevant to the lender: criminal type

[3] This decomposition has legal underpinnings— we exploit the general requirement in American criminal law that proof of both (i) commission of a criminal act and (ii) existence of a criminal intent are necessary conditions for a conviction.

is positively correlated with pre-conviction credit traits such as credit history, likelihood of having a mortgage, and estimated income. Supplied with a measure of observable criminal traits to the lender (conviction) and another of unobservable criminal traits (criminal type), we can evaluate whether informational frictions are distorting lenders' ability to assess post-conviction loan performance. We discover the following: Conviction does not predict better (or worse) loan performance, but criminal type does. Individuals with high criminal type experience fewer defaults, foreclosures, and bankruptcies. The inability of lenders to account for the better unobservable credit traits of high criminal types prevents them from providing high criminal type borrowers with more credit flows as supported by their lower default risk. With respect to income, we summarize the interpretation of this finding as follows: *Distortions in the labor markets can generate informational frictions in credit markets by obscuring information about relevant characteristics of the borrower that are unobservable to the lender and happen to be correlated with labor market income.* In studying this relationship between incarceration, criminal type, and credit, this paper more broadly relates to the literature on externalities in economies with incomplete markets and imperfect information (Greenwald and Stiglitz, 1986).

Finally, we turn to the last component of our analysis: Do restrictions on access to credit triggered by incarceration increase the likelihood of recidivism? We can address this question by exploiting discontinuities in credit limits that naturally occur due to conventional lending practices (Agarwal et al. 2017). These practices frequently appear in the form of "rules of thumb"— borrowers with similar observables are lumped together to receive the same terms of credit;[4] for example, borrowers with credit scores between 700 and 704 are considered to be equally risky, but more risky than borrowers with scores between 705 and 709. These credit limit discontinuities lend themselves to a regression discontinuity design (RDD). By supplementing our random judge assignment with this RDD, we show that, within the former inmates population, reductions in access to credit following incarceration increases recidivism by 6–11 percentage points, with lower effects for individuals with no previous arrests. In this regard, our paper shows the role played by credit constraints in fostering crime.

The paper is structured as follows. In the Section 2, we describe our data and setting. Section 3 describes our research design and overall empirical framework. In Section 4, we provide our main results– namely, the causal effects of incarceration on access to credit. In Section 4.3, we evaluate

[4] Screening borrowers is costly. The optimality of "rules of thumb" has been properly assessed by Agarwal et al. 2017).

heterogeneity in the incidence of incarceration costs across individuals with varying pre-conviction income and proclivity towards crime. We analyze the mechanisms leading to lower credit access in Section 5. In Section 6, we evaluate loan performance and the resulting advantageous selection. Section 7 assesses how lack of access to credit leads to recidivism. In Section 8 we conclude.

2 Data & Summary Statistics

Our setting is Harris County, Texas—the third largest county in the U.S., including the city of Houston. The county is economically and demographically diverse, which is reflected in our sample. Two court systems operate in Harris County: the Criminal Courts at Law (CCL) and the State District Courts (SDC). The 15 CCLs have jurisdiction over misdemeanor cases, including traffic violations, DUI offenses, drug possession, non-aggravated assault, and similarly less serious criminal offenses. The 22 district courts handle felony cases. Importantly for our purposes, after charges are filed, a case is randomly assigned to a courtroom. This assignment method is administered by the District Clerk, and is intended to create equal workloads across judges.

2.1 Data

We rely on several sources of administrative and survey data. Our main explanatory variable is exposure to criminal punishment, which we develop based on initial filings acquired from the District Clerk's office. All felony and misdemeanor charges between 1985 and 2012 are included in the data regardless of final verdict. The filing includes name, date of birth, alleged offense(s), attorneys involved, judge assigned to the case, and final disposition. The administrative court filings allow us to measure whether a defendant received a carceral or probationary sanction, a fine, or was simply released with no punishment. We also have personal information on each defendant, including gender, ethnicity, date of birth, and address of residence. We start with data for each criminal arrest for which there is a court appearance.

We merge the court filing data with detailed individual-level financial history information from Equifax, one of the three major credit bureaus in the U.S. This rich data includes information on a borrower's credit score, borrower liabilities (such as auto loans, installment loans, credit cards, etc.), and debt payments. Equifax follows strict anonymity-preserving obligations (under federal law) when facilitating researcher access. Thus, to create a usable dataset at a feasible cost, we made the following criminal history data restrictions/choices: first, we randomly choose a sample of 400,000 individuals, and removed all within-county geographic information. We then removed specific offense code information. Equifax used the name, date of birth, and address (at a particular point in time) to match our criminal incident files to individual credit data. Equifax then provided us with an anonymized research subsample, with credit characteristics appended for two credit years (2006 and 2013), of around 100,000 unique individuals chosen at random after matching for both years. This file includes coarsened categories related to offense, age, gender, and race.[5]

Lenders often do not have borrowers' income information when making loan decisions but nevertheless, assess borrowers' income capacity using credit information and proprietary algorithms. We thus also include income estimates for everyone in our sample to mirror the algorithms widely used by lenders. Specifically, we estimate personal income from IRS zip code-level income data and the Survey of Consumer Finances (SCF).[6] Using the SCF, we estimate the probability of belonging to an income percentile bracket given the distribution of total loan amounts. Using Bayes' rule, the probability of having income i given loan l, $f_{I|L}(i|l)$, is given by

$$f_{I|L}(i|l) = \frac{f_{L|I}(l|i)f_I(i)}{\int f_{L|I}(l|i)f_I(i)di}$$

We first divide loan amounts into deciles and income into quartiles matching IRS on income distributions by zipcode. We then estimate income by multiplying each income probability given loan amount with average income for each percentile by zipcode. The estimate of income for an individual in zipcode z, with IRS income distribution i_z^{IRS} and loan decile l_z, is given by:

$$i^{Est} = E[i|l_z] = \sum f_{I|L}(i|l_z)i_z^{IRS}$$

2.1.1 Sample Characteristics

We begin by describing characteristics of our final merged criminal defendant-financial history sample. Of this sample, 74.5 percent were ever convicted in Harris County. Of that subset, 12.7 percent were sentenced only to probation and 61.8 incarcerated. 39 percent of the convicted sample recidivate. From all cases, 22 percent are brought to court on account of a felony, while the remaining are only for misdemeanors. The distribution of age for crimes is highly skewed towards a younger population with the median age of individuals at case resolution of 30. The fractions of blacks and Hispanics in the sample are 38 and 22 percent respectively, compared to the 18.9 and 40.8 for Harris County overall, according to the 2010 Census. Hispanic underrepresentation is

[5] The match rate was around 75 percent, and Equifax returned identifiers for the original file to assess match selection.

[6] This approach is also similar to an approach used in Coibion, Gorodnichenko, Kudlyak, and Mondragon (2016).

partly explained by its increasing share in the population over the last few decades.[7] From those arrested, the incarceration rates are 23.6 percent and 18.1 percent compared against 19.4 percent for Caucasians, indicating that Blacks are overrepresented in their probability of being incarcerated. Women make up 27 percent of all the defendants brought to court.

The average credit score for the sample is 575. Figure (2), Panel A, shows the distribution of credit scores for individuals charged with a felony or misdemeanor but that were not convicted. Panel B shows the distribution of credit scores for a convicted individual post-release. Panel C shows the distribution of credit scores for a convicted individual while incarcerated. The mean average credit score is similar in the first two populations, convicts and non-convicts, both groups observed after sentence. The mean credit score is visibly, and expectedly, lower for the population behind bars at the time of the credit report. The percentage holding loans is noticeably different between convicts and non-convicts; it stands at 45 percent for those found guilty and 52 percent for those acquitted. The percentage of individuals with mortgage loans in the sample stands at 13 percent. Similarly, 25 percent of the sample have auto loans. Credit card debt averages $3,844 for the 32 percent of the sample that has a credit card account.

We use a few different subsamples in our analysis. To reduce concerns about match selection we make two main restrictions. We use the no-recidivism restriction throughout the paper, except for Section (7) in which we restrict to prime borrowers. After showing that probation, fine, and bail do not affect the effects of incarceration, we further limit convictions to incarceration sentences. We refer to this as our main sample.[8]

3 Empirical Research Design

We begin by considering a basic empirical set-up with no endogeneity concerns. For person i who is arrested, we relate outcome, Y_i, such as credit score, to an indicator variable for whether the person was incarcerated in the past ($PastIncarceration$):

$$Y_{it} = \beta_0 + \beta_1 PastIncarceration_i + \beta_2 X_{it} + \epsilon_{it} \tag{1}$$

where X_i is a vector of control variables, including current incarceration status, and ϵ_i is the error term. We are interested in the post-release effects of incarceration, which are captured in β_1.

[7] Hispanic population made up 32.9 percent of the population in 2000.

[8] We show in Appendix (D) that focusing on individuals with either no recidivism or prime borrowers leads to no selection.

To identify the impact of incarceration on financial health, a researcher must address the problem of bias. For example, there may be a positive correlation between incarceration and factors such as severity of the crime, criminal history, and characteristics of a person that are also likely to be correlated with credit utilization and history. On the other hand, the process of incarceration generates a selection bias whereby individuals with a greater taste for crime (i.e., higher "criminal type") and better unobservables are more likely to be incarcerated. For instance, holding income constant, individuals with a taste for crime may be more likely to engage in criminal activity. As such, the amount of income needed to dissuade a high criminal type individual from engaging in criminal activity will be higher than for an individual with low criminal type. Post-conviction, this will generate a positive correlation between criminal type and unobservables that will bias OLS estimates upwards.[9]

Our empirical strategy resolves these concerns by using the quasi-experimental variation that arises because criminal cases in Harris County are randomly assigned to courtrooms staffed by judges with different propensities to incarcerate, conditional on offender and offense. We thus use courtroom assignment as an instrument for individual i's final sentence, and interpret any post-assignment difference in credit outcomes as the causal effect of incarceration associated with judges' differences in average harshness. For each individual, we condition on the individual being previously sentenced to incarceration, and then proceed to instrument past incarceration status, $PastIncarceration_{it}$ using a judge fixed effects specification:

$$PastIncarceration_{it} = \pi_0 + \tau_t + \pi_1 Court_i \otimes \tau_t + \epsilon_{it}.$$

where τ_t is the year of disposition. This set-up can be viewed as using marginal cases where judges may disagree about confinement decisions, a margin of policy relevance (Dobbie and Song 2015).

3.1 Instrumental Validity

We first verify that our instrument affects sentencing outcomes, but is uncorreated with initial case characteristics. Specifically, we conduct an F-test of the joint significance of the coefficients in π_1. We repeat this procedure with sentencing outcomes to establish the instrument relevance based on average courtroom differences. Table (2) presents results for this exercise.

While we cannot directly test the exclusion restriction, we can provide evidence consistent with the condition being met. First, judge harshness must be uncorrelated with those characteristics of

[9] We discuss further and formalize this intuition in Appendix B.

the defendant and of the case that might affect the defendants' outcomes of interest. We provide three pieces of evidence in support of random assignment: (i) *prima facie* evidence of randomization stemming from the court rules themselves (see Section (E.2)); (ii) empirical evidence in support of randomization using pre-existing characteristics of the defendant and of the case (Section (E.3)); and (iii) empirical evidence showing that the inclusion of pre-existing characteristics of the defendant and of the case into the first stage estimation does not meaningfully change the results.

To identify the local average treatment effect (LATE) of incarceration, it also must be the case that assignment to a strict judge need not increase the likelihood of incarceration for each type of offender—in other words, the sentencing patterns of judges are monotonic. To test for monotonicity, we conduct several additional tests, with results presented in Appendix F. We first show that the relationship between a judge harshness and the percentile rank across different judges is increasing for different types of offenses. Relatedly, we examine the first stage, and show that the instrument is positively correlated with convictions for several subsamples along demographic characteristics, judge conviction rates, use of credit before disposition, and types of offenses. Finally, following Bhuller et al. (2018), we perform a "reverse-sample" test in which we construct the instrument excluding the subsample to be evaluated and show that there is a positive relationship between the reverse-sample instrument and each subsample along demographic characteristics, use of credit before disposition, and types of offenses. Finally, we note how Bhuller et al. (2018) highlight that judge randomization guarnatees that characteristics of offenders *prior* to case assignment do not determine outcomes. However, this does not guarantee that decisions *aside* from the decision to incarceration do not drive our main effects. In Appendix Table (2).

4 Causal Effects of Incarceration on Access to Credit

We now examine the main effects of incarceration on restricting access to credit for ex-offenders. We focus on the change in credit scores as a measure of financial health, as well as the effect on access to financing for two important durable goods—namely, automobiles and housing.

4.1 Credit Score and Terms of Credit

Credit scores are a summary measure of creditworthiness and take into account payment history, credit utilization, inquiries, and credit length of the borrower.[10] The largest components in calculating credit scores are payment history (which receives 35 percent weight) and credit utilization (30 percent weight). When persons become incarcerated, their ability to service debt is affected by the inability to make payments, and thus their payment history will likely suffer. Similarly, if an individual's income decreases due to incarceration, her credit utilization will go up as she substitutes lost income with debt. This means that the credit score for a formerly incarcerated individual is likely to go down.

We show that this is indeed the case in Table (3). Columns (2)–(8) show that as a consequence of incarceration, credit scores for former inmates decrease by 42 to 68 points relative to their pre-incarceration levels. Columns (6)–(8) also show estimates of the interaction between incarceration and pre-incarceration credit score. Since the addition of the pre-incarceration interaction term does not materially change the general effect due to incarceration, this suggests that the effects of incarceration on credit scores are relatively independent from the pre-incarceration borrower's credit history and hovers around a 50–point drop in credit score. None of the specifications capture a statistically significant effect of this interaction, suggesting that any additional drop in credit scores experienced by crime-prone individuals does not operate through unobservables that determine higher credit scores.[11]

A lower credit score has been connected to lower access to credit, higher interest rates, and generally worse terms of credit. Furletti (2003) estimates that for pre-recession credit card holders, the difference in charge yield between a borrower with good credit and a subprime borrower (below 620) hovers around 8 percent. A drop of 50 points in credit score can lead to an increase in charge yields of up to 4 percent. Using the estimates from Furletti, the drop of around 50 points in credit score due to incarceration implies that an individual of a moderately good credit score (725) would have to pay an additional 1.5 percent in charge yields as a consequence of going to prison. The effect is stronger for a borrower with a 700 credit score, who would have to pay an additional 3 percent. And as pre-incarceration credit scores go down, the additional charge yield goes up.

[10] For details, see https://equifax.com/personal/education/credit/score/how-is-credit-score-calculated.

[11] Indeed, we will show in Section 4.3 that assessed income for high criminal types is differentially more affected by incarceration than assessed income for low criminal types.

4.2 Effects on Access to Durable Goods

In this subsection we evaluate the effects of access to credit on the consumption of durable goods. We analyze effects on auto and home loans, as these are widely-accepted measures of both durable consumption and investments.

4.2.1 Access to Housing

We first examine the effects of incarceration on receiving a home loan. The importance of housing for welfare has been evaluated extensively in the literature (e.g., Green and White 1997, DiPasquale and Glaeser 1999). For example, lack of housing has been linked to worse health outcomes and lower levels of child educational attainment.

To test for home loan effects, we estimate Equation (1) as a linear probability model where Y is a dummy for having a mortgage loan. Table (4) presents our results, which indicate that incarceration leads to a 14–16 percent decline in the likelihood of having a mortgage. All estimates control for current credit scores. Column (1) presents the OLS estimate between having a mortgage and incarceration for the full sample, which contains individuals that may or may not have other loans (i.e., individuals that might be only loosely attached to the formal credit market). We observe a drop of 8 percent. Using the judge-based IV strategy we find a drop of 15 percent (column (2)). The estimate does not change significantly when we restrict to those borrowers with other loans, or add controls (columns (3)–(4)).

While outside the scope of this paper, it is worth noting briefly that reductions in access to housing have important downstream effects. Beyond health and education effects, homeownership also helps households to accumulate wealth. By access to housing, incarceration may force families into poorer neighborhoods—overcrowding affordable housing, and placing together the poor with individuals who are already at risk for crime (Desmond, 2016).

