What's in a Name? The Effect of Social Proximity in Sentencing Decisions

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Abstract

Social divides extend beyond race and gender, but less visible dimensions of social background are rarely recorded in data. We study whether judges treat defendants differently when they are socially closer to them. We measure social proximity as the similarity between judges' and defendants' last names, using name embeddings trained on individuals grouped by the street where they live. We validate the measure by showing that it captures demographic similarity beyond race and gender: national origin, religion, and place of residence. Exploiting the quasi-random assignment of more than 1.2 million criminal cases to judges in Wisconsin, we find that judges are less likely to incarcerate socially closer defendants. Using a model of judicial decisions and a machine-learning prediction of recidivism risk, we find that judges are more responsive to risk among socially closer defendants, suggesting that they perceive this risk more accurately. The leniency effect is driven by victimless crimes and reverses for offenses involving a victim. Since victims tend to share the background of the offenders who harm them, this suggests that judges also weigh the protection of socially closer victims.

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1 Introduction

Social divides extend beyond race and gender. Since 1970, residential segregation between Black and White Americans has declined steadily (Cutler et al., 1999; Logan, 2022), while residential segregation by income has increased (Reardon and Bischoff, 2011), and Americans now perceive conflicts between rich and poor people as more prevalent than those between Black and White people (Schaeffer, 2020).[1] Unlike observable attributes like race and gender, these divides are not marked by a single observable trait and are rarely recorded in administrative data, making them harder to measure. As a result, little is known about whether they matter for individuals' decision-making. In particular, do decision makers favor those who are socially closer to them, beyond a shared race or gender?

Criminal courts are a setting where this question has high stakes. Judges have discretion over whether to incarcerate a defendant, and they observe much more about defendants than the characteristics recorded in court files. People infer others' social background easily from cues like how they talk or how they look (Kraus et al., 2019; Macchi, 2023). Yet research on disparities and in-group bias in the courtroom, as well as the monitoring of judges, has focused on a few recorded characteristics, such as race, gender, and religion (Harris and Sen, 2019; Shayo, 2020).[2] Whether judges behave differently when defendants share their social background remains largely unknown.

This paper studies in-group bias along social proximity in sentencing decisions in more than 1.2 million criminal cases in Wisconsin. We measure social proximity from names, which have been shown to carry information on a person's social background. On the one hand, first names can reflect parents' backgrounds and education (Lieberson and Bell, 1992; Fryer and Levitt, 2004) or deliberate parents' choices (Abramitzky et al., 2020a; Bazzi et al., 2020). On the other hand, last names are inherited and carry information about ethnic and national origin and family status (Clark, 2014; Güell et al., 2015).

To extract this information, we train name embeddings on the names of Wisconsin registered voters and criminal defendants, grouping individuals who live in the same zip code and on the same street. Just as *traditional* word embeddings place words that appear in similar contexts close to each other (Mikolov et al., 2013; Pennington et al., 2014; Bojanowski et al., 2017), our embeddings place names that are common in the same places close to each other. We show that using these embeddings, names of individuals from different ethnicities and national origins occupy distinct regions of the embedding space, suggesting that names contain signals about race and ancestry. Additionally, we show that name embeddings add meaningful information to gender and race in the prediction of other socio-economic information like religion, income, or homeownership.

[1] These divides are not specific to the United States. Residential segregation by socio-economic status has also increased in most large cities around the world (van Ham et al., 2021), and education and income have become dimensions of political division in Western democracies (Gethin et al., 2022).

[2] Rules about judicial conduct already prohibit judges from allowing social relationships to influence their decisions, and include socio-economic status among the characteristics on which judges may not show bias (Wisconsin Supreme Court Rules 60.03(2) and 60.04(1)(e); American Bar Association Model Code of Judicial Conduct, Rule 2.3(B)).

The embeddings, however, also contain some noise. The leading dimensions mainly capture how common a name is, so that two names can appear similar simply because both are common. For this reason, we use principal component analysis and remove the first five principal components, retaining the meaningful information while removing most of the commonness noise (Arora et al., 2017; Mu et al., 2018).

We measure social proximity between a judge and a defendant as the cosine similarity between the embeddings of their names. We build the judges' name embeddings from the character sequences of each name, which allows us to create embeddings for all judges, including those whose names do not appear in the training corpus. Unlike binary indicators of shared race or gender, this measure of social proximity is continuous and varies across judges and defendants of the same race and gender, allowing us to study in-group bias beyond these characteristics.

We compute this similarity for first, last, and full names. We use last names as our main measure for three reasons. First, unlike first names, last names are inherited rather than chosen and therefore reflect a family's background rather than parents' preferences or an individual's cohort. We provide evidence consistent with this and show that in our data, first-name embeddings mainly add information on age and gender. Second, last-name similarity accurately captures where people live. The more similar two last names are, the more likely the individuals live in the same zip codes, whereas for first and full names this relationship is mainly flat. Third, last-name similarity reflects a shared ethnic background between judges and defendants, while first-name similarity mainly captures a shared gender and is, if anything, lower when both the judge and the defendant are Black.

To identify in-group disparities, we exploit the quasi-random assignment of criminal cases to judges within each Wisconsin court, so that the social proximity between a defendant and the judge deciding the case is unrelated to the characteristics of the case. Consistent with this, we show that the defendant's social proximity to the assigned judge is unrelated to observable case and defendant characteristics. We compare defendants in the same county and year charged with offenses of the same severity by including charge-severity fixed effects, which account for differences in the crimes committed by different defendants, prosecutors' charging decisions, and charge-based case assignments. We further include fixed effects for ventiles of predicted recidivism risk, control for defendants' race and gender, and control for whether the judge and the defendant share the same race or gender.

We find that judges treat defendants who are socially closer to them more leniently. The results show that higher social proximity decreases both the likelihood of incarceration and jail length. In particular, moving from the 25th to the 75th percentile of social proximity reduces the probability of incarceration by 0.44 percentage points, around 10% of the racial in-group effect estimated in the same specification. The same change in social proximity reduces the length of the jail sentence among incarcerated defendants by about 1%, or approximately 2 days. The effect is specific to the judge deciding the case, since the defendant's proximity to other judges in the same court does not affect sentencing. Therefore, our estimates do not reflect characteristics that make some

defendants closer to judges in general. The results are robust to controlling for zip code and prosecutor fixed effects, restricting the sample to first-time offenders (whom judges have not seen before), and alternative constructions of the measure.

We then investigate which dimension of social proximity drives the effect. We exclude the possibility that this is driven by shared ancestry, as having a last name that shares the same national origin has no effect on sentencing, and controlling for it leaves the effect of social proximity unchanged. Further, excluding judges and defendants with the same last name does not affect the results, ruling out that the effect masks in-group preferences for defendants with the same surname. Instead, we show that the effect is closest to social proximity among neighborhoods where judges and defendants live. Locating some judges' home zip codes in the voter file, we show that judges and defendants living in neighborhoods with similar socio-economic characteristics have more similar names, and that defendants from neighborhoods closer to the judge's are less likely to be incarcerated. However, when we include both measures in the same regression, the effect of social proximity is unchanged, suggesting that it reflects dimensions of a shared background that neighborhood characteristics capture only in part.

These results alone do not allow us to distinguish the mechanisms driving leniency toward socially closer defendants. On the one hand, the results could reflect taste-based preferences, with judges preferring to release defendants who are socially closer to them (Becker, 1957). Alternatively, they may be better able to assess the risk of these defendants committing another crime because they share a social environment. To distinguish between the two, we develop a simple model of judges' incarceration decisions in which they weigh the expected cost of releasing a defendant after observing a noisy signal of the defendant's recidivism risk, whose precision increases with social proximity (Aigner and Cain, 1977). The model yields two main predictions. If judges have a taste-based preference for socially closer defendants, these defendants are less likely to be incarcerated at the average level of risk. If, on the other hand, judges observe more precise signals about recidivism risk of socially closer defendants, their decisions respond more strongly to these defendants' risk, making judges more lenient toward low-risk and harsher toward high-risk defendants who are socially close to them.

We test these predictions in our setting, leveraging a machine-learning-predicted measure of recidivism risk trained on criminal history and demographic characteristics. In line with the model, judges are more likely to incarcerate defendants with a higher predicted risk of recidivism. At the same time, judges respond more to the risk of socially closer defendants and are more likely to incarcerate socially closer defendants with a higher predicted recidivism risk. This stronger response is larger when we use a measure of risk predicted only from defendants' criminal history than for risk predicted only from their demographic characteristics, consistent with judges interpreting the record of socially closer defendants more accurately. Additionally, the results are robust to training the risk score on the decisions of the most lenient judges to address the problem of selective labels (Kleinberg et al., 2018a).

Finally, the results show that judges' preferences also matter. As in the main results, holding the

level of risk constant, judges are less likely to incarcerate socially closer defendants, with an effect identical to our baseline estimate. Following the model predictions, this is consistent with judges having taste-based preferences for releasing socially closer defendants, although we cannot rule out that it reflects information about defendants that our risk score does not capture. Consistent with a role for preferences, judges are less likely to incarcerate socially closer defendants only in victimless offenses, where such a decision does not have any effect on identifiable victims. In these cases, moving from the 25th to the 75th percentile of social proximity reduces the probability of incarceration by about 0.8 percentage points, almost twice the effect in the full sample. In contrast, in offenses that involve a victim, the sign flips and judges are significantly more likely to incarcerate socially closer defendants. That could reflect a social affinity for victims who are socially close to the judge, as victims tend to come from backgrounds similar to those of the defendants who harm them.

This paper contributes to a large literature on how the identity of judges and other decision makers affects their decisions, particularly the mixed evidence on in-group bias.[3] A first set of papers finds that judges favor defendants who share their ethnicity, religion, or gender (Gazal-Ayal and Sulitzeanu-Kenan, 2010; Shayo and Zussman, 2011; Anwar et al., 2012; Knepper, 2018), while others find null or even negative in-group effects (Lim et al., 2016; Depew et al., 2017; Ash et al., 2025; Ash and Marangon, 2026).[4] These studies define in-group membership through discrete and observable characteristics recorded in the data, such as race, gender, religion, or caste. We go beyond this discrete characterization by measuring in-group membership along a continuous dimension of social proximity that varies among judges and defendants who share the same race and gender, and show that judges favor socially closer defendants.

Works documenting in-group bias have rarely focused on the mechanisms behind it. Following the theoretical and empirical literature on drivers of discrimination that mainly distinguishes between taste-based and statistical discrimination (Becker, 1957; Phelps, 1972; Arnold et al., 2018),[5] we fill this gap by testing model-based predictions that allow us to isolate the mechanisms behind in-group bias. In line with a related line of work documenting that decision makers may be better informed about members of their own group (Fisman et al., 2017; Ash and Marangon, 2026),[6] we show that judges respond more strongly to the predicted risk of socially closer defendants, but we cannot rule out the presence of taste-based preferences. These results also relate to the literature on the effects of victims' characteristics on sentencing decisions (Glaeser and Sacerdote, 2003; Alesina and La Ferrara, 2014), as suggested by the positive coefficient on name similarity for crimes with victims.

Our second contribution is methodological and relates to the literature on social identity and

[3] We also relate to a large literature documenting disparities by race and ethnicity in policing (Knowles et al., 2001; Antonovics and Knight, 2009), pretrial detention (Arnold et al., 2018, 2022), sentencing (Abrams et al., 2012; Rehavi and Starr, 2014), labor markets (Bertrand and Mullainathan, 2004; Charles and Guryan, 2008), and credit and bankruptcy (Bartlett et al., 2022; Argyle et al., 2025).

[4] See Harris and Sen (2019) and Shayo (2020) for reviews on the literature on these topics.

[5] Additional works on this topic include Antonovics and Knight (2009); Anwar and Fang (2015); Arnold et al. (2022).

[6] These papers build on theoretical works like Aigner and Cain (1977); Cornell and Welch (1996)

on names as cues for it. The existing literature has investigated the role of names as signals of identity (Fryer and Levitt, 2004; Clark, 2014; Güell et al., 2015) and used them to study discrimination (Bertrand and Mullainathan, 2004; Bertrand and Duflo, 2017), immigrants' assimilation (Abramitzky et al., 2020a; Fouka, 2020), and cultural transmission (Bazzi et al., 2020; Algan et al., 2022). These studies typically use names to assign individuals to groups, such as race or ethnicity (Imai and Khanna, 2016), or to measure a single dimension of a name, such as how distinctive or foreign-sounding it is. We combine these insights with those from works using word embeddings to measure social and cultural associations in text (Garg et al., 2018; Kozlowski et al., 2019; Ash and Hansen, 2023) to create an embedding-based measure of social proximity based on names. We can compute the resulting relative measure of social proximity between any two individuals using only their names and could therefore be applied to any administrative dataset that records names.

Finally, we contribute to the research on homophily and social distance. Blau (1977) defined social distance as the distance between individuals' positions in a multidimensional social space.[7] In economics, Akerlof (1997) formalized the role of social distance in individual decisions, and a related literature has studied its interaction with homophily both theoretically and empirically (Currarini et al., 2009; Guiso et al., 2009; Chetty et al., 2022). Social distance has received particular attention in the context of migration (Hainmueller and Hangartner, 2013; Fouka, 2019; Abramitzky et al., 2020b), with recent work measuring social distance between natives and immigrant groups continuously using word embeddings (Stampi-Bombelli et al., 2023). In this paper, we propose a method to estimate social distance within groups and show that it shapes decisions in the judicial system.

The rest of this paper is organized as follows. Section 2 describes the data and constructs a measure of social proximity between judges and defendants. Section 3 outlines the empirical strategy and presents the main results on in-group bias based on social proximity. Section 4 introduces a simple model of judges' incarceration decisions and derives testable predictions for the mechanism, which are then tested in Section 5. Section 6 concludes.

2 Data and Social Proximity

This section describes the data and the pipeline to construct the measure of social proximity between judges and defendants. Section 2.1 presents the different types of data we use in this paper, and Section 2.2 describes the construction of the social proximity measure and presents some descriptives on it.

2.1 Data

Wisconsin Circuit Courts Access. The case data come from the API service of Wisconsin Circuit Courts Access (WCCA, available at wcca.wicourts.gov), which includes detailed records on trial

[7] See McPherson et al. (2001) for a review of the evidence on homophily and social distance in sociology.

court cases, with data starting from 1970 (Li et al., 2022; Ash et al., 2023).[8] We use data on criminal cases filed from 2005 to 2021 in 72 county courts in Wisconsin, resulting in around 1.2 million cases.[9]

The case records include information about the charges, the type of offense (felony, misdemeanor, and criminal traffic), defendants' demographic information (e.g., gender, race, and date of birth), and names of judges, attorneys, and prosecutors. The records also contain information about how each charge is resolved and about the sentence. Our sample includes cases in which the defendant is convicted of at least one charge and receives a sentence, while we exclude cases in which all charges are dismissed, or the defendant is acquitted.[10] The main outcome of our analysis is whether the defendant is incarcerated, defined as a sentence to state prison, local jail, or a house of correction. Defendants who are not incarcerated receive probation, a fine, or another non-custodial sentence. We use the number of days of incarceration as a secondary outcome.

We construct a number of additional variables to complement the data. We use a combination of first name, last name, and date of birth as a unique identifier for a defendant. This identifier allows us to conduct a search in the database of case records to match the defendant across multiple cases and construct the additional variables. We obtain the defendant's prior count of each of the three crime types—felony, misdemeanor, and criminal traffic—for each case. We infer age at judgment from the defendant's date of birth and the judgment disposition date of the case. We also construct the age at first offense, which is the age when the defendant committed the first crime.

Table 1 presents summary statistics of the main dataset. The dataset includes five ethnic groups, with about 66% Caucasian and 21% African American. The proportion of male defendants (79%) is significantly higher than that of females, and most cases are committed at a younger age (below 30). The recidivism rate is the highest (54%) among Native Americans, followed by African Americans (43%). Misdemeanors are the most frequent crime type except for Hispanics, with criminal traffic (46%) being the most common crime type. Finally, the average incarceration rate is around 40%, and the average sentence length is 234 days, which is highest for African Americans. Additional summary statistics are reported in the appendix. First, Appendix Tables B.1, B.2, B.3, and B.4 report more detailed case tabulations by crime type and charge severity. Appendix Figure B.1 shows how judge and defendant characteristics vary by charge severity.

Next, we collected data on 651 judges in these courts. We code judges' race and gender by consulting the judges' websites and Ballotpedia. We measure a judge's experience as the number of years between the year she took office and the year the case is filed.[11] Finally, we infer judges' partisanship and ideology by matching judges to personal campaign donations to candidates from the DIME Dataset (Bonica, 2016). Summary statistics on these characteristics are reported in Appendix Table B.5.

[8] This same data source is used by Berdejó (2018, 2019) to examine racial and gender differences in plea bargaining.

[9] See Appendix Section A for details on the data, the sample, and the outcome definition.

[10] Whether a case ends in a conviction is largely outside the judge's control. About 99% of convictions in our sample result from plea bargaining and about 1% from a trial. The records do not report whether a plea was negotiated or its terms, so we cannot fully reconstruct the events of all cases (see Appendix Section A.2 for details).

[11] For readability, we refer to judges as "she" and to defendants as "he" throughout the paper.

Table 1: Summary Statistics by Defendant Ethnicity

	Full sample	Caucasian	African American	Hispanic	American Indian	Asian
	(1)	(2)	(3)	(4)	(5)	(6)
Sample size	1,231,083	805,768	260,572	94,180	57,959	12,604
Sample share		0.655	0.212	0.077	0.047	0.010
Incarcerated	0.38	0.36	0.46	0.34	0.37	0.35
Recidivism Rate	0.40	0.38	0.43	0.36	0.54	0.36
Sentence Length	234	194	379	200	195	237
Sex						
Female	0.21	0.23	0.17	0.13	0.33	0.14
Age						
Below 30	0.51	0.48	0.56	0.55	0.53	0.64
30 to 60	0.48	0.50	0.43	0.44	0.46	0.35
Case type						
Felony	0.35	0.34	0.43	0.22	0.34	0.38
Misdemeanor	0.42	0.43	0.44	0.33	0.46	0.39
Criminal Traffic	0.23	0.23	0.13	0.46	0.20	0.23

Notes. Summary statistics for the estimation sample, in aggregate and by defendant ethnicity. The unit of observation is a defendant-case. Shares are expressed as decimals. *Sentence length* is in days and is computed on incarcerated defendants only. *Recidivism rate* is the share of defendants who recidivate within two years, as defined in Appendix Section C.

Finally, we merge local demographic variables from the 2010 census data. These include population density, the share of people with a college education, the share of people eligible for food stamps, the African American population share, the Hispanic population share, the male population share, the share of people living in rural and urban areas, and median household income. We merge in these variables for each case twice—once at the county level (for the court) and once at the zip code level (for the defendant's address).

Voters Registration Data. Our second source of data is the Wisconsin voter registration file (L2, Inc., 2025). For each registered voter, the file reports the full name, date of birth, sex, and residential address, together with a set of demographic and socioeconomic characteristics, including ethnicity, religion, language, household income, and homeownership. We combine file vintages released between February 2019 and October 2025 and keep one record per voter, for a total of about 6.5 million voters.

We use this data as a training corpus, together with the court records, to train the name embeddings in Section 2.2. Since some defendants are also registered voters, we link the two sources so that the same person does not appear twice in the corpus. We match defendants in criminal cases to voters based on first name, last name, and date of birth. For defendants in civil cases, the date of birth is rarely recorded, and we match them by first name, last name, and residential address. About 10 percent of criminal and civil defendants match to a voter, as reported in Appendix Table B.6.

Recidivism Risk Score. Following Li et al. (2022), we construct a measure for recidivism risk. We train a machine learning model to predict whether the defendant commits another crime within two years of the judgment (see Appendix Figure C.3).[12] The target variable of interest is whether a defendant recidivates or not.[13] We use gender, type of offense, prior criminal count (separately by type), and age at judgment and at first offense as features to predict the target variable. Because recidivism can be observed only for defendants who are not incarcerated for long, we train the model on cases with sentences of at most 180 days (Appendix Section C). To obtain out-of-sample predictions, we split defendants into two random halves, train the model on each half, and predict recidivism risk for each defendant with the model trained on the other half. We use a gradient-boosted classifier from the Python package XGBoost implementation (Chen and Guestrin, 2016). We train the model, including both L1 and L2 regularization, and tune the hyperparameters via randomized search and 5-fold cross-validation.[14]

The quality of the recidivism risk score is evaluated on out-of-sample predictions, using the model trained on the other half of the sample for each defendant. Appendix Table C.8 reports

[12]We define this two-year threshold following Larson et al. (2016), who defined recidivism as a new offense within a two-year period, mainly because Northpointe, the company that designed the COMPAS tool, indicates that its recidivism score is based on that timeline. Further, a study (Hunt and Dumville, 2016) by the U.S. Sentencing Commission showed that most recidivists re-offend within two years after release (if they re-offend at all).

[13]See Appendix C for further details on the construction of this variable.

[14]See Appendix Section C.2 for a detailed explanation of XGBoost.

several standard classifier metrics: accuracy, area under the ROC curve (AUC), false positive rate (FPR), and false negative rate (FNR). At about 67% accuracy and AUC=0.7, the classifier achieves a similar performance to other work predicting recidivism in other jurisdictions (e.g. Lakkaraju et al., 2017; Chouldechova, 2017; Kleinberg et al., 2018b). Further, the model ranks defendants well by recidivism risk, as shown in Appendix Figure C.4. For example, in the top decile of the algorithm's score, around 70% of defendants re-offend. In the bottom decile, under 20% re-offend. Finally, the algorithm has similar error rates across race of the defendant (Appendix Table C.8).

