Discrimination Against Immigrants in the Criminal Justice System: Evidence from Pretrial Detentions

Online Appendix

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A Additional Figures and Tables

Table A.I: Semiparametric Selection Model for Assessing OVB: $\widehat{\beta}_D^o$

	(1)	(2)	(3)	(4)
Model I: Immigrant	-0.091	-0.102	-0.029	-0.030
	(0.0064)	(0.0064)	(0.0061)	(0.0061)
Model II: Immigrant	-0.081	-0.093	-0.006	-0.006
	(0.0064)	(0.0064)	(0.0061)	(0.0061)
Model III: Immigrant	-0.080	-0.092	-0.005	-0.005
	(0.0064)	(0.0063)	(0.0061)	(0.0061)
Mean dep. variable	0.29	0.29	0.29	0.29
Court-by-year characteristics	No	Yes	No	Yes
Individual controls	No	No	Yes	Yes
No. of Immigrants	4,900	4,900	4,900	4,900
No. of Chileans	580,406	580,406	580,406	580,406

Notes: This table presents the results of the OVB test proposed in Section 4 using the semiparametric correction of Newey (2009) that uses series approximations to compute control function corrections. We implement the semiparametric correction following Low and Pistaferri (2015) where the first step uses Gallant and Nychka (1987) estimator to approximate the unknown density by third degree Hermite polynomial expansions and the second step controls for non-linear transformations of the density prediction. As in Low and Pistaferri (2015), we consider three models. Let $\hat{f}$ denote the predicted density. The control function used in Model I is $\hat{f}$ and its square, in Model II is $\Phi\left(\hat{\alpha}_0 + \hat{\alpha}_1 \hat{f}\right)$ and its square –where Φ is the normal cumulative distribution function and $(\hat{\alpha}_0, \hat{\alpha}_1)$ are the estimated coefficients of a Probit model of *Release* on a constant and $\hat{f}$–, and in Model III is $\lambda\left(\hat{\alpha}_0 + \hat{\alpha}_1 \hat{f}\right)$ and its square –where $\lambda(x) = \phi(x)/\Phi(x)$ is the inverse Mills ratio and ϕ the normal density. We report the point estimate for the immigrant indicator (i.e., the coefficient $\widehat{\beta}_D^o$) of equation (8) and its standard error. Standard errors are computed using bootstrap with 500 repetitions to account for the fact that the density is estimated in the first stage. Both sets of controls (*individual controls* and *court-by-year controls*) are always included in the selection equation, but the columns vary in their inclusion in the outcome equation. Judge and attorney controls are defined as in Table 2 and are excluded from the outcome equation. *Individual controls* are defined as in Table 2. To avoid saturating the nonlinear first-stage with court-by-year fixed effects they are replaced in the regressions by court-by-year time varying covariates—namely, the average number of judges, the average pretrial release rate, and the number of prosecutions (within a court in a given year).

Table A.II: Crime Type Distribution

	Chilean		Immigrant	
	%	N	%	N
Homicide	0.01	6,384	0.01	69
Sexual offense	0.02	12,275	0.03	188
Theft or robbery	0.26	180,086	0.17	1,104
Other property crime	0.18	126,335	0.16	1,038
Drug offense	0.12	85,936	0.21	1,306
White-collar or tax crime	0.02	11,946	0.02	142
Crime against public trust	0.06	44,031	0.07	464
Crime against people's freedom and privacy	0.29	198,834	0.29	1,816
Other crimes	0.04	27,179	0.04	235

Notes: This table presents the crime type distribution, by nationality, for the estimation sample. Shares are calculated to sum 100% within Chileans and immigrants.

B Data appendix

This appendix gives a more detailed description of the data, the sample restrictions, and the construction of the variables.

B.1 Sources

We merge two different sources of data to build our database.