One difficulty of evaluating housing consumption is that housing is a commitment good and adjusting the level of its consumption is costly (Shore and Sinai 2010). A household may not find it optimal to sell its house and buy a smaller one following small transitory losses in income but when facing or anticipating a large income loss, the household may find it optimal to actually adjust their housing consumption. Ex-convicts face a permanent loss of income following incarceration that would induce them to reduce their housing consumption. As a benchmark, it is worth noting the state of the literature on consumption based on both housing and nonhousing wealth. Using aggregate data, Carroll, Otsuka, and Slacalek (2010) estimate an immediate marginal propensity to consume of 2 to 9 cents for each dollar of housing wealth. Case, Quigley, and Shiller (2005) estimate the MPC from housing wealth to be between 3 to 4 percent, while their estimates of the MPC out of nonhousing wealth are small and insignificant. Focusing on consumption out of total wealth, Ludvigson and Steindel (1999) find an effect of 2-4 percent. In general, studies find that the marginal propensity to consume out of changes in housing wealth is generally higher than for changes in nonhousing wealth. In our specification, the drop in mortgages is lower than the drop in auto loans by a factor of 1.5 to 3. This is consistent with the evidence from earlier work on consumption out of housing and non-housing wealth using aggregate data.[12]

4.2.2 Auto Loans

Auto purchases are one standard measure of consumption. DiMaggio et al. (2018) document, for example, how pre-Great Recession positive credit shocks led households to increase monthly car purchases.[13]

But how does household consumption respond when instead they experience a *negative* economic shock? Columns (5) through (11) of Table (4) present results using car loans as the measure of consumption. Incarceration spells generate a more than 24 percent decrease in the likelihood of having a car loan, and a 14–28 percent drop in total auto debt. In columns (7) and (8) we analyze how incarceration affects the likelihood of having an outstanding balance on an auto loan account conditional on a borrower having at least one other type of loan (i.e., individuals highly attached to the credit market). We find that incarceration leads to a drop of 34–45 percent for individuals highly attached to credit markets. The inability to obtain an auto loan has deep consequences. Lack of transportation restricts a person's ability to get or keep a job and even to bargain wages. In addition, difficulty getting a car loan makes a borrower vulnerable to predatory lenders.[14]

[12] Studies using microdata, have found more varied effects, however. Campbell and Cocco (2007) find MPCs based on housing wealth of 6-11 percent. In contrast, Disney, Gathergood, and Henley (2010) find that the MPC out of housing wealth decreases to 1 percent after controls for future financial conditions are included, suggesting that financial conditions play a first-order role on the relationship between housing wealth and consumption.

[13] Relative to other durable goods, about 80 percent of car buyers utilize financing through dealerships; auto dealers are the creditor in most transactions (Davis, 2012). This detachment from conventional credit markets, coupled with a high ratio of new car sales to total auto sales (DiMaggio, Kermani, and Ramcharan 2017), make auto purchases a suitable measure of consumption.

[14] The issue of subprime auto lending has received attention by Congress and the CFPB. See, for example, *United States. Cong. House* (2009).

4.3 Heterogeneous Effects

We conclude this section by exploring a few key sources of heterogeneity in our primary effect regarding how incarceration affects access to credit. Specifically, we examine heterogeneity across an individual's pre-incarceration assessed income and underlying taste for criminal behavior.

4.3.1 Heterogeneous Effects Across Income

We first assess the heterogeneous responses of incarceration across varying levels of pre-incarceration assessed income. Recent work demonstrates the myriad ways that incarceration can affect future income—by stigmatizing job-seekers (since criminal records often surface during job applications) or by reducing the wage bargaining power of a worker who has been out of the labor force while confined (Pager, 2003). As we show in Appendix A, individuals with higher pre-incarceration income should experience larger drops in post-confinement income. Recent work on criminal justice policy and labor market performance finds evidence consistent with this prediction. Mueller-Smith (2015), for example, documents that labor market impacts of incarceration are concentrated among individuals with better pre-criminal charge earnings.

We perform a similar test, limiting our sample to those individuals for whom we can estimate their income prior to charging. To test for similar economic effects on credit access, we jointly instrument for incarceration and the interaction between pre-charge income (Y_{pre}) and incarceration:

$$Y_{i,post-trial} = \beta_0 + \beta_1 \widehat{PastIncarceration}_{it} + \beta_2 \widehat{PastIncarceration}_{it} \times Y_{i,pre-trial} + \eta_{it} \quad (2)$$

$$s.t. \quad \begin{cases} PastIncarceration_{it} = \pi_0 + \pi_1 Court_i \otimes \tau_t + \pi_2 Court_i \otimes (\tau_t \times Y_{i,pre}) + \tau_t + \epsilon_{it} \\ PastIncarceration_{it} \times Y_{i,pre} = \pi'_0 + \pi'_1 Court_i \otimes \tau_t + \pi'_2 Court_i \otimes (\tau'_t \times Y_{i,pre}) + \tau'_t + \epsilon'_{it}. \end{cases}$$

Table (A.3) presents results, which confirm that (relatively) high-income earners are indeed affected the most after being released. In column (1), the incarceration-income interaction is strongly negative—a drop of 0.26 percent per additional percentage point of pre-charge assessed income. As a placebo exercise, in column (2), we limit our sample to the pre-incarceration period only. The interaction here compares post-trial income in 2006 against pre-charge income in 2013. Reassuringly, we observe a weakly positive effect. Columns (3)–(4) show alternative specifications with different controls and show that the estimate of the interaction of pre-assessed income and incarceration for individuals convicted after 2006 is consistently around 30 percent. These results are consistent with existing research (and our conceptual intuition) that low earners are less-dependent on formal employment, and that the threat of confinement is less costly in terms of their negotiating benchmark in the labor market.

4.3.2 Heterogeneous Effects by Criminal Type

Conditional on conviction, individuals with high criminal types are more likely to have higher incomes pre-conviction and face steeper drops in credit afterwards as a result of their reduced labor income. The reason for this, which we formalize in Appendix A, is that an individual with a high taste for crime (the criminal "high type") needs greater incentives—income, in this case—to be dissuaded than an individual with a low taste for crime. This has important implications, since criminally-prone individuals may be lured away from good jobs into other activities after spells of incarceration, such as future crime.

We thus expect the pre-conviction assessed income of ex-convicts to increase with their criminal type (see Remark 2 in Appendix A and Figure B.1 in Appendix B.1). This tell us that high crime-type individuals have greater drops in credit outcomes. This is confirmed by our results. In Figures (B.2)-(B.5) we can see that high criminal type individuals have a greater drop in credit scores and probabilities of having auto loans, mortgages, and loans in general. In Figure (B.2), we see that the effect on credit score recovers slightly with time, especially after seven years, when flags of default often disappear from the credit record. Recovery is not complete, though, as low income makes it harder to sustain lower levels of utilization (DiMaggio et al., 2018).

There are several reasons why individuals prone to crime are more affected than individuals with lower type. First, as we have emphasized, in expectation high criminal types have higher pre-incarceration income—meaning larger falls down the job ladder. Moreover, incapacitation may produce deterioration of productive human capital and and the building of criminal capital as well (Bayer et al., 2009). The fact that this population is more adversely-affected than average has important consequences for reentry, given their propensity for criminal behavior.

5 Mechanisms: Obstacles to Credit Access

Limits to credit access are driven by many factors. While incarcerated, households are unable to pay their debts, resulting in worse credit histories. Employers will conduct background checks on prospective employees, making it harder to find employment. Banks may also conduct background checks on households and infer a lower willingness to pay down debt obligations. We test for these in this section.

We will compare the changes in credit access for individuals who went to jail or prison with individuals who got probation. This exercise serves several purposes. Individuals that faced probation instead of jail/prison will not face incapacitation, are less likely to lose their employment, and hence, are less likely to face discrimination in hiring since they do not need to reenter the labor market. They will also not experience deterioration of human capital for time spent outside the labor force. They might experience a reduction in their bargaining position with their employer through a hampered ability to exploit outside options but are less likely to lose the gains due to their negotiating benchmark predating conviction.[15] Yet, a conviction appears in the criminal record regardless of the sentence. Furthermore, knowledge of the sentence requires additional in-depth inquiry which increases the search cost for a criminal background check.

Since the effects on ex-convicts of probation would reflect substantially fewer distortions on their labor market income and credit scores, but still would reflect discrimination in both credit and labor markets, we regard any effects on probation to be an upper bound on the total effect of discrimination. In Section 6, we will show that there is little evidence that these effects come from the discrimination in credit, suggesting that these effects operate in large part through labor market effects.

5.1 Incapacitation

Inability to repay debts leads to a decrease in credit for the formerly incarcerated. To test for immediate effects of incapacitation on credit history, we examine the intensive margin effects of incarceration on credit score. We do so by exploiting variation in sentence length, conditional on being sentenced to carceral confinement.

We present results of this examination in Table (5). In Columns (1) through (2), we analyze the effects of a conviction on credit scores. The results show that on average credit scores go down by about 24 points per year of incarceration. Upon closer examination, we see that about half of this drop comes as a consequence of time spent incarceration, while the other half comes as a direct consequence of going to either jail or prison. This is shown in Column (2). The coefficient of sentence length indicates there is a loss of around 47 points for each year incarcerated, in addition to an immediate drop of about 28 points. This suggests that individuals who are incarcerated face challenges in repaying their debt both at the onset of their sentence and that the effects are

[15] The negotiating benchmark and the flow of outside options are important determinants of wages in job-ladder model of wages. See Jarosch (2017).

compounded as the length of the sentence increases. In contrast, obtaining a probation indictment (Column (3)) shows virtually no change in credit scores with a statistically insignificant drop of around 1 point, which is expected as individuals facing probation do not usually lose their jobs. These effects are similar to those on labor income (Columns (4) through (6)) that we will discuss in the following subsections.

5.2 Assessed Labor Income, Credit Capacity, and Credit Screening Process

Creditors not only underwrite based on credit reputation (i.e., credit scores) but also based on credit capacity. Credit capacity is generally captured by debt to income ratios. As such, it will be affected not only by reductions in income, but also when a borrower substitutes income with debt. People replacing income with debt is well-documented in the literature. Sullivan (2008), for example, finds that unemployed households increase their unsecured debt by 11 to 13 cents per dollar following a decrease in earnings; this response is driven by households with few assets. Using the RAND American Life Panel, Hurd and Rohwedder (2013) note that 18 percent of unemployed households report using credit cards or borrow money to replace income. These dynamics are analogous to the seminal consumption smoothing findings of Gruber (1997). Using the Panel Study of Income Dynamics (PSID), he shows that following periods of joblessness, unemployment insurance helps smooth consumption across the unemployment spell.

Given the importance of debt to income for credit access, we examine labor market effects as a distinct mechanism. Recent research suggests that incarceration has adverse consequences on labor market performance. Most recently, MS (2015) shows (within Harris County) that both employment and earnings are adversely affected following incarceration. This effect of incarceration bears resemblance to the literature on the costs of job loss. For example, Jacobson, Lalonde, and Sullivan (1993) find that following displacement, workers suffer long-term losses of around 25%.

To test income loss as a mechanism for credit loss, we use assessed income estimates, as described in Section (3). Since lenders do not obtain information about income for all loans, assessed income is informative of lenders' inference of a borrower's income based on their credit use. Columns (5)-(8) of Table (2) provide our results. Past incarceration reduces assessed income by between 25 and 29 percent. These results are slightly lower than MS (2015) estimates, which are based on administrative earnings records.[16] In contrast, probation has a statistically insignificant

[16] However, the MS estimates are marginally insignificant while ours are very strong. The reason is important to understand. Low-income defendants are less likely to participate in the formal labor market even in the absence of incarceration, a point also made by MS. Our population, however, is that of those who have credit and who are,

drop of income of between 2 and 3 percent, while bail and fines register effects close to zero. The reason why incarceration has larger effects than probation and other sanctions may include depreciation of human capital, falling off the job ladder, or discrimination in the labor markets.

5.3 Voluntary Delevering?

Given the fact that former inmates experience a decline in income, it is natural to ask whether the reduction in credit they experience is a voluntary decision. A difficulty in answering this question lies in disentangling the reduction in credit that is demand versus supply-driven. That is, as a consequence of lost income, ex-convicts may seek less debt voluntarily, but they may be also facing harsher terms of credit. When individuals are released from confinement and reenter the labor force, their income is generally lower than their preconviction income. This alone will cause a decrease in the borrowing capacity of the individual, which will be internalized by both the lender and the borrower. Evidence suggests that in response to negative income shocks individuals may reduce their consumption (for evidence on a positive income shock see Parker et al. (2013) and Johnson, Parker and Souleles (2006)). There is also evidence that borrowers' *expectations* about home values prior to the Great Recession were an important driver of the crisis (see Adelino, Schoar and Severino, 2017). This evidence points to the possibility that ex-convicts are borrowing less due to lower income or low expectations of income growth but not because of lower access to credit.

To test for this channel, we analyze the effect of incarceration on borrower effort to obtain a credit account. We do so using Equifax's data on credit inquiries normalized by the total number of credit accounts due to incarceration.[17] An increase in inquiries would suggest that borrowers are not delevering voluntarily. However, since borrowers can be discouraged from applying for loans if they expect to be rejected, a decrease in inquiries would be inconclusive. Similarly, because of search costs, any estimates we obtain would be biased downwards, which implies our test provides a conservative assessment of whether delevering occurs in part because of lack of access to credit.

We present our results in Table (6). Incarceration leads to an increase of between 0.76 and 1.08 additional inquiries per credit account. The results are unsurprising, but highlight that reductions in access to credit are not a purely voluntary development from the borrower's perspective. To be clear, though, this does not imply that there is not some voluntary delevering, but we interpret it as evidence that supply side considerations are a main driver of reduced access to credit.

presumably, more tightly attached to the formal labor force.

[17] Equifax maintains information about both credit inquiries and credit accounts.

5.4 Screening and Stigma

Formerly incarcerated individuals may face harsher credit conditions if creditors believe the criminal record is informative about the individual's ability or willingness to pay. Even when income information is available, the bank could interpret the individual's criminal history as evidence of lower ability to repay. This could be so if the individual faces higher levels of unemployment risk following incarceration—i.e., if unemployed, she will be less likely to get another job. Similarly, the bank might use criminal history to assess the "character" of the borrower—if criminal history is a proxy for "character" that signals low or high willingness to pay relative to other borrowers with the same observables.

We will refer to "stigmatization" as occurring when borrowers with a criminal history get less credit than borrowers with the same observables while also default at an equal or lower rate. As can happen with reduced employment due to criminal history, stigma sends lenders an incorrect signal about the borrowers' willingness to pay. And also similar to labor income, stigma operates as a friction in the credit markets that generates advantageous selection of borrowers. In contrast to reduced employment/income, which affects the creditors' decision to lend by muddling the inference made about borrower *unobservables*, stigma operates not through unobservables but *through the observability* of criminal history. Therefore a finding of no advantageous selection based on criminal history but a finding of advantageous selection based criminal type would suggest that stigma is not driving restricted access to credit, and instead lack of access to credit is operating through the labor market.

To fully test for stigma, we need to delve into an analysis of performance and advantageous selection. We will carefully undertake that analysis in the next section. For now, let us consider an alternative approach. We can estimate an upper-bound assessment of the presence of stigma in lending by analyzing the performance of individuals put on probation. Recall from our discussion at the beginning of this section that an individual sentenced to probation gets a conviction in their criminal record, but faces no removal from the labor force, no incapacitation to pay debts, and no depreciation of human capital due to being incarcerated. As we discussed previously and can see in Table (2), individuals facing probation have virtually nonexistent effects on credit scores (a statistically insignificant increase of 3-8 points) and their drop in income of just around three percent. Thus, the performance effects due to muddling of unobservables we obtain for individuals on probation will be smaller than for those individuals who are incarcerated, but the performance effects due to stigma must be the same for both groups. This makes the loan performance of individuals on probation a conservative assessment of the presence of stigma in lending.

Our results are in Table (7). In (Columns (5) and (8)) we can see that persons who underwent probation have about the same probability of 30-day default and bankruptcy, respectively, as individuals found not guilty. In contrast, individuals who are incarcerated have 41 percent lower 30-day defaults and 11 percent lower likelihood of bankruptcy (Columns (1) and (4)). For individuals who underwent probation, we could potentially only find some evidence of stigma in their 60- and 90-day default rates. Columns (6) and (7) show that they are less likely to default at 60 or 90 days than individuals found not guilty by 7 and 10 percent, respectively. Yet these numbers include the effect of unobservables muddled by lower income. We take this as evidence of low levels of stigma in lending and proceed to conduct a more in-depth analysis in the following section.