Using this trained model, we compute out-of-sample predictions of the probability of recidivism (recidivism risk score) for each defendant. Appendix Figure C.5 shows that there are different distributions of risk by race and gender (see also Appendix Figure C.6). The distribution for Black defendants is shifted right, indicating a higher predicted recidivism risk on average. The gender difference is even starker, with male defendants having a much higher average recidivism risk than female defendants. Finally, judge decisions are correlated with recidivism risk. Appendix Figure C.7 shows that as recidivism risk increases, the jail rate increases. This relationship holds when conditioning on court-year fixed effects and charge fixed effects (indicators for the severity of the most severe charge).[15]

2.2 Measuring Judge–Defendant Social Proximity

To measure social proximity between judges and defendants, we use defendants' names and information about where they live. We start by training word embeddings for individuals' full, first, and last names, grouping them by where they live. Then, using the same embeddings, we construct judges' name embeddings and compute the cosine similarity between defendants' names and the names of judges assigned to the defendant's case.

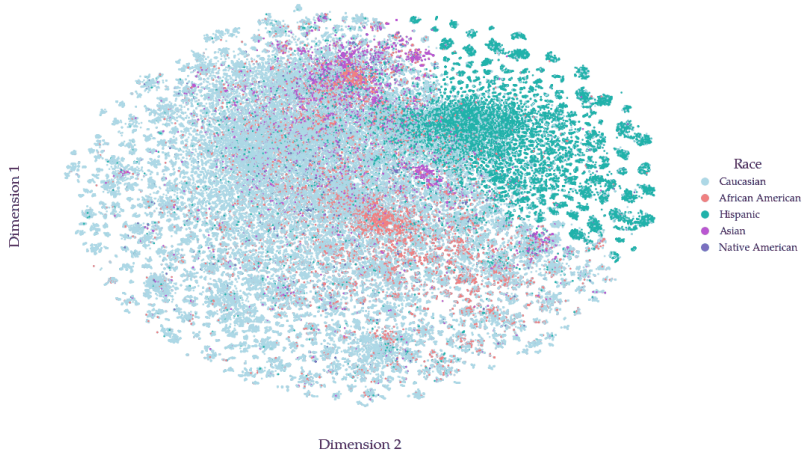
Name Embeddings. We combine both voter registration file and court records to construct the training corpus for the embeddings. Next, we group individuals such that each document in the corpus contains all names of people living in the same street and zip code, for a total of around 605,000 documents (Appendix Table D.9). We train word embeddings on this corpus and create vectors with 200 dimensions for each name using FastText (Bojanowski et al., 2017), which represents each name as the sum of the vectors of its character n-grams. This lets us compute a vector for any name, including judges who do not appear in the training corpus, and learn more precise vectors for rare names by borrowing information from names with similar spellings.[16]

[15] Appendix Figure C.8 shows the incarceration rate by recidivism risk, separately for cases with social proximity above and below the sample mean. Defendants who are socially closer to the judge are less likely to be incarcerated at low and intermediate levels of risk, while the two groups are treated similarly at high levels of risk, a pattern we investigate in Section 5.

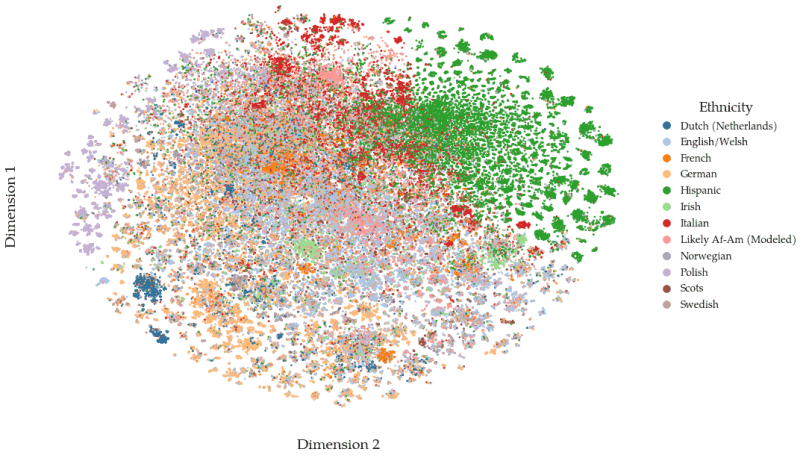
[16] We use the skip-gram architecture, which performs better than the alternative for infrequent words, and we keep all names regardless of how often they appear, since most names in the corpus are rare. We set the context window to 10 names, so that the context of a name includes all other residents in documents up to the 87th percentile, and we let its size vary at random for each name to limit the weight of very long documents. Appendix Section D.1 provides additional details on the training.

Figure 1: Last-Name Embeddings by Race and National Origin

(a) Race



(b) National Origin



Notes. Last-name embeddings projected onto a two-dimensional space using t-SNE. Panel (a) plots the embeddings separately by race and Panel (b) by national origin, for the twelve most frequent origins in the voter registration file. Each dot is a distinct last name, colored by the characteristic of one randomly drawn person with that name.

Figure 1 plots a two-dimensional representation of the last-name embeddings. The figure shows that when clustering names by race (Panel (a)), Hispanic individuals occupy a separate region of the space, while Black and Asian individuals form smaller clusters. Panel (b) shows that the embeddings also capture differences within racial groups, as last names of Polish, Dutch, German, and Italian origin form distinct clusters. In contrast, last names do not cluster by gender (Appendix Figure D.9). Appendix Figures D.10 and D.11 show the same representation for first- and full-name embeddings, which cluster by both race and gender.

However, the raw embeddings also contain noise. Word embeddings tend to be dominated by a few directions, which reflect how frequent a word is more than its meaning (Mimno and Thompson, 2017; Mu et al., 2018). Appendix Figure D.12 shows that our name embeddings suffer from the same issue, with the embeddings predicting how common a name is with an R^2 of 0.51 for last names and 0.71 for first names. As a result, two names can appear similar simply because they are both common. Following Mu et al. (2018), we remove the leading principal components from the embeddings. Specifically, we estimate the principal components on the name embeddings of all defendants in the case data together with all judges, and subtract from each embedding its projection on the first k components. Appendix Figure D.12 shows that after removing the first three components, the R^2 falls from 0.51 to 0.23 for last names, from 0.71 to 0.20 for first names, and from 0.37 to 0.16 for full names, and after removing five components, it falls to 0.18, 0.15, and 0.16, respectively. Removing additional components reduces it only marginally, to 0.14, 0.08, and 0.14 after removing ten components. The leading components also capture broad demographic characteristics.[17]

Appendix Figure D.13 shows how this correction changes what the distance between two names captures. For random pairs of registered voters, we compare the cosine distance between their name embeddings with the distance between their demographic characteristics. While for first names in Panel (b) the correlation is positive across all embedding variations, for last names (Panel (c)) it becomes positive only after removing three or five components, and is zero for the raw embeddings and again after removing ten components. Removing a few components thus makes the embeddings more informative about how different two individuals are, while removing many discards this information. Therefore, we remove five principal components from the embeddings and use these principal component-corrected embeddings as the main measure in the rest of the paper.

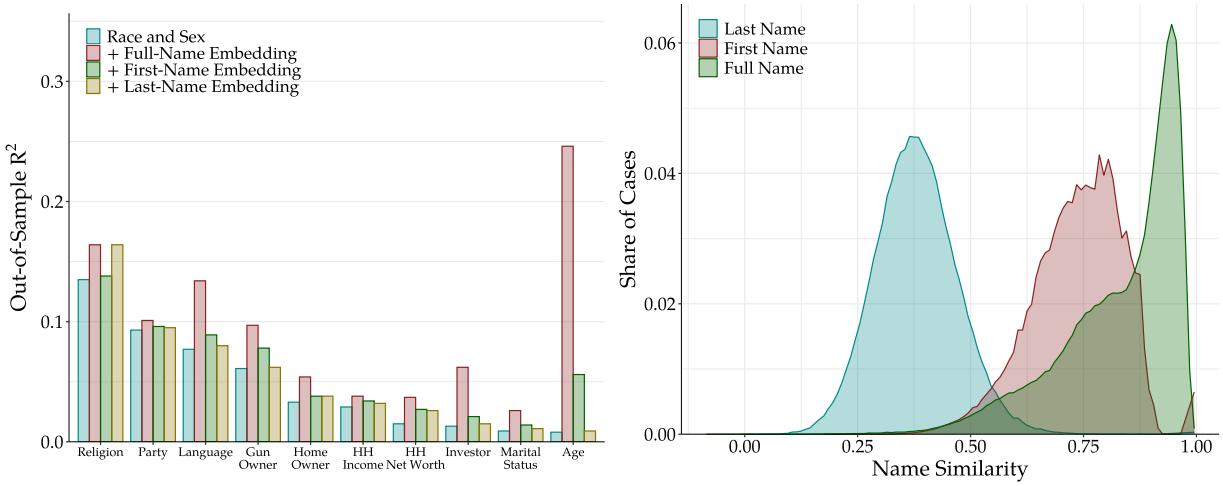
Finally, we investigate whether the name embeddings add additional information with respect to commonly observed characteristics, like race and gender. For this purpose, in Figure 2a we report the out-of-sample R^2 of a ridge regression on race and gender only, and race and gender plus the corrected embeddings of full names, first names, and last names. The figure shows that last names add as much as first names, but on different dimensions. Their contribution is concentrated on religion, driving the increase in the religion R^2 for the full embeddings, while first names add

[17] Appendix Figure D.17 shows that, for last names, the first five components are correlated with race, while for first and full names they are mostly correlated with gender.

Figure 2: Name Embeddings and Judge–Defendant Name Similarity

(a) Predicting Individual Characteristics from Name Embeddings

(b) Distribution of Judge–Defendant Name Similarity



Notes. Panel (a): out-of-sample R^2 of ridge regressions predicting the characteristics of registered voters from race and gender alone, and adding the name embeddings of full, first, and last names after removing the first five principal components. Panel (b): distribution of the similarity between the names of the judge and of the defendant in the estimation sample, reported separately for last names, first names, and full names. Similarity is the cosine similarity between the name embeddings after removing the first five principal components.

little on religion and more on age, where the full-name embeddings add most. The additional information in full names therefore reflects mainly an individual's cohort, while last names reflect a family's religious and national background (Appendix Figure D.14).

Judge–Defendant Social Proximity. We measure the proximity between a judge and a defendant by computing the similarity of the embeddings of their names. For each case, we compute the embeddings of the names of the defendant and of the assigned judge, remove the first five principal components, and compute the cosine similarity between the two corrected embeddings.[18] We construct social proximity between judge j and defendant i as

$$s_{ij} = \cos\left(\tilde{v}_i, \tilde{v}_j\right) \tag{1}$$

where $\tilde{v}_i$ and $\tilde{v}_j$ are the corrected embeddings of defendant i and judge j. We compute this measure separately for full names, first names, and last names. Figure 2b reports the distribution of the three measures in the estimation sample. Last-name similarity is centered around 0.38 and approximately symmetric, while first-name and full-name similarities are, on average, higher. In particular, full-name similarities are concentrated near 1 and are above 0.9 in 42% of the cases,

[18]The embeddings of judges are computed with the same model used for defendants. For the judges whose name does not appear in the training corpus, the embedding is computed from the character n-grams of the name.

Table 2: Name Similarity and Judge–Defendant Characteristics

	Judge–Defendant Social Proximity (PC5)					
	Full Name		First Name		Last Name	
	(1)	(2)	(3)	(4)	(5)	(6)
Black Judge × Black Defendant	0.034**	0.037**	-0.017**	-0.019**	0.028**	0.030**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)	(0.005)
Female Judge × Female Defendant	0.020**	0.021**	0.013**	0.013**	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Hispanic-Named Judge × Hispanic Defendant	0.032**	0.031**	0.005	0.008+	0.013**	0.012**
	(0.007)	(0.007)	(0.004)	(0.004)	(0.004)	(0.004)
Recidivism Risk (std.)	0.001**	0.001**	-0.000	-0.000	-0.001**	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,230,916	1,192,155	1,230,912	1,192,151	1,230,916	1,192,155
R^2	0.217	0.230	0.218	0.229	0.112	0.124
Mean Dep. Var.	0.832	0.832	0.735	0.735	0.380	0.380
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Zip Code FE		X		X		X

Notes. Correlation between judge–defendant name similarity and the characteristics of the judge and of the defendant. The dependent variable is the similarity between the full names (Columns 1 and 2), first names (Columns 3 and 4), and last names (Columns 5 and 6) of the judge and of the defendant, after removing the first five principal components. *Hispanic-Named Judge* indicates judges with a Hispanic last name. All regressions include the main effects of the judge's and the defendant's ethnicity and gender, and county-year and charge severity fixed effects. Columns 2, 4, and 6 also include fixed effects for the zip code of the defendant. Recidivism risk is standardized to have mean 0 and standard deviation 1. Standard errors are clustered at the county-year level (in parentheses): + p < 0.1, * p < 0.05, ** p < 0.01.

so that most of their variation comes from the pairs of names that are unusually different. One potential explanation for these differences in distribution is that they may reflect how the three sets of names are distributed in the training corpus. Full names are long and almost always unique, so their embeddings are built mostly from character sequences shared with many other names. First names are spread evenly across streets and therefore appear in similar contexts, while last names are shared within households and concentrated on specific streets, making their embeddings more distinct and potentially making their similarity a better indicator of social proximity.

We investigate this hypothesis by asking whether the similarity between two names reflects how much the individuals with those names live in the same places. For 300,000 random pairs of names, we compute how much the registered voters with the two names live in the same zip codes, and relate it to the similarity between the two names.[19] Appendix Figure D.15 shows that co-location increases steadily with name similarity for last names, while for first names and full names it is flat for all but the most similar pairs. The similarity between last names is thus informative about who lives close to whom, while the similarity between first names and full names is mostly not, suggesting that last name similarity may be a better proxy for social proximity.

As a final validation, we look at how the measure of social proximity correlates to gender and

[19]For each name, we compute the share of the registered voters with that name who live in each zip code, minus the share of all registered voters who live there. Co-location is the cosine similarity between these vectors for the two names. We consider names with at least 50 registered voters, and define full names as the combination of first and last name.

racial in-group dimensions. Table 2 reports results from regressing each similarity measure on indicators that the judge and defendant share the same race, gender, and Hispanic ethnicity, and on the defendant's predicted recidivism risk, controlling for county-year and charge-severity fixed effects. Last-name similarity is positively correlated with both the judge and defendant being Black or Hispanic, but not with them being the same gender.[20] First-name similarity follows the opposite pattern: it is positively correlated with the judge and defendant being the same gender and negatively correlated when both are Black.

Last-name similarity thus reflects the ethnic and family background that the judge and the defendant share, but not their gender, while the opposite is true for first names. Full-name similarity is a combination of both, and is therefore positively correlated with both gender and race in-group dimension. These patterns are robust to including fixed effects for the zip code of the defendant (Columns 2, 4, and 6). Finally, similarity between last names is negatively correlated with the defendant's recidivism risk: defendants with higher predicted risk have last names less similar to those of their judges. The correlation is statistically significant but small, at around one percent of a standard deviation of similarity for a one standard deviation increase in risk.

Taken together, this evidence suggests that last-name similarity more closely matches the definition of social proximity as the closeness of two individuals' positions in social space (Blau, 1977; McPherson, 1983), along the ethnic, national, and religious dimensions. Unlike first names, which mainly reflect gender and age, last names are inherited and capture the ethnic and family background two individuals share. First names are chosen by parents, and their choices are likely to reflect their education and social context (Lieberson and Bell, 1992; Fryer and Levitt, 2004; Abramitzky et al., 2020a; Bazzi et al., 2020). Last names are not subject to this choice and are passed on across generations (Clark, 2014; Güell et al., 2015). As a result, their similarity tracks both socio-demographic distance and residential co-location. We therefore use last-name similarity, after removing five principal components, as our measure of social proximity in the rest of the paper, and report results based on first and full names in the Appendix.

3 Social Proximity and In-Group Bias

3.1 Empirical Strategy

Our empirical strategy uses regression analysis to estimate the effect of social proximity between judges and defendants on judges' incarceration decisions. A major concern in this setting is that judges may select into cases based on their preferences, expertise, and defendant characteristics. If this is the case, then the disparities we observe across judge/defendant groups could reflect case and/or judges' characteristics, like case severity or judge leniency, rather than actual in-group preferences. For instance, suppose that judges from central districts in Milwaukee are strongly against thefts of automobiles; in turn, they would try to pick cases with such offenses and rule

[20] Appendix Figure B.2 shows that the distribution of social proximity is similar for White and Black defendants, while it is shifted to the right for Hispanic defendants.

harshly on them. Suppose, meanwhile, accused automobile thieves mainly come from similar districts. Then, we would observe greater harshness by judges toward defendants with higher social proximity, but that would reflect the selection of a harsher judge for auto theft cases rather than in-group bias.

We can address this concern in Wisconsin courts because procedural rules prevent trial judges within the same court from selecting into different types of cases (conditional on observables). In most courts, cases are randomly assigned to judges by a computer program. In most others, we were able to verify that case assignment is not intentionally random but "quasi-random" in the sense of being arbitrary—judges cannot intentionally select defendants, and defendants cannot intentionally select judges. These are official rules, so it is possible that they are not being followed with perfect compliance. Further, there are some exceptions where, for example, judges can recuse themselves from a case, or defendants could formally request a new judge.

Therefore, to check that judge assignment is orthogonal to case characteristics, we run a set of balance checks using our data. Since cases are assigned to judges within a county and year, the part of social proximity that depends on which judge is assigned is the difference between the defendant's social proximity to the assigned judge and the defendant's mean social proximity to all judges sitting in the same county-year. We run the following regression to check if this difference is correlated with defendant and case characteristics:

$$x^k_{ijct} = \beta_0 \tilde{d}_{ij} + \alpha_{ct} + \alpha^s_i + \varepsilon_{ijct} \tag{2}$$

where x^k_{ijct} is a characteristic k of case/defendant i (e.g., charge severity, ethnicity, gender) assigned to judge j in court/county c at year/month t. On the right-hand side, $\tilde{d}_{ij} = s_{ij} - \bar{s}_{i,ct}$ is the difference between the defendant's social proximity to the assigned judge and the defendant's mean social proximity to the judges sitting in county c in year t, standardized to mean zero and unit standard deviation, while α_{ct} are county-year fixed effects, allowing block randomization of judges to cases. As judges are (quasi-)randomly assigned to cases within a court and particular time period, court fixed effects are interacted with time fixed effects (court-year or court-month). Further, there can be selection of judges into cases based on the severity of the crime (see Appendix Figure B.1), which can occur, for example, when new judges do not sit on capital cases. Hence α^s_i also includes charge severity fixed effects.

Table 3 tests whether the part of social proximity that depends on which judge is assigned is related to case and defendant characteristics. Columns 1 and 2 show that cases in which the defendant's social proximity to the assigned judge is below and above the bench mean are similar even before any adjustment. Differences in defendant race, gender, age, recidivism risk, and neighborhood characteristics are small, although several are statistically significant given the sample size (Column 3). Once we compare cases within county-year and charge severity, the differences become negligible and are jointly insignificant (Column 4). The table shows that a one-standard-deviation increase in social proximity is associated with a 0.1 percentage point higher share of Black defendants and a 0.003 standard deviation higher recidivism risk score, which we

Table 3: Randomization Checks for Judge Assignment

	Mean		Social Proximity	
	Below Bench Mean (1)	Above Bench Mean (2)	(3)	(4)
Charge Severity	9.8592	9.8333	-0.0237^{+}	-0.0097
	(2.6331)	(2.6289)	(0.0135)	(0.0123)
Recid. Score (std.)	-0.0022	0.0023	0.0027^{+}	0.0032^{+}
	(1.0006)	(0.9994)	(0.0015)	(0.0018)
Black Defendant	0.2118	0.2120	-0.0011	0.0012^{+}
	(0.4086)	(0.4087)	(0.0022)	(0.0007)
Female Defendant	0.2130	0.2124	-0.0003	0.0006
	(0.4095)	(0.4090)	(0.0006)	(0.0005)
Defendant Age	31.7890	31.7310	-0.0177	0.0048
	(11.1238)	(11.1471)	(0.0192)	(0.0138)
First Offense	0.3242	0.3264	0.0008	-0.0013
	(0.4681)	(0.4689)	(0.0010)	(0.0008)
Felony	0.3495	0.3479	-0.0019	-0.0004
	(0.4768)	(0.4763)	(0.0025)	(0.0024)
Misdemeanor	0.4270	0.4220	-0.0039^{+}	-0.0031
	(0.4946)	(0.4939)	(0.0022)	(0.0020)
Criminal Traffic	0.2235	0.2302	0.0057*	0.0036
	(0.4166)	(0.4209)	(0.0029)	(0.0024)
Zip: Share Black	0.0967	0.0977	0.0002	0.0003
	(0.1975)	(0.1994)	(0.0013)	(0.0003)
Zip: Share Male	0.4989	0.4985	-0.0002**	-0.0001
	(0.0329)	(0.0328)	(0.0001)	(0.0000)
Zip: Share Urban	0.5702	0.5744	0.0015	0.0005
	(0.4603)	(0.4603)	(0.0032)	(0.0004)
Zip: Share College	0.2291	0.2271	-0.0015^{+}	-0.0002
	(0.1095)	(0.1069)	(0.0008)	(0.0001)
Zip: Share Food Stamps	0.1235	0.1230	-0.0001	0.0002*
	(0.0774)	(0.0777)	(0.0005)	(0.0001)
Zip: Log Median Income	10.7582	10.7600	0.0009	-0.0002
	(0.2673)	(0.2677)	(0.0014)	(0.0004)
Observations	629,597	599,722	1,229,319	1,229,319
Joint F-Statistic			5.3098	1.3218
Joint F p-Value			0.0000	0.1930
County-Year FE				X
Charge Severity FE				X

Notes. Balance of case and defendant characteristics with respect to the social proximity of the assigned judge, measured as the difference between the defendant's social proximity to the assigned judge and the defendant's mean social proximity to all judges sitting in the same county-year. Columns 1 and 2 report means, with standard deviations in parentheses, for cases in which the defendant's social proximity to the assigned judge is below and above the defendant's mean social proximity to the county-year bench. Columns 3 and 4 report the coefficient from a regression of each characteristic on this difference, standardized to have mean zero and standard deviation one. Column 4 includes county-year and charge-severity fixed effects, except for the rows on charge severity and case type, which include county-year fixed effects only. The joint F-statistic tests that all characteristics are jointly unrelated to social proximity in a regression of social proximity on all of them. Standard errors clustered at the county-year level in parentheses: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

control for in our preferred specification.

