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

Abhay P. Aneja[†]   Carlos F. Avenancio-León[‡]

July 2020

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, auto loans, 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.

JEL Classification: D14, G21, K14, E21, K00

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

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 corrections 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 provide the first evaluation of how episodes of incarceration affect the financial health of ex-offenders. We make three principal empirical contributions. First, we identify the causal effect of incarceration on an individual's post-release credit scores and likelihood of obtaining auto or home loans. Second, we examine the mechanisms through incarceration reduces access to credit. Specifically, we consider how incarceration reduces one's ability to service debts and the role of insufficient information and discrimination (or lack thereof) by lenders toward ex-criminal offenders. Finally, we consider the downstream impact of post-incarceration credit loss by demonstrating that post-release credit constraints increase recidivism.

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 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 propensity to incarcerate as an instrument for incarceration. Exploiting this exogenous variation in the likelihood of being incarcerated arising from quasi-random assignment of cases recovers the

---

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

1

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), Mueller-Smith (2015), and Bhuller et al. (2020) – among others – to estimate the impact of criminal justice outcomes. Using this design we show that ex-convicts face a drop of between 42 to 57 points in their credit scores, reductions in their auto loan financing of around 25 percentage points (p.p.), and declines in mortgages of around 20 p.p.

We next examine the mechanisms that link incarceration with access to credit. 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. This can be direct due to confinement or indirect by impairing workforce participation. Although this incapacitation might be 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 32 points in credit score, meaning that the intensive margin accounts for approximately two-thirds 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.

Another potential mechanism is discrimination when applying for credit. Discrimination against ex-offenders 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 against former offenders. 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 incapacitation. 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 formerly incarcerated ex-convicts are stigmatized by

---

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

lenders. Using both methods, we find little evidence that the reduction in access to credit is due to discrimination in lending.

A third potential mechanism is that lenders, even absent discriminatory intent, lack sufficient information to optimally assess the creditworthiness of formerly incarcerated individuals. How can this happen? Consider a dependable borrower who, after being incapacitated, has worse observable credit traits (e.g., credit scores and income) than before being incapacitated. The borrower likely retains the same dependable mindset she had before incarceration but her new observable credit traits do not reflect this. Because of imperfect information, when lenders use observable credit traits to make credit allocations they must also make inferences about unobservable credit traits. This wedge between *real* and *inferred* unobservable credit traits create informational asymmetries between lenders and ex-convicts that adversely distort the allocation of credit. Using our random judge assignment design, we decompose the effects of being incarcerated into an (exogenous) average component and a residual component summarizing the unobservable characteristics that lead to a conviction. Using this decomposition, 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 the wedge between real and inferred unobservable credit traits does. The inability of lenders to account for the better unobservable credit traits of some former inmates prevents optimal credit allocations.

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;[3] 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 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 meaningfully increases recidivism. 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. Screening borrowers is costly. The optimality of "rules of thumb" has been properly assessed by Agarwal et al. 2017).

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. We analyze the mechanisms leading to lower credit access in Section 5. Section 6 assesses how lack of access to credit leads to recidivism. In Section 7 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. There are 37 courts with jurisdiction over criminal cases. Importantly for our purposes, after charges are filed, a case is randomly assigned to a court. 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 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 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 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. The credit bureau 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 around 450,000 individuals with cases resolved between 2000 and 2010 (i.e., disposition years), and removed all within-county geographic information. The credit bureau used the name, date of birth, and address (at time of arrest) to match our criminal incident files to individual credit data. The credit bureau then provided us with an anonymized research subsample chosen *at random*,

4

with credit characteristics appended for two credit years (2006 and 2013). Note that there are two time dimensions: (i) year of disposition (roughly speaking, year of case resolution) and (ii) year of credit – the point in time we observe credit traits, regardless of case resolution. This file includes coarsened categories related to offense, age, gender, and race.[4]

In total, we receive around 100,000 individuals (close to 200,000 person-years). We focus on past incarceration and reentry for first offenders of working age (age above 18 and below 65). After removing individuals currently serving sentence and first-time offenders, we have around 130,000 person-years. We call this our full sample. To zero-in on incarceration, we can also concentrate on offenses that would carry incarceration time if the defendant is found guilty. We call this our main sample, which has around 70,000 person-years. In the tables, we specify if we add additional restrictions (e.g., exclude violent offenses, restrict to borrowers with at least one loan, restrict to borrowers where we observe credit before incarceration, etc.). A potential concern with matching court filing and credit bureau datasets is that the likelihood of a positive match might be correlated with our instrument (judge harshness). Similar to Dobbie, Goldin, and Yang (2018), we demonstrate that judge harshness does not drive selection into our main sample in Appendix D.1.

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 partly explained by its increasing share in the population over the last few decades.[5] From those arrested, the incarceration rates are 23.6 percent and 18.1 percent compared against 19.4 percent for non-Hispanic Whites, 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 (1), Panel A, shows the distribution

---

4. The match rate was around 75 percent, and the credit bureau returned identifiers for the original file to assess match selection.

5. Hispanic population made up 32.9 percent of the population in 2000.

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.

3 Empirical Methodology

3.1 Instrumental Variable Design

We begin by considering a basic empirical set-up, ignoring 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.

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.[6]

---

6. We discuss further and formalize this intuition in Appendix B.

Our empirical strategy resolves this using a method that is becoming increasingly common in applied econometrics. We measure the tendency of a randomly-assigned judge to incarcerate as an instrument, Z, for person's i's ultimate incarceration status. Essentially, we compare credit outcomes for individuals assigned to judges that have different propensities to incarcerate, and interpret any difference as a causal effect of the change in incarceration associated with the difference in these propensities. Our set-up can be viewed as utilizing marginal cases where the judges may disagree about the custody decision, a margin of particular policy relevance.[7]

In terms of mechanics, for each individual, we construct an instrument that corresponds to the "incarceration propensity" or "judge harshness" of each judge. We define the instrument for each individual i as a leave-out mean for judge $j(i)$:

$$Z_{j,(i)} = \frac{1}{n_{j(i)} - 1} \sum_{k!=i}^{n_{j(i)}-1} Incarceration_k \tag{2}$$

Here, $n_{j(i)}$ is that total number of cases seen by judge j; k indexes an individual's case seen by judge j where $Incarceration$ is equal to 1 if a person was sentenced to jail/prison. Thus, the instrument is the judge's incarceration rate among cases based on all the judge's other cases (i.e., excluding case i). The two-stage least-squares estimator is a Jackknife Instrumental Variable Estimator (JIVE) that is used in similar papers on the effects of criminal justice processes.

Using this instrument, we can proceed to test the first-stage relationship between judge assignemnt and whether a person charged with a crime receives a sentence involving confinement (jail or prison term). We estimate the following equation for person i assigned to judge $j(i)$ using a linear probability model:

$$PastIncarceration_{it} = \alpha_0 + \alpha_1 Z_{j(i),t} + \alpha_2 X_{it} + \eta_{it}. \tag{3}$$

And the second stage estimating equation is:

$$Y_{it} = \beta_0 + \beta_1 \widehat{PastIncarceration}_i + \beta_2 X_{it} + \epsilon_{it}. \tag{4}$$

An alternative to using a JIVE estimator is to directly instrument $PastIncarceration_{it}$ using court fixed effects (FE estimator).[8] The advantage of using the JIVE over the FE estimator is that JIVE, by virtue of excluding the case at hand, is not subject to the small sample bias that affects

---

7. See, for example, Dobbie, Goldin, and Yang (2018) as well as Bhuller et al. (2020).

8. As an alternative specification, we 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

the FE estimator. However, the FE estimator provides an advantage when multiple potentially endogenous regressors need to be instrumented for. This will be useful when we are considering multiple margins of punishment (incarceration vs. probation vs. fines), years since incarceration (event study version of equation 4), or decomposing extensive and intensive margins (the fact of having been incarcerated vs. the length of the sentence served). We do not use the FE estimator for our smallest samples; specifically, we do not use the FE estimator in the regression discontinuity analysis in Section 6.

3.2   Instrumental Validity

We can conduct several tests to verify the validity of our IV strategy.

Relevance.   We first verify that our instrument does affect sentencing outcomes. Figure (2) plots the leave-out-mean "judge harshness" against the probability of incarceration for a defendant. The plot demonstrates a strong positive relationship between the judge harshness instrument and incarceration (i.e., a strong first stage). In Appendix E, we conduct additional tests for the relevance of court fixed effect as an instrument using 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 (E.1) presents results for this exercise.

Judge Randomization (Conditional Independence).   While we cannot directly test the pre-treatment exclusion restriction, we can provide evidence consistent with the condition being met. First, judge harshness must be uncorrelated with those characteristics of the defendant and of the case that might affect the defendants' outcomes of interest. We provide evidence in support of random assignment. First, we provide visual evidence that using defendant demographic characteristics (such as race, age, sex) and credit characteristics to predict incarceration does not correlate with judge harshness. This evidence is shown in Figure (2). And second, we show additional empirical evidence in support of randomization by again using pre-existing characteristics of the defendant in a characteristic by characteristic basis, to assess the individual impact of each trait

---

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).

(Section E.2).

Post-Randomization Exclusion. While judge randomization is sufficient for evaluating the effect of a harsher judge on credit outcomes, we need to show the extent through which the effects are driven by incarceration in contrast to other margins of punishment. (This does not mean that these margins may not have an effect, but that our estimates of incarceration are being confounded with these.) To address this, when we show our main results in Table (3), we include estimate using our FE specification where we instrument for past incarceration, past probation, having had a fine, and having being assigned bail. Our main results are robust to the inclusion of these other margins of punishment.

Monotonicity. To identify the local average treatment effect (LATE) of incarceration, it must be the case that the sentencing patterns of judges are monotonic (Imbens and Angrist, 1994). Monotonicity requires that if a given judge has a higher propensity to incarcerate than other judges, then it must be that the risk of incarceration for everyone assigned to that judge is relatively higher than if they had been assigned to a a judge with a lower propensity to incarcerate. To test for monotonicity, we conduct several additional tests, with results presented in Table (2). To show this, we examine the first stage, and show that the fitted values of the instrument for the full sample is positively correlated with convictions for several subsamples along demographic characteristics, judge conviction rates, use of credit before disposition, and types of offenses. In addition, following Bhuller et al. (2020), we perform a (more stringent) "reverse-sample" test in which we construct the instrument excluding the subsample of interest and show that the fitted values computed within this reverse-sample positively predict incarceration status for within the subsample of interest (which can be along demographic characteristics, use of credit before disposition, or type of offense). The idea behind these tests is that if there is a violation to the monotonicity condition, the predicted values from instruments constructed using the full sample or a reverse-sample should fail to exhibit a positive relationship within some subsamples.

