Racial Disparities in Debt Collection*

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Abstract

Debt collection judgments have important implications for a debtor's financial stability, and these judgments are more common in black neighborhoods than in non-black ones. Using Experian Credit Report data, we find that individuals residing in majority black neighborhoods are 16% more likely to receive a debt collection judgment than individuals residing in majority non-black neighborhoods, even after controlling for differences in income, credit score, default rates, and a multitude of other credit characteristics. We also explore the possibility that collectors are using neighborhood-level information to estimate collection success likelihood using a second source of judgment data from Missouri. We again find that debt collection judgments are more common in majority black neighborhoods than in majority non-black neighborhoods, holding other factors constant. Neighborhood level differences in the previous share of contested judgments, the likelihood of attorney representation, and lending institutions cannot explain the racial gap in debt collection judgments.

JEL Codes: D14, D18, D63, G21, J15

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1 Introduction

Debt collection court cases have significant financial implications for debtors. For example, if a guilty verdict, referred to as a judgment, is issued, the defendant's wages can be garnished, and their bank accounts can be seized. These consequences, particularly when combined with compound interest and various legal fees, hinder debtors' ability to become financially stable and ultimately limit their ability to accumulate wealth. Furthermore, we know that a disproportionate number of judgments occur in predominantly black neighborhoods compared to neighborhoods with a lower share of black residents, even after controlling for differences in income levels across the two neighborhoods (Keil and Waldman, 2019).[1] However, there are several potential explanations for this disparity, including differences in creditworthiness and the composition of debt across neighborhoods.

In this paper, we explore the disparity in debt collection judgments across black and non-black neighborhoods. In particular, we estimate the size of the disparity after controlling for potential confounding factors and analyze mechanisms through which this racial disparity could be entering the debt collection system. We then use two different data sets, the first being an individual-level panel dataset from 2013 to 2018 containing information from individuals' Experian Credit reports that includes if individuals have any outstanding judgments. The second is a zip code-level panel dataset from 2004 to 2013 that links the number of debt collection judgments in each zip code in Missouri to data from Experian credit reports and the American Community Survey. We use a rich set of control variables from the Experian credit report data to track all aspects of both individuals' and neighborhoods' financial liabilities to help mitigate concerns about omitted variable bias. Our control variables include income, credit score, and debt characteristics such as the types of debt incurred, the number of accounts and balances, and detailed delinquency information. We address differences in unobservable characteristics across black and non-black neighbor-

[1] This ProPublica article concludes that the risk of judgment is twice as high in majority black neighborhoods than in non-majority black neighborhoods with similar income levels.

hoods by limiting our samples to only observations with common support over observable characteristics and controlling for county and year fixed effects.

Using the individual-level data, we find that the judgment rate is 1.6 times higher for individuals who live in a majority black neighborhood compared to individuals residing in a majority non-black neighborhood. Differences in incomes, credit scores, default rates, total debt levels, and other debt characteristics can explain a large portion of this baseline judgment gap. However, even after controlling for these differences, individuals in majority black neighborhoods have approximately 16% more outstanding judgments on their credit reports than those in majority non-black neighborhoods. Results are similar when we predict an individual's race based on the demographic distribution of their zip code, with black individuals having a 13% higher judgment rate than non-black individuals.[2]

We next explore potential mechanisms that could be driving this result. When dealing with unpaid balances on unsecured debt, creditors have two main options if negotiating with the borrower has proved unsuccessful: they can write off the debt or take the debt to collections. Several factors drive this decision, including the likelihood the debtor will ultimately repay the debt and the cost of collecting on the debt. While creditors are legally prohibited from using race as a factor when making decisions related to credit, they may use neighborhood characteristics to gather information about a debtor's ability to repay debt or to estimate the potential cost of collecting the debt. For example, it could be the case that defendants from black neighborhoods are less likely to hire an attorney or to contest the debt in court, making it less costly for debt collectors to obtain judgments in black neighborhoods. While creditors do not know this information a priori for any given individual, they could estimate the likelihood of such situations based on historical outcomes in a given zip code. To better understand if and to what extent such neighborhood-level characteristics are driving the judgment gap, we use a neighborhood-level panel dataset from Missouri. In this data, debt collection court cases are 40% more common in black neighborhoods compared to

[2]Such predictions are based on the racial composition and age distribution of neighborhoods.

non-black neighborhoods after controlling for income, credit scores, and a portfolio of debt characteristics at the neighborhood level.

Using this data, we find no statistical difference in the share of contested judgments across black and non-black communities, and controlling for attorney representation has a limited effect on the judgment gap. Another potential theory regarding the racial gap in debt collection judgments is that differences in lending institutions across majority black and majority non-black neighborhoods cause the positive correlation between neighborhood racial composition and the judgment rate. However, the racial gap in judgments remains after controlling for the number of banks and payday lenders in a given area. We also control for differences in homeownership rates, HMDA based mortgage rejection rates, and annual information on housing prices from Zillow to explore if differences in housing-related wealth drive the relationship between race and judgments. The judgment gap remains positive and statistically significant after adding these controls.

Lastly, we explore the extent to which certain plaintiff types (e.g., major banks, debt collectors, high-cost lenders) are driving our results. Judgment rates are higher in black communities for every plaintiff type except medical lenders, although, certain plaintiffs consistently show more racial imbalance in their lawsuits than others. Our results are robust to controlling for an alternative measure of credit score, estimating our models with alternative samples, and to using a double machine learning-based estimation strategy.

There are two potential explanations that we cannot explore using our current data: differences in wealth that are not driven by housing values and discrimination. Laws prohibit using race to make decisions regarding access to credit, and thus many creditors do not collect information on the race of borrowers. Furthermore, juries are not typically used in debt collection cases and, if a case is heard in front of a judge, such cases are usually fairly algorithmic with a limited amount of subjectivity involved. It is more likely that the unexplained racial gap in debt collection judgments results from the broader disadvantages experienced by black communities. For example, according to estimates provided by the

United States Census Bureau in 2016, the typical black household has a net worth of \$12,920, while that of a typical white household is \$114,700 - this is a \$101,780 difference in wealth that could have important implications for a household's ability to mitigate negative financial shocks. About \$35,000 of this wealth gap is not driven by home equity and would not be captured in our current analysis. By translating this wealth gap into differences in annual income and using our estimates of the relationship between income and judgments, we calculate that a wealth gap of this size would explain roughly 80% of the remaining judgment gap across black and non-black communities.[3]

There is a large literature that documents the role that race plays in the legal and criminal justice system. For example, black individuals are more likely to be searched for contraband (Antonovics and Knight 2009), to have biased bail hearings (Arnold et al. 2018), and to be charged with a serious offense (Rehavi and Starr 2014). Discrimination in the labor and housing-related market has also been well documented (e.g., Bartlett et al. 2019, Ritter and Taylor 2011, Bertrand and Mullainathan 2004, and Turner et al. 2002). Such disparities have contributed to racial differences in wealth, and a growing literature explores how the racial wealth gap was generated and how it has persisted over time (e.g., McKernan et al. 2014 and Akbar et al. 2019). While much attention has been given to racial disparities in general, this is the first economic analysis to empirically document racial disparities in debt collection judgments.

Aside from the literature documenting racial disparities across many dimensions, this paper also contributes to a growing literature about the debt collection industry.[4] We know

[3]Our most conservative estimate of the judgment gap is 0.53 more judgments per 100 people in majority black neighborhoods compared to majority non-black neighborhoods and is derived from Oster (2016). We computed the difference in annual savings needed over a 40-year horizon to generate a wealth gap of \$35,000. We found that an annual difference of \$2,910 is sufficient to generate the wealth gap in net present value. For the interest rate, we applied the stock market's historical return, which between 1957 through 2018 is roughly 8%. Consistent with estimates from the U.S. Bureau of Economic Analysis, we assume an 8% personal savings rate. Under these assumptions, this wealth gap translates into an annual income difference of \$36,375. Increasing the median income of majority black neighborhoods by this amount would eliminate roughly 80% of the judgment gap.

[4]See Hunt (2007) for an overview of the debt collection industry and details about its institutional structure and regulatory environment.

that consumers who creditors and debt collectors sue are drawn predominantly from lower-income areas (Hynes, 2008). Other more recent studies have investigated the role of information technology in the collection of consumer debts (Drozd and Serrano-Padial, 2017), documented the link between debt collection regulations and the supply of consumer credit (Fedaseyeu, 2015), and determined if consumers are made better or worse off by settling their debt outside of court (Cheng et al., 2019). Interpreted broadly, the literature has documented the impacts of the debt collection process on consumer outcomes; however, racial disparities in debt collection have not been empirically explored.

The rest of our paper is outlined as follows: Section 2 discusses the typical debt collection litigation process in the United States, as well as the laws regulating access to credit and debt collection procedures; Section 3 describes our data; Section 4 outlines our empirical strategy; Section 5 documents the racial gap in debt collection judgments and discussions potential mechanisms driving the disparity; Section 6 presents various robustness checks; and Section 7 concludes.

2 Background Information

The debt collection industry in the United States is large and growing. According to a 2018 annual report by the Consumer Financial Protection Bureau, debt collection is a $10.9 billion industry that employs nearly 120,000 people across approximately 8,000 collection agencies in the United States. In 2010 alone, U.S. businesses placed $150 billion in debt with collection agencies. When the debt is unsecured, the owner of the debt can either negotiate with the debtor to bring their debt to current, write off the debt, or file a debt collection lawsuit. This section will summarize the key institutional details surrounding debt collection lawsuits and the laws regulating the debt collection industry.

2.1 Debt Collection Litigation Process

Debt collection litigation typically begins when a creditor files a "Summons and Complaint" in a state civil court.[5] This document names the parties involved and states the amount owed (including interest and, in some cases, attorney fees and court costs). The summons is served to the defendant to notify them of the lawsuit. It also provides the defendant with additional information, including the deadline for which the debtor must file a formal response to the court. If the debtor does not meet this deadline, the creditor will usually ask the court to enter a default judgment, at which point the defendant is obligated to abide by the court's ruling and is subject to the punishments requested by the court.

For most routine debt collection lawsuits, if the debtor files a formal response to the lawsuit, a trial date will be requested and set by the court. In some courts, settlement conferences are held to provide both parties with the opportunity to settle the case before the trial. Once the creditor obtains a judgment, the creditor might request a "debtor's examination," which would require the debtor to appear in court and answer questions about their finances. This process informs the creditor how it can collect the judgment. The most common methods for enforcing the judgments are to garnish wages or bank accounts.[6] If a dispute is settled before trial, the creditor gives up the ability to collect on the debt by garnishing the debtor's bank accounts or wages and therefore creditors often requires a one-time lump sum payment to drop the suit.

2.2 Laws Regulating Debt Collection

Debtors are granted some protections throughout the debt collection process. The 1977 Fair Debt Collection Practices Act (FDCPA) is the primary federal law governing debt collection practices. The statute's stated purposes are as follows: to eliminate the abusive practices used to collect consumer debts, such as calling the debtor at all hours of the night and

[5]These courts have many different names, including municipal court, superior court, justice court, county court, etc.

[6]Courts can also seize and sell the debtor's personal property, though this is relatively uncommon.

showing up to their place of employment, to promote fair debt collection, and to provide consumers with an avenue for disputing and obtaining validation of debt information to ensure the information's accuracy.

The Consumer Credit Protection Act (CCPA) of 1968 restricts the amount of earnings that creditors can garnish from defendants' weekly disposable income to 25% or the amount by which disposable earnings are greater than 30 times the minimum wage. State laws can increase the share of wages protected from debt collection garnishments. For example, a creditor in Missouri can only garnish 10% of after-tax wages if the debtor is the head of their household. However, the burden to assert these protections is typically on the debtor, and take-up is relatively low. There is no federal law limiting the amount of savings that can be seized from a debtor's bank accounts.

While not directly related to debt collection, other protections have been put in place to protect consumers in the credit market. For example, the Equal Credit Opportunity Act (ECOA), enacted in 1974, makes it illegal for creditors to discriminate against any applicant based on their race, color, religion, national origin, sex, marital status, age, or participation in a public assistance program.[7] The law applies to everyone who regularly participates in a credit decision, including banks, retail and department stores, bankcard companies, finance companies, and credit unions. The ECOA applies both to the decision to grant credit as well as setting the terms of credit.

Furthermore, the Fair Credit Reporting Act (FCRA) of 1970 promotes the accuracy, fairness, and privacy of consumer information contained in the files of consumer reporting agencies. The law was intended to protect consumers from the inclusion of inaccurate information in their credit reports. More recently, the Credit Card Accountability Responsibility and Disclosure (CARD) Act of 2009 established fair and transparent credit card practices. Key provisions include giving consumers enough time to pay their bills, prohibiting retroactive rate increases, making it easier to pay down debt, eliminating "fee harvester cards",

[7] This law is enforced by the Federal Trade Commission (FTC), the nation's consumer protection agency.

and eliminating excessive marketing to young people. Despite these protections, abusive debt collection practices still exist, and, as we will show, debt collection judgments are more common in majority black neighborhoods.