Next, to investigate in-group bias based on social proximity, we estimate the following regression equation:

$$y_{ijct} = \mathbf{x}_i'\theta_x + \mathbf{x}_i'\mathbf{w}_j'\theta_{xw} + \beta s_{ij} + \alpha_{ct} + \alpha_i^s + \alpha_i^v + \alpha_j + \varepsilon_{ijct} \tag{3}$$

where i denotes a defendant-case, j the judge assigned to that case, c is the court/county, and t is year. The main outcome y_{ijct} is an indicator variable taking the value 1 if the defendant in i is incarcerated and 0 otherwise, which we refer to throughout as releasing the defendant. We also look at the effect on days of incarceration assigned as a proxy for the harshness of the sentence, taking the inverse hyperbolic sine of sentence length and excluding cases with no incarceration. To leverage quasi-random judge assignment, we include court-time fixed effects (α_{ct}) and thus address the possibility that harsher judges select into/out of cases with defendants with high/low social proximity. Similarly, we include charge severity fixed effects (α_i^s) to address the issue that judges may handle socially closer defendants who commit more severe crimes. We also include risk ventile fixed effects (α_i^v) that allow us to compare defendants with similar misconduct potential. These ventile fixed effects are calculated starting from the residualized recidivism risk score, after taking out county-year fixed effects, and are designed to address the issue that recidivism risk is correlated with social proximity, although weakly (Table 2). If judges respond to risk, this relation alone would generate a correlation between similarity and incarceration. When including the risk ventiles fixed effects, we estimate variation in judge decisions according to defendant characteristics while flexibly controlling for such risk.

Finally, given the imperfect nature of random assignment in our context, particularly concerning courts with fewer judges, we control for additional defendant characteristics, like age, gender, and race ($\mathbf{x}_i'$). We also include the interaction between defendants' and judges' characteristics ($\mathbf{x}_i'\mathbf{w}_j'$) to account for potential correlations between social proximity and additional in-group dimensions. This is a particularly important element to include in our regression since social proximity is indeed correlated with judges and defendants being of the same race or gender. For instance, we observe that social proximity is correlated with race. If Black judges are on average harsher than White judges toward Black defendants (Ash and Marangon, 2026), then the coefficient on social proximity would capture racial in-group bias rather than any effect of a higher name similarity between judges and defendants. Finally, we include judge fixed effects (α_j) to account for all time-invariant judges' characteristics. We cluster standard errors at the county×year level.

The coefficient of interest is β, which measures how much judges are more or less likely to incarcerate defendants with higher social proximity. For instance, a negative β means judges are less likely to incarcerate defendants whose last names are closer to their own. Still, the regression results should be interpreted with caution. A β different from zero does not necessarily indicate bias due to in-group preferences, since this might vary with defendants' characteristics. We will explore these issues in more detail in Sections 4 and 5.

Table 4: In-Group Bias: Incarceration and Sentence Length

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.103**	-0.082**	-0.037**	-0.188**	-0.237**	-0.076*
	(0.022)	(0.008)	(0.006)	(0.035)	(0.032)	(0.031)
Female Def.			-0.068**			-0.211**
			(0.002)			(0.009)
Black Def.			0.044**			-0.186**
			(0.003)			(0.016)
Black J. × Black Def.			0.042**			-0.020
			(0.010)			(0.053)
Female J. × Female Def.			0.002			-0.012
			(0.003)			(0.017)
Observations	1,230,916	1,230,888	1,230,832	458,464	458,434	458,412
R^2	0.087	0.098	0.121	0.490	0.498	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
Ratio: Social Prox. / Black J. × Black Def.			-0.889			
Ratio: Social Prox. / Black Def.						0.407
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X	X	X	X	X
Judge FE		X	X		X	X
Other Controls			X			X

Notes. Effect of social proximity on judges' incarceration decision (Columns 1–3) and on the length of the jail sentence (Columns 4–6). Social proximity is computed from name embeddings after removing the first five principal components. All columns include county-year, charge severity, and risk ventiles fixed effects. Columns 2–3 and 5–6 add judge fixed effects, and Columns 3 and 6 add controls for judges' and defendants' race, gender, and age and their interactions. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

3.2 Results

Table 4 reports the results from equation (3). The table shows that judges are less likely to incarcerate defendants who are socially closer to them (Columns 1–3) and sentence them to shorter jail time (Columns 4–6). Without additional controls, moving from the 25th to the 75th percentile of the social proximity distribution, an increase of 1.3 standard deviations, reduces the probability of incarceration by 1 percentage point (Column 2). The estimate falls to about -0.44 percentage points for a 1.3 SD increase once we control for defendants' characteristics, including ethnicity, and for interactions with judges' characteristics (Column 3). Part of the effect in Column 2, therefore, reflects differences across ethnic groups that the controls absorb, but a significant effect of social proximity remains within ethnic groups, equal to 10% of the racial in-group effect.[21] Columns 4 to 6 show a similar pattern for the length of the jail sentence among incarcerated defendants. A 1.3

[21]Consistent with Ash and Marangon (2026), Black judges are more likely than other judges to incarcerate Black defendants, while we find no gender in-group effect.

standard deviation increase in social proximity leads to shorter sentences by 1 to 2.8 percent, with and without additional controls, respectively, which corresponds to approximately 2 to 6.5 days.

Appendix Tables E.10 and E.11 report the results from the same analysis using first-name and full-name similarity. Without controls, first-name similarity increases the probability of incarceration and sentence length, while full-name similarity decreases both outcomes. However, both effects disappear once we control for defendants' characteristics and their interactions with judges' characteristics (Columns 3 and 6). We also investigate which measure of name similarity affects the outcomes the most in Appendix Table E.12, where we include all measures jointly. Including first and last name together (Columns 1 and 4) yields coefficients for first- and last name-based proximity that are very similar to the main corresponding specifications. Including full name similarity leaves the last name coefficient virtually unchanged and moves the first name coefficient by little, while the coefficient on full name is between 30 and 60 percent lower than in Appendix Table E.11. When including additional controls for race, gender, and their interactions, only the coefficient on last name similarity remains significant, with magnitude virtually identical to the one in Table 4, while the coefficients on first name and full name similarity are close to zero. This is consistent with the evidence in Section 2.2 showing that first names mainly carry information on gender and age, which we control for directly, while last names carry additional sociodemographic information that we do not directly observe.

Next, we investigate whether the effect of social proximity is driven by some judges' characteristics. Appendix Table E.13 estimates the heterogeneous effect of social proximity on incarceration by interacting it with indicators for Black, female, and Republican judges. The results show that the effect does not differ between male and female judges, nor between Republican and Democratic judges. However, it becomes positive for Black judges, suggesting that Black judges are harsher toward socially closer defendants beyond the racial match, in line with their higher propensity to incarcerate Black defendants documented in Table 4. Since Black judges decide fewer than 2 percent of the cases, this estimate is imprecise. Appendix Table E.14 reports the same specifications for sentence length and finds no evidence of heterogeneity by any judge characteristic.

Finally, we ask whether the effect comes from the social proximity between the defendant and the assigned judge, or from the defendant's proximity to the judges of the court more generally. In the second case, our measure would not capture a relationship specific to the judge–defendant pair, but rather whether a defendant is close to judges as a group, for instance because they come from a more educated or higher-income background. Defendants whose names are similar to those of local judges may differ in ways we do not observe that also affect their sentences, such as in education, income, or the ability to afford private counsel.[22] To separate these channels, Table 5 adds to the baseline specification the defendant's average social proximity to judges sitting in the same county and year (the judge pool).

[22] Appendix Table B.7 shows that defendants with different levels of social proximity differ on other dimensions as well. Defendants with high social proximity are slightly more likely to be Black and to be charged with criminal traffic offenses, and they live in zip codes with slightly lower median income and a higher share of food-stamp recipients, although these differences are small and shrink further within county-year and charge severity.

Table 5: Social Proximity to the Assigned Judge and to the Judge Pool

	Defendant is Incarcerated			
	(1)	(2)	(3)	(4)
Social Proximity	-0.034**	-0.035**	-0.036**	-0.034**
	(0.009)	(0.008)	(0.008)	(0.009)
Avg. Social Prox. to Judge Pool	-0.005	-0.005	-0.005	-0.013
	(0.013)	(0.011)	(0.011)	(0.012)
Observations	1,230,832	1,229,249	1,205,813	1,081,251
R^2	0.121	0.121	0.121	0.122
Mean Dep. Var.	0.377	0.377	0.378	0.383
County-Year FE	X	X	X	X
Charge Severity FE	X	X	X	X
Risk Ventile FE	X	X	X	X
Judge FE	X	X	X	X
Other Controls	X	X	X	X
Assigned Judge in the Pool	X			
Bench Size Restriction			≥ 3	≥ 5

Notes. Effect on the incarceration decision of the defendant's social proximity to the assigned judge and of the defendant's mean social proximity to the judges sitting in the same county-year (the judge pool). Social proximity is computed from last-name embeddings after removing the first five principal components. In Column 1 the pool includes the assigned judge, while Columns 2–4 exclude it. Columns 3 and 4 restrict the sample to county-years with at least three and five judges, respectively. All columns include county-year, charge severity, risk ventiles, and judge fixed effects and control for judges' and defendants' race, gender, and age and their interactions. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

The results show that the effect comes from the social proximity between the defendant and the specific judge deciding the case, regardless of how we define the judge pool. This suggests that our estimates do not reflect defendants' characteristics that make them closer to judges in general. The coefficient on proximity to the assigned judge is virtually unchanged relative to the main specification in Table 4, while proximity to the judge pool has no effect on sentencing. Similarly, for sentence length, the coefficient on proximity to the assigned judge is negative and significant in most specifications, while proximity to the judge pool is not significant in any specification (Appendix Table E.15). As a placebo test, we also add the defendant's social proximity to a judge drawn at random from the other judges sitting in the same county-year, who did not decide the case. Proximity to this placebo judge has no effect once we control for proximity to either the assigned judge or the judge pool, while the coefficient on proximity to the assigned judge is unchanged (Appendix Tables E.16 and E.17).

Interpreting Social Proximity. As described in Section 2.2, social proximity is constructed from names, which carry information on several dimensions of a person's background, from ancestry to socio-economic background. The effect we estimate may therefore reflect proximity along any of these dimensions, and we now investigate which of them drives it.

First, last names carry information on national origin, so judges might favor defendants who share their ancestry rather than their social background. To test this, we assign each surname to its most frequent national origin among the registered voters who carry it, and construct an indicator for whether the judge's and the defendant's surnames map to the same origin. Pairs with the same origin are indeed socially closer according to our measure (Appendix Figure E.18a). However, sharing the same surname origin has no effect on either the incarceration decision or the length of the jail sentence, and the coefficient on social proximity is unchanged when we control for it (Appendix Table E.18).

Second, names also carry information on a person's socio-economic background, and we can test this dimension directly by measuring the effect of how similar the backgrounds of judges and defendants are independently of their names. For this purpose, we locate judges' home zip codes by matching them to the voter file[23] and measure how close the judge's and the defendant's zip codes are in their socio-economic characteristics, like median income, education, or rurality, and in their racial composition. We also construct a variable indicating the judge and defendant being from the same zip code.

Appendix Figure E.18 shows that judges and defendants who live in the same zip code or in

[23]We match each judge to the registered voters in the voter file with the same first and last name and sex, who are registered in a county where the judge hears cases and are between 30 and 90 years old. We keep only judges with exactly one matching voter and assign them that voter's residential zip code. This yields a unique match for 401 of the 651 judges in our sample, who decide 71% of the cases. We validate this matching looking at judges that also appear as donors in campaign-finance records, which report the donor's address. The zip code matched from the voters files agrees with the one in those records for 62% of judges and the county for 81%; most disagreements involve older contribution records or adjacent zip codes. Since a mismatched zip code adds noise to the neighborhood proximity measures, their estimated effects are likely attenuated toward zero.

neighborhoods with similar socio-economic characteristics have more similar names, consistent with our interpretation of the measure as capturing social proximity. In contrast, our measure is not positively related to the similarity in the racial composition of their neighborhoods. We investigate the effect of these proximity measures in Appendix Table E.19. The results show that defendants who live in neighborhoods socially closer to the judge are less likely to be incarcerated and receive shorter sentences, in line with the main results. This holds when we control for the characteristics of the defendant's zip code, so that the effect does not reflect defendants from richer neighborhoods being treated more leniently. In contrast, proximity in racial composition has a small, non-robust effect, and judges do not treat defendants differently based on whether they live in the same zip code.

We also investigate whether the effect of social proximity varies with the characteristics of the defendant's neighborhood in Appendix Figure E.19. While the effect is similar across terciles of median household income, it is about twice as large for defendants living in the most urban and least educated zip codes as for those in the top terciles of the rural and college graduate shares. Social proximity therefore matters most for defendants from urban, low-education neighborhoods.

Finally, judges and defendants with the same surname may be related, and kinship rather than social proximity could explain a more lenient treatment. However, identical surnames are rare, accounting for about 0.03% of cases, and the results remain unchanged when we exclude them or the top 1% of the social proximity distribution, and when we control for judges and defendants having the same surname (Appendix Table E.20). If anything, defendants who share the judge's surname are treated more harshly, although the difference is not statistically significant.

Overall, this evidence suggests that our measure of social proximity does not reflect shared ancestry or family ties. Instead, its effect is closest to neighborhood social proximity, suggesting that judges are more lenient toward defendants who come from social environments similar to their own. Names, however, capture more than the neighborhood where defendants live, as highlighted by Appendix Table E.19. When we include both measures in the same regression, the coefficient on social proximity is unchanged while neighborhood proximity retains its own effect. Our measure therefore reflects dimensions of a shared social background that the characteristics of the neighborhood capture only in part.

3.3 Robustness

We start by ruling out alternative explanations that could be driving the effect of social proximity. First, it could be that the effect we find reflects differences across zip codes rather than a real shared background between judges and defendants. In Appendix Table F.22, we show that the results are robust to including zip code fixed effects, alleviating this concern. Second, social proximity to the judge could be correlated with the prosecutor assigned to the case. If prosecutors who handle socially closer defendants are more willing to offer lenient plea deals or to request shorter sentences, the lower incarceration rates we observe would reflect the prosecutor rather than the judge. To account for this, we include prosecutor fixed effects in Appendix Table F.23, and show

that the results are very similar to the main specification.

Next, we investigate the effect for first offenders. Repeated offenders are often assigned to the same judge, so a judge may know them from earlier cases, and social proximity could capture this familiarity rather than the closeness of their backgrounds. Appendix Table F.24 restricts the sample to first time offenders whom a judge has never seen before and shows that the results are similar in this sample as well. Similarly, controlling for an indicator for first-time offenders in the full sample does not affect the estimates (Appendix Table F.25).

Finally, we show that our results are robust to different fixed effects specifications and different constructions of the social proximity measure. In Appendix Table F.26 we replace the county-year and charge severity fixed effects with their interaction—county×year×severity fixed effects—so that we compare only defendants charged with offenses of the same severity in the same county and year and show that the results are robust to this more demanding specification. Moreover, we investigate the robustness of our results when removing 3 or 7 principal components. As discussed in Section 2.2, removing three or more components eliminates most of the information on how common a name is. Removing a different number of components yields effects on incarceration that are similar to the main ones (Appendix Tables F.29 and F.30). The results also do not depend on the size of the context window used to train the embeddings, as shown in Appendix Tables F.31–F.33. The effect on sentence length is similar in sign in all specifications, but it is smaller and noisier, and it is close to zero with a window of three names. Finally, the results are robust to clustering standard errors by judge, or two-way by judge and defendant, to account for the same defendant appearing in several cases (Appendix Tables F.27 and F.28).

4 A Model of Judging With Bias and Uncertainty

The previous section shows that judges treat socially closer defendants more leniently. This lenience may reflect two different mechanisms. Judges may prefer to release defendants who are close to them (taste-based discrimination), or they may be better able to assess the risk of defendants who are close to them, because they share a social environment and can interpret their circumstances more accurately (statistical discrimination). This section presents a simple model of the incarceration decision that separates the two mechanisms and derives predictions we can test with our measure of recidivism risk. Following Arnold et al. (2018, 2022), judges decide whether to incarcerate a defendant by weighing the expected cost of releasing them, which depends on their risk of re-offending. Following Phelps (1972) and Aigner and Cain (1977), judges observe a noisy signal of this risk, whose precision increases with social proximity, in line with the idea that individuals have better information about members of their own group (Cornell and Welch, 1996; Fisman et al., 2017).

Setup. We model the decision of a judge j about a defendant i. Let s_{ij} denote the social proximity between the judge and the defendant. Each defendant has true, unobserved recidivism risk r_i, with

prior distribution $r_i \sim \mathcal{N}(\mu_i, 1)$, where the prior mean μ_i may vary with the defendant's observable characteristics, and the variance (and precision) is assumed to always be one for simplicity. The judge observes a signal $\tilde{r}_{ij} = r_i + \varepsilon_{ij}$, with $\varepsilon_{ij} \sim \mathcal{N}\left(0, 1/\rho_{ij}\right)$, whose precision increases with social proximity

$$\rho_{ij} = \rho_0 + \rho_s \, s_{ij}, \tag{4}$$

where $\rho_s \geq 0$ so that judges receive a more precise signal about more similar defendants. The judge's posterior mean of the defendant's risk is the average of the prior mean and the signal, weighted by the relative precision of the signal

$$\bar{r}_{ij} = \mu_i + w_{ij}\left(\tilde{r}_{ij} - \mu_i\right), \qquad w_{ij} = \frac{\rho_{ij}}{1 + \rho_{ij}}. \tag{5}$$

The judge's utility from releasing the defendant is $U_j(\text{release}_i) = \phi \, s_{ij} - \tau \, \bar{r}_{ij}$, where $\tau > 0$ is the cost of releasing a defendant who may re-offend and ϕ captures taste-based preferences: $\phi > 0$ indicates a preference for releasing socially closer defendants and $\phi < 0$ the opposite. The utility from incarcerating the defendant is $U_j(\text{jail}_i) = \kappa_j$, which varies across judges and captures their overall harshness. The judge incarcerates the defendant when

$$I_{ij} \equiv \tau \, \bar{r}_{ij} - \phi \, s_{ij} + \kappa_j > 0 \tag{6}$$

That is, the cost of release is higher than the benefit. Equation (6) shows that observing more lenient treatment of socially closer defendants does not allow us to pin down its source. Comparing defendants with the same signal and the same prior, a lower probability of incarceration for socially closer defendants could reflect a taste-based preference for releasing them ($\phi > 0$). However, social proximity also changes how the judge forms beliefs about the defendant's risk through the precision of the signal, so that judges may treat socially closer defendants differently because they assess their risk differently, even when they have no preference for them. Therefore, the decision rule in (6) does not allow us to directly disentangle why in-group bias could arise.

In-Group Bias and Recidivism Risk A change in the signal affects only the left-hand side of the decision rule. The response of the incarceration decision to the signal therefore is

$$\frac{\partial I_{ij}}{\partial \tilde{r}_{ij}} = \tau \, w_{ij} > 0, \tag{7}$$

which is always larger than zero, leading to the first prediction.

Proposition 1. *On average, defendants with higher observed recidivism risk are more likely to be incarcerated.*

Since the weight on the signal increases with its precision, the response to the signal also

increases with social proximity:

$$\frac{\partial^2 I_{ij}}{\partial \tilde{r}_{ij}\, \partial s_{ij}} = \tau \frac{\rho_s}{\left(1+\rho_{ij}\right)^2} \geq 0, \tag{8}$$

with strict inequality whenever $\rho_s > 0$.

Proposition 2. *If judges have more precise information about socially closer defendants, the incarceration decision responds more strongly to recidivism risk for socially closer defendants.*

Finally, the effect of social proximity on the incarceration decision is

$$\frac{\partial I_{ij}}{\partial s_{ij}} = -\phi + \tau \frac{\rho_s}{\left(1+\rho_{ij}\right)^2} \left(\tilde{r}_{ij} - \mu_i\right). \tag{9}$$

The second term is the information channel: a more precise signal moves the judge's beliefs away from the prior, raising the probability of incarceration for defendants with a signal above the prior mean and lowering it for those below. This implies that in-group bias can arise from differences in information alone. If socially closer defendants tend to have a lower risk than the judge's prior suggests, and judges receive a more precise signal about them, judges will on average treat them more leniently even if they have no preference for them ($\phi = 0$). In that case, a lower incarceration rate of socially closer defendants reflects statistical rather than (or in addition to) taste-based discrimination. Comparing defendants with the same level of risk allows us to separate the two channels.

Proposition 3. *At the mean level of recidivism risk, socially closer defendants are less likely to be incarcerated if and only if judges prefer to release socially closer defendants ($\phi > 0$).*

Empirical Test. We test these predictions by interacting social proximity with the predicted recidivism risk R_i from Section 2, standardized to have a mean of zero and a standard deviation of one:

$$y_{ij} = \beta\, s_{ij} + \gamma\, R_i + \delta\, s_{ij} \times R_i + \mathbf{x}_i' \theta_x + \mathbf{x}_i' \mathbf{w}_j' \theta_{xw} + \alpha_{ct} + \alpha_i^s + \alpha_j + \varepsilon_{ijct}, \tag{10}$$

where α_j are judge fixed effects, which absorb judges' harshness κ_j, α_{ct} are county-year fixed effects, α_i^s are charge severity fixed effects, and $\mathbf{x}_i$ and $\mathbf{x}_i'\mathbf{w}_j'$ includes the controls of the main specification, which absorb differences in the prior mean μ_i across groups of defendants. Proposition 1 implies $\gamma > 0$, and Proposition 2 implies $\delta > 0$ if $\rho_s > 0$. Since R_i is standardized, β measures the effect of social proximity at the mean level of risk, and by Proposition 3 $\beta < 0$ indicates taste-based preferences for socially closer defendants. Since we do not observe the recidivism risk signal observed by the judge we use the predicted risk as a proxy for the information on risk available to the judge.