4 Main Results: 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. We then look at 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.[9] The largest components in calculating credit scores are payment history (which receives 35 percent weight) and credit utilization (30 percent weight). When a person becomes incarcerated, her ability to service debt is affected by the inability to make payments, and thus her 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)–(5) show that as a consequence of incarceration, credit scores for former inmates decrease by around 42 to 57 points relative to their pre-incarceration levels. These effects are many times larger than the OLS estimates of −12 points reported in Column (1). Note that the choice of JIVE or FE estimation does not significantly impact the estimates (Columns 2 & 3). This is important, as the FE estimation allows us to jointly evaluate incarceration and other margins of punishment. In Columns (4) and (5), we jointly estimate the effects of incarceration, probation, imposition of fines, and imposition of bail on credit scores using FE estimation. The only margin that significantly affects credit scores is incarceration, with a drop of around 56 to 57 points. Thus, estimation of the effects of incarceration alone (necessary if using JIVE) does not seem to be driving our results; on the contrary, it is generating slightly more conservative estimates.[10]

In Figure 5, we show the effect of receiving an incarceration sentence dynamically using an event study design. A few things are worth noting. First, we see that the effect of incarceration is immediate, but remains negative for several years – indicating effects on both the extensive and intensive margins. Second, we note the sharp increase in credit score at year 7 (highlighted by the dashed green line, seven years after a conviction leading to incarceration). This point in time is significant for two main reasons: (1) under the Fair Credit Reporting Act (FCRA), credit reporting agencies[11] can only delve 7 years into a person's past (including criminal records), and (2) after seven years, flags for defaults and Chapter 13 bankruptcy are removed (which should raise credit

---

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

10. While there might be many sources of bias, this upward bias in the OLS estimator is consistent with the intuition we put forward in Section 3.1 and formalize in Appendix B.

11. Importantly, credit reporting agencies are the main providers of criminal background checks for employers.

scores and increase access to credit).

We also break down the effects of incarceration on credit scores by type of offense in Table (4). The effects are consistently negative across all types of offenses, but the magnitude of the effect varies. The effect of incarceration is stronger for more serious offenses, such as violent offenses, and weaker for less serious crimes, such as driving offenses. We also discuss other sources of heterogeneity in the Appendix, such as heterogeneity by estimated income (Appendix F) or by estimated criminal propensity (Appendix B).

These reductions in credit are likely to be consequential for ex-offenders. 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.

4.2   Effects on Access to Durable Goods

In this subsection we evaluate the effects of access to credit on the financing of durable goods. We analyze effects on auto and home loans, as barriers to obtaining a car or a home are significant obstacles for the prospect of former inmates to successfully reenter society.

Auto Loans.   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.[12]

To evaluate the effects of incarceration on ability to obtain auto-financing after release, we estimate Equation (4) as a linear probability model where Y is a dummy for having an auto loan. Table (5) presents our results. Column (1) shows that the OLS estimate between having an auto loan and being incarcerated is -14 p.p. Columns (2) and (3) show that incarceration spells result

---

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

in a drop of around 24 p.p. in the likelihood of having a car loan. Given that around 80 percent of car buyers utilize financing (Davis, 2012), these effects suggest that incarceration puts significant mobility restrictions on former inmates, which could impair their chances of obtaining employment and opportunities for successful reentry.

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 (4) as a linear probability model where Y is a dummy for having a mortgage loan. Table (5) 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 concurrent credit scores. Column (4) presents the OLS estimate between having a mortgage and incarceration. We observe a drop of 9 p.p. Using the judge-based IV strategy we find a drop of around 19 to 20 p.p. (Columns 5 & 6).

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 restricting 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).

5 Mechanisms: Obstacles to Credit Access

We now turn to our analysis of mechanisms underlying our causal effects of incarceration on credit outcomes. Limits to credit access are driven by many factors. One such factor is that, because of incarceration, individuals are unable to pay their debts, both because of direct and indirect forms of incapacitation. Direct incapacitation may result from the inability of borrowers to pay while they are physically removed from economic life. Indirect incapacitation can occur if incarceration tenure impairs the ability to pay of a borrower by affecting other mediating outcomes, such as human capital or health. Incapacitation of either kind would result in worse credit histories. A second potential factor is that borrowers might voluntarily delever. A third potential channel is that banks may take into account the criminal history of a potential borrower and infer a higher or lower willingness to pay down debt obligations (holding observable credit traits constant, such

as credit score) and choose to adjust credit accordingly. Fourth, information asymmetries between the lender and the borrower, and caused by incarceration, may prevent optimal allocation of credit even when it would benefit both parties. We test for each of these mechanisms in this section, although we acknowledge this list of potential channels is not exhaustive.

5.1 Incapacitation Effects: Reductions in Debt Repayment and Earnings Potential

Incarceration can have both direct and indirect effects on financial health and access by incapacitating borrowers. In terms of direct effects, when individuals are incarcerated, they are unable to pay their debts – resulting in worse credit histories. Incapacitation may also have indirect effects – for example, by making reincorporation into the labor force more challenging. Such indirect channels have implications for access to credit by increasing an ex-offender's debt to income ratio. We refer to these combined mechanisms as the "incapacitation" channel.

While direct evidence of incapacitation per se is difficult to demonstrate, we probe this channel by comparing the intensive and extensive margin effects of incarceration on credit score (and measures of credit access). Results of this examination are shown in Table (6). We jointly instrument for variation in sentence length and incarceration status using court-year fixed effects, since both are endogenous regressors. This allows us to evaluate the effects caused by time served conditional on having been sentenced to jail or prison. In Column (2), we estimate the unconditional intensive margin effect of incarceration instead of the effect of simply being sentenced to prison. The results indicate that on average credit scores decline by about 36 points per year of incarceration. Upon closer examination, in our primary specification (Column (3) – in which analyze the intensive and extensive margin effects of incarceration jointly), we see that much of this drop comes as a consequence of time spent incarcerated. This is indicated by the large coefficient of sentence length. There is a loss of around 32 points for each year incarcerated, on top of an immediate drop of about 6.5 points – 85 and 15 percent of the total effect, respectively. This suggests that individuals who are incarcerated face accumulating challenges in repaying their debt as the length of the sentence increases. In Columns (4)–(7), we observe similar dynamics with regard to having access to automobile and home loans.

Admittedly, separating the direct incapacitating effects (e.g., debt repayment while in jail/prison) from indirect effects (future loss of income) is challenging. We believe that labor market effects are likely to be an important mechanism in our setting, though, for a few main reasons. First, it is well-known that creditors not only underwrite based on credit history (e.g., credit scores) but also

based on credit capacity, which is in large part captured by debt-to-income ratios. Second, existing research suggests that incarceration has adverse consequences on labor market performance. Most recently, Mueller-Smith (2015) shows (in our setting, Harris County) that both employment and earnings are adversely affected following incarceration.[13] Given the importance of earnings potential to credit access, we provide suggestive evidence of income loss as a mechanism for credit loss in Appendix F. Because the credit data provided to us does not contain administrative earnings records, we use the measure of estimated income proposed by Coibion et al. (2016).[14] Our analysis (Appendix Table F.1) suggests incarceration reduces (estimated) income by between 25 and 29 percent, while non-carceral sanctions have very small effects on our estimates of labor market performance.[15] Further analysis is discussed in Appendix F.

Finally, we also note that our analysis of other court sanctions in Table (3) provides additional corroboration for the importance of incapacitation as a channel. To see this, note that, in Columns (4)–(5), a sentence of probation produces no significant change in credit scores. This may be consistent with individuals facing probation not necessarily losing their jobs or being otherwise physically removed from economic life, and thus preserving their financial health.

5.2   Voluntary Delevering?

We next consider whether reduced demand for credit – i.e., voluntary delevering – is partly responsible for the decline in credit we observe. To the extent that ex-offenders experience a reduction in their earnings potential, it is possible that reductions in credit indicate a voluntary decision to delever. In support of this possibility, existing research indicates that individual consumption does respond to income shocks (see, for example, Parker et al. (2013) and Johnson, Parker and Souleles (2006) documenting responses to positive income shocks).[16] To test for this channel, we consider how incarceration affects potential borrowers' effort to obtain credit. We do so using data on credit inquiries normalized by the total number of credit accounts. An increase in inquiries would suggest that borrowers are not delevering voluntarily. However, since borrowers can be discouraged from

---

13. 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 percent.

14. 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.

15. Notably, these results are a similar magnitude to those obtained by than Mueller-Smith (2015) estimates, which are based on administrative earnings records.

16. Similarly, research shows that borrowers' *expectations* about home values prior to the Great Recession were an important driver of the crisis (Adelino, Schoar and Severino, 2017). This finding suggests the possibility that ex-convicts are borrowing less due to lower income or low expectations of income growth, and not necessarily due to lower *access* to credit.

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.

Table (7) presents results based on estimating our IV specification using credit inquiries as the outcome of interest. Incarceration leads to an increase of between 0.76 and 1.08 additional inquiries per credit account. This result highlights 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. However, this evidence suggests that supply-side considerations are the primary driver of reduced access to credit.

5.3 Screening and Stigma

We also examine whether ex-offenders suffer from stigma that reduces their access to credit. By "stigma" we refer to the situation where an individual receives less credit than borrowers with the same observable credit characteristics and same likelihood of default, solely because of having a criminal record.[17] Ex-offenders may face harsher credit conditions caused by stigma to the extent that creditors believe a criminal record is informative about an individual's ability/willingness to successfully fulfill his debt obligations. For example, a bank may 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. Anecdotal evidence suggests that lenders can and do ask about criminal history (see Appendix C for a sample loan application).

We first consider indirectly whether stigma in lending may exist by comparing the performance of incarcerated individuals to those convicted but put on probation (conditional on the same underlying offense). An individual sentenced to probation has a criminal record,[18] but does not face incapacitation that hinders repayment of debt nor removal from the labor force and its attendant credit effects. As we demonstrated in our main results, convicted individuals facing probation

---

17. For reference, ex-offenders often face stigma in the labor market, leading workers with above-average skill to remain jobless due to previous incarceration spells (Pager, 2003). Stigma may similarly indicate that lenders receive an incorrect signal about formerly-incarcerated borrowers' willingness to pay.