PDO administrative records We use administrative records from the Public Defender Office (PDO, see http://www.dpp.cl/). The PDO is a centralized public service under the oversight of the Ministry of Justice that provides criminal defense services to all individuals accused of or charged with a crime who lack an attorney. The centralized nature of the PDO ensures that the administrative records contain information for all the cases handled by the PDO alone or those handled in coordination with a private attorney (as opposed to cases handled only by a private attorney), which covers more than 95% of the universe of criminal cases in Chile. The unit of analysis is a criminal prosecution and contains defendants characteristics (ID, name, gender, nationality, and place of residence, among other characteristics) and case characteristics (case ID, court, public attorney assigned, initial and end dates, different categories for the type of crime, pretrial detention status and length, and outcome of the case, among other administrative characteristics). We consider cases whose arraignment hearings occurred between 2008 and 2017.

Registry of judges In addition, we have access to information on arraignment judges and their assigned cases for arraignment hearings that occurred between 2008 and 2017. We merge this registry with the administrative records using the cases' IDs. We do not observe other characteristics of the judges other than their names and IDs. This data was shared by the Department of Studies at the Chilean Supreme Court (https://www.pjud.cl/corte-suprema).

B.2 Estimation sample

The initial sample contains $3,571,230$ cases and covers all the cases recorded by the PDO that had an arraignment hearing between 2008 and 2017. To create our estimation sample, we make the following adjustments.

Basic data cleaning Due to potential miscoding, we drop observations where the initial date of the case is later than the end date, and we also drop observations where the length of pretrial detention is greater than the length of the case. After these adjustments, the sample size reduces to $3,559,019$ (i.e., the number of cases reduces by $12,211$).

Sample restrictions We then make the following sample restrictions:

- We exclude hearings due to legal summons ($1,233,909$ observations). We do this because the information set available to the judges is likely to be different.
- We drop cases involving juvenile defendants ($254,243$ observations). We do this because the juvenile criminal justice system works differently, and the mandated selection rule and the preventive measures differ between systems (see Cortés et al., 2019 for details).
- We drop cases where the defendant hires a private attorney as their exclusive defender ($103,092$ observations). We do this because we do not observe the result of the arraignment hearing (and what happens after in the prosecution) in these cases.
- We drop cases that are longer than two years in duration ($55,495$ observations).
- For defendants that are accused of more than one crime in a given case and the records provide multiple observations, we consider the most severe crime (see below for the severity definition). In this step we drop $200,412$ observations. To be clear, we do not drop defendants, only cases. We do this to have at most one case/defendant pair per day of arraignment hearing.
- We drop cases where the detention judge ID is missing ($66,975$ observations).
- We drop the types of crime with a likelihood of pretrial detention that is less than 5% ($942,677$ observations). We do this because we want to study the decisions of judges in cases where pretrial detention is a plausible outcome.
- We drop cases handled by judges that see less than 10 cases in the whole time period ($2,848$ observations). We also resolve to only consider cases in which the assigned public attorney has defended at least 10 cases previously. It was not necessary to drop any data because of this restriction.

After all these adjustments the sample size is $699,368$, which is consistent with the figure in Table 1.

B.3 Variables

Many of the variables used in our estimations are directly contained in the administrative records. In what follows we describe how we construct the other variables.

- Severity: we proxy crime severity by computing the share of cases within the type of crime in which the defendants are detained pretrial.
- Criminal record: we can track all the arrests of a given defendant using their IDs. Then, the variables previous prosecution, number of previous prosecutions, previous pretrial misconduct, previous conviction, and severity of previous prosecution are constructed by looking at the characteristics of the cases associated to the defendant's identification ID that were initiated before the current case. For individuals with no previous prosecutions, these variables are set to zero. To build these variables, we can track cases from 2005 onwards.
- Pretrial misconduct: pretrial misconduct is an indicator variable that takes value one if the defendant does not return to a scheduled hearing or is engaged in pretrial recidivism, or both. Nonappearance in court is recorded in the administrative data. Pretrial recidivism is built by looking at the arrests associated to the particular defendant's ID with an initial date that is between the initial and end dates of the current prosecution.
- Attorney quality and judge leniency: as in Dobbie et al. (2018), we use the residualized (against court-by-time fixed effects) leave-out mean release rate.
- Court-by-year of prosecution fixed effects: we consider the initial date to set the fixed effects.