Table 6: Social Proximity and Recidivism Risk

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.105**	-0.083**	-0.036**	-0.191**	-0.240**	-0.073*
	(0.022)	(0.008)	(0.006)	(0.035)	(0.032)	(0.031)
Recid. Score (std.)	0.039**	0.037**	0.054**	0.093**	0.095**	0.176**
	(0.003)	(0.003)	(0.003)	(0.011)	(0.011)	(0.011)
Social Proximity × Recid. Score (std.)	0.024**	0.030**	0.027**	0.001	-0.006	-0.004
	(0.009)	(0.007)	(0.006)	(0.025)	(0.025)	(0.024)
Observations	1,230,916	1,230,888	1,230,832	458,464	458,434	458,412
R^2	0.086	0.097	0.120	0.489	0.497	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Judge FE		X	X		X	X
Other Controls			X			X

Notes. Effect of social proximity, defendants' recidivism risk and their interaction on judges' incarceration decision (Columns 1–3) and on the length of the jail sentence (Columns 4–6). Social proximity is computed from last-name embeddings after removing the first five principal components. The recidivism risk score is standardized to have mean zero and standard deviation one. All columns include county-year and charge severity fixed effects. Columns 2–3 and 5–6 add judge fixed effects, and Columns 3 and 6 add controls for judges' and defendants' race, gender, and age and their interactions, and for defendants' ethnicity. Jail time is estimated on incarcerated defendants only. Standard errors (in parentheses) are clustered at the county-year level: + p < 0.1, * p < 0.05, ** p < 0.01.

5 Social Proximity and Recidivism Risk

This section takes the predictions of Section 4 to the data. We start by estimating how the effect of social proximity varies with defendants' predicted recidivism risk, which separates the taste-based and the information channels.

Table 6 reports the estimates of Equation (10). In line with Proposition 1, judges are more likely to incarcerate defendants with a higher predicted risk of re-offending. Specifically, a one standard deviation increase in the risk score raises the probability of incarceration by 5.4 percentage points. The coefficient on social proximity measures its effect at the mean level of risk. It remains negative and significant in all specifications and, after we include judge fixed effects and controls, it is identical to the baseline estimate in Table 4. In line with Proposition 3, this is consistent with taste-based preferences of judges for socially closer defendants, independently of what they know about their risk.

To provide additional evidence on the taste-based component, we look at the effect of social proximity separately for offenses with and without a victim. In Appendix Section G, we extend the model to include a cost to the victim when a defendant is released, which judges weigh more for higher social proximity victims. The model predicts that if judges prefer socially closer defendants, this preference should be most visible when a lenient decision imposes no direct cost on

Table 7: Social Proximity in Offenses With and Without a Victim

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Offenses Without a Victim						
Social Proximity	-0.179**	-0.071**	-0.068**	-0.371**	-0.140**	-0.138**
	(0.011)	(0.008)	(0.009)	(0.038)	(0.034)	(0.034)
Recid. Score (std.)			0.019**			0.104**
			(0.004)			(0.012)
Social Proximity × Recid. Score (std.)			0.059**			0.021
			(0.008)			(0.028)
Observations	695,866	695,845	695,845	288,419	288,408	288,408
R^2	0.104	0.125	0.124	0.433	0.461	0.461
Mean Dep. Var.	0.419	0.419	0.419	4.389	4.389	4.389
Panel B. Offenses With a Victim						
Social Proximity	0.048**	0.032**	0.030**	0.056	0.071	0.089
	(0.011)	(0.011)	(0.011)	(0.064)	(0.065)	(0.068)
Recid. Score (std.)			0.094**			0.335**
			(0.004)			(0.020)
Social Proximity × Recid. Score (std.)			-0.019*			-0.118*
			(0.010)			(0.050)
Observations	308,750	308,732	308,732	85,526	85,521	85,521
R^2	0.121	0.151	0.151	0.633	0.643	0.642
Mean Dep. Var.	0.281	0.281	0.281	5.518	5.518	5.518
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity in the sample of offenses without a victim (Panel A) and with a victim (Panel B), classified by the statute of the charge. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

an identifiable victim. Consistent with this, Panel A of Table 7 shows the effect of social proximity is concentrated in victimless offenses, where an increase by 1.3 standard deviations in social proximity reduces the probability of incarceration by about 0.8 percentage points. Conversely, in offenses that involve a victim, socially closer defendants are more likely to be incarcerated (Panel B). This reversal is consistent with judges' preferences extending to victims, who tend to share the background of the offenders who harm them.[24]

Moving to the interaction between social proximity and risk, Columns 1–3 of Table 6 show that the coefficient is positive and significant across all specifications. As predicted by Proposition 2, judges respond more to recidivism risk when handling cases with socially closer defendants, consistent with judges having more precise information about these defendants. In particular, for a defendant one standard deviation below the mean risk, the interquartile increase in social proximity reduces the probability of incarceration by 0.77 percentage points, while for a defendant one standard deviation above the mean the effect falls to 0.11 percentage points. The two forces offset each other at about 1.3 standard deviations above the mean risk.

Taken together, the results suggest that both channels are at work. Judges favor socially closer defendants, but they also seem to read their risk more accurately, and are more lenient with socially closer defendants when they are low-risk. For sentence length, the pattern is different. As for incarceration decisions, higher-risk defendants receive longer sentences, and socially closer defendants receive shorter ones at the mean level of risk, but the length of the sentence does not respond to risk differently depending on social proximity.

We also investigate whether more experienced judges, who are likely to receive more precise signals about all defendants, respond differently to risk and to social proximity. Appendix Table E.21 interacts social proximity and risk with an indicator for judges whose tenure is above the median of their county-year. Experienced judges do not respond more to recidivism risk in general, as the interaction between experience and risk is small and not significant. However, the stronger response to risk among socially closer defendants is larger for experienced judges (Column 2), although the difference is only marginally significant. For sentence length, experienced judges also assign longer sentences to high-risk defendants who are socially close to them (Column 4), suggesting that experience amplifies the advantage in reading the risk of socially closer defendants.

Robustness. We next show that these results do not depend on how we construct the recidivism risk score. First, Appendix Table F.34 shows that the results are robust to predicting risk with a logistic regression on the same features instead of using XGBoost.

A second concern is that we observe recidivism only for defendants who are released or receive short sentences, leading to the selective labeling problem described by Kleinberg et al. (2018a).

[24]We provide evidence consistent with this by looking at social proximity between defendants and plaintiffs in restraining-order cases in the sample of civil cases. Appendix Figure E.20 shows that the two parties have higher average social proximity than random pairs and share the same last name in 19% of cases. Consistent with this, evidence on homicides in the United States shows in 2019 that between 70 and 90% of victims were killed by an offender of the same race or ethnicity (Federal Bureau of Investigation, 2020), and offenders mostly target areas close to where they live and similar to their own in ethnic composition (Bernasco and Block, 2009).

Following their approach, we train the score on the cases decided by the most lenient judges, who release a broader set of defendants, so that recidivism is observed for a sample closer to the full population of defendants. We use two leniency definitions. On the one hand, the third of judges with the highest share of sentences shorter than 180 days (Appendix Table F.35), and on the other, the two most lenient judges in each county-year (Appendix Table F.36). In both cases, the coefficient on social proximity and the interaction with risk are very close to the main estimates.

Additionally, it could be that the introduction of automated risk assessment affected the ability of judges to assess the riskiness of more similar defendants. Wisconsin courts started to use the COMPAS risk assessment tool in 2012. Therefore, we restrict the sample to cases filed before 2012 in Appendix Table F.37. Reassuringly, the table shows that the results are robust to this sample restriction. Appendix Table F.38 also replaces our risk score with one trained on first offenders only. Since these defendants have no criminal record, this score relies only on their age, sex, and the type of the current case, leading its effect on incarceration to be close to zero. In line with this, the interaction with social proximity is also small and not significant, while the main coefficient on social proximity is unaffected.

Finally, our score combines information on defendants' criminal history and on their demographic characteristics. If judges assess the risk of socially closer defendants more accurately, we expect this to apply mostly to the information that is relevant for recidivism and that judges must interpret, such as the defendant's record, rather than to characteristics like age or the neighborhood where the defendant lives. Appendix Tables F.39 and F.40 split the score into its two components. The score predicted from criminal history only has a higher correlation with incarceration and sentence length than the one predicted from demographics only. Similarly, judges respond more to the former risk score when assessing socially closer defendants. The stronger response to risk among socially closer defendants is therefore concentrated in the dimension of risk that comes from criminal history, consistent with judges reading the record of socially closer defendants more accurately.

6 Conclusion

Social divides beyond race and gender are harder to observe or record in administrative data, and therefore remain less studied. In this paper, we construct a continuous measure of social proximity between defendants and judges using last name embeddings and investigate whether judges treat defendants who are socially closer to them differently. Exploiting the quasi-random assignment of cases to judges in more than 1.2 million criminal cases, we find that judges are less likely to incarcerate defendants who are socially closer to them. The effect is specific to the judge deciding the case, not a general characteristic of defendants who are close to judges, and is most closely related to the social proximity of the neighborhoods where judges and defendants live, although names capture more of a shared background than neighborhoods alone.

To investigate why judges are more lenient toward socially closer defendants, we develop a

model in which judges can have taste-based preferences for more similar defendants and observe a noisy signal of defendants' recidivism risk with precision increasing with social proximity. Using a machine-learning predicted measure of recidivism risk, we find evidence of an information channel and results consistent with taste-based preferences. Judges' decisions respond more strongly to the risk of socially closer defendants, and socially closer defendants are less likely to be incarcerated, holding risk constant, which is consistent with taste-based preferences. These preferences, however, do not always favor the defendant, as judges are more lenient toward socially closer defendants only in victimless offenses, but harsher in offenses that involve a victim.

These findings have implications for how bias in the courtroom is monitored and addressed. Judicial conduct rules already prohibit judges from allowing social relationships or specific attributes, including race and socio-economic status, to influence their decisions. Yet the monitoring and training of judges focus on the few protected attributes that are recorded in the data and, for instance, cover racial and ethnic fairness (Casey et al., 2012). Our results show that judges' decisions also depend on dimensions of social proximity that these tools do not capture.

Our measure requires only individuals' names and can therefore be applied to many administrative datasets that record names but not other social-background information. Future work could use our measure to study social proximity between decision makers and the individuals they evaluate in other contexts, such as managers and workers in hiring and promotions, loan officers and borrowers, teachers and students, or police officers and civilians. It could also measure the diversity and integration of workplaces, schools, and neighborhoods continuously, rather than through shares of predefined groups. Finally, since victims are not recorded in our data, our evidence on the role of victims is indirect, and future work could explore more directly how judges' proximity to victims shapes their decisions.

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A WCCA Court Records

A.1 Data Limitations

Following our agreement with the WCCA, we acknowledge the following data limitations:

- WCCA Information includes only court records open to public view under Wisconsin's Open Records Law, Wis. Stat. §§19.31-19.39.
- WCCA Information does not comprise the complete court record. For instance, it does not include information that may be confidential, sealed, or redacted in accordance with all applicable statutes, court orders, and rules related to confidentiality, sealing, and redaction.
- WCCA Information is not the Judgment and Lien Docket under Wis. Stat. §806.10. The Judgment and Lien Docket is available from the Clerk of Circuit Court.
- In criminal cases, any designation in any race field contains subjective information generally provided by the agency that filed the case.

A.2 Sample and Outcome Definition

WCCA records the disposition of each charge, including dismissed charges and acquittals. In the final sample, we exclude cases in which all charges are dismissed, or the defendant is found not guilty at trial, as well as cases with no sentence recorded, most of which involve dismissed charges read in for sentencing in another case and deferred prosecutions. Between 42% and 62% of cases each year have at least one dismissed charge, and remain in our sample if the defendant is convicted on at least one charge.

The disposition codes distinguish convictions resulting from a guilty, no contest, or Alford plea from convictions at a jury or court trial. Among cases filed from 2007 onward, 99% of convictions in our sample result from a plea and about 1% from a trial. In 2005 and 2006, the records use a single code for guilty and no contest dispositions that does not allow us to separate pleas from trials. In general, the records do not systematically report whether a plea was negotiated or its terms, and explicit references to a plea agreement appear in the court-record events of only 7% of cases.

We record a defendant as incarcerated if sentenced to state prison, local jail, or a house of correction on at least one charge. A custodial term imposed on a charge together with probation is not counted as incarceration ($\sim$ 20% of cases in the sample).

B Summary Statistics

B.1 Tables

Table B.1: Summary Tabulations: Misdemeanors

	Cases (1)	Share (2)
Disorderly Conduct	79,920	0.153
Battery	75,369	0.144
Resisting Officer	71,154	0.136
Bail Jumping	52,783	0.101
Drug Possession	45,335	0.087
Theft	32,011	0.061
Retail Theft (Shoplifting)	29,991	0.057
Criminal Damage	29,233	0.056
Drug Paraphernalia	14,562	0.028
Operating While Intoxicated	11,329	0.022
Weapons/Explosives	11,309	0.022
Other Misdemeanor	10,938	0.021
Worthless Checks	9,930	0.019
OAR/OAS	9,756	0.019
Violation of TRO	5,869	0.011
Other	33,021	0.063
Total	522,510	1.000

Notes. Number and share of misdemeanor cases by offense category, for the fifteen most frequent categories; *Other* pools the remaining categories. The offense category is the one recorded for the case in the court records and can differ from the case type. Cases filed between 2005 and 2021 in the estimation sample.

Table B.2: Summary Tabulations: Felonies

	Cases (1)	Share (2)
Drug Possession	76,351	0.178
Bail Jumping	56,755	0.132
Theft	29,385	0.068
Operating While Intoxicated	28,534	0.066
Burglary	28,016	0.065
Drug Manufacture/Deliver	26,011	0.061
Other Felony	20,483	0.048
Battery	16,570	0.039
Other Bodily Security	12,397	0.029
Other Public Safety Crimes	12,299	0.029
Forgery	11,567	0.027
Substantial/Aggravated Battery	11,083	0.026
Weapons/Explosives	9,911	0.023
Other Crimes Against Children	9,503	0.022
Operate Vehicle w/out Consent	8,506	0.020
Other	71,992	0.168
Total	429,363	1.000

Notes. Number and share of felony cases by offense category, for the fifteen most frequent categories; *Other* pools the remaining categories. The offense category is the one recorded for the case in the court records and can differ from the case type. Cases filed between 2005 and 2021 in the estimation sample.

Table B.3: Summary Tabulations: Criminal Traffic

	Cases (1)	Share (2)
Operating While Intoxicated	125,743	0.451
OAR/OAS	102,721	0.368
Operate Without License	29,315	0.105
Hit and Run	5,914	0.021
Other Misdemeanor	5,758	0.021
Bail Jumping	3,376	0.012
Unidentified Misdemeanor Traffic	2,047	0.007
Resisting Officer	1,374	0.005
Drug Possession	801	0.003
Disorderly Conduct	592	0.002
BAC	498	0.002
Drug Paraphernalia	381	0.001
Reckless Driving	209	0.001
Weapons/Explosives	115	0.000
Criminal Damage	57	0.000
Other	142	0.001
Total	279,043	1.000

Notes. Number and share of criminal traffic cases by offense category, for the fifteen most frequent categories; *Other* pools the remaining categories. The offense category is the one recorded for the case in the court records and can differ from the case type. Cases filed between 2005 and 2021 in the estimation sample.

Table B.4: Summary Tabulations: Charge Severity

	Cases (1)	Share (2)
Felony A	155	0.000
Felony B	1,619	0.001
Felony BC	67	0.000
Felony C	12,456	0.010
Felony D	5,904	0.005
Felony E	14,055	0.011
Felony F	37,619	0.031
Felony G	33,341	0.027
Felony H	133,892	0.109
Felony I	84,589	0.069
Felony U	3,093	0.003
Misdemeanor A	384,180	0.312
Misdemeanor B	115,774	0.094
Misdemeanor C	528	0.000
Misdemeanor U	403,644	0.328
Total	1,230,916	1.000

Notes. Number and share of cases by the severity class of the most severe charge in the case, from the most to the least severe. Classes marked U are felonies and misdemeanors whose class is not recorded. Cases filed between 2005 and 2021 in the estimation sample.

Table B.5: Summary Statistics on Judge Characteristics

	Mean		Difference
	High Social Prox. Judge (1)	Low Social Prox. Judge (2)	High − Low (3)
Black judge	0.028	0.037	-0.009
Female judge	0.189	0.207	-0.018
Years of Experience	22.829	23.410	-0.582
Political contributions (CF score)	0.127	0.141	-0.014
Incarceration rate	0.349	0.357	-0.008
Leniency tertile (1 harsh, 3 lenient)	2.045	2.011	0.034
Cases per judge	1,756	2,023	-267^{+}
Number of judges	323	328	

Notes: Summary statistics of judges in the sample calculated using judges as units of observation. *Years of Experience* indicates the years of judicial activity of judges, and *Political Contributions* indicates a political contributions score (where positive values indicate contributions by Republicans). Judges are split at the sample mean of their mean social proximity to the defendants they see, where social proximity is the last-name similarity after removing the first five principal components. Column 3 reports the difference in means with stars from a two-sample t-test with unequal variances: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table B.6: Matching Court Defendants to Registered Voters

	Number (1)	Share (2)
Panel A: Records		
Registered voters	6,500,788	
Court defendants	6,674,178	
Criminal cases	809,678	0.121
Civil cases	5,864,500	0.879
Panel B: Defendants matched to a voter		
Criminal cases (name and date of birth)	78,252	0.097
Civil cases (name and address)	662,797	0.113
Panel C: Voters matched to a defendant		
Any defendant	730,227	0.112
Criminal defendant	80,437	0.012
Match shared with another voter	4,636	0.001
Civil defendant	674,066	0.104

Notes. Counts of records in the voter file and in the court records, and of the matches between them. *Registered voters* are the distinct voters that appear in at least one vintage of the Wisconsin voter file between February 2019 and October 2025, including inactive registrations; the October 2025 vintage includes 5.0 million voters, 3.4 million of them active. *Court defendants* are the distinct defendants in the Wisconsin circuit court records, identified by first name, last name, and date of birth for criminal cases, and by first name, last name, and residential address for civil cases. In Panel A, shares are out of all court defendants. In Panel B, shares are out of the defendants of each type. In Panel C, shares are out of all registered voters.

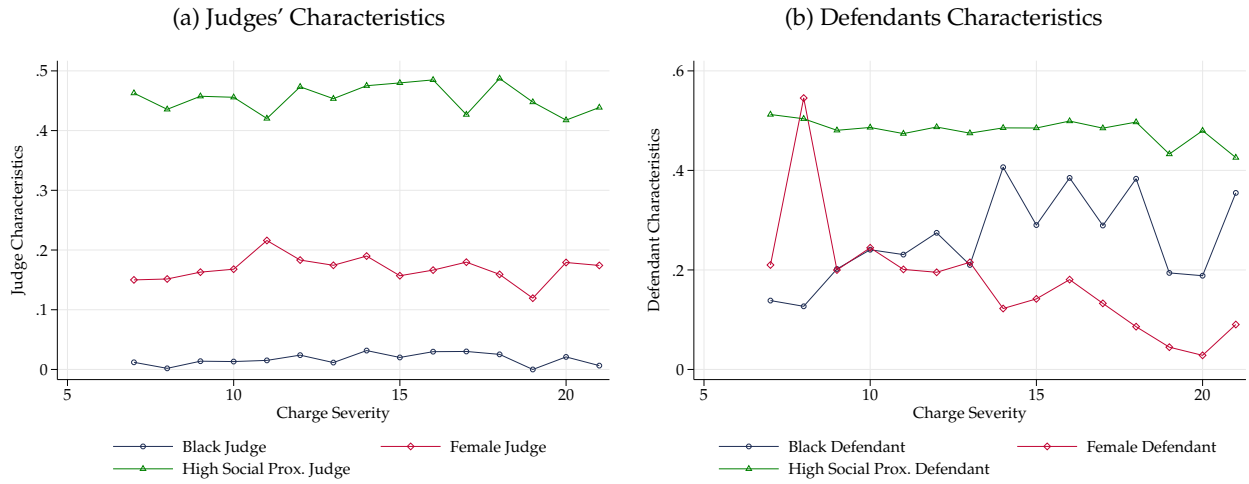
Table B.7: Defendant and Case Characteristics by Social Proximity

	Mean		Difference in Means	
	Low Social Prox. Defendant	High Social Prox. Defendant	Without FE	With FE
	(1)	(2)	(3)	(4)
Charge Severity	9.899	9.793	-0.106**	-0.096**
	(2.624)	(2.637)	(0.005)	(0.012)
Recid. Score	0.001	-0.001	-0.002**	-0.001**
	(0.170)	(0.169)	(0.000)	(0.000)
Black Defendant	0.205	0.219	0.014**	0.013**
	(0.404)	(0.413)	(0.001)	(0.001)
Female Defendant	0.216	0.209	-0.007**	-0.005**
	(0.412)	(0.407)	(0.001)	(0.001)
Defendant Age	31.869	31.657	-0.212**	-0.256**
	(11.215)	(11.052)	(0.020)	(0.030)
First Offense	0.317	0.334	0.017**	0.009**
	(0.465)	(0.472)	(0.001)	(0.001)
Felony	0.356	0.342	-0.014**	-0.002**
	(0.479)	(0.474)	(0.001)	(0.001)
Misdemeanor	0.432	0.417	-0.015**	-0.006**
	(0.495)	(0.493)	(0.001)	(0.001)
Criminal Traffic	0.213	0.241	0.028**	0.009**
	(0.409)	(0.428)	(0.001)	(0.001)
Zip: Share Black	0.092	0.102	0.009**	0.005**
	(0.193)	(0.203)	(0.000)	(0.000)
Zip: Share Male	0.499	0.498	-0.001**	-0.000
	(0.033)	(0.033)	(0.000)	(0.000)
Zip: Share Urban	0.566	0.577	0.011**	0.005**
	(0.460)	(0.461)	(0.001)	(0.001)
Zip: Share College	0.230	0.226	-0.004**	-0.004**
	(0.110)	(0.107)	(0.000)	(0.000)
Zip: Share Food Stamps	0.121	0.126	0.006**	0.004**
	(0.075)	(0.079)	(0.000)	(0.000)
Zip: Log Median Income	10.765	10.753	-0.012**	-0.014**
	(0.265)	(0.270)	(0.000)	(0.001)
Black Judge	0.017	0.012	-0.004**	-0.004^{+}
	(0.128)	(0.111)	(0.000)	(0.002)
Female Judge	0.165	0.162	-0.003**	-0.002
	(0.371)	(0.369)	(0.001)	(0.003)
Observations	623,714	607,202	1,230,916	1,230,916

Notes: Case and defendant characteristics across defendants whose mean social proximity to the judges they face is above and below the sample mean, where social proximity is the last-name similarity after removing the first five principal components. Columns 1 and 2 report group means with standard deviations in parentheses; Column 3 the raw difference in means and Column 4 the difference after county-year and charge severity fixed effects, with standard errors clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$. In the charge severity row, Column 4 absorbs county-year effects only. This table is descriptive: similarity to the assigned judge varies within defendant as a result of assignment, so the differences reported here are not a test of random assignment, which is Table 3 in the main text.