3 Data

This paper uses two different datasets to explore differences in debt collection judgments across black and non-black neighborhoods. The first is an anonymous individual-level panel dataset that spans from 2004Q1 to 2018Q2 with quarterly observations. The panel is nationally representative and contains a random draw of over half a million individuals with an Experian credit report. The data consists of over 200 variables that allow us to track all aspects of individuals' financial liabilities, detailed delinquencies, various types of debt with the number of accounts and balances, and an individual's credit score. The race of each individual is not reported, but the data does contain each individual's age, zip code, and an estimate of their income.[8] Importantly, this data indicates the number of judgments outstanding against a given individual in a given quarter from 2013Q1 to 2018Q2.

The second dataset is a panel dataset containing zip codes in Missouri. This dataset includes debt collection court cases filed in Missouri between 2004 and 2013, along with other neighborhood characteristics.[9] For each zip code in our sample, we know the number of debt collection lawsuits filed and the number of judgments arising from these lawsuits. We also see the number of cases that resulted in a default judgment (meaning the debtor did not show up to court), a consent judgment (meaning the debtor showed up to court

[8]The Equal Credit Opportunity Act (ECOA) generally prohibits a creditor from inquiring about the race, color, religion, national origin, or sex of an applicant. As a result, many data sets lack information about the race of an applicant. One exception is applications for home mortgages covered under the Home Mortgage Disclosure Act (HMDA).

[9]The Missouri court data was generously provided by Kiel and Waldman (2015). They acquired individual court case data from the state court administration. Their white paper focuses on three jurisdictions: Cook County, Illinois (composed of Chicago and surrounding suburbs), St. Louis City and St. Louis County, Missouri, and Essex County, New Jersey (composed of Newark and suburbs). The court data included basic case information such as the plaintiff, the defendant, and the defendant's address. The defendant's race is not reported.

and admitted to owing the debt), and the number of cases that were contested (meaning that some aspect of the debt was disputed). We know if the defendant was represented by an attorney and the plaintiff type (categorized into the following groups: auto, debt buyer, high-cost lender, major bank, medical, utility, and miscellaneous).

We supplement the Missouri court data with Experian credit report data aggregated to the zip code level to control for credit scores, default rates, and other neighborhood-level debt characteristics.[10] Zipcode tabulation data from the IRS from 2004 - 2018 is used to control for the average income and the number of filers as a fraction of all filers with income under \$25,000, \$25,000-50,000, \$50,000-75,000, \$75,000-100,000 and over \$100,000. Also included is zip code tabulation data from the 2009-2013 American Community Survey to control for population, racial composition, the unemployment rate, and other socioeconomic variables of interest at the zip code level.[11] For our neighborhood-level analysis, our outcome variable of interest is the number of judgments per 100 people. Neighborhood racial composition is our primary independent variable of interest. More specifically, we classify a zipcode as a majority black zip code if more than 50% of its residents are black.

Throughout our analysis, we control for other neighborhood-level characteristics that may be biasing our results. For example, we document the number of lending institutions accessible to each neighborhood as an additional proxy for a neighborhood's financial well-being.[12] We use Census ZIP Code Business Patterns (ZCBP) data to get access to the number of banks and payday lenders in each zip code. Following Butta (2014), we use the following two North American Industrial Classification System (NAICS) codes to capture payday lending establishments: non-depository consumer lending (establishments primarily

[10]We calculate the median credit score, the average number of delinquent accounts, and other credit measures for each zip code in our sample.

[11]USPS ZIP Codes are not areal features used by the Census but a collection of mail delivery routes that identify the individual post office or metropolitan area delivery station associated with mailing addresses. ZIP Code Tabulation Areas (ZCTAs) are generalized areal representations of United States Postal Service (USPS) ZIP Code service areas.

[12]This measure can alternatively be thought of as a measure of access to credit markets. However, the presence of payday lenders in a given neighborhood could be the result of financial distress as opposed to the cause.

engaged in making unsecured cash loans to consumers) and other activities related to credit intermediation (establishments primarily engaged in facilitating credit intermediation, including check cashing services and money order issuance services).[13] For each zip code, we use arcGIS to create a weighted average (based on land area) of the number of banks and payday lenders that exist within a five-mile radius from each zip code's centroid. We use Fixed Broadband Deployment Data from Federal Communications Commission to document access to online credit markets for every zip code in our sample. Single-family house price data from Zillow and differential mortgage denial rates by zip code are also used to control for housing-related wealth and access.

3.1 Sample Selection

Our main neighborhood-level specifications focus only on debt collection cases from Missouri. Aside from the fact that Missouri has a centralized database of cases tried in different circuit courts, previous research has documented that Missouri is a representative state in terms of collection (Ratcliffe et al., 2014 and Cheng et al., 2019). More specifically, Missouri is representative in terms of the percentage of delinquent consumers and the average amount of debt in collections (Ratcliffe et al., 2014). Missouri is also not particularly exceptional regarding its law surrounding collections, its share of black residents, or its level of inequality (Cheng et al., 2019).

We have additional debt collection judgment data from all New Jersey counties and Cook County, Illinois (composed of Chicago and surrounding suburbs). However, it is less detailed than the Missouri data. This data includes the number of cases filed in a five-year window from 2008-2012, but it is not broken down by type of judgment (default, consent, contested), defendants' names or representing attorney. As such, all of our main specifications use data from Missouri due to its representative nature, high level of detail, and because we want to keep our sample consistent across specifications. However, when comparable information is

[13] Barth et al. (2016) discuss how this proxy could likely overstate the number of payday lenders. We adjust these measures to correct for states that prohibit payday lending to help reduce this bias.

available, analogous analysis is provided in the appendix.

There are important differences in observable characteristics across majority black and non-majority black neighborhoods and the individuals who live in these neighborhoods.[14] Majority black neighborhoods tend to have lower median credit scores, lower average incomes, lower house values, higher unemployment rates, and a higher share of divorced individuals. These differences cause concern that there could also be important differences in unobservable characteristics that vary with the racial composition of neighborhoods. We mitigate this concern by limiting both our datasets to samples that only contain observations with common support over observables (Crump et al. 2009). More specifically, we use logistic regressions to restrict our dataset to a common support. We estimate:

$$\Phi(M_{ict}) = \beta_0 + \theta X_{ic} + \epsilon_{ic} \tag{1}$$

where M_{ict} is an indicator variable equal to one if observation i in county c in period t is (or resides in) a majority black neighborhood and X_{ict} is a vector of other controls for observation i in county c in period t. Controls include income, credit score, debt levels, default rates, and other credit characteristics for the individual-level analysis. For the neighborhood level dataset, controls included quintiles of the credit score distributions, the fraction of IRS filings under \$25,000, between \$25,000-50,000, \$50,000-75,000, \$75,000-100,000, and over \$100,000, the average income, median credit scores, the Gini index of income inequality, 90+ days-past-due debt balances, unemployment and divorce rates, population density, median house value, and education attainment levels such as the fraction with at least a bachelors degree or with less than a high school diploma.

[14] Examples of such differences are documented in Figure A1. This figure shows kernel density estimates of various covariates that are used throughout this analysis.

3.2 Summary Statistics

Summary statistics for the individual-level data are presented in Table 1 and summary statistics for the neighborhood-level data are presented in Table 2. The individual common support sample contains approximately 500,000 individuals observed quarterly from 2013 to 2018. The neighborhood-level common support sample consists of an unbalanced panel of over 800 zip codes from Missouri observed annually from 2004 to 2013.[15] Both tables report our main variables of interest across majority black and majority non-black zip codes and indicate that majority black neighborhoods had higher judgment rates than non-black neighborhoods. In the individual-level dataset, approximately 3% of individuals in non-majority black neighborhoods had an outstanding judgment on their credit score compared to 6% of individuals in majority black neighborhoods. When focusing on the Missouri neighborhood-level dataset, majority black neighborhoods had 2.7 judgments per 100 people compared to 1.2 judgments per 100 people in non-black neighborhoods. Table 1 suggests that individuals residing in majority black neighborhoods had lower incomes, lower credit scores, and more debt balances that are 90 days past due, patterns similarly reflected in the neighborhood level data presented in Table 2. The neighborhood-level dataset provides some additional insights. Majority black neighborhoods had a higher share of cases result in default judgments and a lower share of cases in which an attorney represented the defendant. Furthermore, black neighborhoods have more payday lenders, lower median house values, lower homeownership rates, higher unemployment rates, and higher divorce rates. Most of these differences are significant at the 1% level.

One of our primary goals is to estimate the disparity in debt collection judgments across black and non-black neighborhoods. Figure 1 graphically illustrates the baseline relationship between the judgment rate and the percentage of black individuals in a zip code using the neighborhood level dataset from Missouri. This figure classifies zip codes into one of one

[15]Table A1 in the appendix documents sample size restrictions imposed based on data availability and restricting our sample to the common support.

hundred bins based on their share of black residents and plots the average share of black population in each bin against the average judgment rate of each bin. The size of the bubbles corresponds to the number of zip codes in each of the bins; as expected, there are many low share black neighborhoods and relatively less high share black observations. The regression line represents the fit between the average judgment rate in the bin and the black population share weighted by the number of observations in the bin. We see that the judgment rate is positively correlated with the black population share of the zip code.

This positive relationship between the racial composition of a neighborhood and debt collection judgments could be driven by a number of factors. Figure 2 graphically explores how this relationship varies by the median income and credit score of each neighborhood. Panel (a) of Figure 2 plots neighborhoods by their median income and the judgment rate per 100 people, with darker red circles representing neighborhoods with a higher share of black residents and darker blue circles representing neighborhoods with a lower share of black residents. This figure documents a negative relationship between the judgment rate and median income, with the judgment rate decreasing as median income increases. However, even looking at neighborhoods with similar income levels, we see higher judgment rates for majority black neighborhoods. Panel (b) of Figure 2 plots neighborhoods by their median credit score and the judgment rate per 100 people, with darker red circles representing neighborhoods with a higher share of black residents and darker blue circles representing neighborhoods with a lower share of black residents. This figure documents a negative relationship between the judgment rate and median credit scores. Once again, racial disparity is evident in this figure, with majority black neighborhoods having higher judgment rates than majority non-black neighborhoods with similar median credit scores. Figure 2 suggests that differences in income and credit scores may not be the primary mechanism driving the racial disparity in debt collection cases, results that are formalized using regression analysis in the following sections.

4 Empirical Specification

We use a fixed effect framework with our common support sample to estimate the impact of neighborhood racial composition on the debt collection judgments. After limiting our sample to only observations within the common support, we further limit omitted variable bias by using a number of control variables combined with county and time fixed effects.

Our empirical specification is given by the following equation:

$$y_{ict} = \alpha + \beta M_{ic} + \theta X_{ict} + \gamma_c + \lambda_t + \epsilon_{ict} \tag{2}$$

where, at the individual level, y_{ict} is a binary variable indicating if individual i in county c has an outstanding judgment on their credit score in quarter t, M_{ict} is an indicator variable equal to one if individual i in county c in quarter t lives in a majority black neighborhood (defined as a neighborhood where the black population is greater than 50%), and X_{ict} is a vector of other controls for individual i in county c in quarter t. Such controls include income, credit score, measures of debt balances by type of debt (credit card, medical, student loans, etc.), debt composition (type of debt as a share of total debt balances), delinquent balances by the length of delinquency (30 days, 60 days, 90 days), and bankruptcy/collection flags. In addition, our main specification includes county fixed effects to control for time-invariant differences across counties and quarter fixed effects to control for any time-varying changes that impact all individuals (such as the Credit Card Accountability Responsibility and Disclosure Act of 2009).

We run a similar specification using our neighborhood level dataset from Missouri. Our outcome of interest is the number of judgments per every 100 people in zip code i in county c in year t. In addition to county and year fixed effects, we control for neighborhood-level income and credit score measures, measures of debt balances by type of debt, debt composition, delinquent balances by the length of delinquency, and bankruptcy/collection flags. We also control for other neighborhood level observable characteristics such as the

Gini index, unemployment rate, median housing values, education levels, and divorce rate. To further limit omitted variable bias and to better understand the mechanisms driving the racial disparity in debt collection judgments, we include a number of additional control variables at the neighborhood level. Such control variables include the number of banks and payday lenders within a five-mile radius of each zip code, the share of unscored borrowers, home prices, and mortgage rejection rates. All regressions are weighted by population in our neighborhood-level analysis, and standard errors are clustered at the county-year level. Due to the small number of clusters, we also report the p-values from a wild cluster bootstrap for inference (Cameron and Miller, 2015).

5 Results

We start with our individual level results, from which we ultimately conclude that debt collection judgments are 16% more common when individuals live in majority black neighborhoods as opposed to majority non-black neighborhoods. We then present our neighborhood-level results to better understand potential mechanisms that could be driving this disparity.

5.1 Individual Level Results

To establish a baseline judgment gap, we begin by documenting the racial disparity in judgments across majority black and majority non-black neighborhoods controlling only for county and year fixed effects. Column (1) of Table 3 shows that majority black zip codes experience a 2.62 percentage point increase (or 87% increase over the 3% average in majority non-black neighborhoods documented in Table 1) in the probability of having a judgment on their Experian credit report after controlling for county and quarter fixed effects.