Crime categories We classify crimes following the PDO classification and group them in the following nine categories.

- Homicides: considers all homicides, including specific categories such as parricide and femicide, among other specific types.
- Sexual offenses: examples include sexual abuse, pedophilia, and rape, among other sex crimes.
- Thefts and robberies: includes robbery, burglary, theft, and larceny.
- Other property crimes: examples include receiving or possession of stolen goods, arson, and criminal damages.

- Drug offenses: includes illegal consumption, drug trafficking, and drug production.
- White-collar and tax crimes: examples include economic fraud and the falsification of money, checks, or credit cards.
- Crimes against public trust: examples include falsification of public, official, and commercial documents, forgery of private documents, falsification of certificates, and identity theft.
- Crimes against the freedom and privacy of people: considers threats against citizens, but also includes threats to police officers and trespassing.
- Other crimes: examples include gun possession and intellectual property theft.

C Kitawaga-Oaxaca-Blinder decompositions

Let $\overline{R}_g = \mathbb{E}[Release_i | I_i = g]$, with $g \in \{0, 1\}$. With KOB decompositions, group differences, $\Delta_R = \overline{R}_1 - \overline{R}_0$, can be explained using a vector of observed covariates, X_i. To do this, we first run an OLS projection for each group, $Release_i = X_i' \beta_g^R + \epsilon_{ig}^R$, where X_i includes a constant and the individual controls of the benchmark equation, and ϵ_{ig}^R is the OLS projection error. By construction, OLS fits group means, so $\overline{R}_g = \overline{X}_g' \beta_g^R$, with $\overline{X}_g = \mathbb{E}[X_i | I_i = g]$. Then

$$\Delta_R = \overline{X}_1' \beta_1^R - \overline{X}_0' \beta_0^R = \left(\overline{X}_1 - \overline{X}_0\right)' \beta_1^R + \overline{X}_0' \left(\beta_1^R - \beta_0^R\right). \tag{C.I}$$

The first term accounts for differences in release rates given differences in observables. The second term accounts for differences in release rates between defendants with the same observables (i.e., for differences in the estimated coefficients). When X_i includes all the relevant characteristics that matter for the release decision, the second term can be interpreted as discrimination. If there are unobserved variables that correlate with I_i and matter for misconduct potential, however, the OLS coefficients will capture their effect and the latter term will mistakenly interpreted as discrimination because of omitted variable bias (OVB).

Our intuition is that if differences in unobservables matter for the release decision in a nondiscriminatory fashion, then they should be relevant to explain differences in pretrial misconduct rates when released. Formally, let $\overline{PM}_g = \mathbb{E}[PM_i | I_i = g, Release_i = 1]$, where PM_i is an indicator that takes value one if defendant i engages in pretrial misconduct. Using the same logic as before, we can estimate $PM_i = X_i' \beta_g^P + \epsilon_{ig}^P$ in the sample of released defendants and write

$$\Delta_{PM} = \overline{X}_1' \beta_1^P - \overline{X}_0' \beta_0^P = \left(\overline{X}_1 - \overline{X}_0\right)' \beta_1^P + \overline{X}_0' \left(\beta_1^P - \beta_0^P\right). \tag{C.II}$$

Then, one way of testing if unobservables are important for interpreting our results is checking whether differences in observables are capable of explaining differences in pretrial misconduct rates. If the second component is large in the release equation but small in the outcome equation, then we can conjecture that unobservables are only playing a small role in explaining release rate disparities in a statistical sense, and we can therefore confidently interpret the benchmark estimations as evidence of discrimination.