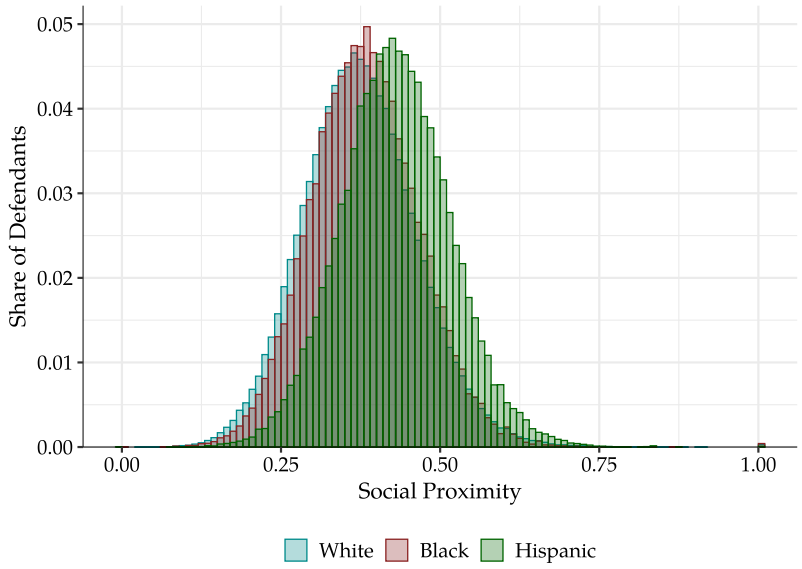
B.2 Figures

Figure B.1: Judges/Defendants' Characteristics Across Charge Severity Risk



Notes: Average share of female and Black judges/defendants, and of judges/defendants with above-average social proximity, plotted by charge severity. Panel (a) shows judges and Panel (b) defendants. Social proximity is the last-name similarity after removing the first five principal components; a judge (defendant) counts as high-proximity when their mean social proximity to the defendants they see (the judges they face) is above the sample mean.

Figure B.2: Distribution of Social Proximity by Defendant Ethnicity



Notes. Distribution of judge–defendant social proximity, the last-name similarity after removing the first five principal components, separately for White, Black, and Hispanic defendants.

C Recidivism Risk Prediction

C.1 Target

The target of the prediction model is a binary variable taking value 1 if the defendant commits a new offense within two years of the judgment date and 0 otherwise (see Figure C.3). We date a new offense by the offense date of a charge in a later case, and count it only if that case is closed and not entirely dismissed or acquitted. One issue we had to deal with when computing this variable was how to decide which cases to include, because whether we observe recidivism or not is affected by the decisions of judges. This is is not trivial because defendants serve different sentence lengths. To see this, consider three defendants who stay in jail for one year, for 22 months, and for 2 years after judgment. The first defendant has 1 year left to re-offend in the follow-up period, the second has only 2 months, and for the third, we can not observe recidivism in the follow-up period of 2 years at all. Yet extending the follow-up period for two years after the assigned sentence period instead of the judgment date is also problematic because defendants often serve more or less than the assigned sentence. Since there is not a comparable data source that has the exact jail record of every defendant in Wisconsin, we don't observe the actual length of the sentence served. Moreover, the sentence itself could affect the probability of recidivism. Further, the defendants who receive sentences are a selected group, so there is the issue of selective labeling (Lakkaraju et al., 2017).

There is no consensus in the literature about how to deal with this problem. We use a cutoff for sentence length of 180 days, such that we don't throw away a lot of useful data and still leave enough time in the follow-up period for the defendant to reveal crime potential. Cases with longer sentences enter the training sample only if a new offense is observed within the follow-up period; otherwise their outcome is treated as missing.

C.2 XGBoost Implementation and Performance

The XGBoost algorithm is an ensemble method that produces forests of decision trees, a state-of-the-art binary classification model for tabular datasets is gradient boosted trees (Friedman, 2001; Hastie et al., 2009).[25] Gradient boosting models consist of an ensemble of decision trees that "vote" on the predicted outcome. Each decision tree iteratively selects informative variables (in our case, e.g., number of previous arrests), splits on a value of that variable (e.g., $x > 2$) to better predict the outcome, branches off for additional splitting, and so on, until reaching a terminal node and an associated prediction ($\hat{Y} = 0$ or $\hat{Y} = 1$). With gradient boosting, additional layers of trees are gradually added during the training process to fit residuals and fix errors in the initial layers. A number of hyperparameters, such as the number of trees, and their depth, can be selected to calibrate the level of regularization.

A popular implementation of gradient boosting is XGBoost ("eXtreme Gradient Boosting"; Chen and Guestrin 2016). Besides being optimized for fast training, XGBoost has a number

[25] For non-tabular data, such as images, text, or audio, neural nets are often preferred (Goodfellow et al., 2016).

of computational adjustments to improve out-of-sample fit. Feurer et al. (2018) systematically compared XGBoost to many other classifiers, including a sophisticated automated ML system, and found that XGBoost consistently performed best on classification tasks with tabular data (see also Grinsztajn et al., 2022). Hence, we take XGBoost as our preferred model for predicting recidivism. In a robustness check, we replicate our results using a Logit model to predict the recidivism risk score, and show that the results are robust to this change in prediction model.

C.3 Predicting Recidivism Risk

We compute the recidivism risk score by cross-prediction. First, we randomly split the sample in two halves at the defendant level, so that the same defendant never appears in both halves. Within each half, we train the model on the cases with an observed target, using gender, type of offense, prior criminal count (separately by type), and age at judgment and at first offense as features, and select the hyperparameters by randomized search with 5-fold cross-validation. We then predict recidivism risk for every case in each half with the model trained on the other half, so that each defendant's score is out of sample. We evaluate the classifier on these out-of-sample predictions.

C.4 Tables

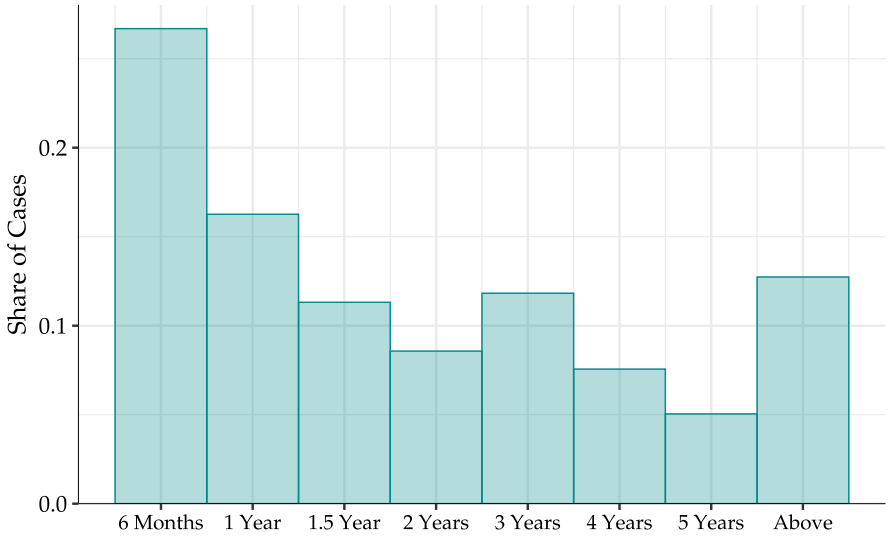
Table C.8: Performance of Classifier

	All (1)	Caucasian (2)	African American (3)
Accuracy	0.659	0.665	0.645
AUC	0.703	0.704	0.700
FPR	0.220	0.213	0.237
FNR	0.509	0.516	0.496
Observations	1,542,972	1,020,149	328,005

Notes. Performance of the XGBoost recidivism classifier on out-of-sample predictions, where the prediction for each defendant comes from the model trained on the other half of the sample. Computed on all cases used to train the model, overall (Column 1) and by defendant race (Columns 2 and 3). Predicted classes use a threshold of 0.5. FPR is the false positive rate and FNR the false negative rate.

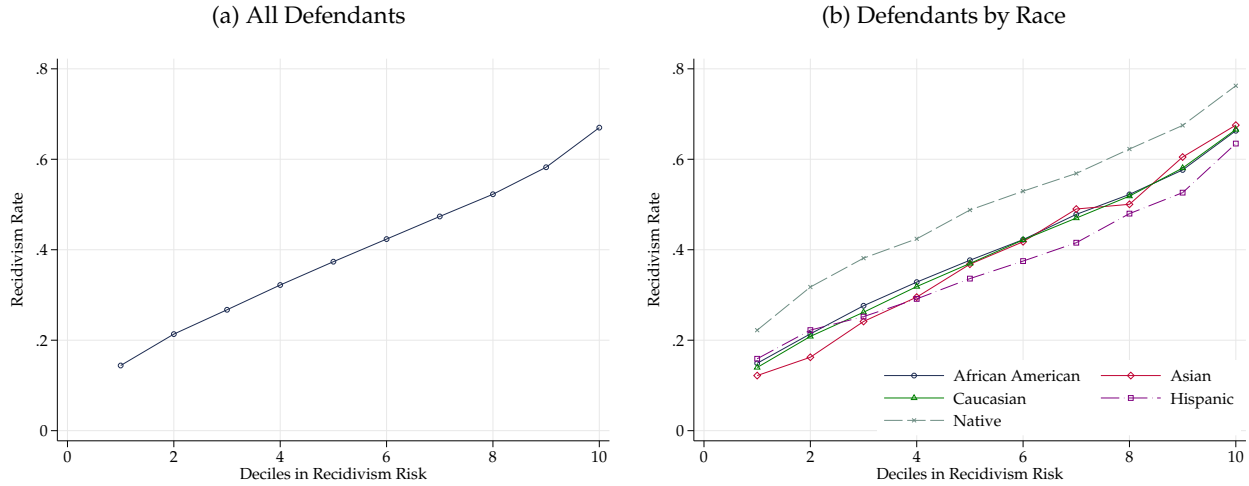
C.5 Figures

Figure C.3: Share of Defendants/Cases by Intervals between Offenses



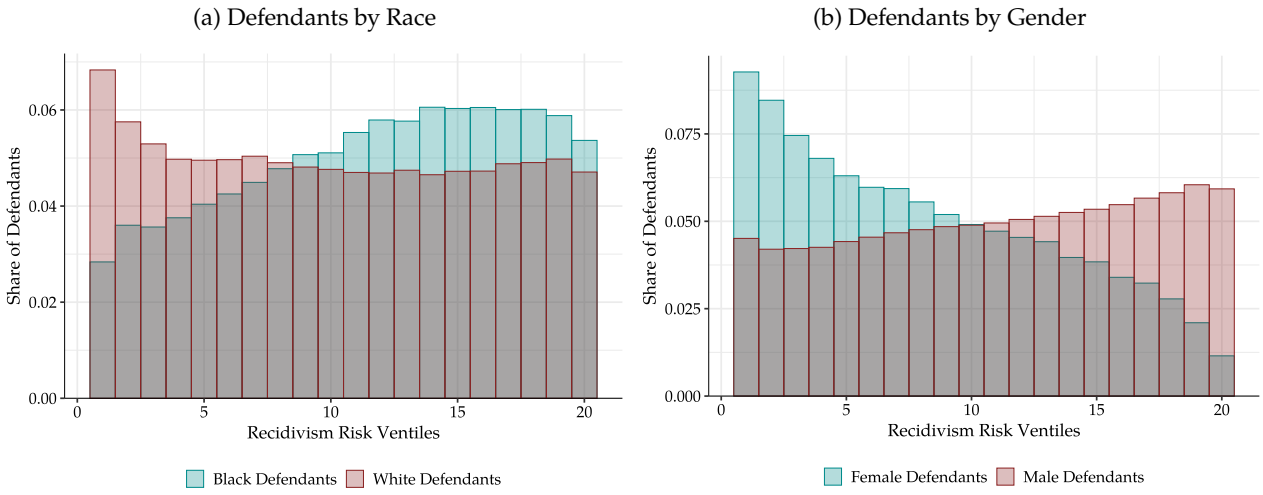
Notes: Share of defendant/cases with recidivism episodes by time intervals between current and subsequent offense.

Figure C.4: Recidivism Rate by Risk Decile in Held-Out Test Set



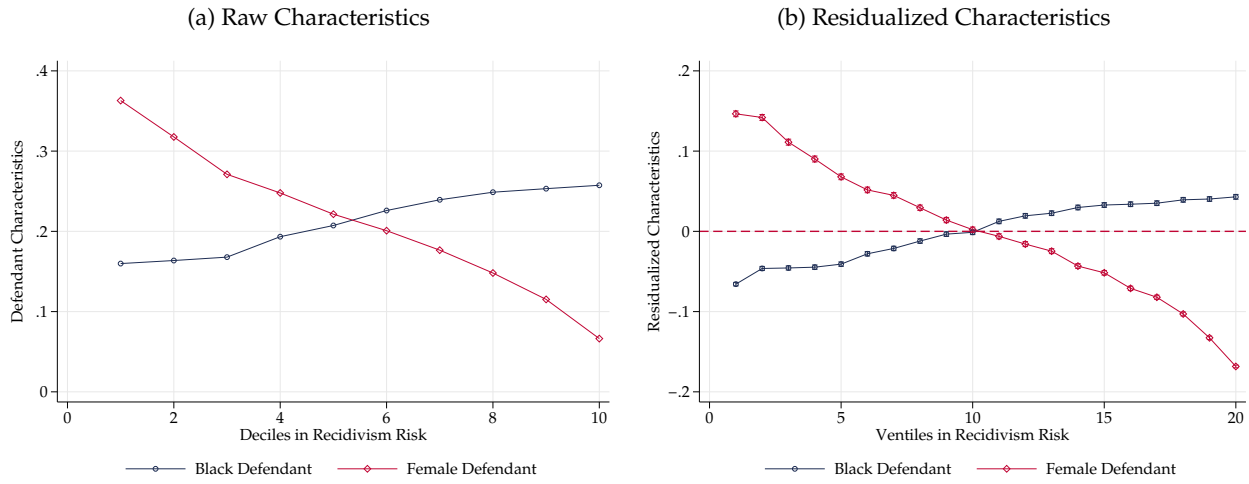
Notes: Average share of defendants who re-offend within a two-year period, plotted by recidivism risk deciles. Recidivism risk deciles are centered by county-year and charge severity. Panel (a) plots all defendants together, while panel (b) plots defendants separately by race.

Figure C.5: Distribution of Defendants by Recidivism Risk



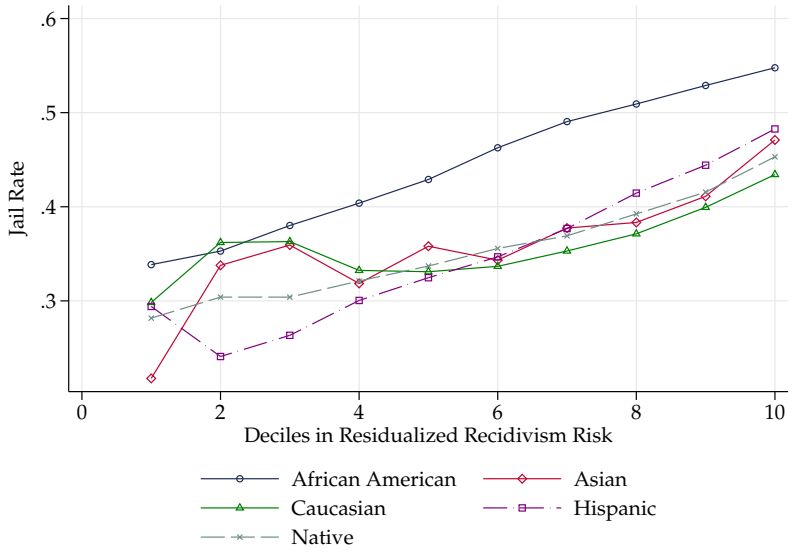
Notes: Share of defendants plotted by recidivism risk ventiles. Panel (a) shows the distribution of defendants separately for Black and White defendants, while Panel (b) shows the distribution of defendants separately by gender.

Figure C.6: Defendant Characteristics Across Recidivism Risk



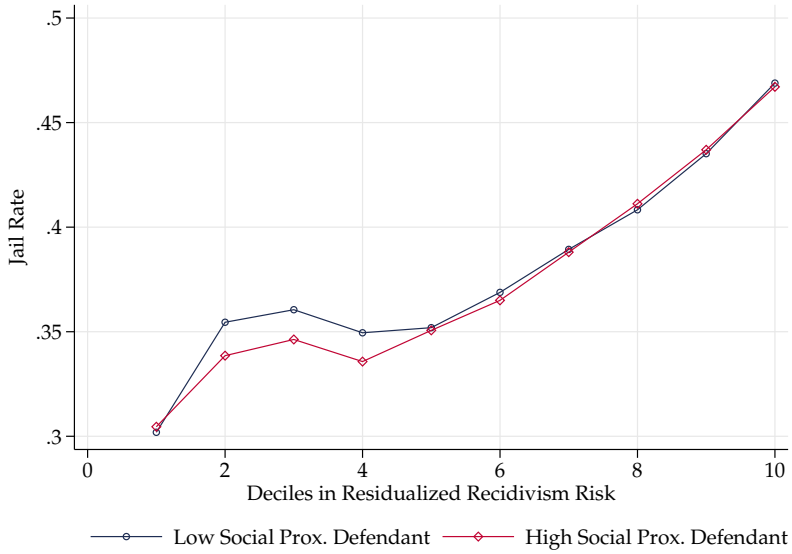
Notes. Panel (a) shows the share of female and Black defendants by recidivism risk decile. Recidivism risk deciles are computed across all ethnicities and are centered by county-year and charge severity. Panel (b) shows the residuals from regressions of the same characteristics on county-year and charge severity fixed effects by recidivism risk ventile, with 95% confidence intervals.

Figure C.7: Incarceration Rate by Recidivism Risk and Race



Notes: Average share of incarcerated defendants plotted by recidivism risk deciles and separately by ethnicity. Recidivism risk deciles are computed across all ethnicities and are centered on court and year and charge severity.

Figure C.8: Incarceration Rate by Recidivism Risk and Social Proximity



Notes: Average share of incarcerated defendants plotted by recidivism risk deciles separately for cases with social proximity above and below the sample mean, where social proximity is the last-name similarity after removing the first five principal components. Recidivism risk deciles are computed across all defendants and are centered by court-year and charge severity.

D Social-Proximity Construction

D.1 Name Embeddings

Table D.9 reports the size of the corpus we use to train the name embeddings. We group registered voters and court defendants by the street and zip code of their residential address, for a total of 604,952 documents and around 11.5 million name records. A name record is a name at a given street and zip code, so a person who appears at more than one address is counted once for each address. We train three models on this corpus, representing each person with their full name (Column 1), first name (Column 2), or last name (Column 3). Before training each model, we drop the documents that are identical to another document. Almost all of these are streets with a single resident, which coincide whenever the two residents share a name. This is common for first and last names and rare for full names. As a result, the last-name model is trained on 339,546 documents and the first-name model on 288,659, compared to 571,754 for the full-name model. Since a document with a single name provides no context to learn from, this step removes less than 3% of the name records. Finally, the last row of the table reports the number of distinct names in each model.

We train the embeddings with FastText (Bojanowski et al., 2017), which represents each name as the sum of the vectors of its character n-grams, of length between 3 and 6. Each name enters the model as a single token, so that in the full-name model the token is the whole name of a person. We set the parameters of the model as follows. First, we use the skip-gram architecture (Mikolov et al., 2013), which predicts the context of a name from the name itself and is commonly preferred when many words are infrequent. This matters in our setting because most names are rare: the corpus contains 7.9 million distinct full names out of 11.5 million name records. Second, for the same reason, we keep all names regardless of how often they appear. More than half of the documents contain a single name, and a minimum frequency threshold would remove a large part of the corpus. Third, we use a context window of 10 names. Since the residents of a street have no natural order, we sort the names within each document at random, and the window determines how many other residents of the same street enter the context of a name. Figure D.16 reports the distribution of the number of names per document. Most documents are short: 53% contain a single name, 81% at most 10 names, and 87% at most 20. However, the remaining documents account for 88% of the name records, and some of them may correspond to institutions like prisons or detention centers and not to residential streets. A larger window gives more weight to these long documents. For this reason, we also let the size of the window vary at random between 1 and 10 for each name, which gives less weight to names that are far apart in a document and therefore to long documents. Appendix Tables F.31, F.32, and F.33 report the results using a context window of 3, 5, and 13 names. Finally, the vectors have 200 dimensions, and we train each model for 5 iterations over the corpus with a fixed random seed.