5.1.1 Income, Credit Score, and Debt Characteristics

Some of the baseline difference in the judgment rate is driven by differences in characteristics that are correlated with both the racial composition of a neighborhood and the likelihood of receiving a debt collection judgment. Two important difference in the observable characteristics of individuals residing in majority black versus non-black neighborhoods is differences in incomes and credit scores. Column (2) of Table 3 adds controls for income (as estimated by Experian) to the previous specification; however, the estimated coefficient of interest does not change significantly from our baseline estimate. Column (3) of Table 3 adds an individual's credit score to the baseline specification. We see that individuals residing in majority black zip codes have a .8 percentage point increase in the likelihood of having an outstanding debt collection judgment on their credit report compared to other individuals. This coefficient implies that differences in the credit score distribution can explain roughly 72% of the judgment gap between individuals residing in black versus non-black neighborhoods. Column (4) adds income and credit score controls into the same specification, and the coefficient changes very little from that presented in Column (3). This result suggests that approximately 28% of the judgment gap remains unexplained after controlling for differences in income and credit score of individuals residing in black versus non-black communities.

It could be the case that controlling for differences in credit scores does not adequately capture all the relevant differences between the credit reports of individuals residing in black and non-black neighborhoods. For example, individuals living in majority black neighborhoods might tend to acquire the type of debt that is more likely to be collected in court. To explore whether differences in the debt portfolios of individuals residing in black versus non-black neighborhoods drive the racial disparity in debt collection judgments independent of credit score, we use the plethora of information from the Experian Credit Report data to control for such differences. Relevant controls include total debt levels, debt composition, payment amounts, utilization ratios, and delinquency rates.

Our results are presented in Table 4. Each specification includes controls for income and credit scores, as well as county and quarter fixed effects. Column (1) adds additional controls for total debt levels, including breakdowns for the type of debt such as credit card debt, mortgage debt, and student loan debt. Column (2) includes controls for payment amounts and utilization rates. Column (3) includes debt composition controls, such as credit card debt as a share of total debt. Column (4) adds additional delinquency and collection controls, including the total debt balances that are 30 days, 60 days, or 90 days delinquent, as well as bankruptcy and collection flags. Lastly, Column (5) includes all of these controls together. With the addition of these controls, we see that individuals residing in majority black neighborhoods have a 0.48 percentage point higher likelihood of having an outstanding debt collection judgment on their credit report, which is a 16% higher judgment probability compared to the average judgment probability of individuals living in non-black neighborhoods. These results indicate that differences in debt characteristics cannot explain the entirety of the racial disparity in debt collection judgments.

5.2 Neighborhood Level Results

Why then do individuals living in majority black neighborhoods experience a higher share of judgments compared to individuals residing in non-majority black neighborhoods, despite having similar incomes, credit scores, and debt characteristics? It could be that creditors are using neighborhood-level information to help determine the profitability of their debt collection efforts[16] For example, they may focus their collection efforts in neighborhoods where defendants are less likely to hire an attory or to contest the debt in court. This section uses neighborhood-level data from Missouri to explore if and to what extent neighborhood-level characteristics can explain the racial disparity in debt collection judgments. We begin by replicating the findings from the individual analysis, which used nationally representative judgment data from Experian, with judgment data from Missouri aggregated to the zip code

[16] Creditors can legally factor proxy variables into their decision-making if there is a legitimate business necessity, such as scoring credit risk, even if these tactics cause disparate outcomes across space (Barlett et al., 2021).

level. We then explore various neighborhood-level characteristics that could be driving the relationship between neighborhood racial composition and debt collection judgments, including attorney representation, previous case outcomes, and the types of lending institutions available to individuals.

5.2.1 Income, Credit Score, and Debt Characteristics

To establish a baseline judgment gap, we start by documenting the racial disparity in judgments across black and non-black neighborhoods controlling only for county and year fixed effects. Column (1) of Table 5 shows that majority black neighborhoods have about 1.4 more judgments per every 100 people compared to non-black neighborhoods. This coefficient implies that the baseline judgment rate in black neighborhoods is more than double that of non-majority black neighborhoods, where the average judgment rate is 1.26 judgments per 100 people.

We next progressively add additional control variables to our baseline specification to explore if and to what extent neighborhood-level measures of income and credit score are driving the relationship between a neighborhood's racial composition and the number of judgments per 100 individuals. Column (2) of Table 5 adds controls for average ZIP code level income and the fraction of IRS filings under \$25,000, between \$25,000-50,000, \$50,000-75,000, \$75,000-100,000, and over \$100,000, to the previous specification. After controlling for differences in the income distribution across black and non-black neighborhoods, we see that black neighborhoods are associated with 0.86 more judgments per 100 people. Column (3) in Table 5 adds credit score quintiles and median credit score to the baseline specification. We see that majority black zip codes are associated with 1.08 more judgments per 100 individuals. Column (4) adds income and credit score controls into the same specification. The coefficient on the black majority indicator variable drops to .79, suggesting that over 50% of the judgment gap remains unexplained after controlling for differences in the income and credit score distributions across black and non-black communities.

Column (5) adds controls for total delinquent debt balances, unemployment rate, median house value, the fraction of the population with a college education, the divorce rate, and population density.[17] In this specification, we see that majority black neighborhoods have a 56% higher judgment rate than non-black neighborhoods. Column (6) uses a one-year lag of our income, credit score, and baseline controls, and the coefficient of interest changes only slightly.

Following the individual-level analysis, we next explore if differences in debt portfolios across black and non-black neighborhoods could explain the remaining racial judgment gap. Results are presented in Table 6 and mirror those shown and discussed in Table 4, although now each debt measure has been aggregated to the neighborhood level. We continue to control for income, credit score, and the baseline controls discussed above. The estimated coefficient is always statistically significant and is approximately equal to 0.70 more judgments per 100 people, or a 56% higher judgment rate in majority black neighborhoods compared to majority non-black neighborhoods.

5.2.2 Attorney Representation

One potential difference in the cost of collecting debt across black and non-black neighborhoods is the likelihood that an attorney represents a given debtor. While this is not known a priori, creditors could theoretically estimate these likelihoods when deciding which of their delinquent accounts to bring to court. We begin by investigating whether debt collectors target neighborhoods where defendants are less likely to have an attorney or contest the debt.

To explore how differences in attorney representation are driving our result, we explore the disparity in attorney representation and investigate whether this impacts our main findings.

[17] According to estimates provided by the United States Census Bureau in 2016, the typical black household has a net worth of $12,920, while that of a typical white household is $114,700. This difference could play an important role in driving the racial gap in debt collection. Since most wealth accumulated to middle or low-income households is through homeownership, we use housing values to help control for differences in wealth levels across majority black and non-majority black neighborhoods.

These results are presented in Columns (1) and (2) of Table 7. Each specification in this table includes the income, credit score, and baseline controls discussed in Table 5, as well as county and year fixed effects. The outcome variable in Column (1) is the share of debt collection court cases where an attorney represented the defendant; this result shows that defendants in majority black neighborhoods are less likely to have an attorney represent them in a debt collection court case. However, as seen in Column (2), where our dependent variable is once again judgments per 100 people, controlling for the share of cases in which an attorney represents defendants cannot explain the racial disparity in debt collection cases.

We take this as evidence that attorney representation does not impact the number of debt collection judgments; this does not imply that attorney representation is not meaningful or important in debt collection court cases. Debt collection laws often place the burden to assert various legal protections, including the share of the debtor's wages that can be garnished as the result of a judgment on the debtor. Thus, attorney representation is important in protecting debtors' rights throughout the debt collection process, even if such cases ultimately end in judgments.

5.2.3 Judgment Type

It could instead be the case that debt collectors target their collection efforts in areas where defendants are less likely to show up to court, resulting in a default judgment, or in areas where defendants don't tend to contest the debt in court. In other words, debt collectors might avoid collecting in areas where defendants tend to argue some aspect of the debt owed, which could result in the plaintiff exerting more effort or spending more money to collect the debt.

To explore the extent to which differences in the share of contested versus uncontested cases could be driving our result, we document the impact of neighborhood racial composition on the share of different types of judgments. Our outcome variable in Column (3) Table 7 is the share of cases in which the defendant admitted to owing the debt, and our outcome variable in Column (4) is the share of cases that were contested. We see no racial differences along these dimensions. Columns (6), (7), and (8) explore the share of cases that resulted in default judgments, were dismissed, or were settled, respectively. We see that a larger share of cases in black neighborhoods resulted in default judgments or was dismissed, while a smaller share was settled before court.

Given that the rank order preference of case outcomes is not obvious, we explore how these differences could be impacting the judgment gap by including the lagged share of case outcomes into our main specification, where our outcome variable is once again judgments per 100 people. The inclusion of lagged case outcomes does not mitigate the judgment gap across black and non-black neighborhoods. These results suggest that previous differences in case outcomes across neighborhoods and plaintiffs' potential strategic decisions to exploit this information for cost savings measures do not explain why judgments are more common in black neighborhoods.

5.2.4 Lending Institutions

We have shown that a racial disparity in debt collection judgments exists, even after controlling for differences in the income and credit score distributions across black and non-black neighborhoods. These differences are unlikely to be driven by strategic decisions of creditors based on the likelihood of attorney representation or the outcomes of previous cases. In this section, we explore another potential mechanism that could be driving the racial disparity in debt collection judgments - differences in lending institutions across black and non-black neighborhoods.[18]

To explore this potential explanation, we use arcGIS to create an index that measures the number of banks and payday lenders within 5 and 10-mile radii from each zip codes' centroid. We also use broadband access as a proxy for access to online credit markets and

[18]The presence of payday lenders is likely the result of financial distress as opposed to the cause. As such, we view these controls as an additional measure of the financial well-being of neighborhoods as opposed to a measure of credit access.

the share of credit reports unscored as a proxy for access to credit.[19] We add these variables as controls to our main specification.

This analysis only used data from 2008-2012, and thus our sample size is slightly smaller. We replicate the main results on this subsample of the data in Column (1) of Table 8 for context. Columns (2) and (3) add controls for broadband access and the share of unscored accounts in a zip code, respectively; both measures have no meaningful impact on our coefficient of interest. Columns (4) and (5) add our controls for the number of banks and payday lenders within 5 and 10-mile radii. We see that the number of banks is negatively correlated with the judgment rate, while the number of payday lenders is positively correlated with the number of judgments. Adding these controls decreases the coefficient on black majority by 14%, though a large racial gap in the number of judgments issued across black and non-black communities remains.

5.2.5 Differences in Plaintiff Type

Lastly, we explore if a specific plaintiff category drives differences in judgment rates across black and non-black zip codes. For each zip code, we know the number of judgments awarded to the following plaintiff types: auto, debt buyer, high-cost lender, major bank, medical, utility, and miscellaneous. Debt buyers account for 48% of plaintiffs in our sample. Medical lenders, major banks, and high-cost lenders are the next largest plaintiff categories accounting for 20%, 13%, and 6% of plaintiffs, respectively. The other plaintiff categories are combined into the miscellaneous category.

Our results are presented in Table 9. Each specification includes the income, credit score, and baseline controls discussed in Table 5, as well as county and year fixed effects. Column (1) repeats the main analysis and includes judgments from all plaintiff types (this is the same result presented in Column (5) of Table 5). Column (2) limits the outcome variable to

[19]An unscored credit report is one in which there is not enough information on a consumer's credit report to issue a formal credit score. This variable could serve as a measure of access to credit if unscored reports correlate with limited access to credit markets instead of a limited desire to obtain credit.

only judgments obtained by debt buyers, Column (3) to major banks, Column (4) to medical companies, Column (5) to high cost lenders, and Column (6) to any other lender.

The coefficients estimated across each specification should not be directly compared due to differences in the baseline judgment rates in non-black neighborhoods across these different plaintiff types. For example, the judgment rate in non-black neighborhoods was 0.57 judgments per 100 people for debt buyers, 0.14 judgments per 100 people for miscellaneous lenders, and 0.08 judgments per 100 people for high cost lenders. These baseline levels imply that majority black neighborhoods have a 38% higher judgment rate than non-black neighborhoods among debt buyers, 13% higher among major banks, and a 156% higher judgment rate among high cost lenders. The racial gap in debt collection judgments is not prevalent among medical lenders. In our estimation, the miscellaneous lenders category consistently showed the highest racial imbalance in their lawsuits, followed closely by high cost lenders.

5.2.6 Differences in Wealth

One potential explanation that we cannot directly test is that differences in wealth levels across black and non-black communities drive the judgment gap. According to estimates provided by the United States Census Bureau in 2016, the typical black household has a net worth of $12,920, while that of a typical white household is $114,700 - this is a $101,780 difference in wealth that could have important implications for a household's ability to mitigate negative income shocks. About $35,000 of this wealth gap is not driven by home equity. By translating this wealth gap into a difference in annual income and using our estimates of the relationship between income and judgments, we calculate that a wealth gap of this size would explain much of our most conservative estimate of the judgment gap across black and non-black communities.[20]

[20] Our most conservative estimate of the judgment gap is 0.53 more judgments per 100 people in majority black neighborhoods compared to majority non-black neighborhoods and is derived from Oster (2016). We computed the difference in annual savings needed over a 40-year horizon to generate a wealth gap of $35,000. We found that an annual difference of $2,910 is sufficient to generate the wealth gap in net present value. For the interest rate, we applied the stock market's historical return, which between 1957 through 2018 is roughly 8%. Consistent with estimates from the U.S. Bureau of Economic Analysis, we assume an 8%

Figure 3 plots the evolution of the racial disparity from 2004-2013. The racial disparity is present over our whole sample period; however, it increases dramatically during the Great Recession. This pattern could be taken as evidence that majority black neighborhoods had less wealth to help mitigate the negative shocks associated with the recession or that the recession disproportionately impacted them.