Table C.I shows the results. For each dependent variable, we present two versions of the KOB decompositions. Columns labeled as *raw* present the standard KOB decomposition using the individual controls of the benchmark regressions as the vector of observables. Columns labeled as

Table C.I: Kitawaga-Oaxaca-Blinder Decomposition

	Release		Pretrial Misconduct	
	Raw	Residualized	Raw	Residualized
	(1)	(2)	(3)	(4)
Total Difference	0.067	0.030	0.050	0.076
	(0.011)	(0.011)	(0.009)	(0.007)
Explained: Due to difference in characteristics	-0.036	-0.056	0.065	0.076
	(0.003)	(0.003)	(0.003)	(0.004)
Unexplained: Due to differences in coefficients	0.103	0.085	-0.014	-0.001
	(0.011)	(0.010)	(0.008)	(0.004)
No. of Immigrants	6,362	6,362	4,900	4,900
No. of Chileans	693,006	693,006	580,400	580,400

Notes: This table presents the Kitawaga-Oaxaca-Blinder decomposition for release and pretrial misconduct estimation, considering raw and residualized covariates (residualized against court-by-year fixed effects).

residualized include dependent variables and the vector of individual controls residualized against court-by-year fixed effects. The release equation (columns 1 and 2) shows that there is a large share of the variation in release rates that cannot be explained by observed characteristics. In absolute value, the unexplained component of the average release gap is between two and three times larger than the share of variation explained by observables. Moreover, and consistent with the analysis so far, both components shift unconditional release disparities in opposite directions. In the absence of relevant unobserved variables, this reinforces the hypothesis of discrimination suggested by the benchmark regressions; however, it could also reflect the presence of OVB.

Columns 3 and 4 replicate the analysis using observed pretrial misconduct rates (among released defendants) as dependent variable. Note that in this case the complete average difference in pretrial misconduct rates can be explained by the same observed characteristics. This suggests that there are no relevant unobservables that explain misconduct potential, which reinforces the idea that the main benchmark regressions are not affected by OVB. Moreover, because observables do a good job of explaining actual misconduct rates, it reinforces the idea that our specific vector of observables makes the analysis closer in spirit to the disparate impact perspective, in the sense that this particular set of variables seem to do a reasonably good job of explaining observed misconduct rates. Or, at least, relevant omitted variables do not seem to be correlated with immigration status.

D Outcome test

This appendix describes the outcome test, specifically the observational implementation proposed by Grau and Vergara (2021), and provides suggestive evidence that the proposed identification argument is valid in our setting.

Outcome test The outcome test identifies a combination of biased beliefs and taste-based discrimination using observed outcomes of marginally selected individuals. Formal proofs are provided in Arnold et al. (2018), Grau and Vergara (2021), and Hull (2021). In what follows, the intuition for the outcome test is presented.

Recall in Section 3 that the release decision can be conceptualized as follows:

$$Release_i = 1\left\{p(I_i, Z_i) \leq t_{j(i)}(I_i, Z_i) - b_{j(i)}(I_i, Z_i)\right\}. \tag{D.I}$$

The outcome test examines whether the effective thresholds, $t_{j(i)}(I_i, Z_i) - b_{j(i)}(I_i, Z_i)$, are systematically different between immigrant defendants and Chilean defendants. Put formally, it tests whether

$$\mathbb{E}\left[t_{j(i)}(1, Z_i) - b_{j(i)}(1, Z_i)\right] - \mathbb{E}\left[t_{j(i)}(0, Z_i) - b_{j(i)}(0, Z_i)\right] \tag{D.II}$$

is different from zero, where the expectation is taken across Z_i and $j(i)$. If the difference is not zero, then defendants with equal "true" pretrial misconduct probabilities will be detained at different rates. Notably, if a group is, on average, more prone to be engaged in pretrial misconduct, this does not affect the results of the outcome test. Differences in pretrial misconduct potential will affect the "LHS" of (D.I); the OT estimates differences in the "RHS". If a group is systematically more risky, then it will cross the threshold more often, which is different from having a different threshold. That is why the outcome test identifies a notion of discrimination that abstracts from accurate sources of statistical discrimination. If judges are engaged in accurate statistical discrimination, then the outcome test should not be rejected (Hull, 2021).