D.2 Tables

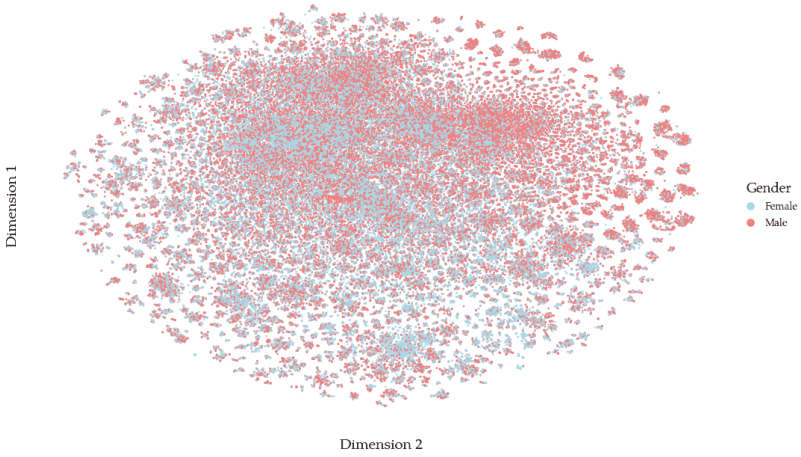
Table D.9: Training Corpus for the Name Embeddings

	Full Name (1)	First Name (2)	Last Name (3)
Street-zip documents	604,952	604,952	604,952
Name records	11,535,081	11,535,081	11,535,081
Documents in training	571,754	288,659	339,546
Name records in training	11,501,303	11,200,254	11,253,947
Distinct names	7,900,937	234,567	523,089

Notes. Number of documents and names in the corpus used to train the name embeddings. Column 1 reports the counts for the model trained on full names, Column 2 for the model trained on first names, and Column 3 for the model trained on last names.

D.3 Figures

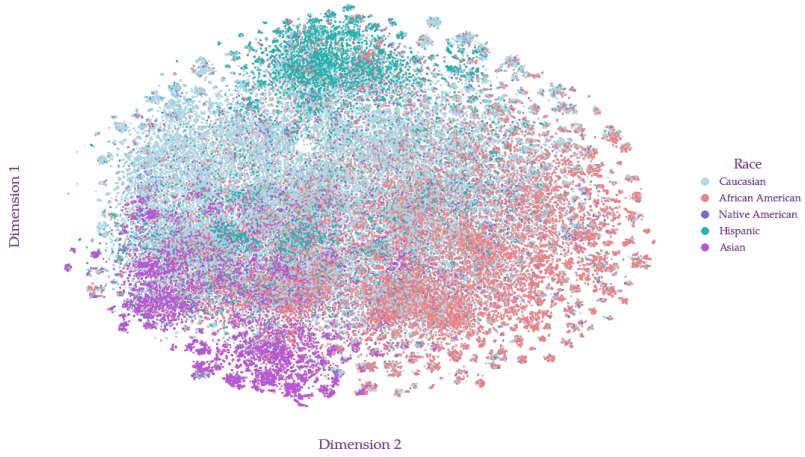
Figure D.9: Last-Name Embeddings and Gender



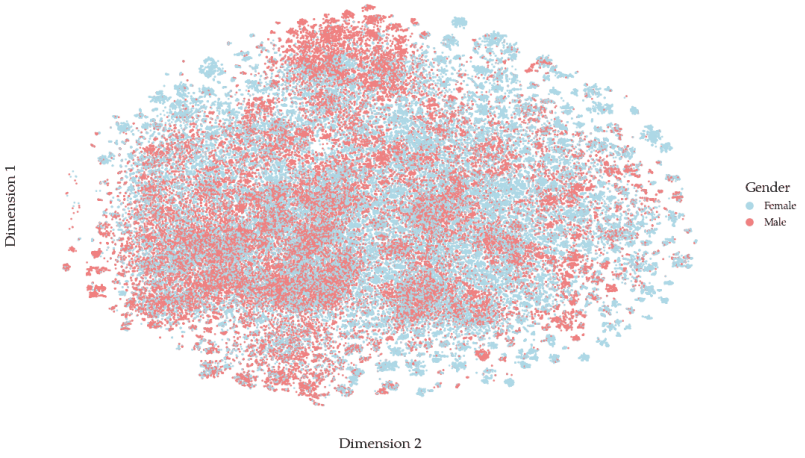
Notes. Last-name embeddings projected onto a two-dimensional space using t-SNE, plotted separately by gender. Each dot is a distinct last name, colored by the gender of one randomly drawn person with that name.

Figure D.10: First-Name Embeddings and Individual Characteristics

(a) Race



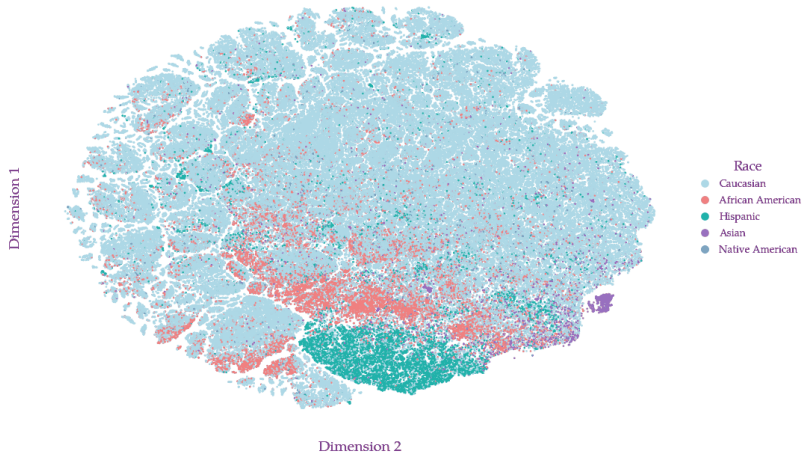
(b) Gender



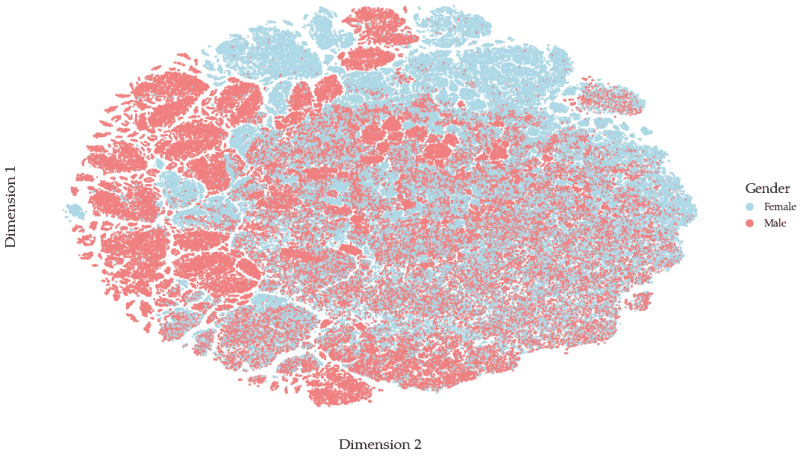
Notes. First-name embeddings projected onto a two-dimensional space using t-SNE. Panel (a) plots the embeddings separately by race and Panel (b) by gender. Each dot is a distinct first name, colored by the characteristic of one randomly drawn person with that name.

Figure D.11: Full-Name Embeddings and Individual Characteristics

(a) Race

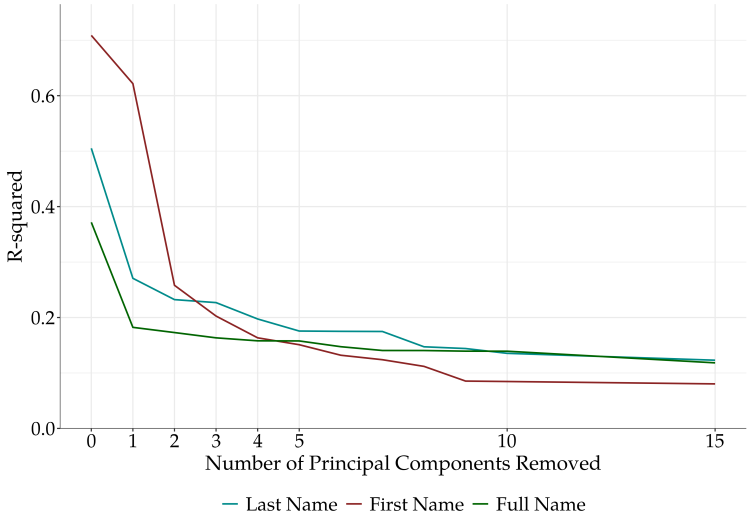


(b) Gender



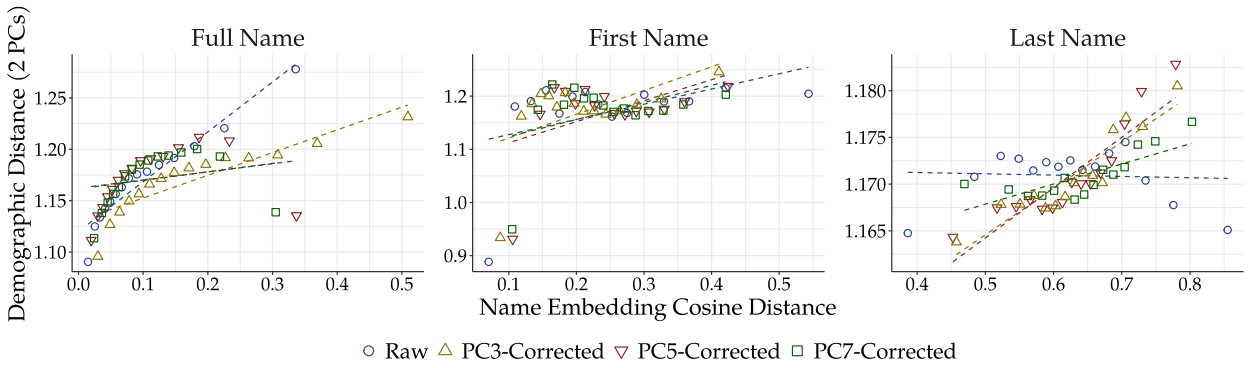
Notes. Full-name embeddings projected onto a two-dimensional space using t-SNE. Panel (a) plots the embeddings separately by race and Panel (b) by gender. Each dot is a person, from a random sample of 150,000 individuals in the training corpus.

Figure D.12: Name Commonness by Number of Principal Components Removed



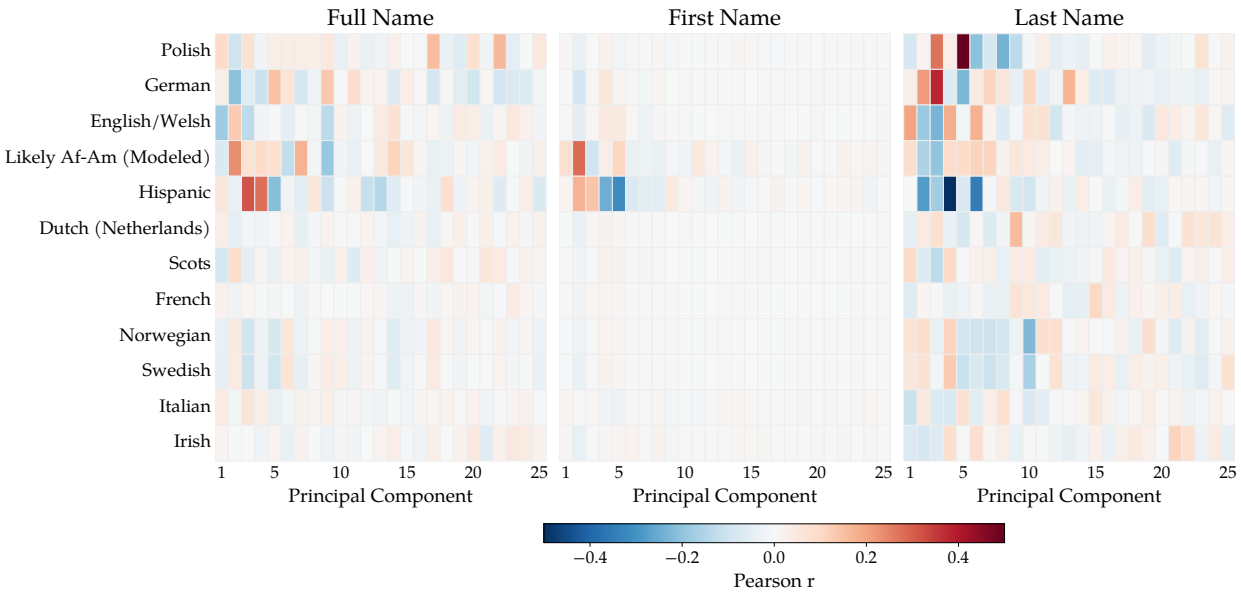
Notes. Out-of-sample R^2 of ridge regressions predicting the commonness of the names of registered voters from their name embeddings, after removing an increasing number of principal components. Commonness is the log number of registered voters with the same name embeddings. The principal components are estimated on the name embeddings of registered voters.

Figure D.13: Name Distance and Demographic Distance by Number of Principal Components Removed



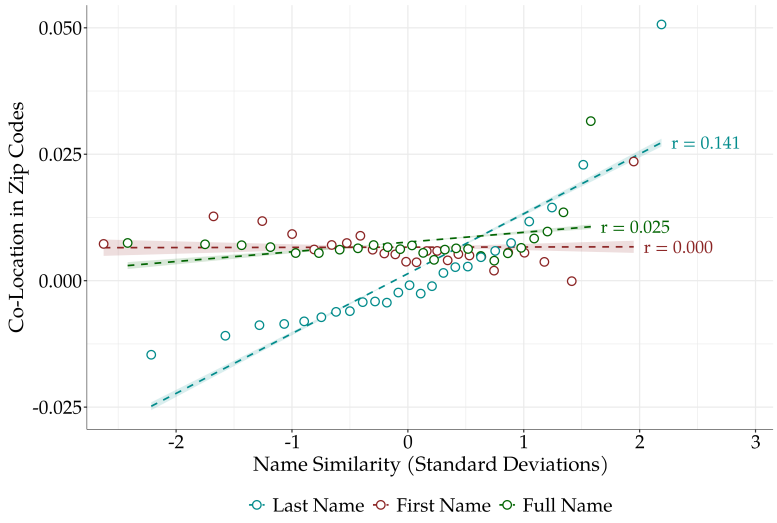
Notes. Correlation between the distance between the demographic characteristics of two registered voters and the cosine distance between their name embeddings, for 5 million random pairs of voters, reported separately for full, first, and last names. Demographic distance is the distance between the first two principal components of nine characteristics from the voter registration file. Each series uses the raw embeddings or the embeddings after removing the first 3, 5, or 7 principal components, which are estimated on the name embeddings of registered voters. Dashed lines are linear fits across all pairs.

Figure D.14: Principal Components of the Name Embeddings and National Origin



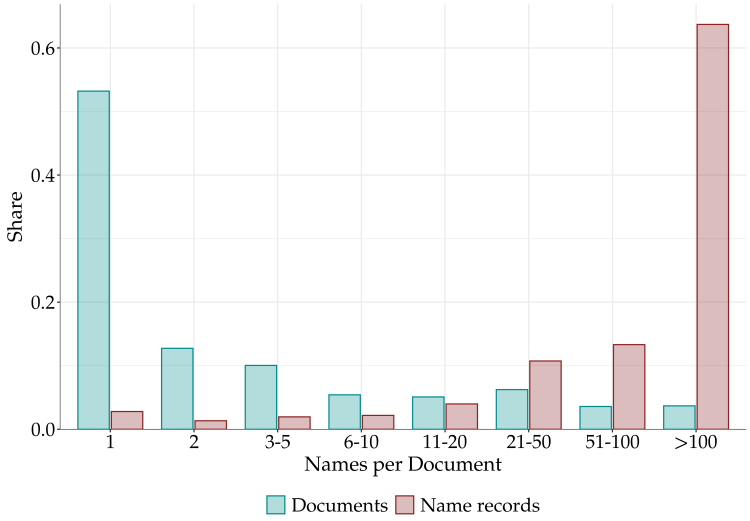
Notes. Correlation between the first 25 principal components of the name embeddings and the national origin of registered voters, reported separately for the embeddings of full names, first names, and last names. The principal components are estimated on the name embeddings of registered voters.

Figure D.15: Name Similarity and Co-Location



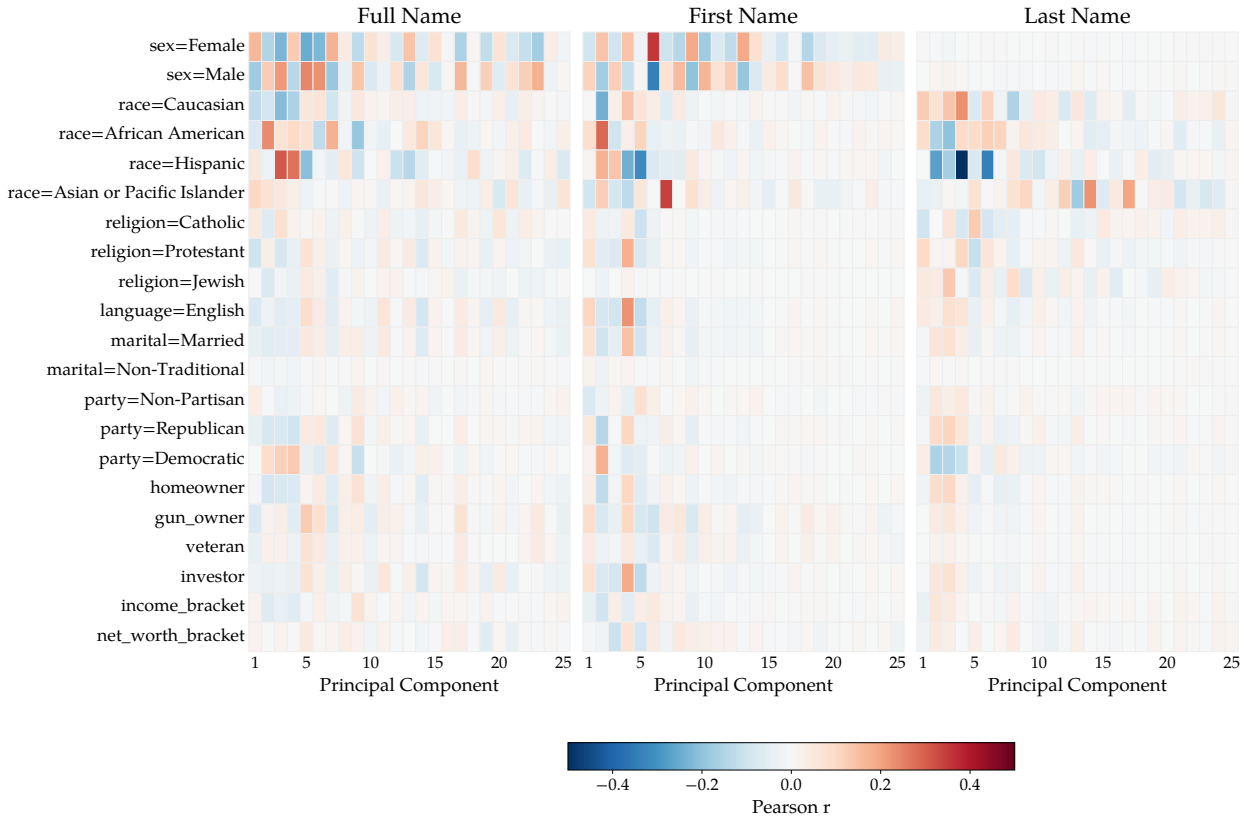
Notes. Correlation between the co-location of two names and their similarity, for 300,000 random pairs of names with at least 50 registered voters. Dashed lines are linear fits with 95% confidence intervals, and the numbers next to the lines report Pearson correlations. Co-location is the cosine similarity between the two names' distributions of voters across zip codes, net of the overall voter distribution. Name similarity is the PC5-corrected cosine similarity, standardized within each set of names.

Figure D.16: Number of Names per Document in the Training Corpus



Notes. Distribution of the number of names per document in the corpus used to train the name embeddings. The bars report the share of documents and the share of name records by number of names in the document.

Figure D.17: Principal Components of the Name Embeddings and Individual Characteristics



Notes. Correlation between the first 25 principal components of the name embeddings and the characteristics of registered voters, reported separately for the embeddings of full names, first names, and last names. The principal components are estimated on the name embeddings of registered voters.