6 Informational Distortions, Selection, and Lender's Role

Lenders screen borrowers for default risk in large part using credit scores and income. Consider a borrower with an unfavorable credit history. Such a history may be attributable to an inability/unwillingness to repay debt, but also to life circumstances like illnesses or job loss. Lenders can use knowledge of these events to protect or improve profits. For example, during Hurricanes Irma and Maria, and more recently during the government shutdown, some lenders voluntarily instituted payment deferments for affected borrowers, confident that their clients would fulfill existing debt obligations at a later point.

However, by adversely affecting both credit history and earnings within the labor market, incarceration makes it harder for lenders to fully evaluate the repayment potential of formerly incarcerated borrowers. Specifically, unobserved heterogeneity may explain both the income and credit score differences for individuals who are incarcerated and those who are not.[18]

Lenders may be aware of these informational distortions, to the extent they also know a person's criminal history.[19] If so, they will be inclined to correct for such distortions to improve profitability. Lenders can potentially correct in two ways: (i) they can correct for the *average* informational distortion due to incarceration; and (ii) they can correct for *heterogeneity* in the

[18] If the lender only observes income, performance conditional on income does not only capture ability to pay but also those unobservables determining willingness to pay that are *correlated* with income—trustworthiness, for example. For instance, in the context of employment disparities between offenders and non-offenders, by paying less to workers that have been incarcerated, employers are effectively changing the signaling value of income regarding creditworthiness.

[19] For a sample loan application asking for criminal history, see Appendix C.

informational distortion (we documented the heterogeneous effects of incarceration in Section 4.3). To correct for an average informational distortion, it suffices to know the criminal history of the applicant. Since criminal history is verifiable to the lender, we will refer to this as correcting on observable information, and as such, we can test whether lenders are able to correct for the average portion of the informational distortion. To correct for heterogeneity in the informational distortion, the lender needs to account, directly or indirectly, for characteristics of the applicant that explain both incarceration *and* default risk. This information is plausibly unavailable to the lender and, thus, we will refer to this as correcting on unobservable information.

In contrast to criminal history, a person's propensity for crime is unobservable and unverifiable. However, we can proxy for this trait in the manner discussed in Section (4.3), allowing us to test whether informational distortions pass through to the screening process because of heterogeneity in informational distortions. The average "high-crime"-type individual will have better pre-conviction unobservables than an average (non-convicted) individual. As we documented in Section (4.3.2), crime-prone individuals experience a larger drop in both income and credit score. Since the lender cannot account for the relatively larger effect on credit occurring *because* of incarceration, we expect a negative correlation between default risk and criminal type. Assuming so, informational distortions pass through to the screening process. These effects, though, should be absent for those sentenced to probation, as their income and credit scores are less-affected by convictions.

In summary, we test the following:

(6.1) Performance of Former Inmates: whether former inmates default at lower rates than non-convicted individuals.[20]

(6.2) Correction of Informational Distortions for Observationally-equivalent Borrowers: whether the lender uses criminal history to mitigate the presence of asymmetric information. Since the lender cannot costlessly distinguish between types of conviction—i.e., jail/prison vs. probation—a correction intended to benefit former inmates will create or exacerbate an adverse problem for individuals who underwent probation.[21]

(6.3) Correction of Heterogeneous Informational Distortions: whether lenders can (or cannot) correct based on criminal type, since this information is unobservable to the lender. In other

[20] In the previous section, we saw individuals that undergo probation default at similar rates to non-convicted individuals, partly because they are less affected by the correctional system.

[21] This is because the observables (credit scores and income) of individuals who underwent probation were not equally affected by the correctional system.

words, high crime-type individuals who were incarcerated should perform better than non-convicted individuals with similar observables.

6.1 Performance of Former Inmates

We first examine the performance of loans given to the formerly incarcerated, ignoring adverse selection problems for the time being. Recall that formerly-incarcerated workers experience negative credit and labor market effects. Due to various unobservable characteristics correlated with credit, income, and criminal propensity, the lender might be unable to properly screen for the default probability of the loan given the negative income shock. As such, one might see observe *better* performance for formerly-incarcerated borrowers, conditional on obtaining a loan.

In Table (7), we establish that formerly-incarcerated individuals do in fact default at lower rates than non-convicts (ignoring negative borrower selection for now). Columns (1)-(4) present the loan performance of individuals going to jail/prison (Columns (5)–(8) present results on individuals sentenced to probation, see Subsection 5.4 for a discussion). Formerly incarcerated individuals experience 41 percent, 30 percent, and 20 percent fewer delinquencies after 30, 60, and 90 days, respectively, than non-convicted individuals. The better performance may be due to lower levels of credit access or several other factors: statistical discrimination, stigma, and suboptimal lending due to information frictions (i.e., muddling of unobservables). In this section we analyze these factors.

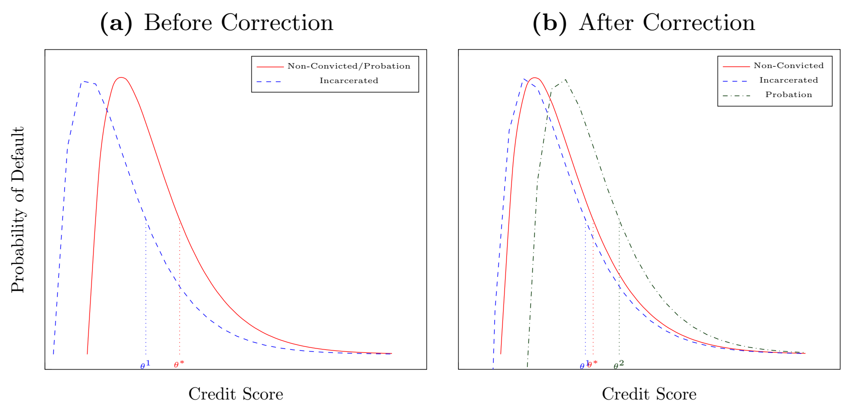
6.2 Can the Lender Correct for Informational Distortions? Testing for Adverse and Advantageous Selection

Incarceration may also lead to stigmatization of potential borrowers separate and apart from the negative stigma that attaches to ex-offenders as re-entrants to the labor market. In contrast to statistical discrimination, though, carceral stigma in lending is a friction that may prevent profitable lending. Thus, lenders that discriminate against former inmates due to the stigma of incareration (independent from any stigma effects in the labor market) should have advantageous selection—i.e., better borrowers conditional on screening criteria. Conversely, if lenders simply statistically discriminate in response to the lower repayment ability of the incarcerated population, the lender should face no advantageous selection and possibly some degree of adverse selection.

We test for the presence of adverse selection by following the positive correlation test of selection introduced by Chiappori and Salanié (2000).[22] The intuition of the test is that for observationally-

[22] For an application of this test in an analysis of asymmetric information in lending markets, see Crawford, Pavanini, and Schivardi (2017).

Figure 1: Hypothetical Lender Correction for Informational Distortions



NOTES: This figure shows hypothetical distributions of default by credit score (holding income and other observable traits equal) for: (1) non-convicted individuals; (2) convicted individuals who are sentenced to incarceration; and (3) convicted individuals who are sentenced to probation. Panel (a) shows the default probability distribution after trial but with no adjustment by lenders. The default distribution for individuals who go to jail or prison shifts to the left as their inability to service debt while incapacitated obscures their true default probability post-release. Individuals who undergo probation do not face this challenge and, hence, their default distribution equals that of non-convicted borrowers. For a fixed credit score threshold θ^*, the lenders will forgo profits by not lending to formerly incarcerated individuals with credit scores between θ^1 and θ^*. Panel (b) shows the default probability after lenders adjust for incarceration effects. Lenders can only see a conviction and cannot distinguish between incarceration and probation. As a result, both the default distribution for the formerly incarcerated and for those that went on probation shifts to the right. By doing so, lenders are able to recover profits from the ex-incarcerated population. However, they endure losses stemming from individuals that went on probation ($\theta^2 > \theta^*$).

equivalent borrowers, a positive correlation between incarceration and default suggests an asymmetric information problem that systematically explains both crime and default. As such, a positive correlation implies adverse selection when lending to ex-offenders. Conversely, if this correlation is negative, there is advantageous selection. We conduct this test in the spirit of outcome tests proposed by Becker (1957, 1993) to detect taste-based discrimination in lending against minorities.[23]

6.2.1 Selection and Incarceration

Following Chiappori and Salanié (2000), we implement the following bivariate probit selection test:

$$
\begin{aligned}
PastIncarceration_{it} &= \mathbb{1}(X_{it}\beta + \nu_{it} > 0) \\
default_{it} &= \mathbb{1}(X_{it}\gamma + \eta_{it} > 0)
\end{aligned}
\tag{3}
$$

$PastIncarceration_{it}$ is a dummy for whether person i was ever incarcerated at time t, and $default_{it}$ is a dummy outcome variable indicating if i defaulted on a debt obligation (defaults

[23] Such tests are used today to analyze discrimination in other settings, such as policing (Knowles, Persico and Todd 2001).

include nonpayment during the past 30, 60, or 90 days, as well as bankruptcy filings). The intuition for this test is that, with adverse selection, unobservables ν_{it} leading to incarceration should be correlated with unobservables η_{it} that lead to default.[24] In a competitive market, ρ should be (weakly) positive (Chiappori and Salanié 2000). Since stigma and muddled information prevent optimal lending, though, these frictions imply a negative ρ. ρ thus provides an upper bound on the stigma level; ρ near zero suggests little stigma, while positive ρ indicates adverse selection.

Table (8), columns (1)-(2) summarize our results. We focus on the correlation, ρ, between residual traits ν leading to default and residual traits, η, leading to a conviction. Table (8) suggests that when lenders screen based on observable information (credit scores, income, age), residual traits do not explain differences in default rates — inconsistent with lenders stigmatizing the ex-incarcerated. Column (1), which describes screening based on credit scores, shows a correlation ρ very close to zero for defaults of all lengths, and also for bankruptcies. Column (2) describes screening based on credit scores and assessed income, and also shows a correlation ρ very close to zero for 3 of 4 default outcomes, although there is modest evidence of adverse selection by the 90-day defaults metric.

6.2.2 Favorable Discrimination?

As we stated before, ρ contains information of both stigma and muddled information. A ρ near zero not only implies that the likelihood of stigma is low, but also that there might be active effort from lenders to reduce the informational distortion generated by incarceration. We use the same approach from previous sections of exploiting the equal conviction signal for offenders sentenced to incarceration or probation (and who thus face different income/credit effects). If lenders seek to resolve the information distortion, we should see evidence of adverse selection for individuals sentenced to probation. Results are in columns (5) and (6) of Table (8). From column (5) we see that, after the lender screens based on credit scores, performance for individuals sentenced to probation is worse. They exhibit strong positive correlation between conviction and 30-day, 60-day, and 90-day delinquencies and, also, a strong positive correlation with bankruptcy. That is, by lending to individuals sentenced to probation, lenders face adverse selection. The finding persists after we account for the lender screening also on income, as column (6) also shows strong positive correlations ρ for all default measures.

[24] Conversely, for a correlation coefficient between ν_{it} and η_{it}, ρ, a negative and significant value signifies advantageous selection.

To summarize the results in this subsection, we find little evidence of discrimination in credit markets on the basis of criminal status alone (i.e., the stigmatization of ex-offenders by lenders). On the contrary, there is some, albeit weak, evidence of favorable treatment by lenders. Formerly incarcerated individuals are less likely to receive a loan than those not incarcerated, but they are only marginally less likely to default than those not confined. And when we extend the analysis to individuals sentenced to probation, we find that there is adverse selection, consistent with the idea that lenders can use observable information (i.e., a criminal record) to correct informational asymmetries arising from frictions in the interplay between labor markets and incarceration. In contrast with this subsection, where we have evaluated the role of potentially observable information, we now turn to evaluating the role of *unobservable* information to further show how labor frictions generated by incarceration spill-over to credit markets.

6.3 Can the Lender Correct for Heterogeneous Distortions? Selection and High-crime Types

As we discussed in Section (4.3.2) (as well as Appendices A and B), average pre-incarceration income is increasing in criminal type. We have also shown that losses in credit scores and overall access to credit are greater for higher criminal types. Following our discussion, the estimated residual of formerly incarcerated status on court-year fixed effects captures an individual's propensity for crime. As in the last section, we run a correlation test à la Chiappori and Salanié (2000), but we include court-year fixed effects as controls in our specification:

$$\begin{aligned} PastIncarceration_{it} &= \mathbb{1}(\beta_0 + \tau_t + \beta_1 Court_i \otimes \tau_t + X_{it}\beta + \nu_{it} > 0) \\ default_{it} &= \mathbb{1}(\beta_0 + \tau_t + \beta_1 Court_i \otimes \tau_t + X_{it}\gamma + \eta_{it} > 0) \end{aligned} \tag{4}$$

where the main difference between equations (3-4) and (4-5) is the inclusion of court-year fixed effects. The inclusion of the fixed effects lets the residual ν_{it} be our estimate of criminal type.

The interpretation of a correlation is different from the previous section. A positive correlation means that high criminal types are less likely to repay, lending support to using criminal history as a proxy for "character" at least in the lending context. A negative correlation, however, would suggest that high criminal types have better repayment ability than it is understood by lenders, which is consistent with our findings thus far and the analytical framework put forward in Appendix A.

Table 8 presents results for this sub-analysis. As we explained above in the heterogeneity results

(Section 4.3), individuals with higher tastes for crime are relatively less likely to receive credit than the average ex-offender. Here, we want to examine if this differential reduction in credit access is due to informational distortions. To perform that inquiry, we now consider the performance of a loan after taking into consideration both incarceration and criminal type (Columns (3) and (4) of Table 8). When criminal type is considered, loans substantially overperform (ρ <0) relative to their unincarcerated counterparts in all categories—30-, 60-, 90-day delinquencies as well as bankruptcy. In column (4) we see a similar pattern for all default measures except 90-day defaults. These results highlight that lenders are unable to fully correct for informational distortions, and that heterogeneity in the effects of incarceration on credit applicants are unaccounted for when allocating credit. To further examine whether informational distortions cause a negative correlation between borrower traits explaining both defaults and propensity for crime, we look at individuals sentenced to probation rather than incarceration as a placebo test. The intuition is that probation should have a smaller effect on an individual's income. In Columns (7) and (8), we see that heterogeneity does not affect loan performance for those who faced probation instead of incarceration.[25]

To sum up this subsection briefly, we have shown here that incarceration not only affects the credit access of ex-inmates, but that the effects are stronger for those with greater propensities for criminal behavior. Presumably, if under more favorable conditions individuals engaged in crime, all else equal, after facing lower access to credit the likelihood of reengaging in crime must be high, especially for high types. This begs the question: Does lack of access to credit lead to recidivism? We approach this question in this following section.

7 Lack of Access to Credit and Recidivism

In general equilibrium, a reduction in credit access has an ambiguous effect on future crime. On one hand, a reduction in future credit access may act as a deterrent against crime. On the other hand, for formerly incarcerated individuals, reduced future credit lowers the opportunity cost of new crimes. In general, evidence on the deterrence effects of criminal punishment is mixed. Levitt (1996) finds that incarceration has strong deterrent effects. However, other studies have failed to show deterrence effects stemming from either longer sentences (Lee and McCrary 2017) or the

[25] These results are also consistent with our conceptual analysis in Appendix A and provide additional evidence of the spillover effects from frictions in the labor market to credit. Labor income and credit scores are proxies for both ability and willingness to pay of a prospective borrower. When there is a disconnect between the information contained in the proxy, income or credit score, and the characteristics of interest, ability + willingness, banks will under provide credit to borrowers with a criminal history.

death penalty (Donohue and Wolfers 2009). Assuming, for example, that the threat of death has a higher order effect than the threat of worsened access to credit, we should expect the deterrence effect of lack of access to credit to be nil.

Acknowledging the conceptually ambiguous effects of credit on crime, in this section we analyze whether lack of access to credit increases the likelihood of recidivism for the formerly incarcerated. To do so, we exploit an approach set forward on Agarwal et al. (2017). We exploit discontinuities in credit limits for borrowers. Before proceeding to our estimation equation, we find it useful to provide some context on the modeling practices of the lenders.

7.1 Estimation and Validity of Credit Limit Discontinuities

When setting out credit limits, lenders establish their tolerance for risk of default given observables. Lower credit scores generally imply a higher likelihood of default. A common practice of banks is to set out credit limits based on cutoff scores, wherein a borrower just below the cutoff score would receive a different credit limit than a borrower just above the cutoff (FDIC, 2007). Agarwal et al. (2017) show that this process can be optimal when there are fixed costs to determine the optimal contracting terms for similar borrowers.