6 Robustness Checks

This section provides various robustness checks, including using a continuous measure of racial composition, using an alternative measure of default risk, and exploring selection on unobservables. We also present our results using alternative judgment data sources and alternative dependent variables.

6.1 Neighborhood Racial Composition

In this section, we show that our results are robust to using the share of black residents in a neighborhood instead of a binary measure. Columns (1) and (2) of Table 10 present this result. Column (1) shows a positive and statistically significant coefficient on the share of black residents within a zip code. This result suggests that a neighborhood with only black residents would have 1.5 more judgments per 100 people than a similar neighborhood with no black residents, more than double the baseline judgment rate in non-majority black neighborhoods. Column (2) shows our preferred specification from Table 5, which uses our binary measure for a black neighborhood for comparison. Tables A2 through A9 in the appendix replicate our main neighborhood-level results using the share of black residents instead of a binary measure. Results are statistically and economically similar to the results from our primary analysis.

personal savings rate. Under these assumptions, the wealth gap translates into an annual income difference of $36,375. Increasing the median income of majority black neighborhoods by this amount would eliminate roughly 80% of the judgment gap.

Since many creditors do not collect racial demographic information, the main analysis focuses on disparities across majority black and majority non-black neighborhoods. One could instead explore the racial composition of defendants within a neighborhood; however, since racial demographic information is often not collected by creditors, it is not available in our data sets. We instead use a Bayesian Improved Surname Geocoding (BISG) to estimate the racial composition of the defendant pool (Elliott et al., 2009). This algorithm uses both geography and surname-based information to calculate the probability a given individual is black.[21] Results are then aggregated up to the neighborhood level, allowing us to examine the racial composition of defendants from a given neighborhood. Columns (3)-(4) present the results. Once again, the results are positive and statistically significant.[22]

6.2 Alternative Credit Score and Other Additional Controls

In Table 11, we replicate Table 5 but add an alternative control for credit score that was calculated using a deep learning algorithm. The model consistently outperforms standard credit scoring models when predicting default rates (Albanesi and Vamossy [2019]). This alternative credit score has more predictive power than credit score in predicting default. However, it does not mitigate the racial disparity we see in judgments across black and non-black communities. This finding provides additional support that differences in credit scores, which measure a borrower's likelihood of default, are not the main factor driving the judgment gap between black and non-black communities.

Each of our main specifications includes a static measure of housing values to control for differences in housing-related wealth across majority black and non-black neighborhoods. However, it is likely the case that neighborhoods experience differential housing market dynamics. To address concerns that our static neighborhood-level measures of housing values

[21] Section A2 in the appendix provides more details.

[22] We also replicate Tables 5 and 6 using our BISG estimate of the racial composition of the defendant pool in Tables A10 and A11 in the appendix. We find results that are similar in size and statistical significance. We similarly expand our individual-level findings in Tables A12 and A13 in the appendix using the predicted race of an individual based on their zip code and age instead of the racial composition of their neighborhood. Results are quantitatively and statistically similar to those presented in Tables 3 and 4.

could miss such dynamic differences in housing values across majority black and majority non-black neighborhoods, we add an annual zip code level measure of home prices from Zillow to our model. We also know that differential mortgage denial rates exist across white and black neighborhoods (Bartlett et al. [2021]). We, therefore, supplement our model with HMDA data documenting mortgage denial rates by zip code to see if such differences can explain the disparity in debt collection judgments across black and non-black neighborhoods.

Table 12 presents these results. Column (1) shows our preferred results on the subsample of data for which HMDA data is available, and column (3) shows our preferred specification on the subsample of data for which Zillow data is available. Columns (2) and (4) show how our coefficient of interest changes when HMDA and Zillow data are added to the empirical model. Adding these controls decreases the coefficient on black majority by 30%, though a large racial gap in the number of judgments issued across black and non-black communities remains.

6.3 Alternative Dependent Variables and Weighting

Throughout our main neighborhood-level analysis, we choose to focus on the number of judgments per 100 individuals as our primary outcome variable of interest. The results from that analysis suggest there are more judgments per 100 people in majority black neighborhoods as opposed to majority non-black neighborhoods. In Column (1) of Table 13 we instead document the share of cases that resulted in a judgment. We see that majority black neighborhoods are 3 percentage points more likely to have a case result in a judgment. A lower share of settled cases before trial is the primary driving force behind this difference. Due to the fact that settling cases pre-trial often requires a large lump sum payment, this finding provides suggestive evidence that defendants from majority non-black neighborhoods are better able to mitigate negative shocks.[23]

[23] Since settling a case often requires a one-time lump sum payment, defendants who settle tend to have worse subsequent credit outcomes (Cheng et al. 2019). This finding suggests that a lower propensity to settle cases before a trial could help defendants from majority black neighborhoods in the long run.

We further alter our outcome variable of interest by normalizing by the number of judgments per 100 people with tax filings under $25,000 as opposed to normalizing by the total population. This specification presents an alternative way to address concerns that the risk set of individuals potentially exposed to debt collection judgments is different across black and non-black neighborhoods. The results are presented in Columns (2) of Table 13. Column (4) presents similar results but with the number of judgments being normalized by 100 people with subprime or worse credit. While these coefficients can not be directly compared to our previous results due to differences in the sample averages of judgments per 100 people and judgments per 100 people with an increased likelihood of default, we still see positive and statistically significant coefficients on our black majority indicator variable.

Column (6) of Table 13 present results that windsorized our main outcome variable at the 99th percentile to mitigate the impact of outliers. The positive and statistically significant coefficient suggests that outliers are not driving our main findings. Lastly, we explore the impact of not using population weighting. Our main neighborhood-level analysis uses population weighting since we are interested in the aggregate and representative gap. However, as shown in Table 2, black zip codes are more populated than non-black zip codes, meaning the judgment gap could be magnified by (or potentially solely driven by) the decision to population weight our regressions. As such, we also run equally weighted regressions. We consistently find coefficients that are of very similar signs, magnitudes, and statistical significance, as evidenced by Columns (3), (5), and (7) that replicate our previous three results.

6.4 Selection on Unobservables

We also investigated the impact of selection on unobservables on coefficient stability (Oster [2019]). In particular, we used Column (5) of Table 5 as our benchmark and found that given a selection on unobservables that is half the size of the selection on observables, our coefficient on black majority is reduced to 0.53 with a 95% confidence interval ranging from

[0.26 to 0.81].[24] This suggests majority black neighborhoods experience approximately 40% more judgments than non-majority black neighborhoods.[25] This finding suggests that a racial gap is unlikely to be zero, even after controlling for any unobservable characteristics.

6.5 Additional Judgment Data Sources

Lastly, we replicate our main results wherever possible with zip code level data from New Jersey and Cook County, Illinois. The results are presented in Table A14 through A16, which replicate Tables 2, 5, and 6 using data from New Jersey and Cook County, Illinois. Table A14 shows that judgments per 100 people are larger in majority black neighborhoods compared to majority non-black neighborhoods, while median income and median credit score tend to be lower. Table A15 confirms that differences in median income or median credit score cannot explain the racial gap in debt collection judgments and A16 shows that other debt characteristics are not driving the racial disparity in debt collection judgments. These results suggest that judgments are 18% higher in black neighborhoods compared to non-black neighborhoods.[26]

7 Conclusion

We find that individuals residing in majority black neighborhoods are 16% more likely to have an outstanding debt collection judgment on their credit report than individuals residing in non-majority black neighborhoods, even after controlling for differences in income, credit score, default rates, and other credit characteristics. We confirm the robustness of this result

[24] We bootstrapped our treatment coefficient estimates 100 times, and assumed a maximum R^2 value of 0.9.

[25] We also examined the proportion of selection of unobservables to observables that would explain away our treatment effect. We found that a ratio of 1.91 with a 95% confidence interval ranging from [0.89, 2.92] is sufficient to explain away our findings.

[26] We also relax the common support assumption and replicate our main results on the entire Missouri sample. These results are presented in tables A17 and A26 in our online appendix. Again, we find very similar results to our main specification, which uses only the common support sample, suggesting that omitted variables correlated with observable neighborhood characteristics are not biasing our results in any particular direction.

using a second dataset containing zip codes in Missouri to explore potential explanations for the disparity in debt collection judgments. The racial gap in debt collection judgments across black and non-black neighborhoods is robust to the inclusion of controls such as homeownership rates, unemployment rates, divorce rates, annual housing prices, annual income, and mortgage denial rates from HMDA. This racial disparity cannot be fully explained by the previous share of contested versus uncontested cases across black and non-black communities, differences in debt characteristics, differences in attorney representation, or differences in lending institutions. Furthermore, the racial gap in debt collection judgments exists for every plaintiff type (albeit at different magnitudes) except medical lenders.

There are two potential explanations that we cannot explore using our current data: differences in wealth and discrimination. It is unclear where discrimination would occur during the legal process, as most cases are fairly algorithmic and heard by a judge with no jury necessary. Furthermore, Keil and Waldman (2015) quote Lance LeCombs, the Metropolitan St. Louis Sewer District's spokesman, who claims his company has no demographic data on its customers and treated them all the same. He said the racial disparity in its suits was the result of "broader ills in our community that are outside of our scope and exceed our abilities and authority to do anything about." One such broader ill is the average difference in wealth across black and white households. Back of the envelope calculations suggest that closing the wealth gap would help to close the judgment gap across black and non-black communities.

As the number of debt collection cases rises, it is crucial to identify both the extent to which racial disparities exist and how they are entering the debt collection system. Future research should explore policies meant to provide more protections to consumers and their impact on the racial disparity in debt collection judgments. For example, such reforms could require debt buying companies to prove they own the debt before they can sue a debtor, preventing companies from winning judgments when the statute of limitations has expired on a debt[27], or require collection attorneys to prove they have a legal right to collect attorney fees and provide an itemized list of their work on the case to win an attorney's fee through a default judgment[28]. When states do provide legal protections for debtors, such as allowing those with children to keep more of their pay under a head of family exemption, the burden is typically on the debtor to assert these protections. Another policy reform could require a clear notice that these are provided to debtors.

[27] In most states, the law currently requires defendants to know that the statute of limitations has expired, and raise it as a defense in court.

[28] Currently when companies sue, they often request such fees, which are usually granted and passed on to the debtor as part of the judgment. For example, in Missouri, the fees are typically set at 15 percent of the debt owed, even though attorneys may spend only a few minutes on a suit.