The insight produced by the outcome test is that although $t_{j(i)}(I_i, Z_i) - b_{j(i)}(I_i, Z_i)$ is not observed, for defendants that were released on a borderline decision $p(I_i, Z_i) = t_{j(i)}(I_i, Z_i) - b_{j(i)}(I_i, Z_i)$, and therefore the average misconduct rates of marginally released defendants identify $t_{j(i)}(I_i, Z_i) - b_{j(i)}(I_i, Z_i)$. Then, the outcome test is reduced to a difference in means that tests whether misconduct rates are different between marginally released immigrant defendants and marginally released Chilean defendants.

The main identification challenge, then, is to identify marginal individuals. Arnold et al. (2018) provide a quasi-experimental approach that relies on the quasi-random assignment of judges. That approach is not applicable in our setting given the small share of immigrant defendants (the required instrument is underpowered). Grau and Vergara (2021) propose an observational approach that does not require instruments, which is the one implemented in this paper.

P-BOT The prediction-based outcome test (P-BOT) proposed by Grau and Vergara (2021) uses the propensity score to identify marginal individuals. More specifically, Grau and Vergara (2021) provide sufficient conditions under which released defendants with lower propensity scores are more likely to be marginal given their observables. The implementation of the outcome test then proceeds as follows. First, we estimate the propensity score and compute the predicted values. Second, we rank released defendants according to their predicted release probabilities and define as marginal the released defendants at the bottom of the distribution. Third, we implement differences in means for pretrial misconduct rates between immigrant defendants and Chilean defendants who were marginally released.

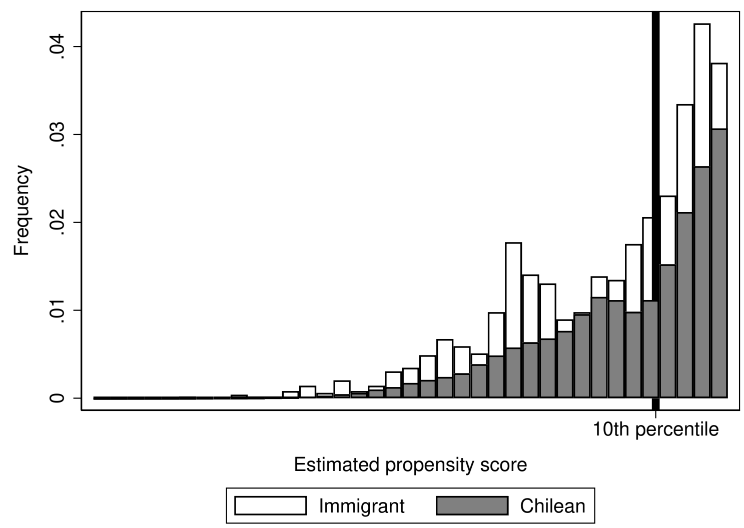
Identification requires three assumptions and here we present a high-level discussion of these sufficient conditions (for technical details see Grau and Vergara (2021)). First, we need a common support assumption on the distribution of latent risk that allows us to claim that “the more marginals” are effectively marginals. Second, the result relies on a separability assumption between observables and unobservables (by the econometrician) in the release equation. This implies that the effect of observables on the likelihood of being released is not affected by unobservables. Because the release decision is based on pretrial misconduct probabilities, this also implies a similar pattern in the outcome equation. Third, the result allows for unrestricted correlation between observables and unobservables but puts restrictions on the patterns of heteroskedasticity.

In what follows we present suggestive evidence that these assumptions are met in this setting implementing the tests as set out in Grau and Vergara (2021).