Even though documentation of the general practices of lenders is readily available, specific lenders and precise cutoffs are unobservable to the researcher and must be estimated. Following closely the procedure set forth in Agarwal et al. (2017), we average credit limits by 5-point risk score bins while restricting our sample to borrowers with credit above 600 (credit scores below 620 are generally considered subprime). From Figure (5), we can identify candidate credit score discontinuities. To formally detect these discontinuities, we run threshold regressions following Hansen (2000):

$$\begin{aligned} \log(CL) &= \delta_1 CS + \eta \qquad \text{if } CS \leq \gamma \\ \log(CL) &= \delta_2 CS + \eta \qquad \text{if } CS > \gamma \end{aligned} \tag{5}$$

where CL is the credit limit, and the credit score, CS, is both the regressor and the threshold variable used to split the sample into two groups or regimes. Our credit limit discontinuity is the estimate of our threshold, γ.[26] We sequentially estimate the remaining credit limit discontinuities by performing threshold tests in each of the regimes.[27] Following this procedure we obtain six

[26] Endogeneity of the estimate γ is not a concern as threshold estimates are super-consistent.
[27] A more rigorous approach can be found in Gonzalo and Pitarakis (2002).

quasi-experiments in the form of credit limit discontinuities at credit scores of: 625, 665, 700, 735, and 770. The results of the LM test for the presence of a discontinuity are shown in Figure (6). To our list of quasi-experiments we add 640 and 655 based on our reading of Figure (5). We pool our seven credit limit quasi-experiments to perform a regression discontinuity analysis in the next section.

In Figure (7), we show the behavior of applicant characteristics around the pooled cutoff, $\bar{\gamma}$. Panels A and B show credit outcomes—in particular, in credit limits and, to a lesser extent, number of credit accounts—are smoothly increasing in credit score except at the cutoff where there is a discontinuous jump. Panel C and D show applicant characteristics typically taken into account during the credit process—estimated income and age are expectedly positively correlated with credit score but exhibit no discontinuous jump, remaining smooth at the cutoff. In Panels E and F, we show applicant characteristics related to their past criminal history—conviction and sentence—and both are also smooth around the cutoff.

7.2 First Stage

Since assignment to each side of the cutoff may depend on other applicant characteristics we implement our estimated credit limit discontinuities in a fuzzy RD research design. In addition, since we are interested in assessing the effects of credit on recidivism, we must supplement our fuzzy RD design with the random judge assignment strategy we have followed thus far for the rest of the paper. We can implement both simultaneously in instrumental variable form. Following Calonico, Cattaneo, and Titiunik (2014), we estimate the optimal bandwidth h to be 12 credit score points. The first stage takes the form:

$$\begin{aligned} CL_{it} = \beta_0 + \beta_1 Past\widehat{Incarceration}_{it} + \beta_2 RD_{it} + \beta_3 Past\widehat{Incarceration}_{it} \times RD_{it} + \\ \beta_4 CS_{i,pre-trial} + \beta_5 Past\widehat{Incarceration}_{it} \times CS_{i,pre-trial} + \beta_6 RD_{it} \times CS_{i,pre-trial} + \\ \beta_7 Past\widehat{Incarceration}_{it} \times RD_{it} \times CS_{i,pre-trial} + \epsilon_{it} \end{aligned}$$

where $RD = \mathbb{1}[CS_{i,pre-trial} > \gamma]$. We have allowed the relationship between pre-trial credit scores, CS, and credit limit, CL, to vary above and below the cutoff, and also to vary for formerly incarcerated and never incarcerated individuals. We are interested in β_2 and β_3.

The results of the estimation are reported in Table (9) Panel A. Column (1) reports OLS results whereby an individual with pre-trial credit scores above the cutoff will still enjoy a higher credit limit after incarceration. Instrumental variable estimation shows this is not the case. Columns (2) to (4) show that the drop in credit limit for a former convict with pre-trial credit scores above the cutoff is between \$4,360 to \$6,960 higher than for a former convict with pre-trial credit scores below the cutoff. This is natural, as individuals with more available credit have more opportunity to default on their debt and, consequently, to adversely affect future borrowing ability.

7.3 2SLS Estimates

In the previous subsection we showed that there is indeed a decrease in credit caused by incarceration. We are interested in assessing whether these reductions in credit due to having been incarcerated increase a formerly incarcerated individual's likelihood of recidivating. Conceptually, an ideal experiment would allow us to see the effects of incarceration on two groups randomly assigned to have either high or low credit prior to treatment. To that end, we use our discontinuity in credit limits to sort out individuals by their credit access prior to incarceration. We implement this using 2SLS based on the following specification:

$$\begin{aligned} Recidivism_{it} = \beta_0 + \beta_1 Past\widehat{Incarceration}_{it} + \beta_2 \widehat{CL}_{i,pre-trial} \\ +\beta_3 Past\widehat{Incarceration}_{it} \times \widehat{CL}_{i,pre-trial} + \beta_4 CS_{i,pretrial} \\ +\beta_5 Past\widehat{Incarceration}_{it} + \beta_6 \widehat{CL}_{it} \times CS_{i,pretrial} \times CS_{i,pretrial} \\ +\beta_7 Past\widehat{Incarceration}_{it} \times \widehat{CL}_{i,pre-trial} \times CS_{i,pretrial} + \epsilon_{it} \end{aligned} \tag{6}$$

where each regressor

$$\begin{aligned} \widehat{X} \in \{Past\widehat{Incarceration}, \widehat{CL}, Past\widehat{Incarceration} \times \widehat{CL}, CS, \\ Past\widehat{Incarceration} \times CS, \widehat{CL} \times CS, Past\widehat{Incarceration} \times \widehat{CL} \times CS\} \end{aligned}$$

is instrumented jointly using court fixed effects and credit limit discontinuities as instruments.[28]

The effect of a credit reduction due to past incarceration on recidivism is given by:

$$\begin{aligned} \Delta Recidivism_{it} &= \mathbb{E}[\mathbb{E}[Recidivism_{it} | CL = CL_{pretrial}] | RD_{i,pre-trial} = 1, Inc_{it} = 1] \\ &- \mathbb{E}[\mathbb{E}[Recidivism_{it} | CL = CL_{pretrial}] | RD_{i,pre-trial} = 0, Inc_{it} = 1] \\ &= (\beta_3 + \beta_2)\mathbb{E}\big[\Delta CL_{pretrial \to postrelease}^{RD_{i,pre-trial}=0, Inc_{it}=1} - \Delta CL_{pretrial \to postrelease}^{RD_{i,pre-trial}=1, Inc_{it}=1}\big]. \end{aligned} \tag{7}$$

[28] This is:

$X_{it} = \pi_0 + \pi_1 RD_{i,pre-trial} + \pi_2 CS_{i,pretrial} + \pi_3 Court_i \otimes \tau_t + \pi_4 Court_i \otimes (\tau_t \times RD_{i,pre-trial}) +$
$\pi_5 CS_{i,pre-trial} \times RD_{i,pre-trial} + \pi_6 Court_i \otimes (\tau_t \times CS_{i,pretrial}) + \pi_7 Court_i \otimes (\tau_t \times CS_{i,pre-trial} \times RD_{i,pre-trial}) + \epsilon_{it}.$

Our results are shown in Table (9) panel B. Columns (2)–(4) show our results using IV estimation. A decrease in pre-trial credit limit of about \$1,000 increases recidivism by around 1.5 percentage points. Combining these estimates with the changes in credit limit from Panel A according to Equation (7), we find that within former inmates, those who lose credit the most (those with high pre-trial credit limits), experience increases the likelihood of recidivism between 6.4 and 10.5 percent points larger than low credit former inmates. Importantly, only about 48% of that increase in recidivism is due to better terms of credit *pretrial* that high-credit individuals had relative to low-credit individuals. The other 52% of the increase in recidivism (between 3.8 and 5.3 percent) is due to credit loses that exceed what formerly-incarcerated but low pre-trial credit individuals face. This is intuitive, as individuals with higher credit have more opportunities to default in their obligations. This is also in line with our earlier results about heterogeneity. Other comparisons are also informative. For example, when we compare formerly incarcerated high-credit individuals against never incarcerated high credit individuals, recidivism rates due to credit differences are expectedly higher, on the order of 18.5–20.3 percentage points.

The U.S. Sentencing Commission (2016) found that 49.3 percent of offenders were arrested again within eight years of being released from incarceration or being placed on probation. In our sample that number is 40 percent. These numbers have two implications: (i) increasing access to credit reduces criminal activity; and (ii) removing access to credit is more damaging than not having had credit at all. For individuals with no previous criminal history, the effects are expectedly smaller. Within the former inmate population, the additional likelihood of recidivism due to credit hovers around 1.2–3.8 percentage points for individuals with high pre-trial credit limits.

These results are consistent with the findings of Raphael and Winter-Ebmer (2001) that increases in unemployment have positive and significant effects on property crime rates and with those of Gruber (1997) on the importance of consumption smoothing during unemployment spells, and Herkenhoff, Phillips, and Cohen-Cole (2017) who find that credit allows individual to take on better matches. These are also consistent with the findings of Garmaise and Moskowitz (2006), who find that bank concentration increases property crimes. Lack of access to credit diminishes the efficacy of incarceration in deterring crime.

8 Conclusion

In this paper, we show that incarceration decreases future access to credit. We also provide evidence that access to credit for the incarcerated is hampered by: (i) inability to pay creditors while confined; and (ii) poor labor market outcomes after reentry. Restricted access to credit due to incarceration also significantly reduces the ex-incarcerated's durable consumption. We find little evidence, though, that there is discrimination within credit markets based on the stigma of incarceration alone.

Digging deeper into the mechanisms underlying out results, we show that labor market effects spill-over to credit markets, and that this has the consequence of amplifying the negative labor market shocks to workers. The interconnection between labor and credit markets amplifies challenges the ex-inmates already have when reentering the labor force following release from jail or prison. This is the case because the need of to smooth consumption and consume durables is impaired by the lender's inability to observe some individual traits of the borrower and because frictions in the labor market obfuscate the signaling value of income and credit history. In summary, crime policy and labor market practices can generate frictions in the credit market, as incarceration aggravates an information incompleteness problem.

Finally, we also show that lack of access to credit aggravates the problem of recidivism. Formerly incarcerated individuals are 6–11 percentage points more likely to recidivate following a decrease in credit that is caused by incarceration. This finding—together with our findings on how the inability of lenders to accurately assess default risk for high criminal type individuals—suggests that recidivism is compounded by the fact that individuals with high criminal types are more affected by incarceration than are individuals with low criminal types. In short, access to credit plays an important role in shaping the dynamics of crime.

Our findings have important welfare implications. As we have shown, lack of access to credit leads to higher levels of recidivism. Other studies have also shown that credit constraints lead to loss of human capital (Hai and Heckman, 2017), and that restricting access to credit harms poor households (Zinman, 2010). Overall, our findings suggest further reentry efforts are necessary to alleviate the consequences generated by the interplay of credit constraints and the carceral state. Moreover, an interesting question arises from the interaction between credit, consumption, and recidivism. If we extrapolate the findings of DiMaggio et al. (2018), and the behavior of ex-convicts following expansionary monetary policy mirrors results we observe in our study, we may expect monetary policy to affect crime.

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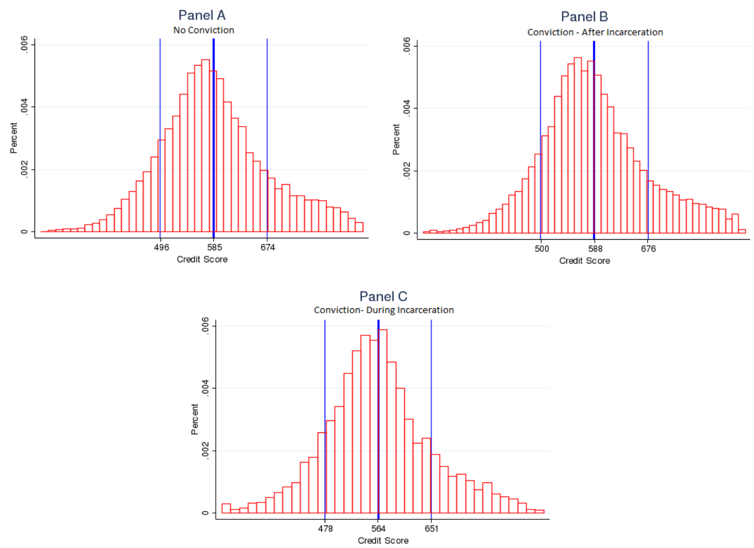
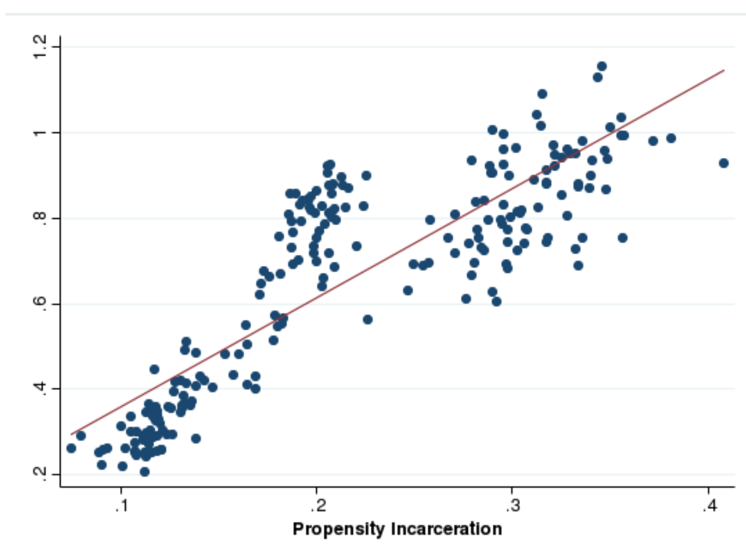
Figure 2: Distribution of Credit Scores



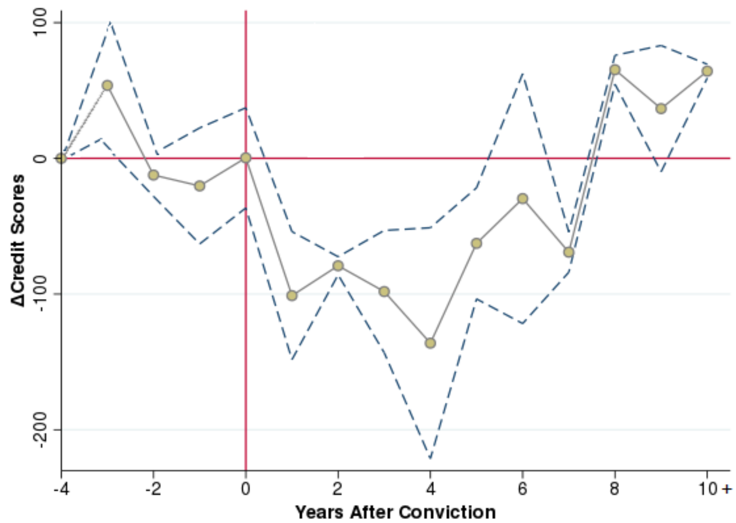
NOTES: This figure shows the credit score distribution for individuals not convicted, formerly incarcerated, and incarcerated at the time of credit report. All credit scores are taken after case resolution. The first, second, and third vertical lines indicate the 25th, 50th and 75th percentiles of credit score, respectively.

Figure 3: Relevance of Instrument



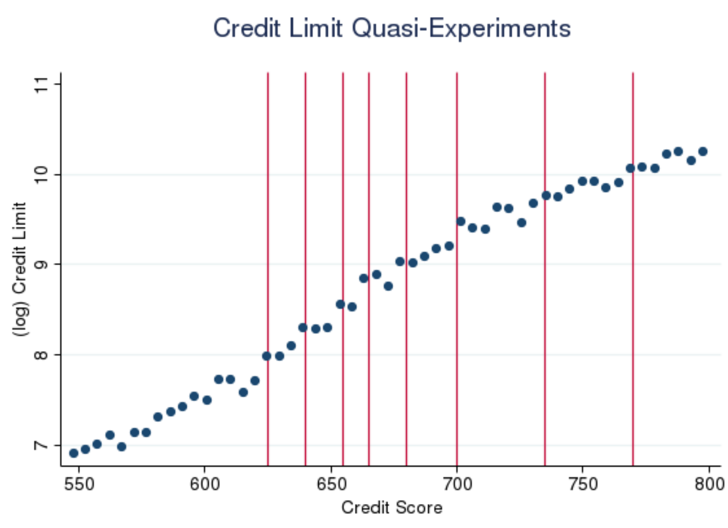
NOTES: This figure shows how judge harshness relates to incarceration by plotting court-year bins of individual incarceration likelihood and overall judge harshness. Judge harshness is the leave-one-out mean of incarceration rate for the assigned court at the year of disposition (verdict and sentence). To construct the binned scatter plot, we regress incarceration on year of disposition fixed effects and calculate residuals based on this regression. We take the average of residuals and judge harshness by each court-year bin.