E Additional Results

E.1 Tables

Table E.10: In-Group Bias: First-Name Similarity

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	0.055**	0.133**	0.009	0.422**	0.601**	0.023
	(0.009)	(0.009)	(0.007)	(0.039)	(0.033)	(0.029)
Female Def.			-0.068**			-0.210**
			(0.002)			(0.009)
Black Def.			0.044**			-0.186**
			(0.003)			(0.016)
Black J. × Black Def.			0.042**			-0.020
			(0.010)			(0.053)
Female J. × Female Def.			0.002			-0.012
			(0.003)			(0.017)
Observations	1,230,912	1,230,884	1,230,828	458,463	458,433	458,411
R^2	0.087	0.099	0.121	0.490	0.498	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
Ratio: Social Prox. / Black J. × Black Def.			0.216			
Ratio: Social Prox. / Black Def.						-0.126
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X	X	X	X	X
Judge FE		X	X		X	X
Other Controls			X			X

Notes. Effect of judge–defendant first-name similarity on the incarceration decision (Columns 1–3) and on the length of the jail sentence (Columns 4–6). Similarity is computed from the first-name embeddings after removing the first five principal components. All columns include county-year, charge severity, and risk ventiles fixed effects. Columns 2–3 and 5–6 add judge fixed effects, and Columns 3 and 6 add controls for judges' and defendants' race, gender, and age and their interactions. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.11: In-Group Bias: Full-Name Similarity

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.053**	-0.070**	-0.003	-0.116**	-0.162**	-0.009
	(0.015)	(0.006)	(0.005)	(0.041)	(0.023)	(0.022)
Female Def.			-0.068**			-0.211**
			(0.002)			(0.009)
Black Def.			0.043**			-0.187**
			(0.003)			(0.016)
Black J. × Black Def.			0.042**			-0.020
			(0.010)			(0.054)
Female J. × Female Def.			0.002			-0.012
			(0.004)			(0.017)
Observations	1,230,916	1,230,888	1,230,832	458,464	458,434	458,412
R^2	0.087	0.098	0.121	0.490	0.497	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
Ratio: Social Prox. / Black J. × Black Def.			-0.074			
Ratio: Social Prox. / Black Def.						0.050
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X	X	X	X	X
Judge FE		X	X		X	X
Other Controls			X			X

Notes. Effect of judge–defendant full-name similarity on the incarceration decision (Columns 1–3) and on the length of the jail sentence (Columns 4–6). Similarity is computed from the full-name embeddings after removing the first five principal components. All columns include county-year, charge severity, and risk ventiles fixed effects. Columns 2–3 and 5–6 add judge fixed effects, and Columns 3 and 6 add controls for judges' and defendants' race, gender, and age and their interactions. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.12: In-Group Bias: Full, First and Last Name Together

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Full-Name Similarity		-0.048**	-0.001		-0.064**	-0.005
		(0.006)	(0.005)		(0.022)	(0.022)
First-Name Similarity	0.130**	0.118**	0.008	0.593**	0.578**	0.021
	(0.008)	(0.008)	(0.007)	(0.032)	(0.032)	(0.029)
Last-Name Similarity	-0.078**	-0.076**	-0.037**	-0.219**	-0.217**	-0.075*
	(0.008)	(0.008)	(0.006)	(0.031)	(0.031)	(0.030)
Observations	1,230,884	1,230,884	1,230,828	458,433	458,433	458,411
R^2	0.099	0.099	0.121	0.498	0.498	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X
Other Controls			X			X

Notes. Effect of judge–defendant full-, first- and last-name similarity on the incarceration decision (Columns 1–3) and on the length of the jail sentence (Columns 4–6). Columns 1 and 4 include first- and last-name similarity; Columns 2–3 and 5–6 add full-name similarity. All similarities are computed after removing the first five principal components. All columns include county-year, charge severity, risk ventiles and judge fixed effects. Columns 3 and 6 add controls for judges' and defendants' race, gender, and age and their interactions. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.13: In-Group Bias by Judge Characteristics

	Defendant is Incarcerated				
	(1)	(2)	(3)	(4)	(5)
Social Proximity	-0.037**	-0.039**	-0.039**	-0.031**	-0.035**
	(0.006)	(0.006)	(0.007)	(0.009)	(0.010)
Social Proximity × Black J.		0.098*			0.095*
		(0.041)			(0.042)
Social Proximity × Female J.			0.009		0.009
			(0.021)		(0.021)
Social Proximity × Republican J.				-0.012	-0.009
				(0.013)	(0.013)
Observations	1,230,832	1,230,832	1,230,832	1,230,832	1,230,832
R^2	0.121	0.121	0.121	0.121	0.121
Mean Dep. Var.	0.377	0.377	0.377	0.377	0.377
County-Year FE	X	X	X	X	X
Charge Severity FE	X	X	X	X	X
Risk Ventile FE	X	X	X	X	X
Judge FE	X	X	X	X	X
Other Controls	X	X	X	X	X

Notes. Heterogeneity of the effect of social proximity on the incarceration decision by judge race, gender and political affiliation. Republican judges are those with a positive DIME campaign-contribution score. All columns include county-year, charge severity, risk ventiles and judge fixed effects and controls for judges' and defendants' race, gender, and age and their interactions. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.14: In-Group Bias by Judge Characteristics: Sentence Length

	Jail Time (IHS Days)				
	(1)	(2)	(3)	(4)	(5)
Social Proximity	-0.076*	-0.076*	-0.083*	-0.079+	-0.089*
	(0.031)	(0.030)	(0.033)	(0.042)	(0.044)
Social Proximity × Black J.		0.008			0.017
		(0.224)			(0.224)
Social Proximity × Female J.			0.050		0.052
			(0.075)		(0.074)
Social Proximity × Republican J.				0.007	0.010
				(0.053)	(0.053)
Observations	458,412	458,412	458,412	458,412	458,412
R^2	0.514	0.514	0.514	0.514	0.514
Mean Dep. Var.	4.665	4.665	4.665	4.665	4.665
County-Year FE	X	X	X	X	X
Charge Severity FE	X	X	X	X	X
Risk Ventile FE	X	X	X	X	X
Judge FE	X	X	X	X	X
Other Controls	X	X	X	X	X

Notes. Heterogeneity of the effect of social proximity on the length of the jail sentence by judge race, gender and political affiliation. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. Republican judges are those with a positive DIME campaign-contribution score. All columns include county-year, charge severity, risk ventiles and judge fixed effects and controls for judges' and defendants' race, gender, and age and their interactions. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.15: Social Proximity to the Assigned Judge and to the Judge Pool: Sentence Length

	Jail Time (IHS Days)			
	(1)	(2)	(3)	(4)
Social Proximity	-0.053	-0.060^{+}	-0.062^{+}	-0.053
	(0.040)	(0.036)	(0.037)	(0.039)
Avg. Social Prox. to Judge Pool	-0.041	-0.036	-0.049	-0.055
	(0.052)	(0.045)	(0.046)	(0.050)
Observations	458,412	457,904	450,167	409,892
R^2	0.514	0.514	0.515	0.517
Mean Dep. Var.	4.665	4.665	4.670	4.689
County-Year FE	X	X	X	X
Charge Severity FE	X	X	X	X
Risk Ventile FE	X	X	X	X
Judge FE	X	X	X	X
Other Controls	X	X	X	X
Assigned Judge in the Pool	X			
Bench Size Restriction			≥ 3	≥ 5

Notes. Effect on the length of the jail sentence of the defendant's social proximity to the assigned judge and of the defendant's mean social proximity to the judges sitting in the same county-year (the judge pool). Social proximity is computed from last-name embeddings after removing the first five principal components. In Column 1 the pool includes the assigned judge, while Columns 2–4 exclude it. Columns 3 and 4 restrict the sample to county-years with at least three and five judges, respectively. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. All columns include county-year, charge severity, risk ventiles and judge fixed effects and controls for judges' and defendants' race, gender, and age and their interactions. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.16: Placebo Judge: Incarceration

	Defendant is Incarcerated			
	(1)	(2)	(3)	(4)
Social Proximity			-0.039**	-0.035**
			(0.007)	(0.008)
Avg. Social Prox. to Judge Pool		-0.040**		-0.010
		(0.010)		(0.012)
Social Proximity to Random Judge	-0.010*	0.005	0.002	0.005
	(0.005)	(0.006)	(0.005)	(0.006)
Observations	1,229,249	1,229,249	1,229,249	1,229,249
R^2	0.121	0.121	0.121	0.121
Mean Dep. Var.	0.377	0.377	0.377	0.377
County-Year FE	X	X	X	X
Charge Severity FE	X	X	X	X
Risk Ventile FE	X	X	X	X
Judge FE	X	X	X	X
Other Controls	X	X	X	X

Notes. Effect on the incarceration decision of the defendant's social proximity to the assigned judge, of the defendant's mean social proximity to the other judges sitting in the same county-year (the judge pool), and of the defendant's social proximity to a placebo judge. Social proximity is computed from last-name embeddings after removing the first five principal components. The placebo is the defendant's social proximity to a judge drawn at random from the other judges sitting in the same county-year, who did not decide the case. The judge pool excludes the assigned judge. The sample is restricted to county-years with at least two judges. All columns include county-year, charge severity, risk ventiles and judge fixed effects and controls for judges' and defendants' race, gender, and age and their interactions. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.17: Placebo Judge: Sentence Length

	Jail Time (IHS Days)			
	(1)	(2)	(3)	(4)
Social Proximity			-0.060^{+}	-0.060^{+}
			(0.032)	(0.036)
Avg. Social Prox. to Judge Pool		-0.051		0.001
		(0.046)		(0.052)
Social Proximity to Random Judge	-0.055*	-0.036	-0.036	-0.036
	(0.022)	(0.026)	(0.023)	(0.026)
Observations	457,904	457,904	457,904	457,904
R^2	0.514	0.514	0.514	0.514
Mean Dep. Var.	4.665	4.665	4.665	4.665
County-Year FE	X	X	X	X
Charge Severity FE	X	X	X	X
Risk Ventile FE	X	X	X	X
Judge FE	X	X	X	X
Other Controls	X	X	X	X

Notes. Effect on the length of the jail sentence of the defendant's social proximity to the assigned judge, of the defendant's mean social proximity to the other judges sitting in the same county-year (the judge pool), and of the defendant's social proximity to a placebo judge. Social proximity is computed from last-name embeddings after removing the first five principal components. The placebo is the defendant's social proximity to a judge drawn at random from the other judges sitting in the same county-year, who did not decide the case. The judge pool excludes the assigned judge. The sample is restricted to county-years with at least two judges. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. All columns include county-year, charge severity, risk ventiles and judge fixed effects and controls for judges' and defendants' race, gender, and age and their interactions. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.18: In-Group Bias: Surname-Ancestry Matching

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Same Surname Origin	-0.001	-0.001	-0.001	0.006	0.007	0.007
	(0.001)	(0.001)	(0.001)	(0.006)	(0.006)	(0.006)
Social Proximity		-0.036**	-0.036**		-0.084*	-0.082*
		(0.007)	(0.007)		(0.035)	(0.035)
Recid. Score (std.)			0.065**			0.175**
			(0.002)			(0.005)
Same Surname Origin × Recid. Score (std.)			-0.004*			-0.009+
			(0.002)			(0.006)
Observations	915,447	915,447	915,447	343,738	343,738	343,738
R^2	0.123	0.123	0.122	0.510	0.510	0.509
Mean Dep. Var.	0.380	0.380	0.380	4.645	4.645	4.645
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls	X	X	X	X	X	X

Notes. Effect of a judge and the defendant having surnames with the same national origin on the incarceration decision (Columns 1–3) and on the length of the jail sentence (Columns 4–6). A surname's origin is the most frequent ethnicity among the registered voters who carry it in the voter file. Columns 3 and 6 replace the risk ventiles fixed effects with the recidivism risk score, standardized to have a mean of 0 and a standard deviation of 1, and its interaction with the origin match. Other controls include judges' and defendants' race, gender, and age, and their interactions. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.19: Judge–Defendant Neighborhood Proximity and Sentencing

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Neighborhood Social Proximity	-0.013**	-0.016**	-0.016**	-0.041**	-0.025**	-0.025**
	(0.002)	(0.002)	(0.002)	(0.009)	(0.008)	(0.008)
Neighborhood Racial Proximity	0.004+	0.007*	0.007*	0.101**	0.014	0.014
	(0.002)	(0.003)	(0.003)	(0.009)	(0.016)	(0.016)
Judge Lives in Defendant's Zip	-0.001	0.002	0.002	-0.004	0.011	0.011
	(0.002)	(0.002)	(0.002)	(0.010)	(0.010)	(0.010)
Social Proximity			-0.034**			-0.043
			(0.007)			(0.034)
Observations	842,999	840,833	840,833	309,725	308,380	308,380
R^2	0.119	0.120	0.120	0.519	0.518	0.518
Mean Dep. Var.	0.371	0.370	0.370	4.685	4.676	4.676
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X
Other Controls	X	X	X	X	X	X
Defendant Zip Characteristics		X	X		X	X

Notes. Effect of the proximity between the judge's and the defendant's neighborhoods on the incarceration decision (Columns 1–3) and on the length of the jail sentence (Columns 4–6). Proximity is defined as the mean absolute difference between the judge's and the defendant's zip codes, socio-economic characteristics (*Neighborhood Social Proximity*), and racial composition (*Neighborhood Racial Proximity*). Other controls include judges' and defendants' race, gender, and age, and their interactions. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table E.20: In-Group Bias: Identical and Near-Identical Surnames

	Defendant is Incarcerated			Jail Time (IHS Days)		
	No Identical Last Name	No Top 1% Soc. Prox.	All	No Identical Last Name	No Top 1% Soc. Prox.	All
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.038**	-0.035**	-0.039**	-0.078*	-0.083**	-0.078*
	(0.006)	(0.006)	(0.006)	(0.031)	(0.031)	(0.031)
Identical Last Name			0.039			0.067
			(0.024)			(0.126)
Observations	1,230,456	1,218,523	1,230,832	458,267	454,529	458,412
R^2	0.121	0.121	0.121	0.514	0.514	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X	X	X	X	X
Judge FE	X	X	X	X	X	X
Other Controls	X	X	X	X	X	X

Notes. Effect of social proximity on the incarceration decision (Columns 1–3) and on the length of the jail sentence (Columns 4–6), excluding judge–defendant pairs with the same or similar last name. *Identical Last Name* equals one when the judge and the defendant have the same last name or last names with a social proximity of one. Columns 1 and 4 exclude pairs with identical last names, Columns 2 and 5 exclude pairs above the 99th percentile of social proximity, and Columns 3 and 6 include the full sample and control for the identical-last-name indicator. All columns include county-year, charge severity, risk ventiles and judge fixed effects and controls for judges' and defendants' race, gender, and age and their interactions. Jail time is the inverse hyperbolic sine of the days of incarceration assigned and is estimated on incarcerated defendants only. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

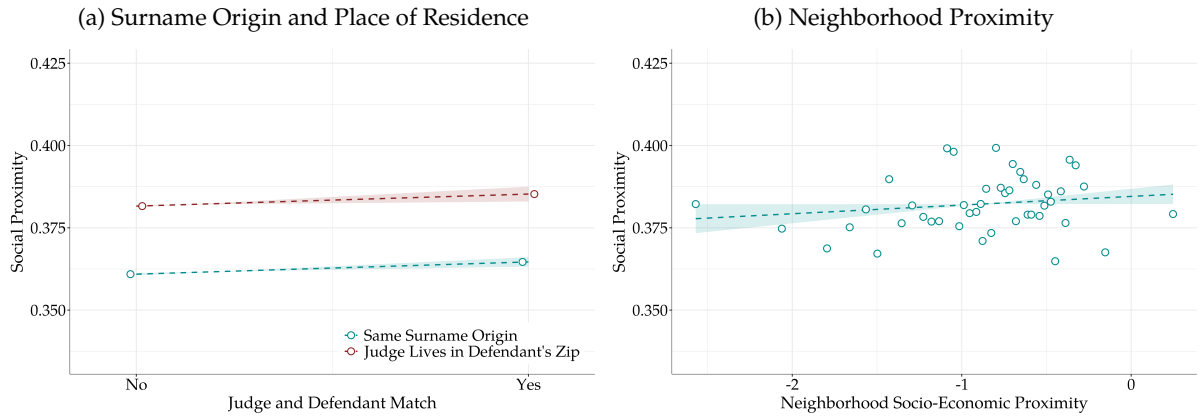
Table E.21: Judge Experience, Social Proximity and Recidivism Risk

	Defendant is Incarcerated		Jail Time (IHS Days)	
	(1)	(2)	(3)	(4)
Social Proximity	-0.079**	-0.031**	-0.273**	-0.111**
	(0.009)	(0.008)	(0.038)	(0.036)
Recid. Score (std.)	0.039**	0.056**	0.104**	0.186**
	(0.003)	(0.003)	(0.013)	(0.013)
Exp. Judge	0.007	0.010	0.018	0.021
	(0.007)	(0.007)	(0.030)	(0.030)
Social Proximity × Recid. Score (std.)	0.022**	0.019*	-0.038	-0.041
	(0.008)	(0.008)	(0.031)	(0.030)
Social Proximity × Exp. Judge	-0.011	-0.017	0.100	0.114+
	(0.014)	(0.014)	(0.068)	(0.068)
Recid. Score (std.) × Exp. Judge	-0.006	-0.007	-0.026	-0.030
	(0.005)	(0.006)	(0.021)	(0.020)
Social Proximity × Recid. Score (std.) × Exp. Judge	0.023	0.024+	0.096+	0.110*
	(0.014)	(0.014)	(0.053)	(0.052)
Observations	1,230,888	1,230,832	458,434	458,412
R^2	0.097	0.120	0.497	0.514
Mean Dep. Var.	0.377	0.377	4.665	4.665
County-Year FE	X	X	X	X
Charge Severity FE	X	X	X	X
Judge FE	X	X	X	X
Other Controls		X		X

Notes. Effect of judges' experience, social proximity, defendants' recidivism risk and their interactions on judges' incarceration decision (Columns 1–2) and on the length of the jail sentence (Columns 3–4). Experienced judges are those with tenure above the median of judges sitting in the same county-year. Social proximity is computed from last-name embeddings after removing the first five principal components. The recidivism risk score is standardized to have mean zero and standard deviation one. All columns include county-year, charge severity, and judge fixed effects. Columns 2 and 4 add controls for judges' and defendants' race, gender, and age, their interactions, and defendants' ethnicity. Jail time is estimated on incarcerated defendants only. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

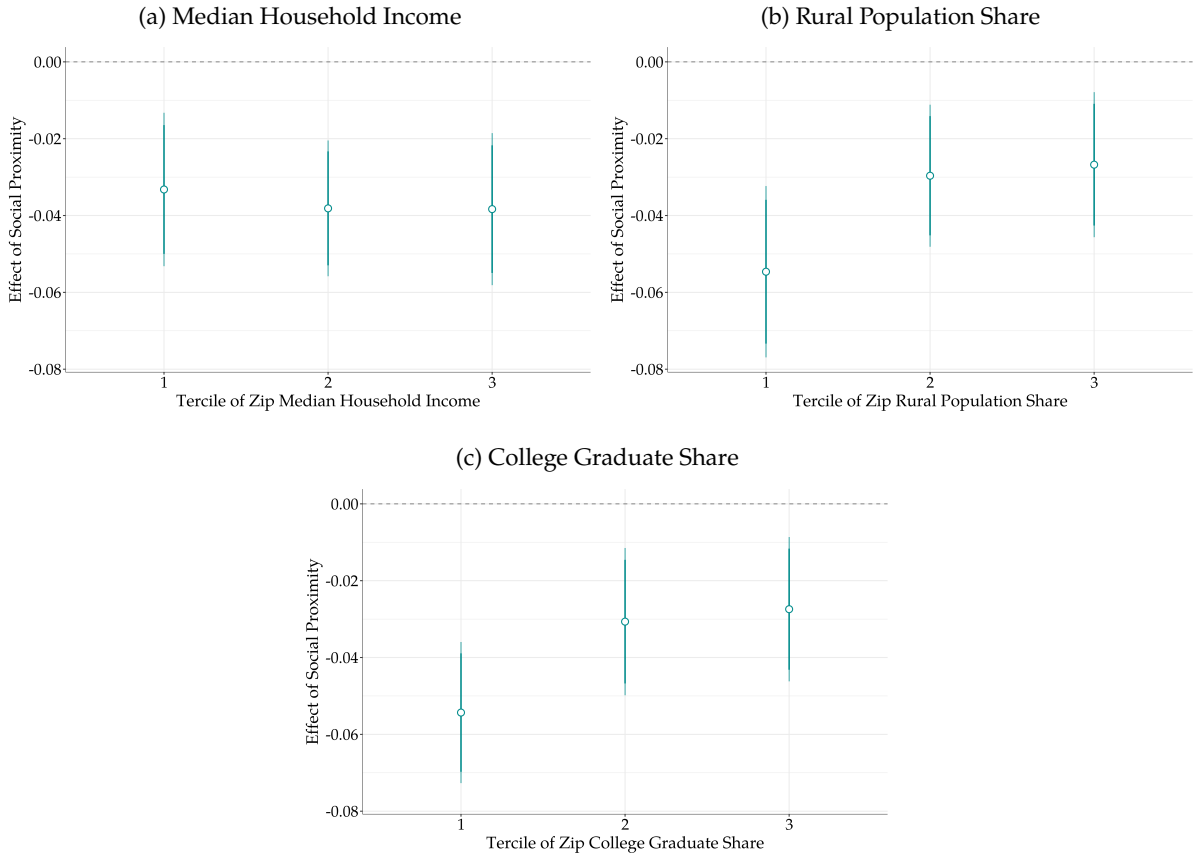
E.2 Figures

Figure E.18: Social Proximity, Surname Origin and Neighborhood Proximity



Notes. Panel (a) shows the mean social proximity of judge–defendant pairs whose surnames map to different and to the same national origin, for White defendants, and of pairs in which the judge does and does not live in the defendant's zip code, for all defendants whose judge has a matched home zip code. Panel (b) shows the correlation of name-based social proximity and socio-economic proximity between the judge's and the defendant's zip codes. Both variables are residualized on the racial-composition proximity between the two zip codes and on an indicator for the judge living in the defendant's zip code. Shaded areas are 95% confidence intervals, based on standard errors clustered at the county-year level.