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Figures

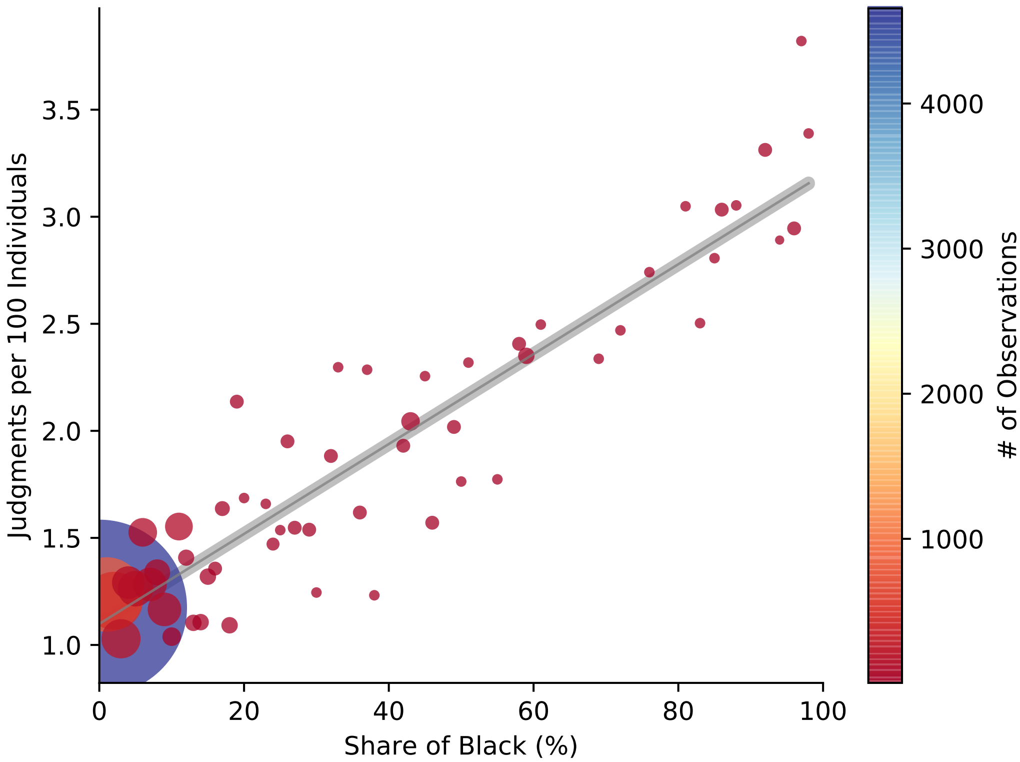
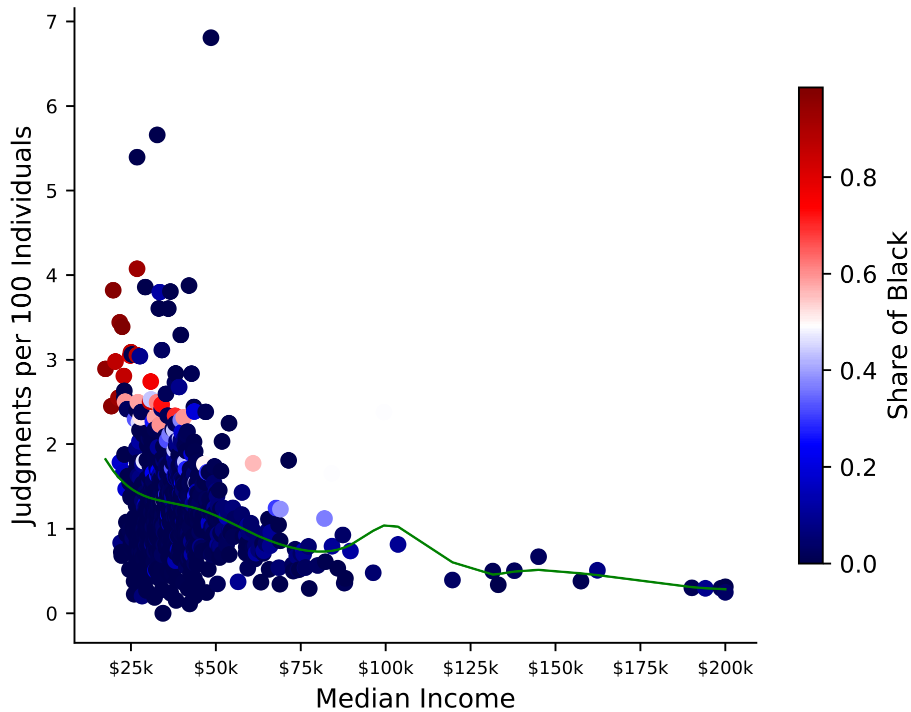
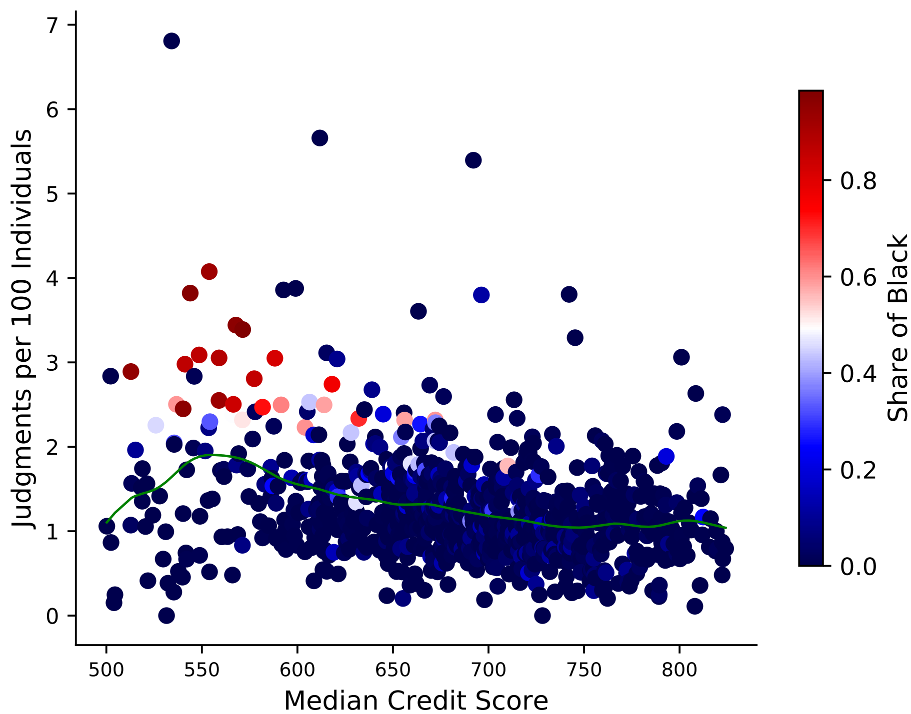


Figure 1: Judgments and Demographic Composition

Notes: Zip code level data from Missouri is used to estimate and illustrate a linear regression estimating the relationship between share of black population and judgment rate. We categorized zip codes into one of one hundred bins based on their share of black residents and plotted the average share black of each bin against the average judgment rate of each bin. The size of the bubbles corresponds to the number of observations in each of the bins. The regression line represents the fit between the average judgment rate in the bin and the share of black population weighted by the number of observations in the bin.



(a) Median Income and Judgment Rate



(b) Median Credit Score and Judgment Rate

Figure 2: Income, Credit Scores, and Judgment Rate

Notes: These figures plot the neighborhood level data from Missouri. The green line represents the non-parametric locally weighted regression line (LOESS) showing the smoothed fit curve of the data. Income is top coded at $200K USD to mitigate the impact of outliers.

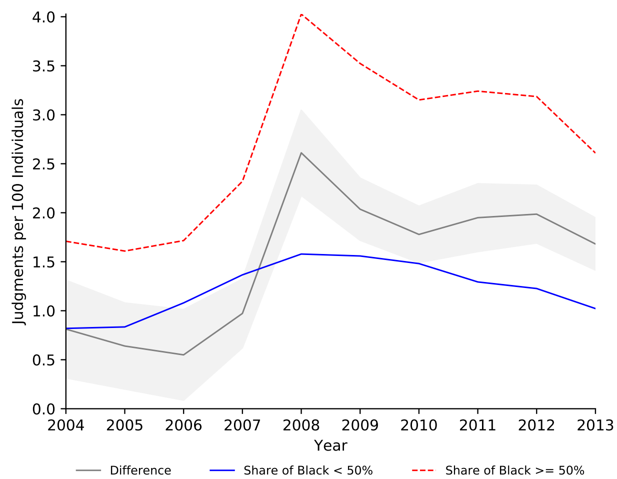


Figure 3: Disparity over Time

Notes: This figure uses our neighborhood level panel dataset from Missouri. Plotted in gray is the estimated coefficient and 95% confidence interval from a regression which estimates the racial disparity in judgments by year. The red dashed lined shows the average number of judgments per 100 individuals for majority black neighborhoods while the blue solid line shows the average number of judgments per 100 individuals for neighborhoods with less than 50% of black residents.

Tables

Table 1: Summary Statistics for Individual Level Experian Data

	Black	Non-Black	Difference
Share with Judgments	0.06	0.03	-0.03***
	(0.23)	(0.16)	
Household Income	43.82	51.61	7.79***
	(71.09)	(52.32)	
Credit Score	610.87	684.70	73.83***
	(120.38)	(113.25)	
90+ DPD Debt Balances	3933.21	2109.77	-1823.44***
	(31475.04)	(23339.70)	
Age	47.92	49.01	1.09***
	(16.34)	(16.54)	
Observations	707919	12229556	12937475

Notes: Individual level common support sample. Standard deviations are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Summary Statistics for Neighborhood Level Judgment Data

	Black	Non-Black	Difference
Panel A: Judgments and Financial Characteristics			
Judgments per 100 People	2.71	1.26	-1.46***
	(1.29)	(0.75)	
Share of Default Judgments	0.45	0.37	-0.08***
	(0.07)	(0.13)	
Share of Consent Judgments	0.16	0.17	0.01*
	(0.07)	(0.11)	
Share of Dismissed Judgments	0.11	0.15	0.04***
	(0.11)	(0.13)	
Share of Contested Judgments	0.05	0.05	-0.01*
	(0.04)	(0.07)	
Share of Settled Cases	0.19	0.2	0.01*
	(0.09)	(0.13)	
Share w/ Attorney	0.04	0.10	0.06***
	(0.02)	(0.08)	
Mean Household Income from IRS (in 000s)	29.21	38.68	9.47***
	(9.75)	(9.88)	
Median Credit Score	587.59	686.36	98.77***
	(49.83)	(72.68)	
90+ DPD Debt Balances	3237.66	1405.79	-1831.87***
	(2629.86)	(3768.89)	
Panel B: Additional Neighborhood Characteristics			
GINI Index	0.46	0.41	-0.05***
	(0.05)	(0.05)	
Unemployment Rate	0.12	0.05	-0.06***
	(0.03)	(0.03)	
Divorce Rate	0.13	0.12	-0.01***
	(0.02)	(0.04)	
Fraction with Bachelors Degree	0.16	0.16	-0.00
	(0.10)	(0.10)	
Median Gross Rent	0.76	0.62	-0.14***
	(0.14)	(0.16)	
Home Ownership Rate	0.50	0.74	0.25***
	(0.16)	(0.12)	
Banks (5 miles)	88.10	11.91	-76.19***
	(39.71)	(30.19)	
Payday Lenders (5 miles)	30.64	3.58	-27.07***
	(9.23)	(8.17)	
Zillow: Single Family House (in 000s)	90.28	113.32	23.04***
	(54.39)	(43.23)	
HMDA: Share Rejected, Black - White	0.19	0.14	-0.05***
	(0.11)	(0.31)	
Zip codes	247	6640	
Average Population	18451.03	7036.03	

Notes: Neighborhood level common support sample drawn from Missouri. Standard deviations are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Judgments, Income, and Credit Scores (Individual Data): ZIP

	(1)	(2)	(3)	(4)
Black Majority: ZIP	0.0262***	0.0258***	0.0075***	0.0073***
	(0.0014)	(0.0014)	(0.0014)	(0.0014)
Income		X		X
Credit Score			X	X
Observations	12935791	12935791	12935791	12935791
R^2	0.0294	0.0298	0.0611	0.0612

Notes: Robust standard errors clustered at the county-quarter level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Indicator variable of having a judgment. County and quarter fixed effects are included in all specifications. Observations from the Experian common support sample, data available from 2013 until 2018Q2.

Table 4: Judgments and Debt Portfolios (Individual Data): ZIP

	(1)	(2)	(3)	(4)	(5)
Black Majority: ZIP	0.0065***	0.0066***	0.0072***	0.0054***	0.0048***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Observations	12935791	12935791	12935791	12935791	12935791
R^2	0.0638	0.0632	0.0615	0.0711	0.0728
Debt Levels	X				X
Monthly Payment and Utilization		X			X
Debt Composition			X		X
Delinquency/Bankruptcy/Collections				X	X

Notes: Robust standard errors clustered at the county-quarter level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Indicator variable of having a judgment. County and quarter fixed effects are included in all specifications. Observations from the Experian common support sample, data available from 2013 until 2018Q2.

Table 5: Judgments, Income, and Credit Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Black Majority: ZIP	1.4356***	0.8629***	1.0786***	0.7939***	0.7084***	0.8384***
	(0.1873)	(0.1917)	(0.1658)	(0.1807)	(0.1662)	(0.0356)
Baseline Controls					X	
Income		X		X	X	
Credit Score			X	X	X	
Lagged Baseline Controls						X
Wild Cluster Bootstrap p-value	0	0	0	.0005	.0009	0
Observations	7365	7365	7019	7019	7019	6105
R^2	0.5961	0.6617	0.6279	0.6684	0.6851	0.6770

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. County and year fixed effects included in all specifications.

Table 6: Judgments and Debt Portfolios

	(1)	(2)	(3)	(4)	(5)
Black Majority: ZIP	0.6910***	0.7134***	0.7070***	0.7095***	0.6962***
	(0.1616)	(0.1657)	(0.1650)	(0.1579)	(0.1511)
Debt Levels	X				X
Monthly Payment and Utilization		X			X
Debt Composition			X		X
Delinquency/Bankruptcy/Collections				X	X
Wild Cluster Bootstrap p-value	.0012	.0009	.0009	.0005	.0004
Observations	7019	7019	7019	7019	7019
R^2	0.6877	0.6860	0.6854	0.6876	0.6926

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications.

Table 7: Attorney Representation and Judgment Type

	(1) Attorney	(2) Judgments	(3) Consent	(4) Contested	(5) Default	(6) Dismissed	(7) Settle	(8) Judgments
Black Majority: ZIP	-0.015***	0.707***	0.003	0.001	0.013**	0.014*	-0.022***	0.768***
	(0.004)	(0.165)	(0.005)	(0.004)	(0.006)	(0.007)	(0.006)	(0.165)
Attorney Representation		X	X	X	X	X	X	X
Lagged Case Outcomes								X
Wild Cluster Bootstrap p-value	.0001	.0007	.0008	.601	.709	.0558	.0702	.0036
Observations	7019	7019	7019	7019	7019	7019	7019	7019
R^2	0.448	0.700	0.685	0.470	0.438	0.395	0.651	0.615

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications. Aside from Column (1), all specifications control for attorney representation.