Figure 4: Impact of Incarceration on Credit Scores: Event Study Estimates



NOTES: This figure presents event-time estimates for the impact of incarceration on credit scores, where time is normalized to be year zero at the year of court disposition. We jointly instrument for each year before or after the year of incarceration using court-year fixed effects, and cluster at the court × year level.

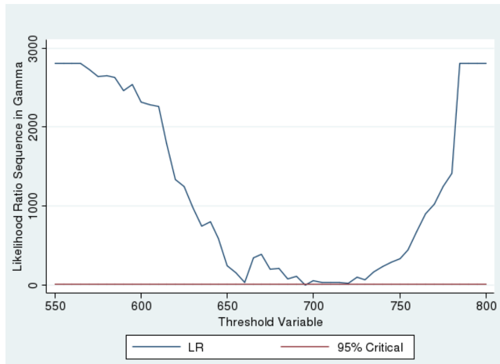
Figure 5: Credit Limit Quasi-Experiments



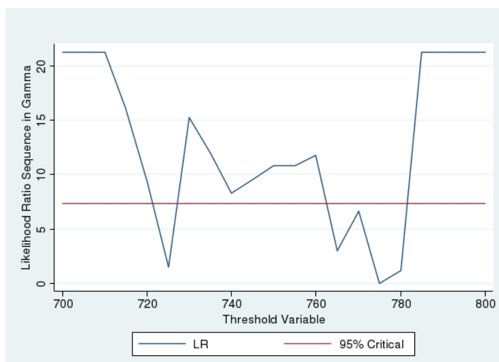
NOTES: This figure plots (log) credit limit against credit scores. To construct the binned scatter plot, we construct 5-point credit scores bins and take the average credit limits for each. The red lines indicate credit limit discontinuities for prime borrowers (credit score > 620).

Figure 6: Tests for Credit Limit Discontinuities

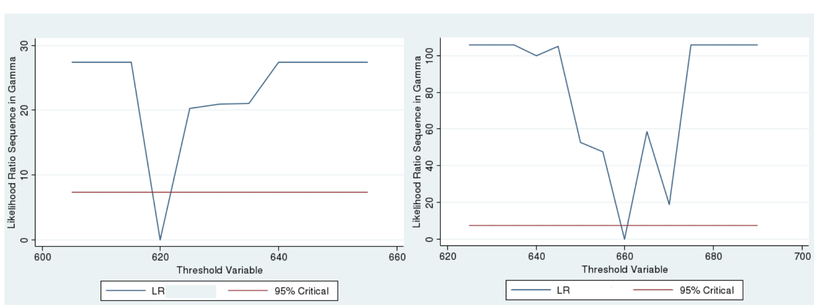
Panel A: Hansen Test For Global Threshold (γ_{global})



Panel B: Hansen Test For Upper Thresholds ($\gamma_{global} < \gamma$)

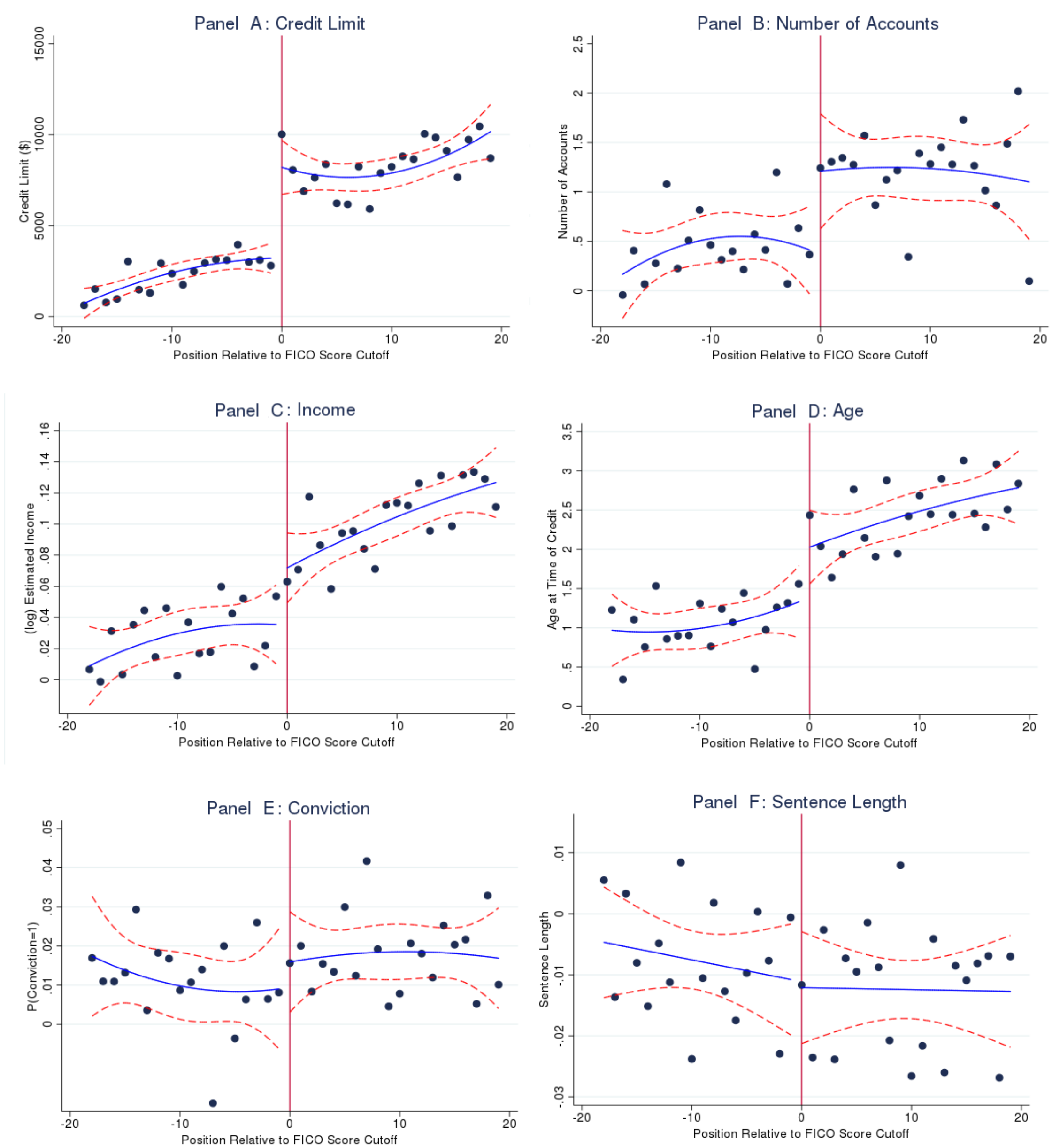


Panel C: Hansen Test For Lower Thresholds ($\gamma_{global} > \gamma$)



NOTES: This figure shows Lagrange multiplier (LM) estimates for the likelihood of the presence of a discontinuity, based on Hansen (2000). The Y axis shows the likelihood ratio of a discontinuity at a given credit score. The likelihood ratio crossing through the red line indicates with 95% confidence that there is a discontinuity. Panel A shows the main discontinuity (global) in credit scores. Panel B shows additional discontinuities above the global discontinuity. Panel C shows additional discontinuities below the global discontinuity.

Figure 7: Borrower Characteristics Around Credit Limit Quasi-Experiments



Notes: Each panel in this figure presents borrower outcomes or characteristics around the credit score discontinuity. To construct the scatter plot we pool all the discontinuities together and average at each credit score point above or below the cutoff. The optimal bandwidth in credit score for the sample, following Calonico, Cattaneo and Titiunik (2014), is 12. Panels A and B plot credit outcomes around the discontinuity. Panels C and D plot borrower characteristics typically used by lenders. Panels E and F show characteristics related to criminal history.

Table 1A: Summary Statistics for Full Sample of Individuals (N = 199,710 Person-Year Observations)

	Mean	Median	SD
General:			
Age	36.19	34.00	10.33
% Female	0.27	0.00	0.44
% Black	0.38	0.00	0.49
% Latino	0.22	0.00	0.41
Credit:			
Credit Score	575.12	569	82.99
Loans	0.47	0.00	0.50
(Log) Estimated Income	10.18	10.08	0.52
Loan Amt	55,285	22,241	95,730
Mortgages	0.13	0.00	0.34
Auto Loans	0.25	0.00	0.43
Incarceration:			
Age Sentence	31.39	29.00	10.01
Misdemeanor (out of Total Cases)	0.78	1.00	0.41
Lesser Offense (out of Felonies)	0.22	0.00	0.42
Recidivism (out of Convicted)	0.39	0.00	0.49
Probation (out of Convicted)	0.13	0.00	0.45
Sentence Length in Years (out of Incarcerated)	0.46	0.16	0.56

Table 1B: Summary Statistics for Cases Processed

	Not Convicted			Convicted		
	Mean	Median	SD	Mean	Median	SD
General:						
Age	36.02	35	9.02	37.59	36	10.55
% Female	0.29	0.00	0.46	0.25	0.00	0.43
% Black	0.42	0.00	0.49	0.38	0.00	0.48
% Latino	0.17	0.00	0.38	0.23	0.00	0.42
Credit:						
Credit Score	576	571	84	579	572	83
Loans	0.52	1.00	0.50	0.45	0.00	0.50
(Log) Estimated Income	10.23	10.13	0.52	10.18	10.08	0.52
Loan Amt	60,682	25,568	86,572	54,496	22,726	78,683
Mortgages	0.14	0.00	0.35	0.13	0.00	0.33
Auto Loans	0.29	0.00	0.46	0.24	0.00	0.43

Table 1C: Top 10 Offenses in Sample

Criminal Offense	%
DWI 1st Time Offender	33.20
Driving While Lic. Suspended	10.47
Theft $50-$500	8.13
Assault-Family Member	6.12
Assault-Bodily Injury	3.42
DWI 2nd Time Offender	3.13
Posession Controlled Substance Less than 1G*	2.38
Unlawfully Carrying a Weapon	1.71
Failure to Stop & Give Info	1.29
Theft $500-$1,500	1.25
Total	71.30

NOTES: Each panel of this table presents summary statistics. Panel A provides descriptive statistics for the full sample. Panel B provides the post-sentence information, and separated by conviction status. Panel C reports the prevalence of the top ten most frequent offenses for our sample. Felonies are presented in bold. All others are misdemeanors. * denotes state jail felony.

Table 2: Incarceration on Credit Score and Estimated Income by Conviction Type

	Credit Score				Assessed Income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incarceration	-60.83	-56.45	-61.26	-56.84	-0.25	-0.29	-0.25	-0.28
	(11.23)	(9.43)	(11.14)	(9.42)	(0.02)	(0.01)	(0.02)	(0.02)
Probation	3.73	8.39	2.69	7.52	-0.02	-0.02	-0.03	-0.03
	(16.11)	(12.90)	(16.07)	(12.92)	(0.03)	(0.04)	(0.04)	(0.04)
Fine	0.00	-0.00	0.00	-0.00	-0.00	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Bail			0.21	0.34			0.00	0.00
			(2.24)	(1.98)			(0.01)	(0.01)
Income 2006					0.82	0.82	0.82	0.82
					(0.01)	(0.01)	(0.01)	(0.02)
N	130,783	130,783	129,700	129,700	19,809	19,809	19,614	19,614
Year Disposition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes	No	No	Yes	Yes
Sample	Full	Full	Full	Full	Sentence>2006	Sentence>2006	Sentence>2006	Sentence>2006

NOTES: This table reports instrumental variable (IV) estimates of the effects of incarceration, probation, fines, and bail on credit scores and assessment of income. Columns (1) through (4) present estimates using the full sample. Columns (5)-(8) restricts the sample to individuals with cases adjudicated after 2006 whose credit outcomes are measured in 2006 and 2013. Errors clustered at the court $\times$ year of disposition level.

Table 3: Past Incarceration on Credit Score Measures

	Main Sample		Prime Borrowers					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	IV	IV	IV	IV	IV	IV
Past Incarceration	-12.10	-53.71	-68.69	-41.73	-54.61	-62.38	-55.54	-45.77
	(1.35)	(8.33)	(7.02)	(5.39)	(9.57)	(4.12)	(11.30)	(12.35)
Past Incarceration $\times$ Credit Score 2006						0.03	-0.10	-0.20
						(0.12)	(0.22)	(0.27)
Credit Score 2006					0.93	0.94	0.94	0.91
					(0.01)	(0.01)	(0.01)	(0.02)
Income 2006					5.25		5.25	3.00
					(0.45)		(0.46)	(0.62)
N	67,115	67,115	20,889	20,889	7,654	13,159	7,654	7,654
Year Disposition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	Yes	No	No	No	Yes
Sub-Sample	N/A	N/A	N/A	N/A	Sentence>2006	Sentence>2006	Sentence>2006	Sentence>2006

NOTES: This table reports OLS and IV estimates of the impact of past incarceration on credit scores. Columns (1) and (2) estimate the effects for the main sample as defined in data section. Columns (3)-(8) estimate the effects for prime borrowers (credit scores $\geq$ 600). $PastIncarceration$ and $PastIncarceration \times CreditScore2006$ are jointly instrumented in a similar manner to Equation (2). Errors clustered at the court $\times$ year of disposition level.

Table 4: Past Incarceration on Financing of Durables

	P(Mortgage=1)				P(Auto Loan=1)				Auto Loan Amount		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	OLS	IV	IV	IV	OLS	IV	IV	IV	OLS	IV	IV
Past Incarceration	-0.08	-0.15	-0.16	-0.14	-0.13	-0.24	-0.34	-0.45	-0.08	-0.14	-0.28
	(0.01)	(0.02)	(0.06)	(0.06)	(0.01)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.13)
Credit Score	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	67,115	67,115	18,110	18,110	67,115	67,115	22,879	18,110	20,357	20,357	10,222
Year Disposition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes
Income	No	No	No	Yes	No	No	No	Yes	No	No	Yes
Sample	Main	Main	Has Other Loans	Has Other Loans	Main	Main	Has Other Loans	Has Other Loans	Has Other Loans	Has Other Loans	Has Other Loans

Notes: This table reports OLS and IV estimates of the effect of incarceration on the probability of obtaining a mortgage or auto financing loan using a linear probability model, as well as for the effect of incarceration on auto loan amount conditional on obtaining such a loan. Columns (1)-(4) present estimates on the probability of obtaining a mortgage loan. Columns (5)-(8) shows estimates on the probability of obtaining an auto loan. Columns (9)-(11) condition on individuals having an auto loan, and show the estimates of incarceration on the (log) size of the loan. Columns (1)-(2) and (5)-(6) use the main sample as defined in data section. Columns (3)-(4) and (7)-(8) restrict the main sample to individuals with additional loans. Errors clustered at the court $\times$ year of disposition level.

Table 5: Intensive Margin of Past Incarceration on Access to Credit

	Credit Scores		P(Loan=1)		P(Mortgage=1)		P(Auto Loan=1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Sentence Length	-0.07	-24.45	0.03	-0.07	0.02	-0.06	0.02	-.04
	(1.18)	(1.15)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)
Credit Score				0.00		0.00		0.00
				(0.00)		(0.00)		(0.00)
N	19,812	19,812	19,812	19,812	19,812	19,812	19,812	19,812
Year Disposition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Incarceration	Incarceration	Incarceration	Incarceration	Incarceration	Incarceration	Incarceration	Incarceration

Notes: This table reports instrumental variable (IV) estimates of the effects of sentence length on credit outcomes. Columns (1)-(2) report the effect of sentence length on credit scores. Columns (3)-(4), (5)-(6), and (7)-(8) report linear probability estimates of sentence length on probability of having loans, a mortgage, or an auto loan, respectively. Sample is main sample restricted to individuals formerly incarcerated. Errors clustered at the court $\times$ year of disposition level.