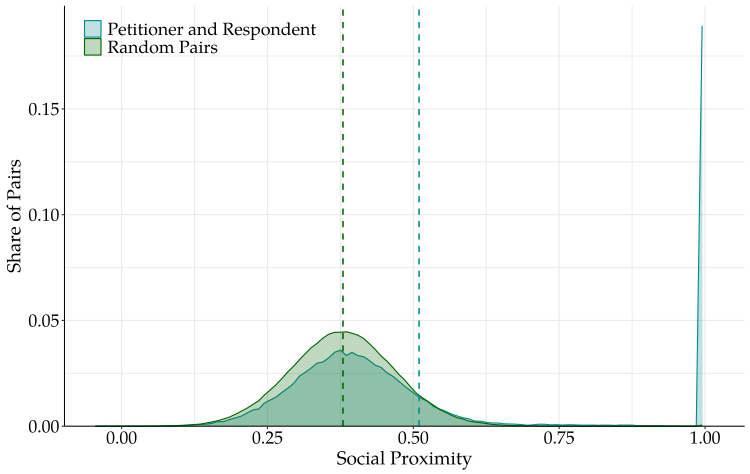
Figure E.19: Social Proximity by Zip Characteristics



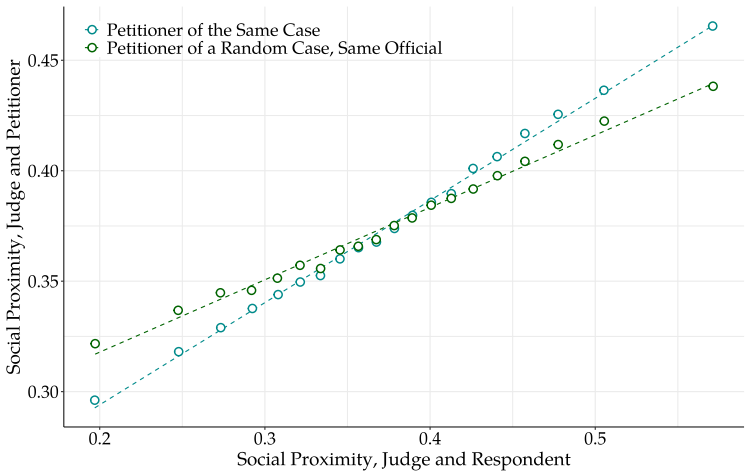
Notes. Effect of social proximity on the incarceration decision by tercile of the characteristics of the defendant's zip code. Social proximity is interacted with indicators for each tercile, whose main effects are included. The specification includes county-year, charge severity, risk ventiles and judge fixed effects, controls for judges' and defendants' race, gender, and age and their interactions, and indicators for defendants' ethnicity. The figure reports 90% and 95% confidence intervals, based on standard errors clustered at the county-year level.

Figure E.20: Social Proximity Between Petitioners and Respondents in Restraining-Order Cases

(a) Petitioner and Respondent



(b) Judge, Petitioner and Respondent



Notes. Panel (a) shows the distribution of the social proximity between the petitioner and the respondent of the same restraining-order case and of one million random petitioner–respondent pairs. Social proximity is the last-name similarity after removing the first five principal components. Dashed lines are the means of the two distributions. Panel (b) plots the correlation between the social proximity between the judge and the petitioner and the social proximity between the judge and the respondent of the same case, for the petitioner of the same case and for the petitioner of a randomly drawn other case decided by the same judge.

F Robustness

F.1 Tables

Table F.22: Robustness: Zip Code Fixed Effects

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.085**	-0.043**	-0.041**	-0.184**	-0.059*	-0.058*
	(0.008)	(0.006)	(0.006)	(0.029)	(0.029)	(0.029)
Recid. Score (std.)			0.052**			0.160**
			(0.003)			(0.011)
Social Proximity × Recid. Score (std.)			0.030**			0.010
			(0.006)			(0.024)
Black J. × Black Def.		0.040**	0.040**		0.018	0.018
		(0.010)	(0.010)		(0.045)	(0.045)
Female J. × Female Def.		0.003	0.003		-0.009	-0.009
		(0.003)	(0.003)		(0.016)	(0.016)
Observations	1,192,129	1,192,074	1,192,074	438,542	438,520	438,520
R^2	0.115	0.135	0.135	0.513	0.527	0.527
Mean Dep. Var.	0.374	0.374	0.374	4.653	4.653	4.653
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Zip Code FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity including fixed effects for the defendant's zip code. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + p < 0.1, * p < 0.05, ** p < 0.01.

Table F.23: Robustness: Prosecutor Fixed Effects

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.077**	-0.032**	-0.031**	-0.194**	-0.050+	-0.047+
	(0.008)	(0.006)	(0.006)	(0.030)	(0.028)	(0.028)
Recid. Score (std.)			0.054**			0.174**
			(0.003)			(0.011)
Social Proximity × Recid. Score (std.)			0.024**			-0.002
			(0.006)			(0.025)
Black J. × Black Def.		0.031**	0.031**		0.076+	0.076+
		(0.010)	(0.010)		(0.042)	(0.042)
Female J. × Female Def.		0.002	0.003		-0.001	-0.001
		(0.004)	(0.004)		(0.016)	(0.016)
Observations	1,158,998	1,158,942	1,158,942	420,232	420,210	420,210
R^2	0.108	0.129	0.129	0.530	0.545	0.545
Mean Dep. Var.	0.367	0.367	0.367	4.694	4.694	4.694
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Prosecutor FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity including prosecutor fixed effects. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.24: Robustness: First Offenders Only

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.164**	-0.047**	-0.026*	-0.250**	-0.097+	-0.208**
	(0.014)	(0.010)	(0.013)	(0.052)	(0.051)	(0.078)
Recid. Score (std.)			0.012**			0.223**
			(0.004)			(0.026)
Social Proximity × Recid. Score (std.)			0.027**			-0.136*
			(0.010)			(0.060)
Black J. × Black Def.		0.009	0.008		-0.016	-0.017
		(0.013)	(0.013)		(0.098)	(0.097)
Female J. × Female Def.		0.007	0.007		-0.035	-0.036
		(0.005)	(0.005)		(0.022)	(0.022)
Observations	400,228	400,203	400,203	125,017	125,009	125,009
R^2	0.152	0.177	0.176	0.529	0.546	0.546
Mean Dep. Var.	0.317	0.317	0.317	4.265	4.265	4.265
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity in the sample of first-time offenders. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.25: Robustness: First Offense as a Control

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.071**	-0.035**	-0.034**	-0.207**	-0.066*	-0.063*
	(0.008)	(0.006)	(0.006)	(0.032)	(0.031)	(0.031)
Recid. Score (std.)			0.045**			0.125**
			(0.003)			(0.011)
Social Proximity × Recid. Score (std.)			0.025**			-0.011
			(0.006)			(0.024)
First Offense	-0.105**	-0.044**	-0.031**	-0.395**	-0.230**	-0.182**
	(0.003)	(0.003)	(0.003)	(0.011)	(0.012)	(0.009)
Black J. × Black Def.		0.041**	0.041**		-0.026	-0.026
		(0.010)	(0.010)		(0.055)	(0.054)
Female J. × Female Def.		0.002	0.002		-0.011	-0.010
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.105	0.122	0.121	0.504	0.516	0.515
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity controlling for the defendant's first recorded offense. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.26: Robustness: County-Year×Severity Fixed Effects

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.073**	-0.035**	-0.034**	-0.228**	-0.065*	-0.065*
	(0.007)	(0.006)	(0.006)	(0.032)	(0.030)	(0.031)
Recid. Score (std.)			0.054**			0.171**
			(0.003)			(0.010)
Social Proximity × Recid. Score (std.)			0.025**			0.031
			(0.006)			(0.024)
Black J. × Black Def.		0.038**	0.038**		-0.054	-0.053
		(0.009)	(0.008)		(0.056)	(0.056)
Female J. × Female Def.		0.000	0.001		-0.019	-0.019
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,229,438	1,229,382	1,229,382	456,569	456,546	456,546
R^2	0.128	0.149	0.149	0.519	0.536	0.536
Mean Dep. Var.	0.377	0.377	0.377	4.656	4.656	4.656
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
County-Year-Severity FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity including county-year×charge severity fixed effects. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.27: Robustness: Standard Errors Clustered by Judge

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.082**	-0.037**	-0.036**	-0.237**	-0.076*	-0.073*
	(0.008)	(0.007)	(0.007)	(0.036)	(0.030)	(0.030)
Recid. Score (std.)			0.054**			0.176**
			(0.005)			(0.016)
Social Proximity × Recid. Score (std.)			0.027*			-0.004
			(0.011)			(0.038)
Black J. × Black Def.		0.042**	0.042**		-0.020	-0.020
		(0.013)	(0.013)		(0.085)	(0.085)
Female J. × Female Def.		0.002	0.002		-0.012	-0.011
		(0.005)	(0.005)		(0.020)	(0.020)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.098	0.121	0.120	0.498	0.514	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity with an alternative clustering of the standard errors. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the judge level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.28: Robustness: Standard Errors Clustered by Judge and Defendant

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.082**	-0.037**	-0.036**	-0.237**	-0.076*	-0.073*
	(0.009)	(0.007)	(0.007)	(0.036)	(0.030)	(0.030)
Recid. Score (std.)			0.054**			0.176**
			(0.005)			(0.017)
Social Proximity × Recid. Score (std.)			0.027*			-0.004
			(0.012)			(0.038)
Black J. × Black Def.		0.042**	0.042**		-0.020	-0.020
		(0.013)	(0.013)		(0.085)	(0.085)
Female J. × Female Def.		0.002	0.002		-0.012	-0.011
		(0.005)	(0.005)		(0.020)	(0.020)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.098	0.121	0.120	0.498	0.514	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity with an alternative clustering of the standard errors. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are two-way clustered by judge and by defendant: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.29: Robustness: Three Principal Components Removed

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.114**	-0.041**	-0.040**	-0.115**	-0.058+	-0.050
	(0.008)	(0.007)	(0.007)	(0.032)	(0.031)	(0.031)
Recid. Score (std.)			0.060**			0.208**
			(0.003)			(0.013)
Social Proximity × Recid. Score (std.)			0.011			-0.086**
			(0.007)			(0.027)
Black J. × Black Def.		0.042**	0.042**		-0.020	-0.022
		(0.010)	(0.010)		(0.053)	(0.053)
Female J. × Female Def.		0.002	0.002		-0.012	-0.011
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.099	0.121	0.120	0.497	0.514	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity computed after removing the first three principal components. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.30: Robustness: Seven Principal Components Removed

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.081**	-0.035**	-0.033**	-0.192**	-0.049	-0.050+
	(0.008)	(0.006)	(0.006)	(0.030)	(0.030)	(0.030)
Recid. Score (std.)			0.049**			0.163**
			(0.003)			(0.009)
Social Proximity × Recid. Score (std.)			0.046**			0.035
			(0.006)			(0.025)
Black J. × Black Def.		0.042**	0.042**		-0.020	-0.020
		(0.010)	(0.010)		(0.053)	(0.053)
Female J. × Female Def.		0.002	0.002		-0.012	-0.012
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.098	0.121	0.120	0.497	0.514	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity computed after removing the first seven principal components. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.31: Robustness: Corpus Window $w = 3$

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.084**	-0.028**	-0.027**	-0.027	-0.006	-0.004
	(0.006)	(0.005)	(0.005)	(0.026)	(0.022)	(0.022)
Recid. Score (std.)			0.052**			0.180**
			(0.003)			(0.009)
Social Proximity × Recid. Score (std.)			0.029**			-0.014
			(0.005)			(0.019)
Black J. × Black Def.		0.042**	0.042**		-0.020	-0.021
		(0.010)	(0.010)		(0.053)	(0.053)
Female J. × Female Def.		0.002	0.002		-0.012	-0.011
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.098	0.121	0.120	0.497	0.514	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity computed from last-name embeddings trained with a context window of three names. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + p < 0.1, * p < 0.05, ** p < 0.01.

Table F.32: Robustness: Corpus Window $w = 5$

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.093**	-0.036**	-0.035**	-0.100**	-0.037	-0.036
	(0.007)	(0.006)	(0.006)	(0.027)	(0.026)	(0.026)
Recid. Score (std.)			0.050**			0.174**
			(0.003)			(0.009)
Social Proximity × Recid. Score (std.)			0.038**			0.001
			(0.005)			(0.022)
Black J. × Black Def.		0.042**	0.042**		-0.020	-0.020
		(0.010)	(0.010)		(0.053)	(0.053)
Female J. × Female Def.		0.002	0.002		-0.012	-0.012
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.098	0.121	0.120	0.497	0.514	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity computed from last-name embeddings trained with a context window of five names. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.33: Robustness: Corpus Window $w = 13$

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.072**	-0.034**	-0.033**	-0.232**	-0.044	-0.046
	(0.008)	(0.006)	(0.006)	(0.033)	(0.031)	(0.031)
Recid. Score (std.)			0.049**			0.162**
			(0.003)			(0.010)
Social Proximity × Recid. Score (std.)			0.042**			0.033
			(0.006)			(0.025)
Black J. × Black Def.		0.042**	0.042**		-0.020	-0.020
		(0.010)	(0.010)		(0.053)	(0.053)
Female J. × Female Def.		0.002	0.002		-0.012	-0.012
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.098	0.121	0.120	0.498	0.514	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity computed from last-name embeddings trained with a context window of thirteen names. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + p < 0.1, * p < 0.05, ** p < 0.01.

Table F.34: Robustness: Logistic Risk Score

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.079**	-0.038**	-0.039**	-0.219**	-0.083**	-0.085**
	(0.008)	(0.006)	(0.006)	(0.032)	(0.031)	(0.031)
Recid. Score (std.)			0.058**			0.153**
			(0.003)			(0.010)
Social Proximity × Recid. Score (std.)			0.027**			0.010
			(0.006)			(0.023)
Black J. × Black Def.		0.043**	0.043**		-0.020	-0.019
		(0.010)	(0.010)		(0.054)	(0.054)
Female J. × Female Def.		0.003	0.003		-0.012	-0.010
		(0.003)	(0.004)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.102	0.122	0.121	0.500	0.513	0.513
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity with the risk score and its ventiles predicted by a logistic regression on the same features. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.35: Robustness: Risk Score Trained on Lenient Judges

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.082**	-0.037**	-0.036**	-0.237**	-0.074*	-0.072*
	(0.008)	(0.006)	(0.006)	(0.032)	(0.031)	(0.031)
Recid. Score (std.)			0.052**			0.181**
			(0.003)			(0.011)
Social Proximity × Recid. Score (std.)			0.032**			0.005
			(0.006)			(0.024)
Black J. × Black Def.		0.041**	0.042**		-0.022	-0.022
		(0.010)	(0.010)		(0.053)	(0.053)
Female J. × Female Def.		0.002	0.002		-0.012	-0.012
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.097	0.121	0.120	0.497	0.515	0.515
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity with the risk score and its ventiles trained on cases of lenient judges, the third of judges with the highest share of sentences shorter than 180 days. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.36: Robustness: Risk Score Trained on the Most Lenient Judges

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.082**	-0.037**	-0.036**	-0.237**	-0.075*	-0.072*
	(0.008)	(0.006)	(0.006)	(0.032)	(0.030)	(0.031)
Recid. Score (std.)			0.054**			0.177**
			(0.003)			(0.011)
Social Proximity × Recid. Score (std.)			0.027**			-0.002
			(0.006)			(0.024)
Black J. × Black Def.		0.042**	0.041**		-0.021	-0.021
		(0.010)	(0.010)		(0.053)	(0.053)
Female J. × Female Def.		0.002	0.002		-0.012	-0.012
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.098	0.121	0.121	0.498	0.514	0.514
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity with the risk score and its ventiles trained on cases of the two most lenient judges in each county-year. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.37: Robustness: Before COMPAS

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.061**	-0.032**	-0.031**	-0.275**	-0.083+	-0.084+
	(0.011)	(0.009)	(0.009)	(0.045)	(0.045)	(0.045)
Recid. Score (std.)			0.061**			0.181**
			(0.005)			(0.014)
Social Proximity × Recid. Score (std.)			0.022*			0.007
			(0.010)			(0.033)
Black J. × Black Def.		0.033**	0.033**		-0.021	-0.021
		(0.012)	(0.012)		(0.066)	(0.066)
Female J. × Female Def.		-0.006	-0.006		-0.023	-0.023
		(0.006)	(0.005)		(0.025)	(0.025)
Observations	605,546	605,507	605,507	245,453	245,440	245,440
R^2	0.109	0.136	0.136	0.494	0.515	0.515
Mean Dep. Var.	0.411	0.411	0.411	4.573	4.573	4.573
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity on the sample of cases filed before 2012. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.38: Robustness: Risk Score Trained on First Offenders

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.163**	-0.047**	-0.048**	-0.226**	-0.100^{+}	-0.110*
	(0.014)	(0.010)	(0.010)	(0.053)	(0.052)	(0.052)
Recid. Score (std.)			0.009**			0.093**
			(0.003)			(0.019)
Social Proximity × Recid. Score (std.)			0.007			-0.076^{+}
			(0.007)			(0.040)
Black J. × Black Def.		0.005	0.007		-0.010	-0.014
		(0.013)	(0.013)		(0.099)	(0.099)
Female J. × Female Def.		0.008^{+}	0.007		-0.034	-0.035
		(0.005)	(0.005)		(0.023)	(0.023)
Observations	400,228	400,203	400,203	125,017	125,009	125,009
R^2	0.155	0.178	0.175	0.532	0.544	0.544
Mean Dep. Var.	0.317	0.317	0.317	4.265	4.265	4.265
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity on the sample of first-time offenders, with the risk score and its ventiles trained and predicted on first-time offenders. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.39: Robustness: Criminal-History-Only Risk Score

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.064**	-0.034**	-0.033**	-0.200**	-0.067*	-0.069*
	(0.008)	(0.006)	(0.006)	(0.031)	(0.031)	(0.031)
Recid. Score (std.)			0.043**			0.147**
			(0.003)			(0.010)
Social Proximity × Recid. Score (std.)			0.032**			-0.003
			(0.007)			(0.024)
Black J. × Black Def.		0.041**	0.041**		-0.025	-0.022
		(0.010)	(0.010)		(0.054)	(0.053)
Female J. × Female Def.		0.003	0.003		-0.013	-0.012
		(0.003)	(0.003)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.107	0.119	0.119	0.502	0.514	0.513
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity with the risk score and its ventiles predicted from prior charge counts and the current case type only. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

Table F.40: Robustness: Demographics-Only Risk Score

	Defendant is Incarcerated			Jail Time (IHS Days)		
	(1)	(2)	(3)	(4)	(5)	(6)
Social Proximity	-0.091**	-0.051**	-0.051**	-0.239**	-0.115**	-0.115**
	(0.008)	(0.006)	(0.006)	(0.032)	(0.031)	(0.031)
Recid. Score (std.)			0.006*			0.106**
			(0.002)			(0.010)
Social Proximity × Recid. Score (std.)			0.018**			-0.009
			(0.005)			(0.022)
Black J. × Black Def.		0.037**	0.038**		-0.034	-0.036
		(0.010)	(0.010)		(0.053)	(0.054)
Female J. × Female Def.		0.004	0.004		-0.009	-0.009
		(0.004)	(0.004)		(0.017)	(0.017)
Observations	1,230,888	1,230,832	1,230,832	458,434	458,412	458,412
R^2	0.089	0.108	0.107	0.494	0.509	0.508
Mean Dep. Var.	0.377	0.377	0.377	4.665	4.665	4.665
County-Year FE	X	X	X	X	X	X
Charge Severity FE	X	X	X	X	X	X
Risk Ventile FE	X	X		X	X	
Judge FE	X	X	X	X	X	X
Other Controls		X	X		X	X

Notes. Effect of social proximity with the risk score and its ventiles predicted from defendants' sex, age and zip code characteristics only. Columns 1–3 report the incarceration decision and Columns 4–6 the inverse hyperbolic sine of jail days assigned to incarcerated defendants. Columns 1 and 4 include county-year, charge severity, risk ventiles, and judge fixed effects. Columns 2 and 5 add controls for judges' and defendants' race, gender, and age and their interactions. Columns 3 and 6 replace the risk ventiles fixed effects with the standardized recidivism risk score and its interaction with social proximity. Standard errors (in parentheses) are clustered at the county-year level: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$.

G Model Extension: Offenses With and Without a Victim

We extend the model of Section 4 to allow judges' decisions to depend on the presence of a victim. Let $v_i \in \{0, 1\}$ indicate that the offense of defendant i involves a victim, and let s_{ij}^V denote the social proximity between judge j and the victim. Releasing a defendant who harmed a victim imposes a cost on the victim, and judges may weigh this cost more when the victim is socially closer to them. The judge's utility from releasing the defendant becomes

$$U_j(\text{release}_i) = (1 - v_i)\,\phi\, s_{ij} + v_i \left(\phi\, s_{ij} - \psi\, s_{ij}^V\right) - \tau\, \bar{r}_{ij}, \tag{11}$$

where $\psi \geq 0$ captures the judge's preference for protecting socially closer victims. In victimless offenses ($v_i = 0$), the utility coincides with the one in Section 4. If there is a victim, the judge incarcerates the defendant when

$$I_{ij} \equiv \tau\, \bar{r}_{ij} - \phi\, s_{ij} + v_i\, \psi\, s_{ij}^V + \kappa_j > 0. \tag{12}$$

We do not observe victims, but victims tend to share the background of the defendants who harm them. We therefore assume that the victim's proximity to the judge is related to the defendant's proximity to the judge as

$$s_{ij}^V = \lambda\, s_{ij} + \xi_{ij}, \qquad \lambda > 0, \quad \mathbb{E}\left[\xi_{ij} \mid s_{ij}\right] = 0. \tag{13}$$

Since we do not observe victims in criminal cases, restraining-order cases are the closest available setting in which we observe both the person who seeks protection and the person they accuse. In these cases, the petitioner and the respondent have a mean social proximity of 0.51, compared with 0.38 for random pairs, and share the same last name in 19% of cases (Appendix Figure E.20), consistent with $\lambda > 0$.

Substituting Equation (13) into Equation (12) and taking expectations over ξ_{ij}, the effect of the defendant's social proximity on the incarceration decision becomes

$$\frac{\partial\, \mathbb{E}\left[I_{ij}\right]}{\partial s_{ij}} = -\phi + v_i\, \psi\, \lambda + \tau\, \frac{\rho_s}{\left(1 + \rho_{ij}\right)^2} \left(\tilde{r}_{ij} - \mu_i\right). \tag{14}$$

As in Section 4, the information term is zero at the average risk level, leading to the following prediction.

Proposition 4. *Holding risk constant, socially closer defendants are less likely to be incarcerated in victimless offenses if and only if judges prefer to release socially closer defendants ($\phi > 0$). In offenses with a victim, the effect of social proximity increases by $\psi\lambda$, and socially closer defendants are more likely to be incarcerated if $\psi\lambda > \phi$.*