P-BOT implementation For implementing the test, we estimate the propensity score using the same set of observables included in the main benchmark estimations: the immigrant indicator, individual controls, judge and attorney controls, and court-by-time fixed effects. Then, we rank released defendants according to the predicted values and define the bottom 10% of the distribution as marginals. Standard errors are bootstrapped considering that the sample selection rule is based on an estimated value.

Common support Figure D.I shows the (estimated) propensity score distributions for released defendants, separating immigrant defendants and Chilean defendants. The figure suggests that the continuity and full support assumptions are met in our setting.

Figure D.I: Immigrant Released Defendant and Chilean Released Defendant Propensity Score Histograms (Zoom up to 20th Percentile)



Notes: This plot shows the propensity score histograms for immigrant released defendants and Chilean released defendants. The vertical line represents the 10th percentile of the distribution. For presentation purposes, we only show each histogram up to the 20th percentile. However, the histogram is calculated considering the entire population of released defendants.

Monotonicity One way to assess this assumption is to check whether the coefficients of a regression of $Release_i$ on covariates are stable (in terms of sign) when considering subsamples with (probably) different unobservables. Likewise, monotonicity also implies that the coefficients of a regression of pretrial misconduct on covariates are stable (in terms of sign) when considering subsamples with (probably) different unobservables.

Tables D.I and D.II show the results using $Release_i$ and PM_i as dependent variables, respectively. Each cell reports the estimated coefficient of the regressor specified in the column, using the sample specified in the first column. Each row represents a different estimation. The first row reports the coefficients using the whole sample. Thereafter, rows are paired by mutually exclusive sample categories that are (probably) characterized by different unobservables. For example, row 2 shows the results for the immigrant subsample, and row 3 shows the results for the Chilean subsample. Then, rows 4 and 5 split the sample by gender, and so on. The results strongly support the monotonicity assumption. In all but eight cases (i.e., 90% of cases) the sign of the coefficient is consistent across samples. Moreover, the magnitudes are also similar. This suggests that the direction of the effect of observables is unlikely to be affected by the unobserved variables.

Table D.I: Testing for Monotonicity in Observables (Dep. Variable: Release Status)

Estimation sample	Previous case	Previous pretrial misconduct	Previous conviction	Severity previous case	Severity current case
All	-0.004	-0.026	-0.013	-0.136	-1.009
	(0.003)	(0.001)	(0.003)	(0.004)	(0.003)
Immigrant	0.044	-0.027	0.061	-0.129	-1.082
	(0.033)	(0.017)	(0.032)	(0.058)	(0.034)
Chilean	-0.006	-0.026	-0.014	-0.136	-1.007
	(0.003)	(0.001)	(0.003)	(0.004)	(0.003)
Male	-0.004	-0.027	-0.012	-0.119	-1.014
	(0.003)	(0.001)	(0.003)	(0.004)	(0.003)
Female	0.011	-0.010	-0.030	-0.341	-0.958
	(0.007)	(0.003)	(0.007)	(0.013)	(0.009)
Low income	-0.006	-0.021	-0.015	-0.131	-1.019
	(0.004)	(0.002)	(0.004)	(0.006)	(0.005)
High income	-0.003	-0.029	-0.012	-0.140	-1.002
	(0.003)	(0.001)	(0.003)	(0.005)	(0.004)
Low judge	-0.002	-0.028	-0.016	-0.151	-1.050
leniency	(0.004)	(0.002)	(0.004)	(0.005)	(0.004)
High judge	-0.005	-0.023	-0.011	-0.121	-0.968
leniency	(0.004)	(0.002)	(0.003)	(0.005)	(0.004)
Low attorney	-0.004	-0.025	-0.016	-0.144	-1.073
quality	(0.004)	(0.002)	(0.004)	(0.005)	(0.004)
High attorney	-0.005	-0.026	-0.010	-0.127	-0.940
quality	(0.004)	(0.002)	(0.003)	(0.005)	(0.004)
Small court	0.002	-0.022	-0.017	-0.153	-1.084
(No. of cases)	(0.004)	(0.002)	(0.004)	(0.005)	(0.004)
Big court	-0.009	-0.028	-0.011	-0.126	-0.943
(No. of cases)	(0.004)	(0.002)	(0.003)	(0.005)	(0.004)
Small court	0.003	-0.022	-0.017	-0.149	-1.077
(No. of judges)	(0.004)	(0.002)	(0.004)	(0.005)	(0.004)
Big court	-0.009	-0.028	-0.012	-0.127	-0.946
(No. of judges)	(0.004)	(0.002)	(0.003)	(0.005)	(0.004)
Low severity	-0.005	-0.019	-0.010	-0.111	-0.879
court	(0.003)	(0.001)	(0.003)	(0.005)	(0.004)
High severity	-0.002	-0.028	-0.019	-0.158	-1.132
court	(0.004)	(0.002)	(0.004)	(0.005)	(0.004)