Table 8: Judgments and Access to Credit Markets

	(1)	(2)	(3)	(4)	(5)
Black Majority: ZIP	1.0910***	1.0897***	1.0914***	0.9375***	0.9417***
	(0.1398)	(0.1394)	(0.1399)	(0.1099)	(0.1135)
Broadband		0.0565**			
		(0.0222)			
Unscored			0.1838		
			(0.2691)		
Banks (5 miles)				-0.0056***	
				(0.0011)	
Payday Lenders (5 miles)				0.0177***	
				(0.0028)	
Banks (10 miles)					-0.0025***
					(0.0003)
Payday Lenders (10 miles)					0.0098***
					(0.0013)
Wild Cluster Bootstrap p-value	0	0	0	0	0
Observations	4183	4183	4183	4183	4183
R^2	0.8181	0.8186	0.8181	0.8279	0.8295

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications.

Table 9: Judgments and Plaintiff Type

	(1) Judgments	(2) Debt Buyer	(3) Major Bank	(4) Medical	(5) High-Cost	(6) Misc.
Black Majority: ZIP	0.708***	0.217***	0.058**	0.045	0.119***	0.321***
	(0.166)	(0.051)	(0.029)	(0.055)	(0.024)	(0.082)
Mean	1.253	.568	.453	.297	.076	.139
Effect Size	56.5	38.2	12.8	15.1	156.3	231
Wild Cluster Bootstrap p-value	.0009	.0006	.0652	.5694	0	.0001
Observations	7019	7019	7019	7019	7019	7019
R^2	0.685	0.699	0.725	0.613	0.602	0.533

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications.

Table 10: Judgments and an Alternative Proxy for Race

	(1)	(2)	(3)	(4)
Share Black: ZIP	1.4731***			
	(0.2865)			
Black Majority: ZIP		0.7084***		
		(0.1662)		
Share Black: BISG			1.3776***	
			(0.2563)	
Black Majority: BISG				0.6283***
				(0.1347)
Wild Cluster Bootstrap p-value	.0001	.0009	0	.0005
Observations	7019	7019	7015	7015
R^2	0.6965	0.6851	0.6981	0.6860

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications.

Table 11: Judgments and an Alternative Credit Score

	(1)	(2)	(3)	(4)	(5)
Black Majority: ZIP	1.6061***	1.2321***	0.9537***	0.8287***	1.0213***
	(0.1885)	(0.1717)	(0.1961)	(0.1764)	(0.0360)
Baseline Controls				X	
Income			X	X	
Default Probability		X	X	X	X
Lagged Baseline Controls					X
Wild Cluster Bootstrap p-value	0	0	.0001	.0007	0
Observations	5909	5909	5909	5625	4738
R^2	0.5977	0.6319	0.6813	0.7001	0.7291

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. County and year fixed effects included in all specifications. Column (5) controls for lagged default probability instead of default probability. Default probability data is available from 2006.

Table 12: Judgments and Local Housing Markets

	(1)	(2)	(3)	(4)
Black Majority: ZIP	0.9451***	0.8416***	0.7136***	0.6891***
	(0.1581)	(0.1453)	(0.1610)	(0.1584)
HMDA: Share Rejected		1.0981***		
		(0.3638)		
Zillow: Single Family House				-0.0060***
				(0.0013)
HMDA Available	X	X		
Zillow Available			X	X
Wild Cluster Bootstrap p-value	0	0	.0006	.0005
Observations	3846	3846	5231	5231
R^2	0.7593	0.7628	0.6767	0.6861

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications.

Table 13: Judgments and Alternative Dependent Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Judgments/Cases	Judgment: Low Income		Judgment: Low Credit		Judgment: Winsorized	
Black Majority: ZIP	0.028***	2.843***	2.649***	0.503***	0.492***	0.708***	0.666***
	(0.010)	(0.704)	(0.570)	(0.122)	(0.115)	(0.165)	(0.138)
Population Weighted		X		X		X	
Wild Cluster Boostrap p-value	.0111	.0011	0	.003	.0005	.0008	.0002
Observations	7019	7019	7019	6999	6999	7019	7019
R^2	0.173	0.660	0.548	0.744	0.576	0.686	0.512

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Observations on the common support sample. Baseline controls, county, year fixed effects, and attorney representation included in all specifications. For Column (1) our dependent variable is the fraction of cases that result in a judgment, for Columns (2-3), our outcome variable is judgments per 100 individuals with tax filings under $25,000 USD, for Columns (4-5) our outcome variable is judgments per 100 individuals with subprime credit, and for Columns (6-7) we winsorize our main outcome variable, judgments per 100 individuals, at the 99th percentile.

Appendix

A.1 Judgment Data

We obtained our judgment data from Paul Kiel and Annie Waldman at ProPublica. This data included all debt collection judgments in New Jersey, Missouri, and Cook County Illinois from 2008 to 2012.[29] Both Missouri and New Jersey have state-wide databases. The Missouri dataset was provided by Missouri's Office of the State Courts Administrator (OSCA) and included all debt collection cases filed in Associate Circuit Court for which OSCA has an electronic record through early 2014.[30] For each case in Missouri, the data contained the following information: court (judicial circuit), county, case ID, filing Date, case type, disposition, plaintiff, plaintiff attorney, defendant, defendant date of birth, defendant address, defendant attorney, judgment amount, date of judgment satisfaction, date of first garnishment attempt.[31] Kiel and Waldman added two fields: a standard name for each plaintiff and a plaintiff type. Missouri's court system has some variation among the judicial circuits in how case types are categorized, so Keil and Waldman selected a range of case types that could be reasonably construed as debt collection cases in consultation with OSCA employees. For St. Louis City and County courts, these were: Breach of Contract, Promissory Note, Suit on Account, Contract /Account (Bulk), Misc Associate Civil-Other, Small Claims under \$100, Small Claims over \$100.[32] They limited the dataset to cases that had resulted in a judgment.

A.2 BISG Algorithm

Our estimate for the racial composition of defendants is constructed using Bayesian Improved Surname Geocoding (BISG) (Elliott et al. 2009).[33] This method combines publicly available geography- and surname-based information into a single proxy probability for race of an individual using the Bayes updating rule. This method involves constructing a probability of assignment to race based on demographic information associated with surname and then updating this probability using the demographic characteristics of the zip code associated with place of residence. The updating is performed through the application of a Bayesian

[29] The focus in their ProPublica article was Essex County, St. Louis City, St. Louis County, and Cook County due to the cities high segregation indexes. Due to a peculiarity of the court system database, the Essex County window is slightly different: July 1, 2007, through June 30, 2012.

[30] The max amount sought in associate circuit courts in Missouri is \$25,000.

[31] The judgment amount was determined to be unreliable and is not used throughout this analysis.

[32] Together, the small claims and "misc associate" cases comprised less than four percent of cases.

[33] Consumer Finance Protection Bureau's Office of Research (OR), the Division of Supervision, Enforcement, and Fair Lending (SEFL) rely on a Bayesian Improved Surname Geocoding (BISG) proxy method.

algorithm, which yields an integrated probability that can be used to proxy for an individual's race and ethnicity. We once again classify the defendant pool as being majority black if at least 50% of the defendants in the debt collection data were predicted to be black. Information on the racial and ethnic composition of the U.S. population by geography comes from the Summary File 1 (SF1) from the 2010 Census, which provides counts of enumerated individuals by age, race, and ethnicity for various geographic area definitions, including zip code tabulation areas.[34] Research has found that this approach produces proxies that correlate highly with self-reported race and national origin and is more accurate than relying only on demographic information associated with a borrower's last name or place of residence alone (CFBP Report, 2014).

Vectors of six racial/ethnic probabilities for each listed surname (corrected for suppression and for low-frequency surnames) are used as the first input into the BISG algorithm. This information is used to calculate a prior probability of an individual's race/ethnicity. The algorithm updates these prior probabilities with geocoded ZCTA proportions for these groups from the 2010 Census SF1 files to generate posterior probabilities. Let J equal the number of names on the enhanced surname list plus one to account for names not on the list and let K equal the number of ZCTA in the 2010 census with any population. We define the prior probability of a person's race on the basis of surname, so that for a person with surname j = 1, ..., J on the list, the prior probability for race, i = 1,...,6, is p(i—j) = proportion of all people with surname j who report being of race i in the enhanced surname file (the probability of a selected race given surname). This probability is updated on the basis of ZCTA residence. For ZCTA k = 1,...,K, r(k—i) = proportion of all people in redistributed SF1 file who self report being race i who reside in ZCTA k (the probability of a selected ZCTA of residence given race/ethnicity). Let $u(i, j, k) = p(i|j) * r(k|i)$. According to Bayes' Theorem and the assumption that the probability of residing in a given ZCTA given a person's race does not vary by surname, the updated (posterior) probability of being of race/ethnicity i given surname j and ZCTA of residence k can be calculated as follows:

$$q(i|j,k) = \frac{u(i,j,k)}{u(1,j,k) + u(2,j,k) + u(3,j,k) + u(4,j,k) + u(5,j,k) + u(6,j,k)} \tag{3}$$

Note that all parameters needed for BISG posterior probabilities are derived only from Census 2010 data, and that none are derived from administrative sources.

We similarly use the estimated probability that an individual is black (based on their zip code of residence and their age) as the covariate of interest as opposed to the racial

[34]Census block are the highest level of disaggregation (the smallest geography).

composition of an individual's neighborhood more broadly when using the individual level data.

A.3 Other Demographics

In Table A27 in our online appendix, we replicate Table 5 with additional controls for the share of Hispanic and Asian population within each zip code. We do not observe any consistent judgment gap for Hispanic across our various specifications. However, we do find that the share of Asian population is negatively related to judgments. The share of black residents remains positive and statistically significant in each of the specifications.

A.4 Non-linearities & Higher Order Interactions

We next investigate if causal machine learning techniques that allow for high order interactions of observable characteristics can mitigate the disparity. This relies on the assumption that more information can be extracted from the interaction of multiples variables. More specifically, we adopt Double Machine Learning by Chernozhukov et al. [2018], and implement Gradient Boosted Trees (GBT) and Random Forests in the first stages, with a linear regression and robust standard errors in the second.[35] Gradient Boosted Trees (GBT) is an ensemble learning approach that mitigates the tendency of tree-based models' to overfit to training data. This is accomplished by recursively combining the forecasts of many oversimplified trees. The theory behind boosting proposes that a collection of weak learners as an ensemble create a single strong learner with improved stability over a single complex tree.

At each step m, $1 \leq m \leq M$, of gradient boosting, an estimator, h_m, is computed on the residuals from the previous models predictions. A critical part of gradient boosting method is regularization by shrinkage as proposed by Friedman [2001]. This consists in modifying the update rule as follows:

$$F_m(x) = F_{m-1}(x) + \nu \gamma_m h_m(x), \tag{4}$$

where $h_m(x)$ represents a weak learner of fixed depth, γ_m is the step length and ν is the learning rate or shrinkage factor.

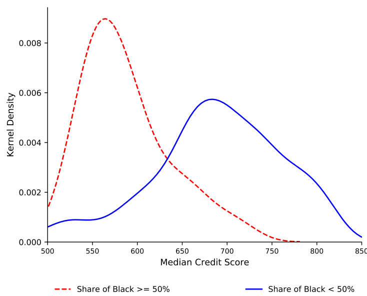
The estimation procedure begins with fitting a shallow tree (e.g., with depth L = 1). Using the prediction residuals from the first tree, you then fit a second tree with the same shallow depth L. Weight the predictions of the second tree by $\nu \in (0, 1)$ to prevent the model from overfitting the residuals, and then aggregate the forecasts of these two trees.

[35] For more information about the GBT model, see Friedman [2001].

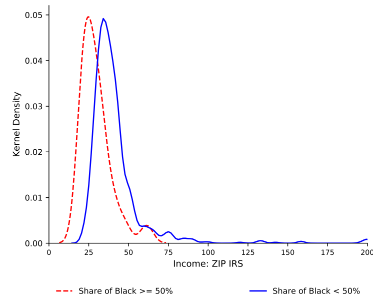
At each step k, fit a shallow tree to the residuals from the model with k-1 trees, and add its prediction to the forecast of the ensemble with a shrinkage weight of ν. Do this until a total of K trees is reached in the ensemble. For our GBT model, we split the data into three chunks: training set (60%), holdout set (20%), and testing set (20%). We relied on XGBoost for the implementation of our GBT model (Chen and Guestrin [2016]).

We find that Double Machine Learning is unable to explain away the disparity. In particular, the estimates obtained in Table A28 are similar to the ones obtained in Table 5 and Table A2.