18. In Texas, the DPS Computerized Criminal History System background check would reflect information on arrest, prosecution, and disposition (conviction, non-conviction, etc.) but won't include sentence or outcome (e.g., probation). This information can be obtained through other means but it is costly to do so and could potentially increase litigation risk.

experience no drop in credit scores, while incarcerated individuals do (Columns 4–5 of Table 3).

One may expect that the performance effects due to stigma for individuals sentenced to probation or incarceration should be similar, as criminal records for both groups, at face value, appear the same. (Our benchmark is individuals with a non-conviction finding, who should face little or no stigma.) In contrast, performance effects due to incapacitation should be stronger for individuals that were incarcerated, while there should be no incapacitation effects for both probationers and non-convicted individuals. Thus, if there is no stigma, we should find that individuals facing probation and non-convicted individuals should perform similarly. We consider this in Table (8), where we assess the effects of incarceration and of probation on loan performance, separately. Because any affects that operate through stigma *in employment* would be reflected in the estimates, this approach provides a conservative assessment of the presence of stigma in lending.

In Table (8), we see that individuals who are incarcerated have 41 p.p. fewer 30-day defaults and an 11 p.p. lower likelihood of bankruptcy (Columns 1 and 4). In contrast, in Columns (5) and (8), we see that persons sentenced to probation have about the same probability of 30-day default and bankruptcy, respectively, as individuals found not guilty. For individuals sentenced to probation, we find only 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 p.p., respectively. Yet these numbers include the effect of stigma in employment, which individuals undergoing probation also face – a concern we will address in the next subsection. All things considered, we believe Table (8) provides *prima facie* evidence of low levels of stigma against ex-offenders in lending.

5.4 Informational Distortions, Adverse Selection, and the Lender's Role

As we just demonstrated in Section 5.3, we find that people leaving prison have *lower* default rates than similar individuals who are released by luck of the draw. One potential explanation for the better performance is that these potential borrowers have fewer loans because their observable characteristics (e.g., credit score) are worse due to being incarcerated. This does not tell us, though, whether banks are lending to the ex-incarcerated optimally. It is possible that creditors are providing lower levels of credit to the ex-incarcerated than they should, even conditional on these potentially lower observable borrowers' characteristics, because former inmates present lower credit risk to lenders than otherwise identical borrowers.

Why might former inmates present lower credit risks? Consider any borrower with an unfa-

vorable credit history; that person has demonstrated an inability or unwillingness to repay loans granted. A dependable borrower, however, likely retains a dependable financial mindset even in the face of inability to repay due to adverse events such as natural disasters, accidents, illnesses, job loss, or incarceration. Lenders can use knowledge of such special circumstances to protect or improve their profits. To consider whether lenders are behaving optimally, we test the following:

(5.4.1) Correction for **Average** Informational Distortions within Observationally-equivalent Borrowers: Lenders could "correct" for the wedge between observable traits and "true" repayment ability of a formerly incarcerated individual by using knowledge of the formerly incarcerated population credit performance 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 selection problem for individuals who underwent probation.[19]

(5.4.2) Correction of **Individual-Specific** Informational Distortions: whether lenders have the ability (or not) to correct for how incarceration distorts the true repayment ability of specific individuals, since incarceration has heterogeneous effects across borrowers, and this information is potentially unobservable to the lender. In other words, after correcting for the average information distortions, are individuals who receive a harsher sentence still performing better than non-convicted individuals with similar observables traits?

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

We begin by assessing whether lenders are optimally providing access to credit to ex-incarcerated persons. To the extent that lenders are using the mark of incarceration against ex-offenders leaving prison, we should observe advantageous selection – i.e., better borrowers conditional on screening criteria. Conversely, if lenders "correct" for the wedge between observable traits and "true" repayment ability of the incarcerated population (in other words, if lenders attempt to correct for ex-offenders having lower credit scores, lower earnings potential, etc., but keeping their willingness to repay), the lender should face no advantageous selection (and possibly some degree of adverse selection if lenders over-correct).

We test for the presence of adverse/advantageous selection by following the positive correlation

---

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

test of selection introduced by Chiappori and Salanié (2000).[20] The intuition of the test is that for observationally-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.[21]

Selection and Incarceration

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

$$
\begin{aligned}
PastIncarceration_{it} &= \mathbb{1}(X_{it}\beta + \nu_{it} > 0) \\
default_{it} &= \mathbb{1}(X_{it}\gamma + \eta_{it} > 0)
\end{aligned}
\tag{5}
$$

$PastIncarceration_{it}$ 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 include non-payment 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. We call the correlation coefficient between ν_{it} and η_{it}, ρ. In an otherwise frictionless competitive market, ρ should be (weakly) positive (Chiappori and Salanié 2000). Since stigma and information asymmetries prevent optimal lending, though, these frictions imply a negative ρ. A negative and significant value of ρ implies advantageous selection arising from suboptimal levels of lending, ρ near zero suggests there is little or no stigma, and a positive ρ indicates adverse selection.

Table (9), Columns (1)–(2) summarize our results.[22] We focus on the correlation, ρ, between residual traits ν leading to default and residual traits, η, leading to a conviction. Table (9) suggests that when lenders screen based on observable information (credit scores, income, age), residual traits do not explain differences in default rates – which is inconsistent with lenders stigmatizing the ex-

---

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

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

22. For outcome tests to be informative, the distribution of defaults should be similar for the groups compared (e.g., formerly incarcerated vs. acquitted) up to an additive constant. We verify this is the case in our setting in Figure (D.1).

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 it also shows a correlation ρ very close to zero for 3 out of 4 default outcomes (although there is modest evidence of adverse selection by the 90-day defaults metric).

Favorable Discrimination?

As we stated before, ρ is informative about both stigma and information distortions. A ρ near zero not only implies that the likelihood of stigma is low, but also that there might be an active effort from lenders to reduce the informational distortion problem 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 (9). From Column (5) we see that, after the lender screens based on credit scores, performance for individuals sentenced to probation exhibits signs of adverse selection. They exhibit strong positive correlation between conviction and 30-, 60-, and 90-day delinquencies and, also, a strong positive correlation with bankruptcy. That is, by lending to individuals sentenced to probation, lenders are extending credit suboptimally. 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. This evidence seems to suggest that lenders are aware it is optimal to improve lending conditions for former inmates, even though they might "overshoot" when lending to probationers.

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.

5.4.2 Can the Lender Correct for Heterogeneous Distortions?

In the last subsection, we evaluated whether lenders could "correct" for the wedge between observable traits and "true" repayment ability of former inmates *as a population*. This correction implies that there is an awareness that formerly incarcerated individuals are on average good borrowers after considering observable credit traits. That analysis, however, does not evaluate whether lenders are able to correct the information distortion problem at the individual level, since incarceration will have heterogeneous effect across different individuals.

In this subsection, we consider this problem. As in the last subsection, we use a correlation test à la Chiappori and Salanié (2000) to test for adverse or advantageous selection. But in contrast to our previous analysis, we now include court-year fixed effects as covariates in our specification – that is, we control for the harshness of the judge in this analysis. Judge harshness is unobservable to the lender. Lenders can correct for this harshness only up to the point this is reflected in former inmates' observable credit traits, but it won't capture any effect judge harshness has on information unobservable to the lender. We formalize this intuition in Appendix B. The modified specification now takes the form:

$$PastIncarceration_{it} = \mathbb{1}(\beta_0 + \tau_t + \beta_1 Court_i \otimes \tau_t + X_{it}\beta + \nu'_{it} > 0) \tag{6}$$

$$default_{it} = \mathbb{1}(\beta_0 + \tau_t + \beta_1 Court_i \otimes \tau_t + X_{it}\gamma + \eta'_{it} > 0)$$

where the main difference between equations (5–6) and (6–7) is the inclusion of court-year fixed effects. The inclusion of the fixed effects absorbs judge harshness effects out of the residuals ν'_{it} and η'_{it}.

The interpretation of a correlation is different from the previous section. A positive correlation means that individuals who undergo long spells of incarceration are less likely to repay, providing support for using criminal history as a proxy for "character" at least in the lending context. A negative correlation, however, would suggest that the individuals that are the most affected by incarceration are also the ones that have better repayment ability than understood by lenders.[23] In other words, the individuals that face the most obstacles to reenter society after release are the ones receiving suboptimal credit.

Columns (3) and (4) of Table (9) presents results for this sub-analysis. When judge harshness

---

23. This would be consistent with hetoregeneity effects documented in Appendix B and the analytical framework put forward in Appendix A.

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 observable credit traits. In Columns (7) and (8), we see that heterogeneity does not affect loan performance for those who faced probation instead of incarceration.To sum up briefly, we find that incarceration not only affects the credit access of ex-inmates, but that the effects are stronger for those facing harsher criminal sentences because of information distortions (which is consistent with lenders overcorrecting for probationers as documented in the previous subsection).

6 Lack of Access to Credit and Recidivism

To this point, we have focused on how incarceration affects access to credit, the channels underlying credit reduction, and whether the post-prison allocation of credit is efficient. In the final section of our study, we consider downstream effects of reduced credit access for those leaving prison – specifically, does lack of access to credit lead to recidivism? Recent research highlights that economic constraints that ex-incarcerated individuals face after imprisonment may lead ex-offenders to commit new crimes (Tuttle 2019). To date, there is no research linking credit access to prisoner reentry and recidivism.[24]

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 discontinuities in credit limits for borrowers.