Table 6: Effects of Past Incarceration on Search for Credit

	(1)	(2)	(3)	(4)
Past Incarceration	0.25	0.76	0.97	1.08
	(0.03)	(0.14)	(0.17)	(0.18)
Income 2006			-0.55	-0.41
			(0.02)	(0.02)
Credit Score	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
N	60,112	60,112	15,596	15,596
Estimation	OLS	IV	IV	IV
Year Disposition	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes
Controls	No	No	No	Yes
Sample	Main	Main	Sentence>2006	Sentence>2006

NOTES: This table reports OLS and IV estimates of the effects of past incarceration on the inquiries to accounts ratio. Columns (1)-(2) report estimates for the main sample. Columns (3)-(4) report estimates for individuals with cases adjudicated after 2006. Errors clustered at the court × year of disposition level.

Table 7: Effects on Performance

	(1) 30d	(2) 60d	(3) 90d	(4) Bankruptcy	(5) 30d	(6) 60d	(7) 90d	(8) Bankruptcy
Past Incarceration	-0.41	-0.30	-0.20	-0.11				
	(0.04)	(0.04)	(0.05)	(0.03)				
Past Probation					0.01	-0.07	-0.10	0.01
					(0.04)	(0.03)	(0.02)	(0.04)
Credit Score	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	24,380	24,380	24,380	24,380	9,932	9,932	9,932	9,932
Year Disposition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Loans	Loans	Loans	Loans	Loans	Loans	Loans	Loans

NOTES: This table reports IV estimates of the effects of past incarceration on defaults within either 30, 60, or 90 days after payment is due, as well as bankruptcy discharge for individuals with cases adjudicated after 2006. Columns (1)-(4) report estimates for individuals sentenced to incarceration while columns (5)-(8) report estimates for individuals sentenced to probation. Controls include age, race and pre-incarceration income. Errors clustered at the court × year of disposition level.

Table 8: Adverse Selection in Loan Performance (Outcome Test)

η \ ν	**Selection Test** $\rho = corr(\eta, \nu)$							
	Past Incarceration				Past Probation			
	Observables		Unobservables		Observables		Unobservables	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Default Last 30 days	-0.007	-0.007	-0.030	-0.056	0.039	0.019	-0.009	-0.018
	(0.009)	(0.014)	(0.012)	(0.019)	(0.015)	(0.018)	(0.013)	(0.018)
Default Last 60 days	-0.008	-0.006	-0.046	-0.032	0.042	0.027	-0.009	0.007
	(0.007)	(0.008)	(0.010)	(0.009)	(0.014)	(0.014)	(0.015)	(0.015)
Default Last 90 days	-0.008	0.028	-0.015	0.039	0.059	0.074	0.052	0.081
	(0.008)	(0.009)	(0.010)	(0.010)	(0.016)	(0.014)	(0.016)	(0.015)
Bankruptcy	-0.002	0.000	-.074	-0.069	0.074	0.064	-0.003	-0.007
	(0.016)	(0.013)	(0.017)	(0.018)	(0.028)	(0.024)	(0.029)	(0.026)
N	58,317	51,150	58,317	51,150	27,797	25,053	27,797	25,053
Age Disposition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Court-Year FX	No	No	Yes	Yes	No	No	Yes	Yes
Credit Score	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Assessed Income	No	Yes	No	Yes	No	Yes	No	Yes

NOTES: This table reports bivariate probit estimates of the correlation (ρ) between residuals explaining conviction (η) and residuals explaining default (ν). The sample of convicted individuals can be either formerly incarcerated (columns 1 through 4) or formerly in probation (columns 5 through and 8). Default can be either 30, 60, or 90 days defaults, or bankruptcy. Columns (1)-(2) and (5)-(6) control for observable information to the bank (credit scores, age). To assess the correlation between criminal type (unobservable to the lender) and default. In columns (3)-(4) and (7)-(8) we control for court year fixed effects making η a proxy for criminal type. Errors clustered at the court $\times$ year of disposition level.

Table 9: Effects of Access to Credit on Recidivism

	All			No Previous Arrests		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	IV	OLS	IV	IV
	Panel A: First Stage (Credit Limit/$1,000)					
Past Incarceration × Discontinuity	-4.02	-13.50	-12.50	-3.56	-2.93	-6.36
	(1.71)	(4.00)	(4.39)	(2.42)	(5.30)	(6.02)
Discontinuity	6.19	6.54	8.14	6.17	6.01	7.77
	(0.77)	(0.90)	(0.97)	(1.00)	(1.06)	(1.12)
Past Incarceration	-2.18	-2.02	-3.88	-2.64	-2.42	-2.97
	(1.42)	(3.11)	(3.63)	(1.87)	(3.61)	(4.97)
	Panel B: 2SLS					
Past Incarceration x Credit Limit	0.001	0.015	0.015	0.002	0.007	0.009
	(0.001)	(0.007)	(0.007)	(0.001)	(0.004)	(0.004)
Past Incarceration	-0.063	0.484	0.461	0.937	0.303	0.305
	(0.029)	(0.111)	(0.110)	(0.028)	(0.067)	(0.067)
Credit Limit	-0.001	-0.007	-0.007	-0.000	-0.003	-0.003
	(0.000)	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)
N	11,359	11,359	11,359	6,816	6,816	6,816
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	No	No	Yes

NOTES: This table presents regression discontinuity (RD) estimates of the effects of credit limits on future crime. The regression discontinuity is implemented jointly with random judge assignment within a 2SLS framework according to equation (6). Panel A shows the first-stage results of credit limit discontinuities and incarceration on credit limits. Panel B shows the main effects of credit limit, incarceration and incarceration times credit limit on recidivism. Columns (1) through (4) show results for all individuals regardless of previous criminal history. Columns (5) through (8) show results for individuals with no previous criminal history. Sample is restricted to pre-charge prime borrowers and includes individuals who recidivate. Errors clustered at the court × year of disposition level.

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Appendix

A Spillover of Labor Market Distortions into Credit Markets: Institutional Overview and Conceptual Framework

A.1 The Institution of the Carceral State in the U.S.

There has been significant empirical research on the collateral consequences of exposure to the criminal justice system. In economics, much of this work has focused on the employment effects of the criminal justice system. Pager (2003), for example, documents using an audit study in Milwaukee and New York City that employers strongly disfavored job seekers with a criminal record (with reductions in callbacks of 30-60 percent). Aizer and Doyle (2014) assess the consequences of incarcerating juveniles on future outcomes, such as high school completion and adult criminal outcomes. Several recent studies have also analyzed employment consequences using administrative data linking court or correctional records to earnings data obtained from state unemployment insurance (UI) systems. Grogger (1995), for example, uses UI earnings data and California court records to study the impact of arrests on labor market outcomes. He reports reductions in employment of around 5 percent and earnings losses of 10–30 percent. MS uses the same geographical context as us, and documents how both the extensive and intensive margins of incarceration significantly affect employment over the life-cycle of a criminal offender.

A.2 Distortion of Labor Income

As we just mentioned, previous studies have shown that a criminal record creates a substantial barrier to obtaining employment. To fix ideas, in the next two subsections we provide a simple framework with the purpose of illustrating the interconnection between income, criminal types and borrower screening. For simplicity, we abstract away from depreciation of human capital and loss of negotiating benchmark, but the intuition we explore here extends to those cases.

Consider a two-period simple screening model of labor supply and crime. Firms freely enter the market. Workers inelastically supply one unit of labor in each period for a wage $w\ e$ where a worker's productivity is denoted by $e \in [\underline{e}, \bar{e}]$. There are hiring costs γ that include the cost of screening and conducting background checks on criminal history. Workers and firms commit only to one-period contracts, and matches are separated afterwards. Private information about the worker's productivity coupled with hiring costs gives rise to endogenous discrimination against ex-convicts. Firms must break even from hiring a worker:

$$P\mathbb{E}[e|X] - w\mathbb{E}[e|X] - \gamma = 0$$

where P is the output per efficiency unit and X is a vector of screening characteristics that include background checks on a worker's criminal history. The competitive wage offered by the firm is:

$$w = P - \frac{\gamma}{\mathbb{E}[e|X]}$$

this is, wages are increasing with expected worker's productivity.

There are two periods in the lifetime of a worker, youth and maturity, and we denote each period by the subscript $t \in \{Y, M\}$. The discount factor is one. Agents engage in crime only when they are young. Denote by w^c the competitive wage of a worker with a record of criminal history. Their utility at period 2 is given by:

$$\begin{aligned} U_M^n(e) &= \frac{1}{2}\log(w\ e) \\ U_M^c(e) &= \frac{1}{2}\log(w^c\ e) \end{aligned}$$

In period 1, some agents engage in criminal activity. The felicity value of engaging in criminal activity, χ, is drawn from a uniform distribution on $[\underline{\chi}, \bar{\chi}]$ and is independent of ability. If agents choose to engage in crime they can be apprehended with probability μ, and they would lose all labor income and go to jail or prison. Consumption in jail or prison is c_P. The lifetime utility at period 1 is given by:

$$U_Y(e) = \max\left\{\log(w\ e), (1-\mu)[\chi + \log(w\ e)] + \mu\frac{\log c_P + \log w^c e + \phi(w^c - w)}{2}\right\} \tag{8}$$

where $\phi(w)$ is increasing in wages and denotes potential gains or losses due to access to credit. Equation (8) implies that the agent could engage in criminal activity if and only if $e \leq \frac{c_P w^c}{w^2}\exp\{2\frac{1-\mu}{\mu}\chi + \phi(w^c - w)\}$— this is, high types are less likely to engage in crime. Hence, it is weakly profitable for the firm to screen on criminal history and consequently, $w^c \leq w$:

Remark 1: *Average wages for workers with criminal histories are less than or equal to average wages. The inequality is strict for low enough prison consumption, c_P.*

From equation (8) we also know that in order for high ability individuals to engage in crime they must have a high criminal type. Hence, conditional on conviction, the expected ability of an individual is no longer independent of criminal type:

Remark 2: *Conditional on conviction, the expected value of ability for individuals with a criminal record increases with criminal type. This is,* $e(\chi) \equiv \mathbb{E}^c[e|\chi]$ *is increasing in* χ.

The intuition of Remark 2 is simple, it says that conviction induces a positive selection bias. As an example, one might think that giving a million dollars to an individual would dissuade her from stealing if her motive is poverty more so than if her reason for stealing is kleptomania. This finding is important if we want to understand the bias of the OLS estimator. When criminal type and ability are ex-ante uncorrelated, the OLS estimator will exhibit positive bias (see Appendix B for details), since criminal type and ability are positively correlated ex-post. Of course, there may exist unobserved factors driving an ex-ante negative correlation between criminal type and ability but, in order to have negative bias in the OLS estimator, the bias induced by these factors must exceed the ex-post positive bias that arises due to selection.

A.3 Spillover into Credit Markets

Lenders face borrowers with characteristics ν. Characteristics include income and credit history, for example, but exclude traits that are private information of the borrower, like repayment character and criminal type. Let L denote total loan amount. To a borrower with observable characteristics ν, lenders offer a contract $\psi = (L, \nu)$ and choose the number, a_ψ, and price, q_ψ for each contract so as to maximize profits:

$$\pi = \sum_{\psi \in \Psi} (1 - p_\psi) a_\psi q_\psi - \sum_{\psi \in \Psi} a_\psi L_\psi$$

where p_ψ is the probability that contract ψ defaults. In frictionless competitive markets, the expected profit of each contract must equal zero

$$\mathbb{E}[\pi^\psi|\nu] \equiv \mathbb{E}[(1 - p_\psi)a_\psi q_\psi - a_\psi L_\psi|\nu] = 0.$$

Now consider the case when the only relevant observable characteristic is income, i.e. $\nu \equiv Income$. We can assess the performance of two individuals with the same income but different criminal histories—$\nu_c = w_c e_c$ and $\nu_{\neg c} = we$, with $\nu_{\neg c} = \nu_c$. Productivity, e and criminal type, χ, are unobservable to the lender. When there is no relationship between unobservables and default probability p_ψ—i.e., $Cov(p_\psi, e) = Cov(p_\psi, \chi) = 0$— ability to pay is the only determinant of default. This implies that lending to an ex-felon or an individual with no convictions yields the same performance:

$$\mathbb{E}[\pi^\psi|\nu_c] = \mathbb{E}[\pi^\psi|\nu_{\neg c}] = 0$$

which says that it is irrelevant for the lender to discriminate between individuals with and without a criminal history. Now consider the case where individuals with higher ability also default less, $Cov(e, p_\psi) < 0$. This can happen, for example, if more responsible individuals both develop more skills and care more about honoring their credit agreements, i.e. their willingness to pay. Then,

$$w > w_c \implies e_c > e \implies \mathbb{E}[\pi^\psi|\nu_c] > \mathbb{E}[\pi^\psi|\nu_{\neg c}]$$

which states that, holding income constant, lending to formerly incarcerated individuals has better performance. We can extend this logic to criminal types. By Remark 2, *post-conviction* there is a positive correlation between ability and criminal types, and hence formerly incarcerated individuals with high criminal type should exhibit the best performance.

There cannot be advantageous selection on observable characteristics. Since criminal history is public information, lenders should face no advantageous selection from lending to formerly incarcerated individuals. Conversely, if stigma[29] is not competed away in the market, we should find evidence of advantageous selection. We summarize as follows:

Remark 3: *In the absence of stigma, lending to applicants with a criminal record should not lead to advantageous selection for the lender. In contrast, criminal type may provide selection advantages or disadvantages to the lender. If ability is a better predictor of creditworthiness than criminal type, high criminal types must be advantageous to the lender.*

[29] By stigma we refer to a set of beliefs about a group or individual that are unsupported by evidence or that when applied lead to outcomes inconsistent with those same beliefs. In the present context, stigma would manifest itself on the form of lower access to credit *and* better repayment history outcomes on the part of the formerly incarcerated.

Table A.3: Past Incarceration on Assessed Income of Borrower Population

	(1) IV	(2) Placebo	(3) IV	(4) IV
Past Incarceration × 2006 Income	-0.26	0.17	-0.28	-0.28
	(0.07)	(0.09)	(0.07)	(0.07)
Past Incarceration	-0.10	-0.07	-0.12	-0.14
	(0.03)	(0.02)	(0.03)	(0.03)
Credit Score	0.00	0.00		0.00
	(0.00)	(0.00)		(0.00)
2006 Income	0.87	0.66	0.89	0.90
	(0.01)	(0.08)	(0.01)	(0.01)
Year Disposition	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes
Controls	No	No	No	Yes
Sample	Sentence>2006	Sentence<2006	Sentence>2006	Sentence>2006

NOTES: This table reports instrumental variable (IV) estimates of the effects of past incarceration and (past incarceration × 2006 assessed income) on 2013 assessed income. Columns (1) and (3)-(4) restrict to individuals with disposition (verdict and sentence) issued after 2006. This restriction allows us to compare the effect of incarceration on 2013 assessed income accounting for heterogeneity in 2006 assessed income. As a placebo check, column (2) restricts to individuals with disposition before 2006, such that changes in income between 2006 and 2013 do not account for incarceration. $PastIncarceration$ and $PastIncarceration \times AssessedIncome2006$ are jointly instrumented according to equation (2). Errors clustered at the court × year of disposition level.

B Criminal Types and OLS Bias

Individuals are randomly assigned to courtrooms. Under the monotonicity assumption, we can exploit this to compute a proxy for criminal intent. The intuition is as follows: If an individual is incarcerated in a court with low proclivity towards incarceration there is less reasonable doubt than if the individual is incarcerated by a stricter court. Formally we construct:

$$\xi_{it} = Incarcerated_{it} - (\hat{\beta}_0 + \tau_t + \hat{\beta}_1 Court_i \otimes \tau_t) \tag{9}$$

The approach is similar to the one followed in some empirical literature assessing adverse selection, e.g. Einav, Jenkins and Levin (2012). A large positive ξ means that an individual was convicted despite being randomly assigned to a lenient courtroom. Conversely, a small negative ξ says that an individual was found not guilty in a courtroom that is relatively more likely to send defendants to jail or prison. Although, having a high criminal type does not imply engaging more in criminal activity —other factors like income and age strongly affect this likelihood—as a matter of robustness, we show in Appendix (B) that criminal type is indeed correlated with past criminal history, future dispositions after first arrest, and future dispositions regardless of past criminal history.