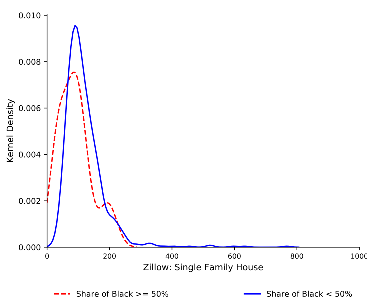
Appendix Figures



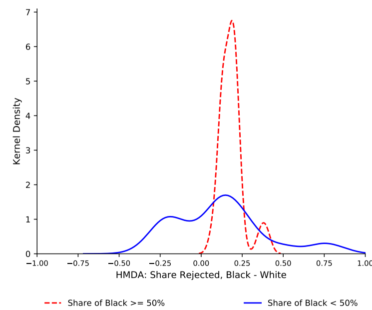
(a) Credit Score



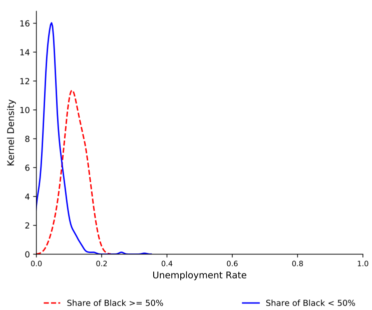
(b) Household Income



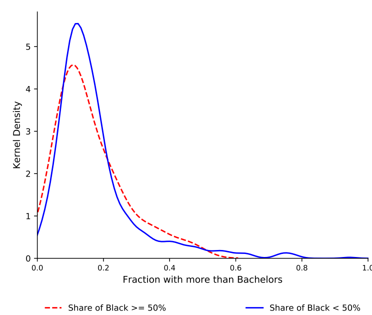
(c) House Value



(d) Mortgage Denials



(e) Unemployment Rate



(f) >= Bachelor Degree

Figure A1: Kernel Density Estimates of Selected Covariates

Notes: These figures plot the densities of various neighborhood characteristics cross majority black and majority non-black neighborhoods.

Appendix Tables

Table A1: Itemized Sample Restrictions: Missouri Data

	ZIP Codes	% Black
ACS	1,021	4.8%
Demographic & Judgment Information Available	1,015	4.8%
Credit Report Information Available	902	5.5%
Common Support Sample	840	5.6%

Table A2: Judgments, Income, and Credit Scores: Share Black

	(1)	(2)	(3)	(4)	(5)	(6)
Share Black: ZIP	2.4081***	1.6669***	1.9877***	1.5634***	1.4731***	1.7233***
	(0.2939)	(0.3308)	(0.2640)	(0.3064)	(0.2865)	(0.0583)
Baseline Controls					X	
Income		X		X	X	
Credit Score			X	X	X	
Lagged Baseline Controls						X
Wild Cluster Bootstrap p-value	0	0	0	.0001	.0001	0
Observations	7365	7365	7019	7019	7019	6105
R^2	0.6403	0.6769	0.6559	0.6816	0.6965	0.6921

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 28.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. County and year fixed effects included in all specifications.

Table A3: Judgments and Debt Portfolios: Share Black

	(1)	(2)	(3)	(4)	(5)
Share Black: ZIP	1.4651***	1.4758***	1.4727***	1.5054***	1.4915***
	(0.2789)	(0.2851)	(0.2850)	(0.2750)	(0.2655)
Debt Levels	X				X
Monthly Payment and Utilization		X			X
Debt Composition			X		X
Delinquency/Bankruptcy/Collections				X	X
Wild Cluster Bootstrap p-value	0	.0001	0	0	0
Observations	7019	7019	7019	7019	7019
R^2	0.6995	0.6973	0.6968	0.6995	0.7045

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 28.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications.

Table A4: Judgments and Access to Credit Markets: Share Black

	(1)	(2)	(3)	(4)	(5)
Share Black: ZIP	2.1184***	2.1157***	2.1185***	1.9213***	2.0197***
	(0.2550)	(0.2546)	(0.2551)	(0.2065)	(0.2115)
Broadband		0.0531**			
		(0.0223)			
Unscored			0.0927		
			(0.2716)		
Banks (5 miles)				-0.0053***	
				(0.0010)	
Payday Lenders (5 miles)				0.0116***	
				(0.0028)	
Banks (10 miles)					-0.0019***
					(0.0003)
Payday Lenders (10 miles)					0.0028**
					(0.0014)
Wild Cluster Bootstrap p-value	0	0	0	0	0
Observations	4183	4183	4183	4183	4183
R^2	0.8371	0.8375	0.8371	0.8447	0.8460

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 28.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications.

Table A5: Attorney Representation and Judgment Type: Share Black

	(1) Attorney	(2) Judgments	(3) Consent	(4) Constested	(5) Default	(6) Dismissed	(7) Settle	(8) Judgments
Share Black: ZIP	-0.036***	1.409***	1.474***	0.014	0.011*	0.045***	0.010	1.575***
	(0.006)	(0.282)	(0.283)	(0.011)	(0.006)	(0.009)	(0.011)	(0.289)
Attorney Representation		X	X	X	X	X	X	X
Lagged Case Outcomes								X
Wild Cluster Bootstrap p-value	0	.0002	0	.2137	.0987	0	.3535	0.0001
Observations	7019	7019	7019	7019	7019	7019	7019	6093
R^2	0.450	0.710	0.697	0.470	0.438	0.397	0.650	0.730

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 28.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications. Aside from Column (1), all specifications control for attorney representation. Column (8) controls for the share of default judgments.

Table A6: Judgments and Plaintiff Type: Share Black

	(1) Judgments	(2) Debt Buyer	(3) Major Bank	(4) Medical	(5) High-Cost	(6) Misc.
Share Black: ZIP	1.473***	0.483***	0.104**	0.024	0.277***	0.645***
	(0.286)	(0.096)	(0.051)	(0.079)	(0.044)	(0.144)
Wild Cluster Bootstrap p-value	.0001	0	.0604	.8287	0	0
Observations	7019.000	7019.000	7019.000	7019.000	7019.000	7019.000
R^2	0.697	0.706	0.726	0.612	0.625	0.547

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 28.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications. Effect size computed for going from 0% share black to 100%.

Table A7: Judgments and Local Housing Markets: Share Black

	(1)	(2)	(3)	(4)
Share Black: ZIP	1.8918***	1.8166***	1.5111***	1.4096***
	(0.2836)	(0.2818)	(0.2957)	(0.3001)
HMDA: Share Rejected		0.3642		
		(0.3448)		
Zillow: Single Family House				-0.0046***
				(0.0013)
HMDA Available	X	X		
Zillow Available			X	X
Wild Cluster Bootstrap p-value	0	0	.0001	.0001
Observations	3846	3846	5231	5231
R^2	0.7765	0.7768	0.6898	0.6953

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 28.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. Baseline controls, county, and year fixed effects included in all specifications.

Table A8: Judgments and an Alternative Credit Score: Share Black

	(1)	(2)	(3)	(4)	(5)
Share Black: ZIP	1.6996***	0.8755***	0.9238***	0.7283***	0.8163***
	(0.2524)	(0.1223)	(0.1388)	(0.1754)	(0.0525)
Baseline Controls				X	
Income			X	X	
Default Probability		X	X	X	X
Lagged Baseline Controls					X
Wild Cluster Bootstrap p-value	.0095	.0015	.0159	0	0
Observations	2088	2088	2088	2088	1611
R^2	0.6071	0.7032	0.7434	0.7623	0.8124

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 28.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. County and year fixed effects included in all specifications. Column (5) controls for lagged default probability instead of default probability.

Table A9: Judgments and Alternative Dependent Variables: Share Black

	(1) Judgments/Cases	(2) Judgment: Low Income	(3) Judgment: Low Income	(4) Judgment: Low Credit	(5) Judgment: Low Credit	(6) Judgment: Winsorized	(7) Judgment: Winsorized
Share Black: ZIP	0.072***	6.160***	4.467***	1.002***	0.747***	1.476***	1.096***
	(0.015)	(1.226)	(0.815)	(0.210)	(0.175)	(0.283)	(0.204)
Population Weighted		X		X		X	
Wild Cluster Boostrap p-value	0	0	.0004	.0003	0	0	0
Observations	7019	7019	7019	6999	6999	7019	7019
R^2	0.174	0.671	0.551	0.752	0.576	0.697	0.514

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 28.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions are weighted by population only in Columns (2), (4), and (6). Observations on the entire Missouri sample. Baseline controls, county, year fixed effects, and attorney representation included in all specifications. For Column (1) our dependent variable is the fraction of cases that result in a judgment, for Columns (2-3), our outcome variable is judgments per 100 individuals with tax filings under $25,000 USD, for Columns (4-5) our outcome variable is judgments per 100 individuals with subprime credit, and for Columns (6-7) we winsorize our main outcome variable, judgments per 100 individuals, at the 99th percentile.

Table A10: Creditworthiness

	(1)	(2)	(3)	(4)	(5)	(6)
Black Majority: BISG	1.2637***	0.7413***	0.9180***	0.6791***	0.6283***	0.6899***
	(0.1571)	(0.1505)	(0.1290)	(0.1401)	(0.1347)	(0.0310)
Baseline Controls					X	
Income Details		X		X	X	
Credit Quintiles			X	X	X	
Lagged Baseline Controls						X
Wild Cluster Bootstrap p-value	0	0	0	.0001	.0005	0
Observations	7361	7361	7015	7015	7015	6103
R^2	0.5799	0.6585	0.6207	0.6661	0.6860	0.6739

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Observations on the common support sample. Since the effective number of clusters is small, Wild Cluster Bootstrap p-values are reported for our main parameter of interest. County and year fixed effects included in all specifications.

Table A11: Debt Portfolios

	(1)	(2)	(3)	(4)	(5)
Black Majority: BISG	0.6222***	0.6329***	0.6264***	0.6302***	0.6266***
	(0.1308)	(0.1349)	(0.1343)	(0.1274)	(0.1229)
Debt Levels	X				X
Monthly Payment and Utilization		X			X
Debt Composition			X		X
Delinquency/Bankruptcy/Collections				X	X
Wild Cluster Bootstrap p-value	.0004	.0004	.0005	.0002	.0002
Observations	7015	7015	7015	7015	7015
R^2	0.6893	0.6869	0.6861	0.6884	0.6940

Notes: Standard errors in parentheses Observations on the common support sample. Since the effective number of clusters is small, Wild Cluster Bootstrap p-values are reported for our main parameter of interest. Baseline controls, county, and year fixed effects included in all specifications.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Judgments, Income, and Credit Scores (Individual Judgment Data)

	(1)	(2)	(3)	(4)
Probability of Being Black	0.0479***	0.0469***	0.0088***	0.0084***
	(0.0018)	(0.0018)	(0.0018)	(0.0018)
Income		X		X
Credit Score			X	X
Observations	12939752	12939752	12939752	12939752
R^2	0.0303	0.0307	0.0611	0.0612

Notes: Robust standard errors clustered at the county-quarter level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Indicator variable of having a judgment. County and quarter fixed effects are included in all specifications. Observations from the Experian common support sample, data available from 2013 until 2018Q2. We use the individuals age and ZIP code to infer their probability of being black.

Table A13: Judgments and Debt Portfolios (Individual Data)

	(1)	(2)	(3)	(4)	(5)
Probability of Being Black	0.0072***	0.0073***	0.0082***	0.0047***	0.0040***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Debt Levels	X				X
Monthly Payment and Utilization		X			X
Debt Composition			X		X
Delinquency/Bankruptcy/Collections				X	X
Observations	12939752	12939752	12939752	12939752	12939752
R^2	0.0638	0.0631	0.0614	0.0711	0.0727

Notes: Robust standard errors clustered at the county-quarter level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Indicator variable of having a judgment. County and quarter fixed effects are included in all specifications. Observations from the Experian sample, data available from 2013 until 2018Q2. We use the individuals age and ZIP code to infer their probability of being black.

Table A14: Summary Statistics: IL & NJ

	Black	Non-Black	Difference
Panel A: Judgments and Financial Characteristics			
Judgments per 100 People	2.70	1.72	-0.98***
	(2.00)	(1.79)	
Median Credit Score	588.98	707.15	118.17***
	(36.72)	(54.62)	
90+ DPD Debt Balances	7135.13	4260.03	-2875.10***
	(6609.97)	(6448.52)	
Panel B: Additional Neighborhood Characteristics			
GINI Index	0.46	0.41	-0.06***
	(0.05)	(0.05)	
Unemployment Rate	0.12	0.07	-0.06***
	(0.03)	(0.02)	
Divorce Rate	0.11	0.10	-0.02***
	(0.02)	(0.03)	
Fraction with Bachelors Degree	0.18	0.30	0.11***
	(0.10)	(0.14)	
Gross Rent: Median	0.97	1.06	0.09***
	(0.25)	(0.29)	
Home Ownership Rate	0.51	0.71	0.20***
	(0.20)	(0.17)	
Banks (5 miles)	166.92	113.30	-53.63***
	(138.48)	(170.14)	
Payday Lenders (5 miles)	33.65	11.84	-21.81***
	(39.86)	(25.73)	
Zillow: Single Family House	176.37	189.89	13.53
	(92.22)	(116.21)	
HMDA: Share Rejected, Black - White	175.91	252.34	76.43***
	(90.21)	(123.72)	
Mean Household Income (IRS)	0.16	0.13	-0.03***
	(0.11)	(0.19)	
Observations	200	705	905

Notes: Summary statistics for observations on the common support sample. Data is drawn from Illinois and New Jersey. Standard deviations are in parenthesis.

Table A15: Judgments, Income, and Credit Scores (NJ and IL Data)

	(1)	(2)	(3)	(4)	(5)	(6)
Black Majority: ZIP	0.9667***	0.8630***	0.3650***	0.5833***	0.2981***	0.3978***
	(0.1297)	(0.1058)	(0.0940)	(0.0876)	(0.0885)	(0.0844)
Baseline Controls					X	
Income		X		X	X	
Credit Score			X	X	X	
Lagged Baseline Controls						X
Wild Cluster Bootstrap p-value	0	0	.0001	0	.0008	0
Observations	3397	3397	3103	3103	3103	2118
R^2	0.8733	0.8994	0.8909	0.9034	0.9089	0.9119

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 30.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. County and year fixed effects included in all specifications.