6.1 Estimation and Validity of Credit 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 credit limits based on cutoff scores, wherein a borrower just below the cutoff score would

---

24. A study that is closely related to ours in this regard is Garmaise and Moskowitz (2006), who find that bank concentration increases property crimes. While this section focuses on the individual reentry dynamics, their analysis focuses on the relationship between aggregate credit provision and aggregate crime levels.

receive a different levels of credit 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. For this analysis, we restrict our sample to borrowers with credit above 600 (credit scores below 620 are generally considered subprime). We divide our sample into an estimation sample – which we use to estimate the discontinuities but is purposefully not used in the RD design – and an analysis sample – where we implement the RD design. In the estimation subsample, we estimate the propensity to have credit for each 1-point credit score bin and then detect discontinuities using threshold regressions (Hansen 2000):

$$\text{Prob}(Credit) = \delta_1 CS + \eta \quad \text{if } CS \leq \gamma \tag{7}$$
$$\text{Prob}(Credit) = \delta_2 CS + \eta \quad \text{if } CS > \gamma$$

where $\text{Prob}(Credit)$ indicates the probability a borrower in the estimation sample with credit score, CS, has credit (in the form of installment loans, auto loans, or open credit cards). The credit score, CS, serves as both the regressor and the threshold variable used to split the sample into groups or regimes. Our credit discontinuity is the estimate of the threshold, γ.[25] We sequentially estimate the remaining credit discontinuities by performing threshold tests in each of the regimes. Following this procedure we obtain four cutoffs at credit scores of: 652, 675, 718, 760. From Figure (5), we can identify candidate credit score discontinuities. We pool our four credit discontinuities to perform a regression discontinuity analysis (which we implement in the next subsection).

We then validate the RD design using the analysis sample. In Figure (6), 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 related to their past criminal history – conviction and sentence – are smooth around the cutoff.

---

25. Endogeneity of the estimate γ is not a concern as threshold estimates are super-consistent.

6.2 Results: Credit Access and Recidivism

Since what changes at each cutoff is the *probability* of obtaining credit, our credit discontinuities constitute a fuzzy RD design. In addition, since we are interested in assessing the effects of credit on recidivism, we must instrument for past incarceration status. Because a fuzzy RD design is a form of instrumental variable design, we implement an IV strategy where we jointly instrument for past incarceration status and access to credit, using both random judge assignment and credit discontinuities. Following Calonico, Cattaneo, and Titiunik (2014), we estimate the optimal bandwidth h to be 8 credit score points.

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 after release. To that end, we use our credit discontinuities to sort out individuals by their credit access following incarceration. We implement this using 2SLS estimation based on the following specification:

$$Recidivism_{it} = \beta_0 + \beta_1 \widehat{PastIncarceration}_{it} + \beta_2 \mathbf{1}(CS_{it} < Threshold)$$

$$+ \beta_3 \widehat{PastIncarceration}_{it} \times \mathbf{1}(CS_{it} < Threshold) + \gamma \mathbf{X}_{it} + \epsilon_{it} \tag{8}$$

where the running variable, CS_{it} and interactions with $\widehat{PastIncarceration}_{it}$ and $\mathbf{1}(CS_{it} < Threshold)$ are included in $\mathbf{X}_{it}$. All $\widehat{PastIncarceration}_{it}$ interaction and single-order terms are jointly instrumented by the JIVE instrument duly interacted with the running variable and the discontinuity indicator up to a linear polynomial. Our main coefficient of interest is β_3.

Our results on how incarceration affects recidivism through its effects on access to credit are presented in Table (10). We find that reduced access to credit significantly increases the likelihood of recidivism for former inmates. Column (1) presents our main finding (pooling across all offenders in our RD sample), and suggests that conditional on exogenously receiving a jail or prison sentence, the quasi-exogenous reduction in credit leads to an 18 p.p. increase in the likelihood of recidivism ($\beta_1 + \beta_3$). The remainder of the table highlights the robustness of this result. When we look separately at the causal effects of reductions in credit for former inmates across offense types (we focus on DUI vs. non-DUI due to the reduced size of the RD sample), we find increased probabilities of incarceration of 19 p.p. and 17 p.p. respectively (Columns 2 and 3). Finally, in Column (4) we limit the sample to first-time offenders (i.e., those with no prior arrests), and again observe that reduced credit after incarceration leads to a 20 p.p. increase in recidivism.

Our findings in this subsection are consistent with research in labor economics highlighting the importance of financial resources for successful ex-offender reintegration. For example, Schneppel (2016) and Raphael and Winter-Ebmer (2001) demonstrate that increases in unemployment have positive and significant effects on property crime. Our results demonstrate another constraint that will similarly limit the ability of ex-offenders to purchase necessary goods (such as food, housing, and transportation) after leaving jail/prison: lack of access to credit. Our analysis thus highlights an important way that government policy has unintended consequences. Rather than deterring criminal activity, incarceration *increases* the likelihood of offense by reducing offenders' future access to credit.

7   Conclusion

By recent estimates, roughly 1 in 4 American adults has a criminal record, and 1 in 20 will be imprisoned at some point during his or his lifetime. The latter number is, of course, significantly higher for Black and Latinx Americans.

In this paper, we demonstrate for the first time how imprisonment affects the financial health of ex-offenders. We demonstrate that episodes of incarceration lead to significant reductions in the credit outcomes of inmates once they leave jail or prison, as measured by credit scores and access to consumer loans. These reductions are driven both by the fact of being carcerally confined (extensive margin), as well as the length of confinement (intensive margin). Our analysis of mechanisms suggests that incapacitation is a significant channel underlying our main effect; this channel includes both the direct effect of being physically unable to access the formal financial sector (and repay debts), as well as the indirect effect of being imprisoned on future earnings, which affects credit access by shaping a borrowers' debt-to-income ratio. We find little empirical support for stigma of incarceration leading to adverse financial outcomes, though. Interestingly, we document inefficiencies in the allocation of credit due to lenders' inability to accurately assess default risk of formerly incarcerated individuals.

Finally, we show that the credit effects of incarceration have important downstream effects. Using differences in access to credit generated by credit score discontinuities, we show that formerly incarcerated individuals are nearly 20 p.p. more likely to recidivate when they have lower access to credit after leaving jail/prison. This finding thus suggests that access to credit plays an important role in shaping the dynamics of crime over the life cycle.

Our findings have important implications for the design of social policy, given that incarceration

hampers future financial health, which in turn leads to higher levels of future crime. Our findings suggest that reentry efforts should consider interventions that address ex-inmates' access to credit in order to alleviate the consequences generated by the interplay of credit constraints and punitive criminal justice police. How reentry policy can be optimally designed to account for ex-offenders' financial constraints, however, remains a question for further inquiry.

References

Agan, Amanda, and Sonja Starr. 2016. "Ban the Box, Criminal Records, and Statistical Discrimination: A Field Experiment." *Working Paper.*

Agarwal, Sumit, Souphala Chomsisengphet, Johannes Stroebel, and Neale Mahoney. 2017. "Do Banks Pass Through Credit Expansions to Consumers Who Want to Borrow?" *Quarterly Journal of Economics* Forthcoming.

Aizer, Anna, and Joseph J. Doyle Jr. 2015. "Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges." *Quarterly Journal of Economics* 130 (2): 759–803.

Bayer, Patrick, Randi Hjalmarsson, and David Pozen. 2009. "Building Criminal Capital behind Bars: Peer Effects in Juvenile Corrections." *Quarterly Journal of Economics* 124 (1): 105–147.

Becker, Gary. 1993. "Nobel Lecture: The Economic Way of Looking at Behavior." *Journal of Political Economy* 101 (3): 385–409.

———. 1957. *The Economics of Discrimination.* Univ.Pr.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82 (6): 2295–2326.

Carroll, Christopher D., Misuzu Otsuka, and Jiri Slacalek. 2011. "How Large Are Housing and Financial Wealth Effects? A New Approach." *Journal of Money, Credit and Banking* 43 (1): 55–79.

Chiappori, Pierre-Andre, and Bernard Salanie. 2000. "Testing for Asymmetric Information in Insurance Markets." *Journal of Political Economy* 108 (1): 56–78.

Davis, Delvin. 2012. *The State of Lending in America & its Impact on US Households.* Technical report. Center for Responsible Lending.

Davis, Morris A., Andreas Lehnert, and Robert F. Martin. 2008. "The Rent-Price Ratio for the Aggregate Stock of Owner-Occupied Housing." *Review of Income and Wealth* 54 (2): 279–284.

Desmond, Matthew. 2016. *Evicted: Poverty and Profit in the American City.* New York: Crown.

DiPasquale, Denise, and Edward Glaeser. 1999. "Incentives and Social Capital: Are Homeowners Better Citizens?" *Journal of Urban Economics* 45 (2): 354–384.

Disney, Richard, John Gathergood, and Andrew Henley. 2010. "House Price Shocks, Negative Equity, And Household Consumption In The United Kingdom4." *Journal of the European Economic Association* 8 (6): 1179–1207.

Dobbie, Will, and Jae Song. 2015. "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection." *American Economic Review* 105 (3): 1272–1311.

E., Case Karl, Quigley John M., and Shiller Robert J. 2005. "Comparing Wealth Effects: The Stock Market versus the Housing Market." *The B.E. Journal of Macroeconomics* 5 (1): 1–34.

Furletti, Mark. 2003. *Credit Card Pricing Developments and Their Disclosure.* Technical report. Federal Reserve Bank of Philadelphia.

Garmaise, Mark J., and Tobias J. Moskowitz. 2006. "Bank Mergers and Crime: The Real and Social Effects of Credit Market Competition." *The Journal of Finance* 61 (2): 495–538.

Gonzalo, Jesus, and Jean-Yves Pitarakis. 2002. "Estimation and model selection based inference in single and multiple threshold models." *Journal of Econometrics* 110 (2): 319–352.

Green, Richard, and Michelle J. White. 1997. "Measuring the Benefits of Homeowning: Effects on Children." *Journal of Urban Economics* 41 (3): 441–461.

Greenwald, Bruce C., and Joseph E. Stiglitz. 1986. "Externalities in Economies with Imperfect Information and Incomplete Markets." *Quarterly Journal of Economics* 101 (2): 229–264.

Grogger, Jeffrey. 1995. "The Effect of Arrests on the Employment and Earnings of Young Men." *Quarterly Journal of Economics* 110 (1): 51–71.

Gruber, Jonathan. 1997. "The Consumption Smoothing Benefits of Unemployment Insurance." *American Economic Review* 87 (1): 192–205.

Hai, Rong, and James J. Heckman. 2017. "Inequality in human capital and endogenous credit constraints." *Review of Economic Dynamics* 25:4 –36.

Hansen, Bruce E. 2000. "Sample Splitting and Threshold Estimation." *Econometrica* 68 (3): 575–603.

Henrichson, Christian, and Ruth Delaney. 2012. "The Price of Prisons: What Incarceration Costs Taxpayers." *Center on Sentencing and Corrections.*

Hunt, Kim Steven, and Robert Dumville. 2016. *Recidivism Among Federal Offenders: A Comprehensive Overview.* Technical report. United States Sentencing Commission.