B.0.1 Legal Foundations for the Interpretation of Residual

Criminal cases generally adhere to the doctrine of *mens rea*, meaning that it is in general necessary to show intent in the commission of a crime. Salmond (1924) provides what is generally considered the classic definition of *mens rea* for common law countries:

> The general conditions of penal law liability are indicated with sufficient accuracy in the legal maxim, *Actus non facit reum, nisi nisi mens sit rea*- the act alone does not amount to guilt; it must be accompanied by a guilty mind. That is to say, there are two conditions to be fulfilled before penal responsibility can rightly be imposed[...] The material condition is the doing of some *act* by the person to be held liable[...] The formal condition, on the other hand, is the *mens rea* or guilty mind with which the act is done. It is not enough that a man has done some act which on account of its mischievous results the law prohibits; before the law can justly punish the act, an inquiry must be made into the mental attitude of the doer.

At the moment of making a decision to convict an individual, courts look at both the acts and the “criminal-type” of the individual. In a criminal case, the verdict is usually rendered by a jury, and occasionally by the judge. But even when a trial is by jury, the judge still directs the jury on process, including *mens rea* or guilty mind.

Following randomization, we interpret the residual as being a proxy for “guilty mind” or our criminal type. An individual sentenced to carceral confinement in a court that generally is lenient towards its accused either has faced clearer proof of a criminal act or a higher assessment of the “guilty mind” of the accused. Since juries are case specific, appreciation of the facts should not be persistent inside a particular courtroom and, thus, we interpret the extensive margin of a judge’s propensity to incarcerate as differences in her standard for a finding of *mens rea*.

The naive relationship we want to explore is given by:

$$Y = \beta Incarcerated + \nu$$

where $Cov(\nu, Incarcerated) \neq 0$. Decompose ν into an intensive margin component $\hat{\xi} = Incarcerated - \widehat{Incarcerated}$—which captures factors such as severity of crime and intent, and its orthogonal component, η. This will implement a control function version of 2SLS:

$$Y = \beta Incarcerated + \gamma\hat{\xi} + \eta \tag{10}$$
$$= (\beta + \gamma)Incarcerated - \gamma\widehat{Incarcerated} + \eta \tag{11}$$

As usual with this type of control function, η is uncorrelated with $Incarcerated$ and $\hat{\xi}$. The bias on the OLS estimate is given by:

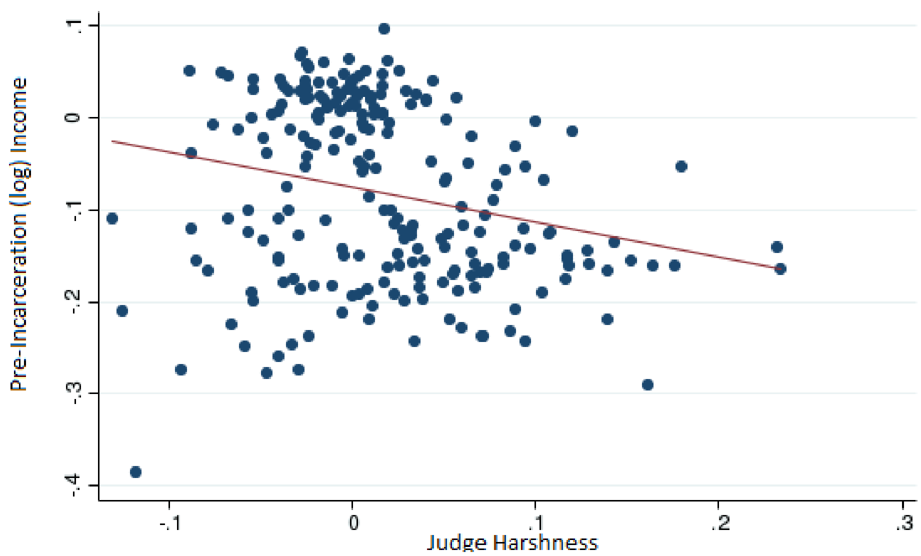
$$\begin{aligned}\hat{\beta}_{OLS} - \beta &= \tfrac{Cov(Y,Incarcerated)}{Var(Incarcerated)} - \beta = \gamma\tfrac{Cov(\hat{\xi},Incarcerated)}{Var(Incarcerated)} + \tfrac{Cov(\eta,Incarcerated)}{Var(Incarcerated)} \\ &= \gamma\left\{1 - \tfrac{Var(\widehat{Incarcerated})}{Var(Incarcerated)}\right\} \end{aligned} \tag{12}$$

Importantly, we can interpret equation (10) as the effect of criminal type on Y conditional on incarceration and, hence, invoke Remark 2 of the conceptual framework above. As we know from Remark 2, conditional on $Incarcerated$, $\hat{\xi}$ can be positively correlated with ability, and if ability is correlated with higher credit scores, we may expect γ to be positive. This makes the bias positive. This type of bias is one of selection post assignment to treatment and, conditional on the assignment being random, can be overcome by using assignment as an instrument in the same spirit of a randomized trial with partial compliance.[30]

Columns (1) and (5) in Table (B.1) show the OLS regression of credit scores and log income on incarceration. The estimates are lower than our IV estimates, suggesting that γ is positive. In columns (2) and (6) we show the OLS estimates for equation 11. Since $\gamma\{1 - \frac{Var(\widehat{Incarcerated})}{Var(Incarcerated)}\} < \gamma$, controlling for $\widehat{Incarcerated}$ drives β_{OLS} closer to zero than in columns (1) and (5). In columns (3)–(4) and (7)–(8), we show the Control Function estimates (equation 10) which show that, as expected, $\hat{\xi}$ is positively correlated with credit scores and log income, respectively.

B.1 Criminal Types

Figure B.1: Pre-Conviction Income Conditional on Future Incarceration by Judge Harshness

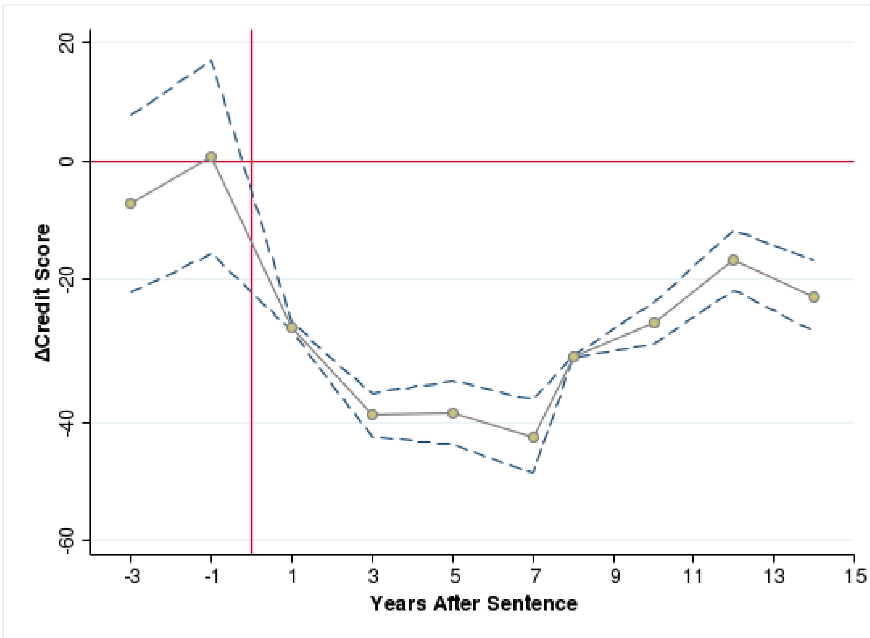


NOTES: This figure plots pre-incarceration income for incarcerated individuals against judge harshness. Judge harshness in the leave-one-out mean of incarcerating for the assigned court at the year of disposition (verdict and sentence). To construct the scatter bin plot, we average 2006 income for individuals with year of conviction after 2006 by court-year. We plot against each court-year's judge harshness.

Below we also present event-study plots of the effects of incarceration on various credit outcomes by criminal type:

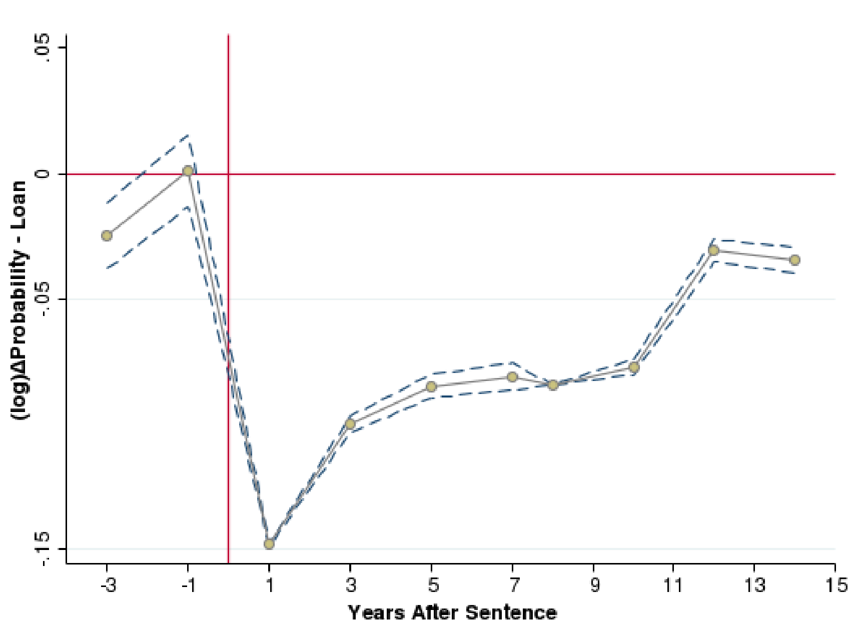
[30] See, Chapter 4.4.2, Angrist and Pischke (2009).

Figure B.2: Heterogeneous Effects of Incarceration on Credit Score by Criminal Type



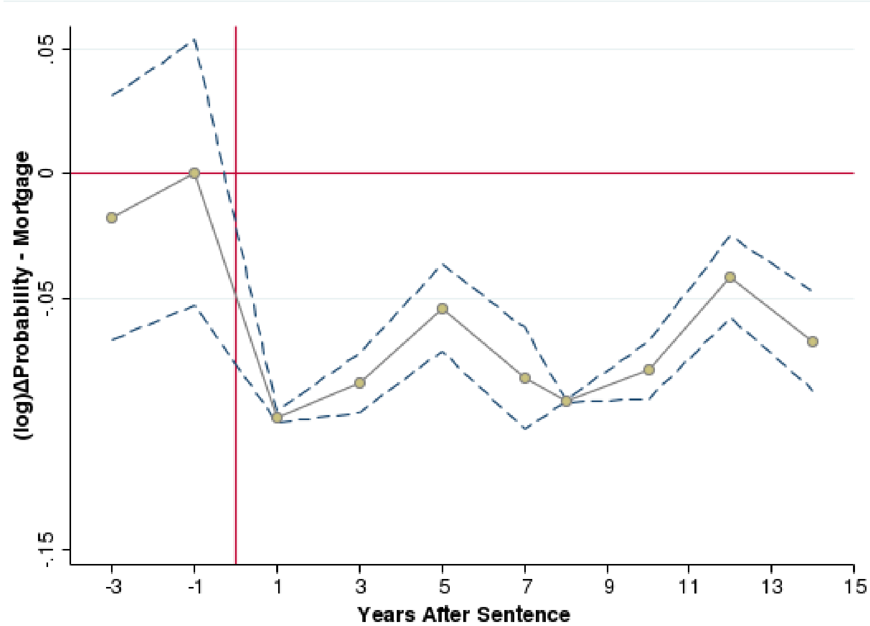
NOTES: This figure shows the effects of incarceration on credit scores by criminal type. Criminal types are computed according to equation (9). The plot shows the coefficient of the interaction of years since conviction × criminal type.

Figure B.3: Heterogeneous Effects of Incarceration on Having a Loan by Criminal Type



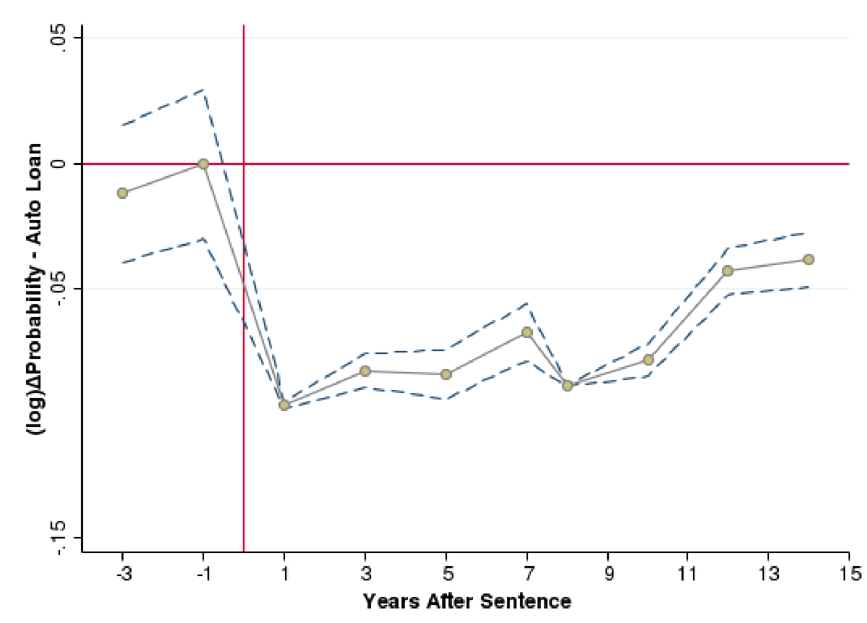
NOTES: This figure shows the effects of incarceration on loan approval by criminal type. Criminal types are computed according to equation (9). The plot shows the coefficient of the interaction of years since conviction × criminal type.

Figure B.4: Heterogeneous Effects of Incarceration on Mortgage Loans by Criminal Type



NOTES: This figure shows the effects of incarceration on probability of having a mortgage by criminal type. Criminal types are computed according to equation (9). The plot shows the coefficient of the interaction of years since conviction × criminal type.

Figure B.5: Heterogeneous Effects of Incarceration on Auto Loans by Criminal Type



NOTES: This figure shows the effects of incarceration on probability of obtaining an auto loan by criminal type. Criminal types are computed according to equation (9). The plot shows the coefficient of the interaction of years since conviction × criminal type.

Table B.1: Correlation between Criminal Types and Number of Arrests

	Credit Scores				Assessed Income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	CF	CF	OLS	OLS	CF	CF
Past Incarceration	-12.10	-4.64	-59.15	-61.80	-0.16	-0.11	-0.54	-0.58
	(1.20)	(1.28)	(3.68)	(4.40)	(0.01)	(0.01)	(0.03)	(0.04)
Residual$_{IV}$/Criminal Type			54.51	55.90			0.44	0.46
			(3.91)	(4.37)			(0.03)	(0.04)
Past Incarceration$_{IV}$		-54.51				-0.44		
		(3.91)				(0.03)		
Sentence Length				2.83				0.04
				(1.33)				(0.01)
	(2.63)	(3.72)	(3.72)	(4.06)	(0.02)	(0.03)	(0.03)	(0.03)
N	67,115	67,115	67,115	58,707	31,592	31,592	31,592	27,795
Year Disposition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS and Control Function (CF) estimates of the effect of incarceration on credit scores and (log) estimated income. Columns (1) and (5) presents the OLS results. Comparing equations (11) and (12) indicates that controlling for the instrumented incarceration $Incarcaration_{IV}$ should increase the bias of the OLS estimate upwards. Columns (2) and (6) control for instrumented incarceration and reflect this upward bias. Columns (3)-(4) and (7)-(8) show the control function estimates of incarceration on access to credit. As predicted by the theory in this subsection, controlling for the first stage residual of incarceration on court-year fixed effects is positive as it reflects the bias induced by the correctional system documented in Remark 2 above. Errors clustered at the court × year of disposition level.

Table B.2: Correlation between Criminal Types and Number of Arrests

	(1)	(2)	(3)	(4)	(5)	(6)
	Before	After	After 1st Arrest	Before	After	After 1st Arrest
Crime Type Measure	0.10	0.13	0.07	0.05	0.08	0.04
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Income 2006	-0.23	-0.25	-0.14	-0.24	-0.26	-0.15
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Credit Score	-0.12	-0.12	-0.09	-0.13	-0.13	-0.09
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N	58,643	58,643	40,569	58,643	58,643	40,569
Year Disposition	No	No	No	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents OLS estimates of the relationship between criminal type and arrests. As a matter of comparison, recidivism in our sample is 40%. Columns (1) and (4) presents the relationship between criminal type and past arrests. Columns (2) and (5) present the relationship between criminal type and future arrests. Columns (3) and (6) present the relationship between criminal type and future arrests conditional on individual being arrested for the first time. Errors clustered at the court × year of disposition level.