Table A16: Debt Portfolios (NJ and IL Data)

	(1)	(2)	(3)	(4)	(5)
black_majority	0.2748***	0.2753***	0.2876***	0.2840***	0.2592***
	(0.0858)	(0.0850)	(0.0871)	(0.0924)	(0.0870)
Wild Cluster Bootstrap p-value	.0012	.0011	.0012	.0018	.0033
Debt Levels	X				X
Monthly Payment and Utilization		X			X
Debt Composition			X		X
Delinquency/Bankruptcy/Collections				X	X
Observations	3103	3103	3103	3103	3103
R^2	0.9105	0.9113	0.9101	0.9110	0.9145

Notes: Observations on the common support sample. Since the effective number of clusters is small, Wild Cluster Bootstrap p-values are reported for our main parameter of interest. Baseline controls, county, and year fixed effects included in all specifications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Online Appendix to "Racial Disparities in Debt Collection"

Table A17: Summary Statistics: Entire Sample

	Black	Non-Black	Difference
Panel A: Judgments and Finacial Characteristics			
Judgments per 100 People	2.65	1.32	-1.32***
	(1.35)	(1.53)	
Share of Default Judgments	0.45	0.37	-0.08***
	(0.07)	(0.17)	
Share of Consent Judgments	0.16	0.17	0.02***
	(0.07)	(0.14)	
Share of Dismissed Judgments	0.12	0.15	0.03***
	(0.11)	(0.15)	
Share of Constested Judgments	0.05	0.05	-0.01*
	(0.05)	(0.09)	
Share of Settled Cases	0.19	0.21	0.02***
	(0.09)	(0.17)	
Share w/ Attorney	0.04	0.10	0.06***
	(0.02)	(0.11)	
Median Credit Score	586.65	688.71	102.06***
	(50.98)	(76.50)	
90+ DPD Debt Balances	3193.18	1416.45	-1776.73***
	(2676.72)	(4253.49)	
Panel B: Additional Neighborhood Characteristics			
GINI Index	0.46	0.40	-0.06***
	(0.05)	(0.07)	
Unemployment Rate	0.12	0.05	-0.07***
	(0.05)	(0.04)	
Divorce Rate	0.13	0.12	-0.01***
	(0.03)	(0.06)	
Fraction with Bachelors Degree	0.16	0.17	0.00
	(0.10)	(0.13)	
Home Ownership Rate	0.47	0.76	0.29***
	(0.19)	(0.14)	
Banks (5 miles)	85.44	11.88	-73.56***
	(40.79)	(30.95)	
Payday Lenders (5 miles)	29.98	3.23	-26.75***
	(9.86)	(7.76)	
Zillow: Single Family House (in 000s)	90.15	122.26	32.11***
	(54.29)	(68.87)	
HMDA: Share Rejected, Black - White	0.19	0.15	-0.04***
	(0.11)	(0.31)	
Mean Household Income from IRS (in 000s)	28.67	41.01	12.34***
	(10.00)	(24.25)	
Zip Codes	258	9530	
Population	17697.00	5807.79	

Notes: Summary statistics for observations on the entire Missouri sample. Standard deviations are in parenthesis.

Table A18: Judgments, Income, and Credit Scores: Entire Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Black Majority: ZIP	1.5127***	0.8940***	1.0934***	0.8200***	0.7071***	0.8402***
	(0.2148)	(0.1912)	(0.1741)	(0.1816)	(0.1612)	(0.0325)
Baseline Controls					X	
Income Details		X		X	X	
Credit Quintiles			X	X	X	
Lagged Baseline Controls						X
Wild Cluster Bootstrap p-value	0	0	0	.0006	.0005	0
Observations	9788	9020	7837	7527	7484	6737
R^2	0.5805	0.6694	0.6304	0.6814	0.7016	0.6903

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.8. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the entire Missouri sample. County and year fixed effects included in all specifications.

Table A19: Judgments and Debt Portfolios: Entire Sample

	(1)	(2)	(3)	(4)	(5)
Black Majority: ZIP	0.6926***	0.7086***	0.7058***	0.6957***	0.6871***
	(0.1549)	(0.1599)	(0.1594)	(0.1558)	(0.1476)
Debt Levels	X				X
Monthly Payment and Utilization		X			X
Debt Composition			X		X
Delinquency/Bankruptcy/Collections				X	X
Wild Cluster Bootstrap p-value	.0004	.0005	.0005	.0004	.0003
Observations	7484	7484	7484	7484	7484
R^2	0.7037	0.7021	0.7020	0.7038	0.7079

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.8. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the entire Missouri sample. Baseline controls, county, and year fixed effects included in all specifications.

Table A20: Judgments and Access to Credit Markets: Entire Sample

	(1)	(2)	(3)	(4)	(5)
Black Majority: ZIP	1.0704***	1.0699***	1.0706***	0.9528***	0.9185***
	(0.1436)	(0.1434)	(0.1437)	(0.1256)	(0.1279)
Broadband		0.0374*			
		(0.0214)			
Unscored			0.1337		
			(0.2643)		
Banks (5 miles)				-0.0047***	
				(0.0008)	
Payday Lenders (5 miles)				0.0161***	
				(0.0026)	
Banks (10 miles)					-0.0021***
					(0.0003)
Payday Lenders (10 miles)					0.0083***
					(0.0009)
Wild Cluster Bootstrap p-value	0	0	0	0	0
Observations	4488	4488	4488	4488	4488
R^2	0.8248	0.8250	0.8248	0.8335	0.8344

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.8. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the entire Missouri sample. Baseline controls, county, and year fixed effects included in all specifications.

Table A21: Judgments and Local Housing Markets: Entire Sample

	(1)	(2)	(3)	(4)
Black Majority: ZIP	0.9253***	0.8022***	0.7012***	0.6903***
	(0.1591)	(0.1401)	(0.1590)	(0.1563)
HMDA: Share Rejected		1.2362***		
		(0.3948)		
Zillow: Single Family House				-0.0040***
				(0.0007)
HMDA Available	X	X		
Zillow Available			X	X
Wild Cluster Bootstrap p-value	0	.0003	.0002	.0001
Observations	4171	4171	5679	5679
R^2	0.7764	0.7806	0.6960	0.7018

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.8. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the entire Missouri sample. Baseline controls, county, and year fixed effects included in all specifications.

Table A22: Attorney Representation and Judgment Type: Entire Sample

	(1) Attorney	(2) Judgments	(3) Judgments	(4) consent	(5) Contested	(6) Default	(7) Dismissed	(8) Settle	(9) Judgments
Black Majority: ZIP	-0.016***	0.685***	0.706***	0.004	0.001	0.014***	0.010	-0.022***	0.752***
	(0.003)	(0.155)	(0.160)	(0.005)	(0.004)	(0.005)	(0.007)	(0.006)	(0.162)
Attorney Representation		X	X	X	X	X	X	X	X
Lagged Case Outcomes									X
Wild Cluster Bootstrap p-value	0	.0004	.0004	.4475	.8498	.0096	.1681	.0036	0.0008
Observations	7484	7484	7484	7484	7484	7484	7484	7484	6701
R^2	0.440	0.715	0.702	0.479	0.407	0.389	0.654	0.643	0.732

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.8. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the entire Missouri sample. Baseline controls, county, and year fixed effects included in all specifications. Aside from Column (1), all specifications control for attorney representation. Column (8) controls for the share of default judgments.

Table A23: Judgments and Plaintiff Type: Entire Sample

	(1) Judgments	(2) Debt Buyer	(3) Major Bank	(4) Medical	(5) High-Cost	(6) Misc.
Black Majority: ZIP	0.707***	0.220***	0.052*	0.045	0.116***	0.322***
	(0.161)	(0.050)	(0.029)	(0.052)	(0.022)	(0.077)
Mean	1.325	.584	0.489	.304	.079	.152
Effect Size	53.4	37.6	10.7	15	146	211.3
Wild Cluster Bootstrap p-value	.0005	.0003	.1048	.5348	0	.0001
Observations	7484	7484	7484	7484	7484	7484
R^2	0.702	0.710	0.734	0.623	0.607	0.534

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.8. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the entire Missouri sample. Baseline controls, county, and year fixed effects included in all specifications.

Table A24: Judgments and an Alternative Proxy for Race: Entire Sample

	(1)	(2)	(3)	(4)
Share Black: ZIP	1.4731***			
	(0.2890)			
Black Majority: ZIP		0.7071***		
		(0.1612)		
Share Black: BISG			1.3774***	
			(0.2604)	
Black Majority: BISG				0.6305***
				(0.1284)
Wild Cluster Bootstrap p-value	.0001	.0005	.0001	.0002
Observations	7484	7484	7479	7479
R^2	0.7133	0.7016	0.7147	0.7027

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.8. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the entire Missouri sample. Baseline controls, county, and year fixed effects included in all specifications.

Table A25: Judgments and an Alternative Credit Score: Entire Sample

	(1)	(2)	(3)	(4)	(5)
Black Majority: ZIP	1.7143***	1.2269***	0.9769***	0.8158***	1.0181***
	(0.2216)	(0.1776)	(0.1921)	(0.1734)	(0.0332)
Baseline Controls				X	
Income			X	X	
Default Probability		X	X	X	X
Lagged Baseline Controls					X
Wild Cluster Bootstrap p-value	0	0	.0001	.0005	0
Observations	6666	6666	6352	6017	5275
R^2	0.5779	0.6325	0.6991	0.7230	0.7388

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.8. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the entire Missouri sample. County and year fixed effects included in all specifications. Column (5) controls for lagged default probability instead of default probability. Default probability data is available from 2006.

Table A26: Judgments and Alternative Dependent Variables: Entire Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Judgments/Cases	Judgment: Low Income		Judgment: Low Credit		Judgment: Winsorized	
Black Majority: ZIP	0.026**	2.830***	2.607***	0.506***	0.499***	0.707***	0.693***
	(0.010)	(0.680)	(0.615)	(0.117)	(0.121)	(0.160)	(0.150)
Population Weighted		X		X		X	
Wild Cluster Boostrap p-value	.0146	.0004	.0002	.0016	.001	.0004	.0002
Observations	7484	7484	7484	7484	7464	7464	7484
R^2	0.185	0.671	0.551	0.754	0.576	0.702	0.519

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.8. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Observations on the entire Missouri sample. Baseline controls, county, and year fixed effects, attorney representation included in all specifications. For Column (1) our dependent variable is the fraction of cases that result in a judgment, for Columns (2-3), our outcome variable is judgments per 100 individuals with tax filings under $25,000 USD, for Columns (4-5) our outcome variable is judgments per 100 individuals with subprime credit, and for Columns (6-7) we winsorize our main outcome variable, judgments per 100 individuals, at the 99th percentile.

Table A27: Judgments and Other Demographic Groups

	(1)	(2)	(3)	(4)	(5)	(6)
Share Black: ZIP	2.2881***	1.5574***	1.9195***	1.4544***	1.4865***	1.6260***
	(0.2748)	(0.3184)	(0.2505)	(0.2955)	(0.2777)	(0.0610)
Share Asian: ZIP	-5.6745***	-3.3050***	-4.8311***	-3.1980***	-1.6851**	-3.6100***
	(0.9938)	(0.9074)	(0.9953)	(0.8989)	(0.7889)	(0.4954)
Share Hispanic: ZIP	1.0901***	-0.3160***	0.7415***	-0.3814***	0.5452***	-0.3274*
	(0.1345)	(0.1217)	(0.1357)	(0.1297)	(0.1421)	(0.1820)
Baseline Controls					X	
Income		X		X	X	
Credit Score			X	X	X	
Lagged Baseline Controls						X
Wild Cluster Bootstrap p_{Black}	0	0	0	0	0	0
Wild Cluster Bootstrap p_{Hispanic}	0	.0137	.0001	.008	.0003	.1084
Wild Cluster Bootstrap p_{Asian}	0	.0031	.0002	.0026	.0552	0
Observations	7365	7365	7019	7019	7019	6105
R^2	0.6496	0.6796	0.6618	0.6841	0.6973	0.6952

Notes: Robust standard errors clustered at the county-year level are in parentheses. Effective number of clusters: 36.0. Wild Cluster Bootstrap p-values are reported for our main parameter of interest. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable: Judgments per 100 individuals. All regressions are weighted by population. Observations on the common support sample. County and year fixed effects included in all specifications.

Table A28: Judgments and Double Machine Learning

	R2: Y Model	R2: T Model	Estimates	T-values
T: Binary	0.5121	0.9877	1.020	1.824
	[0.0069]	[0.0022]	[0.2682]	[0.4028]
T: Continuous	0.5121	0.9904	3.909	6.8267
	[0.0069]	[0.0015]	[0.4592]	[1.3375]

Notes: We use five-fold cross-validation in the first stage. Features used correspond to Column (5) of Table 5. For the classification tasks, we use Gradient Boosted Trees, while for the regression tasks, we use Random Forest. We repeat the estimation with different seeds 1,000 times with distinct train-test splits and the statistics reported are the averages obtained from this exercise. Standard deviations are in brackets.