Hurd, Michael D., and Susann Rohwedder. 2013. "Expectations and Household Spending." *Working Paper.*

Jacobson, Louis S, Robert LaLonde, and Daniel Sullivan. 1993. "Earnings Losses of Displaced Workers." *American Economic Review* 83 (4): 685–709.

Jappelli, Tullio. 1990. "Who is Credit Constrained in the U. S. Economy?" *Quarterly Journal of Economics* 105 (1): 219–234.

Johnson, David S., Jonathan A. Parker, and Nicholas S. Souleles. 2006. "Household Expenditure and the Income Tax Rebates of 2001." *American Economic Review* 96 (5): 1589–1610.

Kaplan, Greg, and Giovanni L. Violante. 2014. "A Model of the Consumption Response to Fiscal Stimulus Payments." *Econometrica* 82 (4): 1199–1239.

Knowles, John, Nicola Persico, and Petra Todd. 2001. "Racial Bias in Motor Vehicle Searches: Theory and Evidence." *Journal of Political Economy* 109 (1): 203–229.

Lee, David S., and Justin McCrary. 2017. "The Deterrence Effect of Prison: Dynamic Theory and Evidence." Chap. 3 in *Regression Discontinuity Designs (Advances in Econometrics, Volume 38),* 73–146. Emerald Publishing Limited.

Mueller-Smith, Michael. 2015. "The Criminal and Labor Market Impacts of Incarceration." *Working Paper.*

Pager, Devah. 2003. "The Mark of a Criminal Record." *American Journal of Sociology* 108 (5): 937–975.

Parker, Jonathan A., Nicholas S. Souleles, David S. Johnson, and Robert McClelland. 2013. "Consumer Spending and the Economic Stimulus Payments of 2008." *American Economic Review* 103 (6): 2530–53.

Raphael, Steven, and Rudolf Winter-Ebmer. 2001. "Identifying the Effect of Unemployment on Crime." *The Journal of Law and Economics* 44 (1): 259–283.

Sabol, William J. 2007. "Local Labor Market Conditions and Post-Prison Employment Experiences of Offenders Released from Ohio State Prisons." In *Barriers to Reentry?: The Labor Market for Released Prisoners in Post-Industrial America,* edited by Weiman DF Bushway S Stoll MS, 257–303. B. New York: Russell Sage.

Salmond, John W. 1924. *Jurisprudence (7th ed.)* Sweet & Maxwell, Limited.

Shore, Stephen H., and Todd Sinai. 2010. "Commitment, Risk, and Consumption: Do Birds of a Feather Have Bigger Nests?" *The Review of Economics and Statistics* 92 (2): 408–424.

Sullivan, Daniel, and Till von Wachter. 2009. "Job Displacement and Mortality: An Analysis Using Administrative Data." *Quarterly Journal of Economics* 124 (3): 1265–1306.

Sullivan, James. 2008. "Borrowing During Unemployment: Unsecured Debt as a Safety Net." *Journal of Human Resources* 43 (2): 383–412.

Tuttle, Cody. 2019. "Snapping Back: Food Stamp Bans and Criminal Recidivism." *American Economic Journal: Economic Policy* 11 (2): 301–327.

Visher, Christy A., Sara A. Debus-Sherrill, and Jennifer Yahner. 2011. "Employment After Prison: A Longitudinal Study of Former Prisoners." *Justice Quarterly* 28 (5): 698–718.

Wachter, Till von, Jae Song, and Joyce Manchester. 2009. "Long-Term Earnings Losses due to Mass Layoffs During the 1982 Recession: An Analysis Using U.S. Administrative Data from 1974 to 2004." *Working Paper.*

Yang, Fang. 2009. "Consumption over the life cycle: How different is housing?" *Review of Economic Dynamics* 12 (3): 423–443.

Zeldes, Stephen P. 1989. "Optimal Consumption with Stochastic Income: Deviations from Certainty Equivalence." *Quarterly Journal of Economics* 104 (2): 275–298.

Zinman, Jonathan. 2010. "Restricting consumer credit access: Household survey evidence on effects around the Oregon rate cap." *Journal of Banking and Finance* 34 (3): 546 –556.

Figure 1: Distribution of Credit Scores
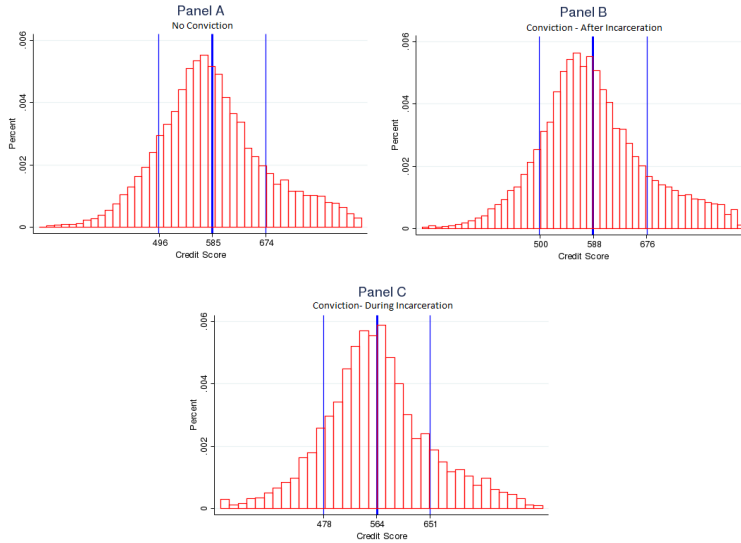
NOTES: This figure shows the credit score distribution for individuals not convicted, formerly incarcerated, and incarcerated at the time of credit report. All credit scores are taken after case resolution. The first, second, and third vertical lines indicate the 25th, 50th and 75th percentiles of credit score, respectively.

Figure 2: Relevance of Instrument
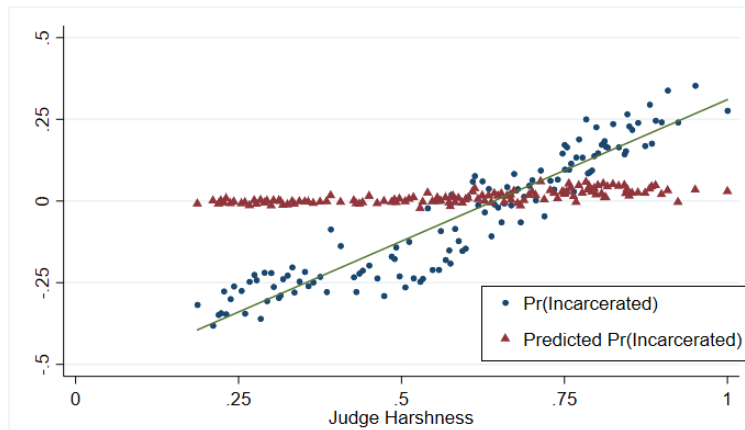
NOTES: This figure shows both the first-stage relationship between judge harshness and incarceration, as well as the exogeneity of the instrument. This blue circles show how judge harshness relates to incarceration by plotting court-year bins of individual incarceration likelihood and overall judge harshness. 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. The red triangles graph the predicted incarceration rate for a given harshness bin, where we predict incarceration as a function of predetermined observable characteristics. Reassuringly, there is no relationship between these predicted values and the measure of judge harshness.

Figure 3: Impact of Incarceration on Credit Scores: Event Study Estimates
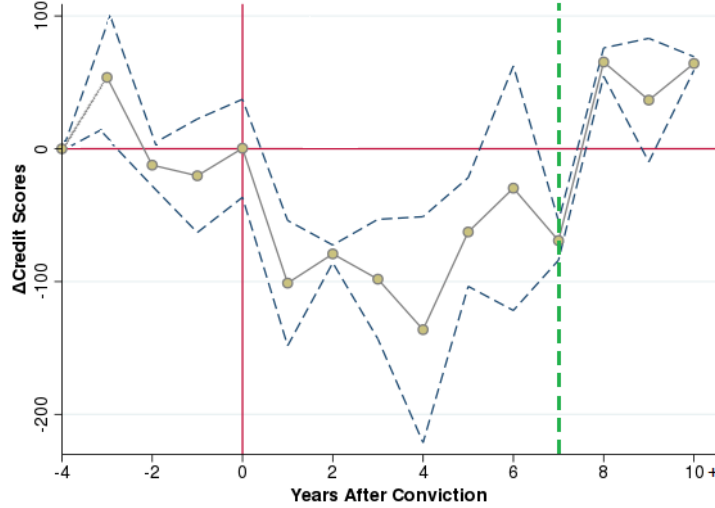
NOTES: This figure presents event-time estimates for the impact of incarceration on credit scores, where time is normalized to be year zero at the year of court disposition. We jointly instrument for each year before or after the year of incarceration using court-year fixed effects, and cluster at the court × year level. The dashed green line indicates when seven years after a conviction leading to incarceration. This point in time is significant for two main reasons: (1) under the Fair Credit Reporting Act (FCRA), credit reporting agencies can only delve 7 years into a person's past (including criminal records), and (2) after seven years, a Chapter 7 bankruptcy flag is removed (which is known to increase access to traditional credit and raise credit scores).

Figure 4: Hypothetical Lender Correction for Informational Distortions
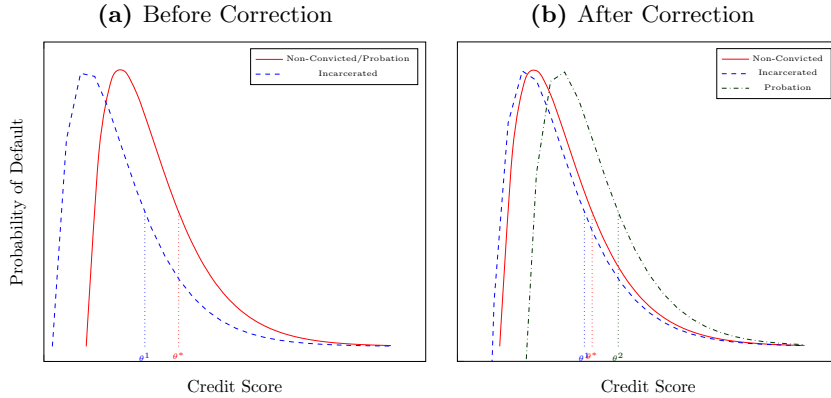

NOTES: This figure shows hypothetical distributions of default by credit score (holding income and other observable traits equal) for: (1) non-convicted individuals; (2) convicted individuals who are sentenced to incarceration; and (3) convicted individuals who are sentenced to probation. Panel (a) shows 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 $(\theta^2 > \theta^*)$.