Notes: This table presents the results of the test for monotonicity in observables. Each reported value is the marginal effect of the variable of the column on the probability of release, estimated using a different sample in each row. The continuous variables were discretized using the respective median as the threshold. The values in parentheses are standard errors.

Table D.II: Testing for Monotonicity in Observables (Dep. Variable: Pretrial Misconduct)

Estimation sample	Previous case	Previous pretrial misconduct	Previous conviction	Severity previous case	Severity current case
All	0.050	0.106	0.036	0.036	0.034
	(0.004)	(0.002)	(0.003)	(0.005)	(0.005)
Immigrant	0.041	0.096	0.044	0.076	0.111
	(0.040)	(0.020)	(0.039)	(0.073)	(0.045)
Chilean	0.050	0.106	0.036	0.036	0.033
	(0.004)	(0.002)	(0.004)	(0.005)	(0.005)
Male	0.051	0.107	0.034	0.041	0.039
	(0.004)	(0.002)	(0.004)	(0.006)	(0.005)
Female	0.049	0.096	0.046	-0.024	-0.012
	(0.010)	(0.005)	(0.010)	(0.020)	(0.014)
Low income	0.045	0.097	0.038	0.036	0.075
	(0.006)	(0.003)	(0.006)	(0.008)	(0.007)
High income	0.051	0.112	0.035	0.037	0.000
	(0.005)	(0.002)	(0.004)	(0.007)	(0.006)
Low judge	0.041	0.102	0.044	0.043	0.035
leniency	(0.005)	(0.002)	(0.005)	(0.008)	(0.007)
High judge	0.059	0.110	0.027	0.029	0.034
leniency	(0.005)	(0.002)	(0.005)	(0.008)	(0.007)
Low attorney	0.047	0.110	0.042	0.048	0.033
quality	(0.005)	(0.002)	(0.005)	(0.008)	(0.007)
High attorney	0.054	0.102	0.029	0.023	0.034
quality	(0.005)	(0.002)	(0.005)	(0.008)	(0.007)
Small court	0.039	0.102	0.035	0.043	0.094
(No. of cases)	(0.005)	(0.002)	(0.005)	(0.008)	(0.007)
Big court	0.061	0.107	0.038	0.025	-0.024
(No. of cases)	(0.005)	(0.002)	(0.005)	(0.007)	(0.006)
Small court	0.054	0.103	0.030	0.040	0.057
(No. of judges)	(0.005)	(0.002)	(0.005)	(0.008)	(0.007)
Big court	0.047	0.107	0.042	0.030	0.013
(No. of judges)	(0.005)	(0.002)	(0.005)	(0.007)	(0.006)
Low severity	0.045	0.101	0.039	0.042	0.053
court	(0.005)	(0.002)	(0.005)	(0.007)	(0.006)
High severity	0.053	0.108	0.034	0.028	0.016
court	(0.005)	(0.002)	(0.005)	(0.008)	(0.007)

Notes: This table presents the results of the test for monotonicity in observables. Each reported value is the marginal effect of the variable of the column on pretrial misconduct, estimated using a different sample of released defendants in each row. The continuous variables were discretized using the respective median as the threshold. The values in parentheses are standard errors.