C Sample Loan Application Form with Criminal History Inquiry

2013-14 LOAN APPLICATION

PLEASE MAIL THIS APPLICATION ALONG WITH YOUR COMPLETED, SIGNED PROMISSORY NOTE
Awards are distributed on a first come basis – based on the date the application packet is determined to be complete.
Failure to respond or submit required documentation will delay the completed application date.

PLEASE READ THE GUIDELINES & TERMS OF AGREEMENT FOR ELIGIBILITY CRITERIA
(located at www.wsac.wa.gov/slp)

Last Name:	First Name:	MI	SSN:
Address:	City:	State:	Zip:
Driver's License #:	State:	Phone:	

Ethnicity (optional) ☐ African-American ☐ Asian-Pacific Islander ☐ Vietnamese ☐ Korean ☐ Alaskan Native ☐ White Caucasian ☐ Filipino ☐ Chinese ☐ Other

☐ Male ☐ Female | Birth date: | Email (required):

How long have you lived in Washington state? years If less than five, previous state of residence:

Are you a U.S. Citizen ☐ Yes ☐ No | If no - VISA Type: ☐ I-151 ☐ I-551 ☐ I-551C *See Guidelines & Terms of Agreement for eligible non U.S. citizen requirements.* | Visa Number:

Contacts: Provide two contacts with addresses <u>different from your own and different from each other</u> that will always know your current address. The first contact should be a relative but not a spouse.

	Contact One:	Contact Two:
Name		
Permanent Address		
City, State, Zip Code		
Area Code/Telephone		
Relationship to Recipient		

Are you delinquent on any Federal/State debts? ☐ No ☐ Yes (example: Federal Income Tax, Student Loans)
If yes, submit a Cosigner Application Form.

Are you delinquent on child support payments? | ☐ No ☐ Yes If yes, submit a Cosigner Application Form.

Have you filed a Bankruptcy in the last seven years? | ☐ No ☐ Yes If yes, submit a Cosigner Application Form.

A Credit Report and Criminal Background Check will be run upon submission of your application. Applicants with derogatory credit history will be required to submit a Cosigner Application Form. If you believe you have poor credit history you may submit a Cosigner Application Form along with your application to expedite the loan application process.

Are you receiving unemployment benefits? ☐ Yes ☐ No | Do you have dependents? ☐ Yes ☐ No If yes # of dependents:
(do not count spouse as a dependent)

<u>Current</u> work status: **Working** : ☐ full time ☐ part time **Not Working**: ☐ but looking for work ☐ not looking work

2012 Adjusted Gross Income $
From your most current Federal Income Tax Form
If you did not file in 2012, write 0 in the blank above.

Current monthly Gross Income $
Spouse's Monthly Gross Income $
Total monthly gross income $
Do not count unemployment benefits as income
***Be sure to check the Financial Need Criteria Chart** in the Guidelines and Terms of Agreement to make sure you do not exceed the annual income eligibility criteria. (To calculate, take your current total income above- multiply times 12 = annual income.)*

Page 1 of 3

Criminal History Background Information:

You must fill out this section accurately and completely, **disclosing all convictions and/or pending criminal charges including any felony or misdemeanors.** Please be aware that the nature, severity and intentionality of a criminal conviction or pending criminal charge may be a factor in you obtaining employment in this industry which negates the purpose of this loan which is to gain training and skills for a career in the Aerospace Industry.

"Crime" includes a misdemeanor, felony or a military offense. "Convicted" includes, but is not limited to, having been found guilty by verdict of a judge or jury, having entered a plea of guilty or nolo contendre, or having been given probation, a suspended sentence or a fine. You may exclude misdemeanor traffic citations.

Please note – if you do not check the box above and criminal history is found on your background report it will result in your application being removed from further review. You may submit a statement regarding the circumstances.

☐ No ☐ Yes I have been convicted of a crime, had a judgment withheld or deferred, or are currently charged with committing a crime.

☐ No ☐ Yes I have been convicted of a felony or a robbery. **If yes – stop here** – you are not eligible.

☐ No ☐ Yes I have been convicted of theft or shoplifting in the last seven years. **If yes – stop here** – you are not eligible.

☐ No ☐ Yes I am or have been a registered sex offender. **If yes – stop here** – you are not eligible.

☐ No ☐ Yes I have had more than 1 (one) DUI in the last five years. **If yes – stop here** – you are not eligible.

I agree that the WSAC may conduct a criminal history background check. To the best of my knowledge, the information provided on this form is true and complete. I understand that falsification or omission of information constitutes grounds for not receiving this loan. I also understand that if I do receive this loan, regardless of my ability to find employment in the Aerospace industry, I am obligated to repay this loan plus interest to the State of Washington per my signed Agreement.

Confidentiality
All persons receiving and reviewing criminal background information regarding an individual shall maintain strict confidence to the extent permitted by the Washington Student Achievement Council. Information and records gathered or created in the course of criminal background reviews will be securely maintained by this office in a locked file.

Applicant Signature ______________ *Printed Name* ______________ *Date* ______________

Before you mail this application:

- ***Make a copy for your records.***
- ***If mailing via the U.S. Postal service consider using a return receipt for documentation that the application was mailed and delivered, or use an alternate method of delivery that can provide documentation of delivery and tracking if lost. Remember loans will be made on a first come first served basis.***

Mail completed Application AND Promissory Note and any other required documents
(listed on our website: www.wsac.wa.gov/alp) to:
WSAC/ALP PO Box 43430 Olympia WA 98504-3430

Faxed copies of the application are not accepted. For questions contact: alp@wsac.wa.gov or (360) 596-4817

D Data Merge Statistics

Table D.1: Merge Sample Selection

	Misdemeanors		Misdemeanors – No Recidivism		Felonies		Felonies – No Recidivism		Prime Borrowers	
									Misdemeanors	Felonies
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Incarceration	0.026	0.022	0.056	0.054	-0.046	-0.044	0.008	0.004	0.016	-0.006
	(0.035)	(0.035)	(0.042)	(0.042)	(0.022)	(0.022)	(0.028)	(0.027)	(0.020)	(0.006)
Sent Sample	285,240	285,240	169,341	169,341	108,483	108,483	43,714	43,714	285,240	108,483
Returned Sub-Sample	78,596	78,596	54,062	54,062	21,259	21,259	9,911	9,911	34,901	6,729
Year Disposition	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	Yes	Yes

Notes: For our analysis of incarceration on access to credit, we restrict our data to individuals with no recidivism. For analysis of how access to credit affects recidivism, we limit our sample to prime borrowers.

E Instrument Validity

E.1 Relevance

Table E.1: Relevance of Instrument By Outcome Variable

Panel A:	Demographic Outcomes
Age	1.93
Female	1.96
Caucassian	1.98
Black	1.99
Latino	1.71
Panel B:	Prison Outcomes
Conviction	9.44
Prison	13.48
Sentence Length	112.56
Probation	11.98
Probation Length	36.02

Notes: This table reports F-statistics for the first-stage regression of outcomes onhe instrument. F-statistics for demographic outcome variables are reported in Panel A, while those for incarceration outcomes are reported in Panel B. Naturally, model fit is better for outcomes under the direct control of the court.

E.2 Randomization I: Court Rules of Case Assignment

Court rules require randomization, which serves as *prima facie* evidence. We provide further evidence on the next subsection.

> LOCAL RULES OF THE HARRIS COUNTY CIVIL COURTS AT LAW
>
> [...]
>
> RULE 3. FLOW OF CASES 3.1.1 Filing and Assignment. Upon being filed, a case in the county civil courts at law shall be **assigned randomly** to the docket of one of the courts. Once assigned to a court, a case will remain on the docket of that court for all purposes unless transferred as provided in Rule 3.2.
>
> 3.2 Transfer
> 3.2.1 Prior Judgment. Any claim for relief based upon a prior judgment shall be assigned to the court of original judgment.
>
> 3.2.2 Nonsuit. If a case is filed in which there is a substantial identity of parties and causes of action as in a nonsuited case, the later case shall be assigned to the court where the prior case was pending.
>
> 3.2.3 Consolidation. A motion to consolidate cases shall be heard in the court where the lowest numbered case is pending. If the motion is granted, the consolidated case will be given the number of the lowest number case and assigned to that court.
>
> 3.2.4 Severance. If a severance is granted, the new case will be assigned to the court where the original case pends, bearing the same file date and the same number as the original case with a numeric suffix designation; provided, however, that when a severed case has previously been consolidated from another court, the case shall upon severance be assigned to the court from which it was consolidated.
>
> 3.2.5 Agreement. Any case may be transferred from court to another court by written order of the Administrative Judge of the County Civil Courts at Law division or by written order of the judge of the court from which the case is transferred; provided, however, that in the latter instance the transfer must be with the written consent of the court to which the case is transferred.
>
> 3.2.6 Presiding for Another. In cases where a court presides for another court, the case shall remain pending in the original court, except as follows: 1) in any hearing on a motion for contempt, the judge who issued the order which is claimed to have been disobeyed must preside over the motion for contempt, except as otherwise provided in Sec. 21.002, Tex.Gov.Code. and 2) in any hearing on a temporary restraining order, temporary injunction or writs of mandamus and certiorari, the judge who issues the order thereby consents pursuant to 3.2.5 for the case to be transferred from the original court.
>
> 3.2.7 Improper Court. If a case is on the docket of a county civil court at law by any manner other than as prescribed by these rules, the Administrative Judge of the County Civil Courts at Law or Administrative Judge of Harris County shall transfer the case to the proper court. (**Emphasis Ours**)
>
> [...]

E.3 Randomization II: Test of Randomization

To further test whether assignment of judge is independent of defendant's characteristics we run the following specification:

$$JudgeHarshness_{j(i)t} = \beta_0 + \beta_1 PreSentenceTrait_{it} + \tau_t + \epsilon_{ijt} \tag{13}$$

Comparing the results effects of several defendant's characteristics with judge harshness (on average .152) reflects no economically significant effects on being assigned to a less harsh judge. This holds true for demographic characteristics (like gender or race), economic characteristics (like income and credit score) or the power of the attorney (measured by the size of her clientele).

Table E.2: Test of Randomization

Pre-Sentence Trait	(1) Judge Harshness	(2) Baseline Mean	N
Judge Harshness	1	.152	129,721
	(.)	(.051)	
Minority	.000849	.598	129,721
	(.000284)	(.490)	
Female	.000087	.291	129,721
	(.000489)	(.454)	
Age	.000006	34.25	129,721
	(.000021)	(12.48)	
Attorney's Clientele	-.000005	412.02	129,721
	(.000001)	(503.80)	
Pre-Charge Credit Limit	-.000000	5,337	35,474
	(.000000)	(43.2)	
Pre-Charge Number of Accounts	-.000117	11.33	35,474
	(.000029)	(10.42)	
Pre-Charge Credit Score	-.000007	523.37	35,474
	(.000001)	(190.48)	
Pre-Charge (log) Assessed Income	-.002147	5.60	23,660
	(.000288)	(.529)	

NOTES: This table reports OLS estimates of equation 13 for various pre-sentence traits. Column (1) presents the OLS coefficients. Column (2) shows baseline means for each trait to allow comparison. Errors clustered at the court $\times$ year of disposition level.

F Tests of Monotonicity

Table F.1: Test of Monotonicity

	Baseline Instrument	Reverse-Sample Instrument
	(1)	(2)
	First Stage	**First Stage**
Dependent Variable:	***Pr(Incarcerated)***	***Pr(Incarcerated)***
A. INCARCERATION PROPENSITY		
1. Subsample: Incarceration Propensity1st quartile (lowest)		
Estimate	1.06045	N/A
Standard Error	0.07846	N/A
Dependent Mean	0.10424	N/A
Number of Observations	24,320	N/A
2. Subsample: Incarceration Propensity2nd quartile		
Estimate	1.01269	N/A
Standard Error	0.03944	N/A
Dependent Mean	0.20364	N/A
Number of Observations	23,595	N/A
3. Subsample: Incarceration Propensity3rd quartile		
Estimate	0.99932	N/A
Standard Error	0.03005	N/A
Dependent Mean	0.48664	N/A
Number of Observations	24,260	N/A
4. Subsample: Incarceration Propensity4th quartile (highest)		
Estimate	0.99713	N/A
Standard Error	0.02914	N/A
Dependent Mean	0.76062	N/A
Number of Observations	23,786	N/A
B. TYPE OF CRIME		
1. Subsample: Violent Crimes		
Estimate	0.61433	0.57188
Standard Error	0.01688	0.01653
Dependent Mean	0.20596	0.20596
Number of Observations	7,749	7,749
2. Subsample: Drug-related Offenses		
Estimate	1.07226	1.06393
Standard Error	0.018	0.01815
Dependent Mean	0.4667	0.4667
Number of Observations	5,421	5,421
3. Subsample: Property-related Offenses		
Estimate	0.80706	0.76472
Standard Error	0.01102	0.01108
Dependent Mean	0.20917	0.20917
Number of Observations	14,027	14,027
4. Subsample: Economic Offenses		
Estimate	0.60376	0.56963
Standard Error	0.01171	0.0118
Dependent Mean	0.09422	0.09422
Number of Observations	7,695	7,695
5. Subsample: Traffic Offenses (includes DWI)		
Estimate	0.68261	0.56168
Standard Error	0.0124	0.01221
Dependent Mean	0.17404	0.17404
Number of Observations	16,628	16,628
6. Subsample: Other Offenses		
Estimate	1.01122	0.82402
Standard Error	0.00708	0.00871
Dependent Mean	0.5149	0.5149
Number of Observations	54,696	54,696

	Baseline Instrument	Reverse-Sample Instrument
	(1)	(2)
	First Stage	**First Stage**
Dependent Variable:	***Pr(Incarcerated)***	***Pr(Incarcerated)***
C. PREVIOUS CREDIT ACCESS		
1. Subsample: High Credit Score (¿600)		
Estimate	1.0685	1.0036
Standard Error	0.03639	0.03585
Dependent Mean	0.14908	0.14908
Number of Observations	13,912	13,912
2. Subsample: Low Credit Score (¡600)		
Estimate	1.00542	0.75613
Standard Error	0.02396	0.02043
Dependent Mean	0.17773	0.17773
Number of Observations	31,277	31,277
3. Subsample: Loans Before Disposition		
Estimate	0.97699	0.81357
Standard Error	0.02665	0.02444
Dependent Mean	0.14393	0.14393
Number of Observations	24,450	24,450
4. Subsample: Mortgage Before Disposition		
Estimate	1.01049	0.91018
Standard Error	0.0502	0.04817
Dependent Mean	0.12845	0.12845
Number of Observations	6,921	6,921
D. DEMOGRAPHIC CHARACTERISTICS		
1. Subsample: Age ¡= 30		
Estimate	0.97916	0.91113
Standard Error	0.00735	0.00723
Dependent Mean	0.38981	0.38981
Number of Observations	43,665	43,665
2. Subsample: Age ¿ 30		
Estimate	1.02521	1.00283
Standard Error	0.00646	0.00666
Dependent Mean	0.38234	0.38234
Number of Observations	55,780	55,780
3. Subsample: Nonhispanic White		
Estimate	0.96836	0.87027
Standard Error	0.0077	0.00742
Dependent Mean	0.33948	0.33948
Number of Observations	38,904	38,904
4. Subsample: Black		
Estimate	0.96622	0.75207
Standard Error	0.00753	0.00672
Dependent Mean	0.40579	0.40579
Number of Observations	40,560	40,560
5. Subsample: Hispanic		
Estimate	1.1292	1.13873
Standard Error	0.01093	0.01145
Dependent Mean	0.43451	0.43451
Number of Observations	19,981	19,981
6. Subsample: Female		
Estimate	0.75231	0.70385
Standard Error	0.00975	0.00977
Dependent Mean	0.17329	0.17329
Number of Observations	17,878	17,878

NOTES: This table reports estimates of instrument on multiple subsamples. Column one constructs the instrument using the full sample. Column two constructs the instrument excluding the relevant subsample. Errors clustered at the year of disposition level.