Figure 5: Credit Limit Quasi-Experiments
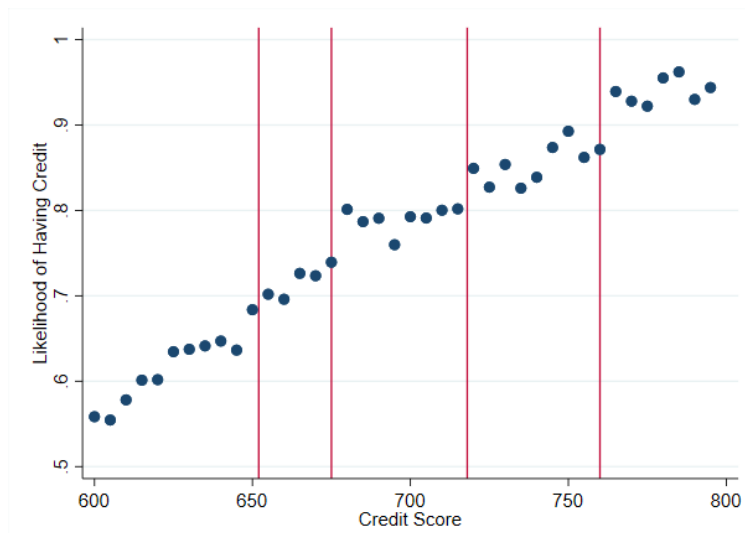
NOTES: This figure plots likelihood of having installment credit or credit cards against credit scores. To construct the binned scatter plot, we construct 5-point credit score bins and estimate the likelihood of having credit for each bin. The red lines indicate credit limit discontinuities for prime borrowers (credit score > 600) as estimated by threshold regressions.

Figure 6: Borrower Characteristics Around Credit Limit Quasi-Experiments
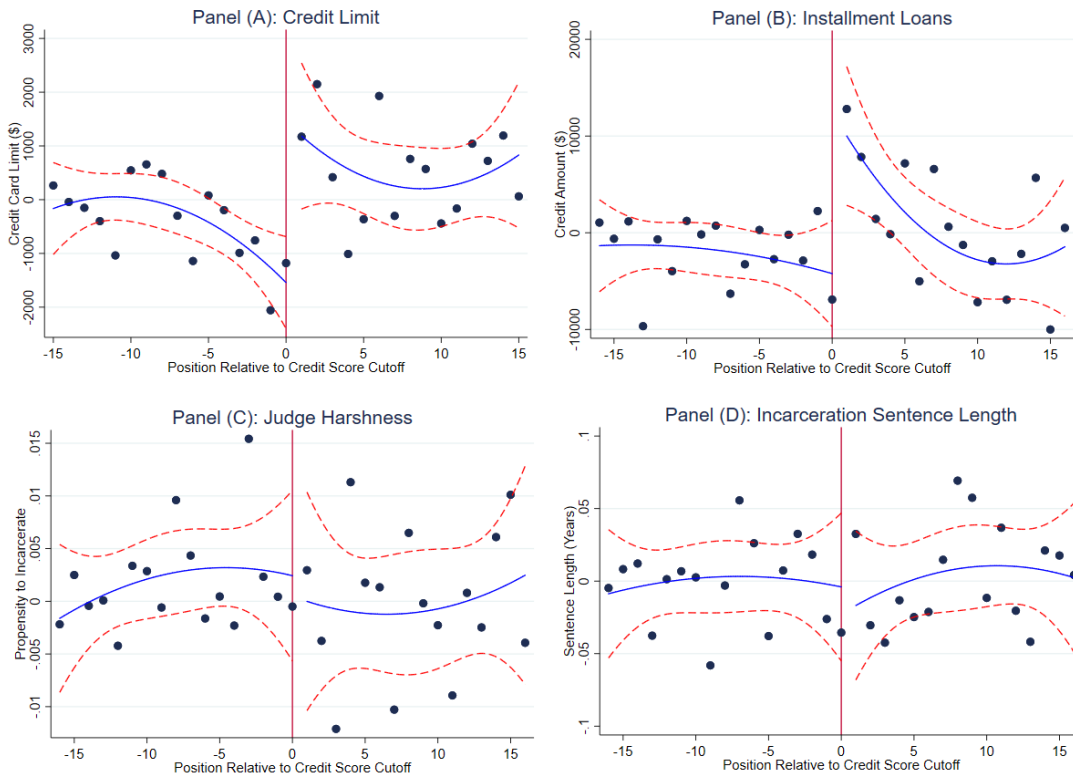
NOTES: Each panel in this figure presents borrower outcomes or characteristics around the credit score discontinuity. To construct the scatter plot we pool all the discontinuities together and average at each credit score point above or below the cutoff. The optimal bandwidth in credit score for the 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

NOTES: This table presents summary statistics for our sample. Panel A provides descriptive statistics for the full sample. Panel B provides the post-sentence information, and separated by conviction status.

Table 2: Test of Monotonicity

Dependent Variable:	Baseline Instrument (1) First Stage Pr(Incarcerated)	Reverse-Sample Instrument (2) First Stage Pr(Incarcerated)
A. INCARCERATION PROPENSITY		
1.Subsample: Incarceration Propensity 1st 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 Propensity 2nd 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 Propensity 3rd 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 Propensity 4th 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

Dependent Variable:	Baseline Instrument	Reverse-Sample Instrument
	(1) First Stage Pr(Incarcerated)	(2) First Stage 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 subsample where the instrument is used – that is a "reverse-sample" instrument (Bhuller et al. 2020). In Panel A, to assess if the instrument in monotonic along the harshness of the judge, we focus on quartile subsamples where the lowest quartile have the judges least likely to incarcerate and the highest quartile has the judges most likely to incarcerate. Within each subsample, we evaluate the performance of the full sample instrument. In Panel B, we construct subsamples by limiting to a particular type of offense. In Panel C, we construct subsamples along credit characteristics of the individuals. In Panel D, we construct subsamples along demographic characteristics. For Panels B, C and D, we estimate the performance of both the full sample and the reverse-sample instruments within each subsample.

Table 3: Impact of Past Incarceration on Credit Scores

	(1) OLS	(2) JIVE	(3) FE	(4) FE	(5) FE
Past Incarceration	-12.15	-44.23	-42.15	-56.45	-56.84
	(1.02)	(2.64)	(2.46)	(9.43)	(9.42)
Past Probation				8.39	7.52
				(12.90)	(12.92)
Fine				-0.00	-0.00
				(0.00)	(0.00)
Bail					0.34
					(1.98)
N	68,279	68,279	68,279	130,783	129,700
Year Disposition	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes
Sample	Main	Main	Main	Full	Full

NOTES: This table reports OLS and IV estimates of the impact of incarceration on credit scores. Columns (1) estimates the baseline OLS relationship between incarceration and credit score (ignoring concerns related to endogeneity), as in Equation 1. Column (2) estimates our main JIVE specification, based on the framework described in Equations 3 and 4. Finally, Columns (3)–(5) presents our IV estimates using court room fixed effects, to both demonstrate robustness (Column 3) and jointly instrument for additional punishment margins (Columns 4–5). All regressions control for fixed effects for both the year of disposition and the year in which credit is observed. Sample descriptions are discussed in Section 2. Errors clustered at the courtroom level.

Table 4: Impact of Past Incarceration on Credit Scores by Type of Offense

Panel A: By Offense Category					
	(1)	(2)	(3)	(4)	(5)
	DUI	Property	Drug	Violent	Other
Past Incarceration	-7.63	-32.59	-13.89	-40.28	-43.30
	(5.87)	(4.09)	(4.06)	(6.32)	(4.47)
N	19,617	9,631	6,820	9,836	13,909
Year Disposition	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes
Sample	Main-DUI	Main-Property	Main-Drug	Main-Violent	Main-Other

Panel B: Excluding Offense Category					
	(1)	(2)	(3)	(4)	(5)
	Non-DUI	Non-Property	Non-Drug	Non-Violent	Non-Other
Past Incarceration	-30.72	-38.85	-47.85	-37.53	-39.48
	(2.35)	(2.55)	(3.26)	(2.29)	(2.48)
N	48,662	58,648	61,459	58,443	54,370
Year Disposition	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes
Sample	Main/DUI	Main/Property	Main/Drug	Main/Violent	Main/Other

NOTES: This table reports IV estimates of the impact of incarceration on credit scores, by type of offense. In Panel (A), we estimate the impact of incarceration on credit score individually for each type of crime (DUI, property, drug, violent, and all other crimes, respectively). Regressions are based on the JIVE strategy described in Equations 3 and 4. In Panel B, we estimate the impact of incarceration for the full sample, *omitting* each type of offense. All regressions control for fixed effects for both the year of disposition and the year in which credit is observed. Errors clustered at the courtroom level.

Table 5: Past Incarceration on Financing of Durables

Outcome:	**1**(Auto Loan)			**1**(Mortgage)		
	(1) OLS	(2) JIVE	(3) FE	(4) OLS	(5) JIVE	(6) FE
Past Incarceration	-0.14	-0.25	-0.24	-0.09	-0.20	-0.19
	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
N	68,279	68,279	68,279	68,279	68,279	68,279
Year Disposition	Yes	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Main	Main	Main	Main	Main	Main

NOTES: This table reports OLS and IV estimates of the impact of incarceration on access to financing for durable goods (automobiles and homes). In Columns (1)–(3) we show the effect of incarceration on an individual's receipt of an automobile loan (an indicator variable). Column (1) estimates an OLS linear probability model of the relationship between incarceration and having an auto loan. Column (2) presents estimates from the JIVE specification, based on the framework described in Equations 3 and 4. Finally, Columns (3) presents our IV estimates using court room fixed effects. Columns (4)–(6) repeat this exercise, using receipt of home mortgage loan (an indicator variable) as the outcome of interest. All regressions control for fixed effects for both the year of disposition and the year in which credit is observed. Errors clustered at the courtroom level.