Ranking validity Assume that our set of observed variables, X_i, is a good approximation (up to some small well-behaved noise) of the (complete) information set of a judge. Under this assumption, the identification of marginally released defendants using the ranking based on the propensity score is accurate. We fit the propensity score and label as marginal the bottom 10% of the predicted probability distribution (among released defendants). Then, we omit one observable and (i) estimate the propensity score with the restricted set of observables and identify marginals using the ranking strategy, and (ii) compute the conditional probabilities of being marginal—namely, the shares of marginals identified in the first step for different combinations of the observables used in the restricted estimation. We then compute the rank correlation between (i) the share of marginals using the restricted propensity-score ranking and the conditional probabilities, and (ii) the estimated propensity score using the restricted set of observables and the conditional probabilities of being marginal. In case (i), the correlation is expected to be positive. In case (ii), the correlation is expected to be negative. If the identification argument holds, we should expect these rank correlations to be large.

We perform this exercise by using each of the 14 observables used in the estimation.[1] To compute the rank correlations, we discretize the nondiscrete regressors (using the median) to define $2^{(14-1)} = 8,192$ categories of observables. For each of these categories, we compute the average restricted estimated propensity score, the average share of marginals using the restricted propensity score, and the conditional probability of being marginal using the base estimation as the true share of marginals. Table D.III presents the results. We report both Spearman's-ρ and Kendall's-τ statistics for rank correlation. In all variables bar one (severity of current case) the correlations are very large. We interpret this as strong suggestive evidence of the validity of the identification argument.

[1]The variables are number of previous cases, severity of previous case, severity of current case, average severity by year-court, number of cases by year-court, judge leniency, judge leniency squared, attorney quality, attorney quality squared, immigrant indicator, previous case indicator, previous pretrial misconduct indicator, and previous conviction indicator.

Table D.III: Rank Correlations

	Corr. btw. $\Pr(Marg \mid X = x, Release = 1)$ and $\mathbb{E}[Marg \mid X = x]$ using restricted p-score		Corr. btw. $\Pr(Marg \mid X = x, Release = 1)$ and $\mathbb{E}[Release \mid X = x]$ using restricted p-score	
Excluded predictor	Spearman	Kendall	Spearman	Kendall
No of previous cases	0.972	0.948	-0.668	-0.549
Severity previous case	0.980	0.960	-0.672	-0.555
Severity current case	0.434	0.379	-0.264	-0.217
Average severity (year/court)	0.960	0.927	-0.671	-0.557
No of cases (year/court)	0.997	0.990	-0.666	-0.551
No of judges (year/court)	0.986	0.974	-0.675	-0.559
Judge leniency	0.988	0.976	-0.673	-0.556
Judge leniency square	1.000	0.999	-0.678	-0.560
Attorney quality	0.975	0.955	-0.676	-0.559
Attorney quality square	1.000	1.000	-0.675	-0.558
Immigrant	0.997	0.992	-0.806	-0.661
Previous case	0.998	0.996	-0.666	-0.551
Previous pretrial misconduct	0.989	0.981	-0.678	-0.562
Previous conviction	1.000	0.998	-0.670	-0.556

Notes: This table presents the rank correlations between the ranking of the conditional probabilities of being marginal and (i) the ranking of the conditional share of marginals using the restricted propensity score estimation, and (ii) the ranking of the predicted propensity score using the restricted estimation. We report Spearman's-ρ and Kendall's-τ_b rank correlation statistics. The excluded predictor is specified in the first column. All regressions include year fixed effects. The unit of analysis to build the ranking is the combination of all possible values of the predictors, without considering the excluded category (i.e., 13 predictors). The continuous predictors were transformed into binary variables using the median among released defendants as the threshold.