Table 6: Impact of Past Incarceration on Credit Scores

Outcome:	Sentence Length	Risk Scores		**1**(Auto Loan)		**1**(Mortgage)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Past Incarceration	1.12		-6.52		-0.06		-0.08
	(0.03)		(3.99)		(0.02)		(0.01)
Sentence Length		-36.19	-31.85	-0.17	-0.17	-0.15	-0.10
		(1.92)	(3.01)	(0.01)	(0.02)	(0.01)	(0.01)
N	68,279	68,279	68,279	68,279	68,279	68,279	68,279
Year Disposition	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Main	Main	Main	Main	Main	Main	Main

NOTES: This table reports IV estimates of both the extensive and intensive margin effects of incarceration on access to credit. In Column (1), we estimate the average effect of incarceration on sentence length. In Column (2), the outcome of interest is years of incarceration, where we estimate the (unconditional) intensive margin effect of incarceration. In Column (3) we jointly instrument for variation in sentence length and incarceration status using court-year fixed effects (since both are endogenous regressors). In Columns (4)–(5), we repeat the same exercise as Columns (2)–(3) where the outcome is an indicator variable for whether a person making a criminal court appearance has an automobile loan. In Columns (6)–(7), we repeat the same exercise where the outcome is an indicator variable for whether a person making a criminal court appearance has a home mortgage. Because the specification presented in Columns (3), (5), and (7) each include multiple endogeneous regressors, all regressions presented rely on court-year fixed effects as our instruments instead of the leave-one-out harshness instrument. All regressions control for fixed effects for both the year of disposition and the year in which credit is observed. Errors clustered at the courtroom level.

Table 7: 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 courtroom level.

Table 8: 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 courtroom level.

Table 9: Adverse Selection in Loan Performance (Outcome Test)

	Selection Test $\rho = corr(\eta, \nu)$							
	Past Incarceration				Past Probation			
	Observables		Unobservables		Observables		Unobservables	
η \ ν	(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 courtroom level.

Table 10: Effects of Access to Credit on Recidivism

	(1) JIVE All	(2) JIVE DUI	(3) JIVE Non-DUI	(4) JIVE No Prior Arrest
Past Incarceration × **1**(Credit Score < Threshold)	0.31	0.38	0.27	0.32
	(0.08)	(0.16)	(0.10)	(0.08)
Past Incarceration	0.39	0.49	0.27	0.20
	(0.07)	(0.16)	(0.07)	(0.08)
1(Credit Score < Threshold)	-0.13	-0.19	-0.10	-0.12
	(0.04)	(0.07)	(0.04)	(0.03)
N	10,174	3,581	6,593	6,503
Year Disposition	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes
Sample	Prime Borrowers	Prime Borrowers	Prime Borrowers	Prime Borrowers

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, instrumenting for incarceration using the JIVE approach. We show the main effects of credit limit, incarceration and incarceration times credit limit on recidivism. Column (1) is the effect for the main RD sample (which limits to prime borrowers). Columns (2)-(4) provide results for the same estimation, but limiting the sample to DUI offenders, non-DUI offenders, and first-time offenders, respectively. Errors clustered at the courtroom level.

Appendix: For Online Publication Only

Table of Contents

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:

$$U_Y(e) = \max\left\{\log(w\ e), (1-\mu)[\chi + \log(w\ e)] + \mu \frac{\log c_P + \log w^c e + \phi(w^c - w)}{2}\right\} \quad (9)$$

where $\phi(w)$ is increasing in wages and denotes potential gains or losses due to access to credit. Equation (9) 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 (9) 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[26] 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.*

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:

$$\xi_{it} = Incarcerated_{it} - (\hat{\beta}_0 + \tau_t + \hat{\beta}_1 Court_i \otimes \tau_t) \tag{10}$$

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.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:

---

26. 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.

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:

$$Y = \beta Incarcerated + \gamma \hat{\xi} + \eta \tag{11}$$
$$= (\beta + \gamma)Incarcerated - \gamma \widehat{Incarcerated} + \eta \tag{12}$$

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:

$$\hat{\beta}_{OLS} - \beta \quad = \frac{Cov(Y,Incarcerated)}{Var(Incarcerated)} - \beta = \gamma \frac{Cov(\hat{\xi},Incarcerated)}{Var(Incarcerated)} + \frac{Cov(\eta,Incarcerated)}{Var(Incarcerated)}$$
$$= \gamma \left\{ 1 - \frac{Var(\widehat{Incarcerated})}{Var(Incarcerated)} \right\} \tag{13}$$

Importantly, we can interpret equation (11) 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.[27]

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 12. 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 11) which show that, as expected, $\hat{\xi}$ is positively correlated with credit scores and log income, respectively.

B.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.

---

27. See, Chapter 4.4.2, Angrist and Pischke (2009).

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/alp)

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 different from your own and different from each other 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)

Current 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 Statistics

D.1 Merge Sample Selection

In Table (D.1), we evaluate whether matching court filing and credit bureau records creates a correlation between our instrument and entrance into the analysis sample. Importantly, to preserve individual privacy, the credit bureau returns only a random subsample of the matches – i.e., not all positive matches are returned to us. To test whether our instrument is correlated with being in the final analysis sample, we use all records sent to the credit bureau and estimate the relationship between judge harshness and a dummy variable equal to one if the record is in the returned analysis sample.

D.2 Top 10 Offenses in Sample

In Table (D.2), we show the frequency of the top 10 offenses in the sample. Since driving offenses are common, throughout the analysis we also show estimates excluding these.

D.3 Distribution of Defaults and Bankruptcies by Credit Score

In Figure (D.1), we show the distribution of 30-day defaults (Panel a), 90-day defaults (Panel b), and bankruptcies (Panel c). Distribution of defaults are important, for example, to determine how informative outcome tests are. We perform outcome tests in our analysis of information distortions (Section 5.4).

Each panel shows the distribution of defaults for individuals with past incarceration, former probationers, and acquitted individuals. Notably, in all panels, all three groups follow the same distribution of defaults up to an additive constant.

E Instrument Validity

E.1 Relevance

In Table (E.1), we show F-statistics for our first-stage specification (Eqs. 3) for demographic characteristics (Panel A) and disposition outcomes (Panel B). Two points are important to note. First, the F-statistics for disposition outcomes are large for all outcomes, but are especially large at the intensive margin (e.g., sentence length). Second, as a benchmark, we fit the model to demographic outcomes. The model makes a meaningfully better job at fitting disposition outcomes than demographic outcomes. Overall, this table complements Figure (2) to provide further evidence that the instrument is relevant both in absolute and relative terms.

E.2 Test of Randomization

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

$$JudgeHarshness_{j(i)t} = \beta_0 + \beta_1 PreSentenceTrait_{it} + \tau_t + \epsilon_{ijt} \tag{14}$$

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).

F Assessed Income

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).[28] 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:

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

F.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

---

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

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:

$$Y_{i,post-trial} = \beta_0 + \beta_1 \widehat{PastIncarceration}_{it} + \beta_2 \widehat{PastIncarceration}_{it} \times Y_{i,pre-trial} + \eta_{it} \quad (15)$$

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

Table (F.1) 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.

Appendix Figures and Tables

Figure B.1: Pre-Conviction Income Conditional on Future Incarceration by Judge Harshness
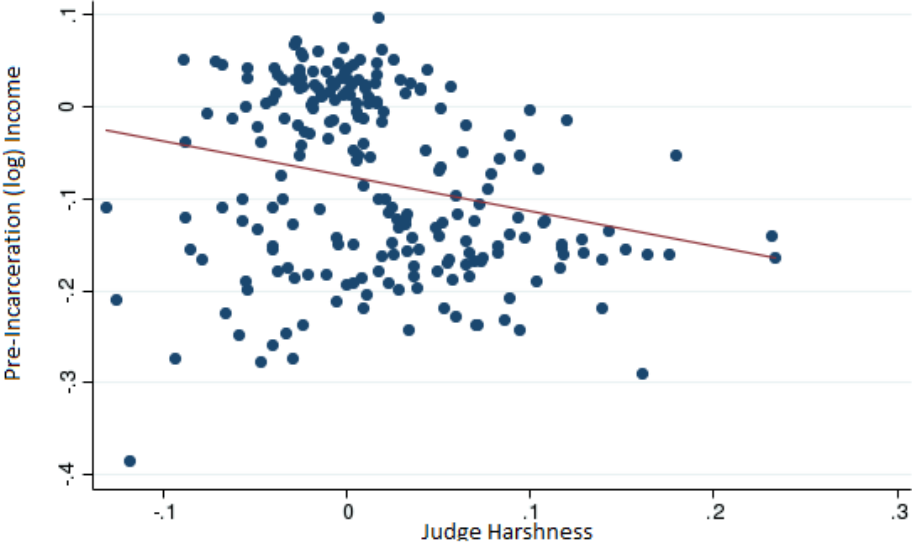
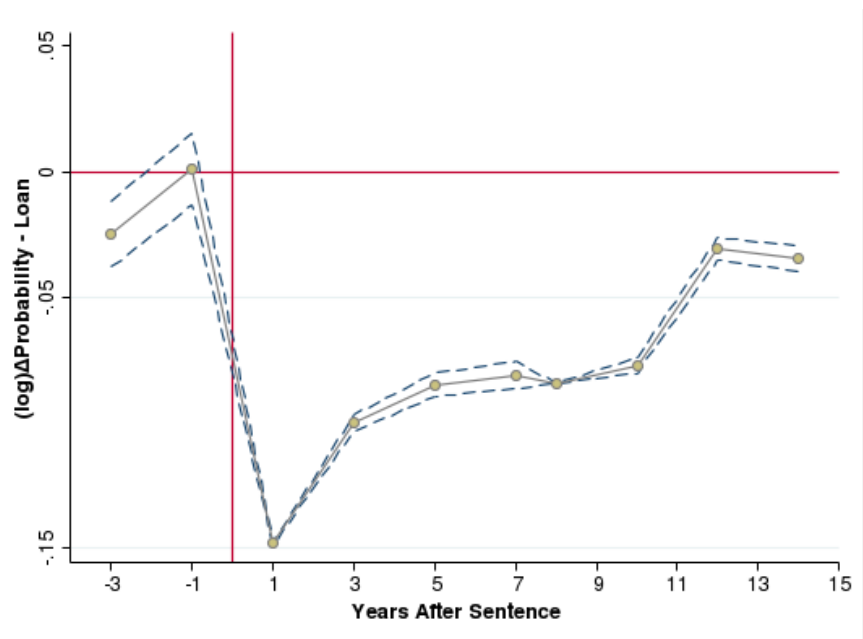NOTES: This figure plots pre-incarceration income for incarcerated individuals against judge harshness. Judge harshness in the leave-one-out mean of incarcerating for the assigned court at the year of disposition (verdict and sentence). To construct the scatter bin plot, we average 2006 income for individuals with year of conviction after 2006 by court-year. We plot against each court-year's judge harshness.

Figure B.2: Heterogeneous Effects of Incarceration on Credit Score by Criminal Type
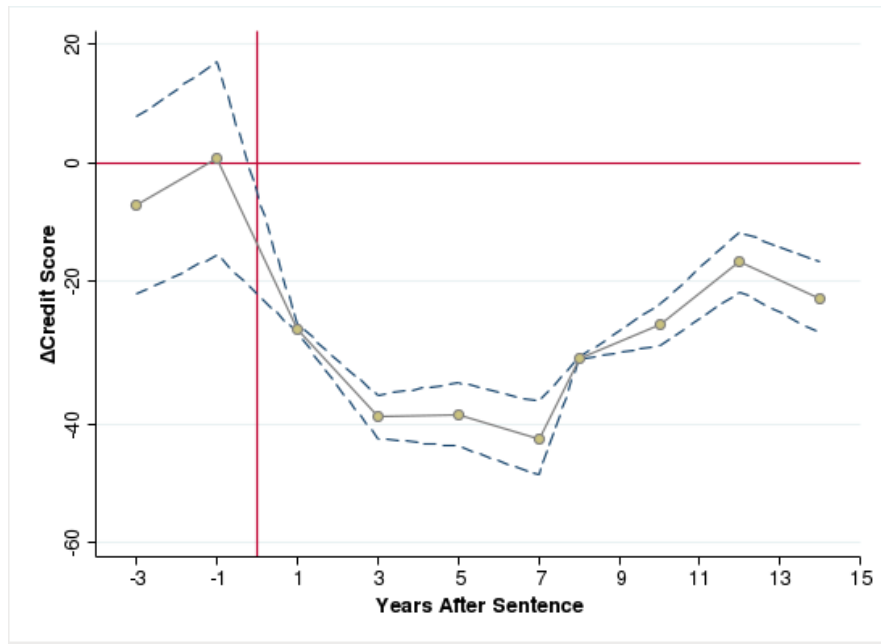
NOTES: This figure shows the effects of incarceration on credit scores by criminal type. Criminal types are computed according to equation (10). The plot shows the coefficient of the interaction of years since conviction × criminal type.

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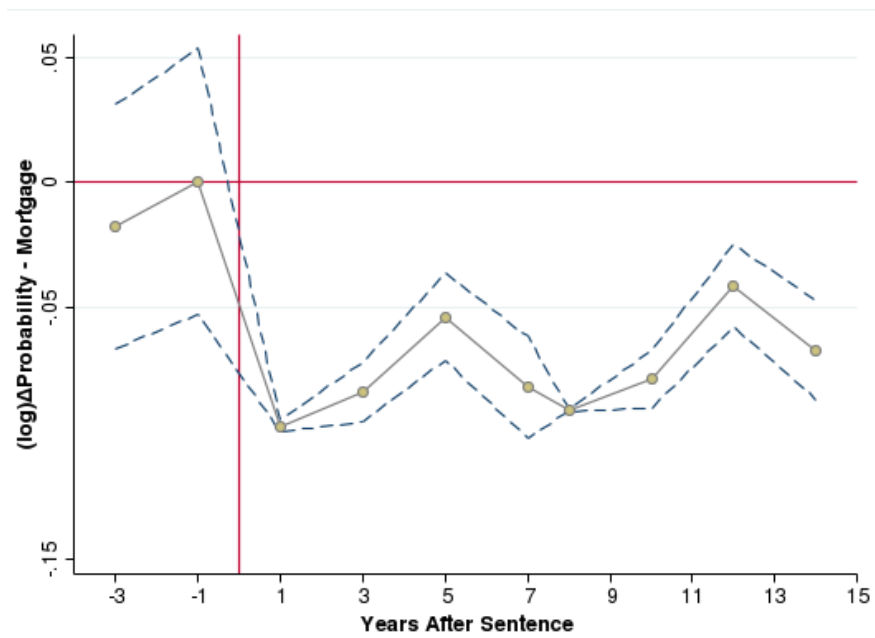

NOTES: This figure shows the effects of incarceration on loan approval by criminal type. Criminal types are computed according to equation (10). The plot shows the coefficient of the interaction of years since conviction × criminal type.

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 (10). 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

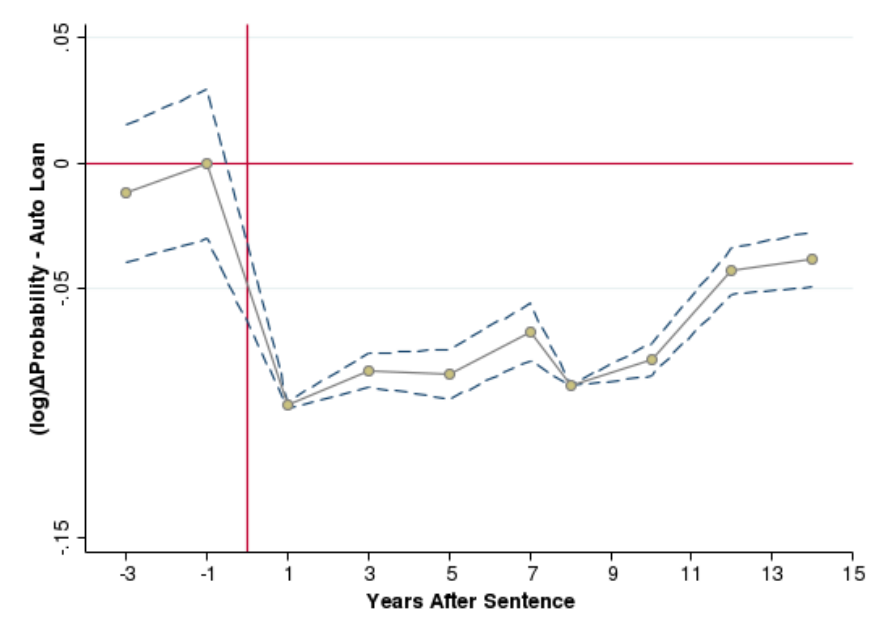NOTES: This figure shows the effects of incarceration on probability of obtaining an auto loan by criminal type. Criminal types are computed according to equation (10). The plot shows the coefficient of the interaction of years since conviction × criminal type.

Figure D.1: Realized Default Distributions

(a) 30-day defaults

(b) 90-day defaults
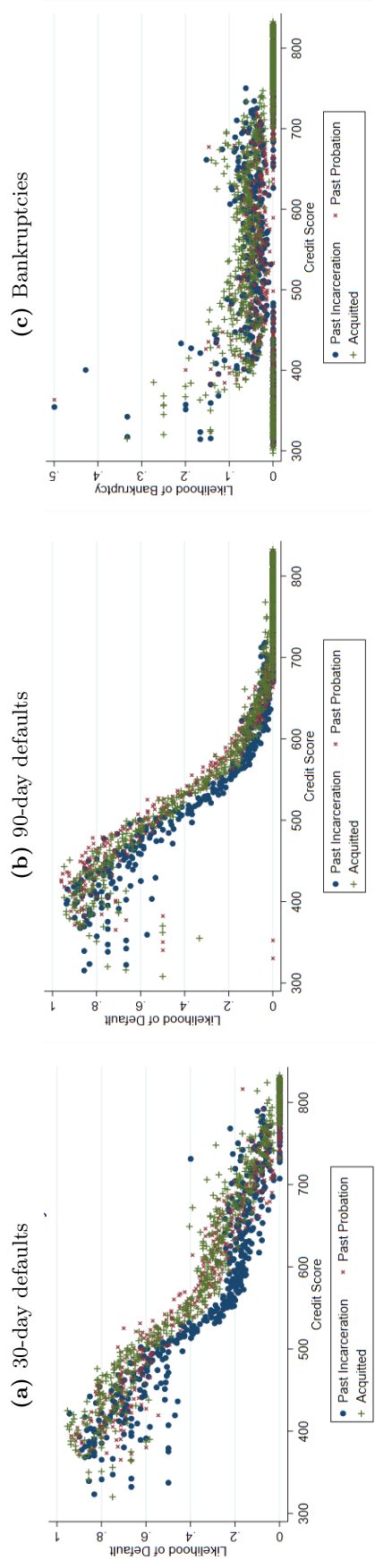
(c) Bankruptcies

NOTES: This figure shows the distribution of 30- and 90- day defaults (panels a & b), as well as bankruptcies (panel c). To make the figure we compute the probability of default or bankruptcy within each 1-credit score point bin, for formerly incarcerated individuals, former probationers, and acquitted individuals.

58

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 (12) and (13) 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) Before	(2) After	(3) After 1st Arrest	(4) Before	(5) After	(6) 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.

Table D.1: Merge Sample Selection

DV:1(Main Sample)	(1)	(2)	(3)	(4)
	All	Property	Violent	Other
Judge Harshness	-0.014	0.002	-0.002	-0.013
	(0.004)	(0.001)	(0.001)	(0.003)
Sent Sample	454,581	454,581	454,581	454,581
Year Disposition	Yes	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.

Table D.2: 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: This table 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 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:	Disposition Outcomes
Conviction	9.44
Incarceration	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 on the 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.

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 14 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.

Table F.1: Incarceration on Estimated Income by Conviction Type

	(1)	(2)	(3)	(4)
Incarceration	-0.25	-0.29	-0.25	-0.28
	(0.02)	(0.01)	(0.02)	(0.02)
Probation	-0.02	-0.02	-0.03	-0.03
	(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)
Bail			0.00	0.00
			(0.01)	(0.01)
Income 2006	0.82	0.82	0.82	0.82
	(0.01)	(0.01)	(0.01)	(0.02)
N	19,809	19,809	19,614	19,614
Year Disposition	Yes	Yes	Yes	Yes
Year Credit	Yes	Yes	Yes	Yes
Age	No	No	Yes	Yes
Sample	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 × year of disposition level.

Table F.2: 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 (15). Errors clustered at the court × year of disposition level.