

Testing for Racial Discrimination in Bail Setting Using Nonparametric Estimation of a Parametric Model

Shawn D. Bushway
School of Criminal Justice
University at Albany

AND

Jonah B. Gelbach*
Senior Research Fellow
Program in Applied
Economics and Policy
Yale University Department
of Economics
and
Yale Law School
Class of 2013

February 14, 2011

Abstract

Black defendants are assigned systematically greater bail levels than whites accused of similar offenses and, partly as a result, have systematically lower probabilities of pre-trial release. We construct a simple model of optimal bail setting that allows us to measure how much of the bail difference is due to judicial bias against blacks, holding constant defendant heterogeneity that judges observe, regardless of whether we also observe it. We show how to use nonparametric methods to consistently estimate the model's key parameter by using the judge's first-order condition to form an auxiliary projection relationship involving defendants' conditional choice probabilities. While the behavioral model requires parametric assumptions, they have a substantial payoff: under these assumptions, we need not make any assumptions at all on the conditional distribution of heterogeneity observed by judges but not researchers. We implement the model using 2000 and 2002 data for five counties, from the State Courts Processing Statistics. While our point estimates are somewhat imprecise, they suggest that judges set bail as if they value blacks' lost freedom from a typical pre-trial jail stay by thousands of dollars less than they value whites' lost freedom.

*We thank David Abrams, David Card, John Donohue, Joe Hotz, Jon Klick, Pat Kline, Justin McCrary, Peter Reuter, Jesse Rothstein, Seth Sanders, Eric Talley, participants at the 2010 Institute for Research on Poverty Summer Research Workshop; law-school workshop participants at the University of Pennsylvania, the University of California at Berkeley, Florida State University, and George Mason University; and economics-department workshop participants at the University of California at Berkeley, the University of South Florida, and Syracuse University. We also thank Jeff Racine for his generous help in working with the **R** *np* package. Finally, we are grateful for financial support through NSF award number SES0718955.

[M]ore than a half-million inmates sit[] in America's jails not because they're dangerous or a threat to society or because a judge thinks they will run. It's not even because they are guilty; they haven't been tried yet.

They are here because they can't make bail – sometimes as little as \$50. Some will wait behind bars for as long as a year before their cases make it to court. And it will cost taxpayers \$9 billion this year to house them....

[P]eople with money get out. They go back to their jobs and their families, pay their bills and fight their cases. And according to the Justice Department and national studies, those with money face far fewer consequences for their crimes.

–Sullivan (2010)

1 Introduction

In this paper, we construct a simple model of judges' optimal bail setting that allows us to test for racial discrimination in bail levels. We estimate this model's key parameter using a two-step econometric method. The first step involves nonparametric estimation of certain conditional choice probabilities. In the second step, we estimate an auxiliary projection equation whose variables are generated using the first step's nonparametric estimates. This second step yields a consistent estimate of the model's key parameter, R , which is an affine function of the value the judge implicitly places on defendants' lost freedom. Our method allows for the model's systematic unobserved heterogeneity component to have an arbitrary distribution, even conditional on observables. The price we pay for this generality is that we must assume that defendants behavior is described by a multinomial logit model, conditional on all heterogeneity that judges observe.

A judge who follows the objective function we assume will choose bail to balance two kinds of marginal social costs against each other: the marginal expected social costs of holding a defendant until trial, and the marginal expected social costs that the defendant would impose on society if he is instead released pending trial. The social costs of jailing a defendant include both the pecuniary costs related to the physical jailing itself, and the value the judge places on the defendant's lost freedom. Because a judge who is biased against blacks will value their freedom less than whites' freedom, the defendant's value of

lost freedom is the variable through which any taste-based discrimination operates in our model.

We use the form of the judge's first-order condition to construct an auxiliary projection equation. This equation allows us to estimate, via least-squares estimation, the parameter R . This parameter is a function of the social costs of jailing a defendant conditional on his not making bail, as well as the expected social costs imposed by defendants who are released pending trial. Under certain homogeneity assumptions concerning (i) the pecuniary costs of jailing the defendant, and (ii) the expected social costs imposed by a released defendant, black-white differences in R will be due only to differences in the value of lost freedom judges accord to blacks and whites.¹

To implement our auxiliary-equation approach, we require the information that enters the judge's first-order condition. According to the model, this information includes both the levels and partial derivatives, with respect to bail, of two conditional choice probabilities: the probability that the defendant makes bail, and the probability that he fails to appear in court as ordered, given that he has made bail. The conditioning in these probabilities involves two types of defendant characteristics. The first type, x , includes those characteristics that both we and the judge observe. Examples of these characteristics include a host of criminal history variables, as well as the defendant's age and the most serious offense with which he is charged. The second type, u , includes any defendant characteristics that judges can observe, but we cannot. We refer to such characteristics collectively as unobserved heterogeneity (though we emphasize again that they are observable to the judge).

The existence of u complicates our task, because a judge who sets bail optimally uses information on defendants' choice probabilities conditional on the bail level, x , and u . Estimating choice probabilities conditional on unobservable information would seem to be impossible. However, we show that when judges set bail optimally according to our model, conditioning on x and the optimal bail level is sufficient to also condition on u . Intuitively, if defendants A and B facing the same charge have the same x , but a judge sets higher bail for A than B, then A must have unobservable characteristics that signal a higher probability

¹In our preferred specifications below, we allow the pecuniary costs of jailing defendants and the expected social costs imposed by released defendants to vary by county and major offense category.

that a defendant will fail to appear (FTA). At the optimum, then conditional choice probabilities that we can estimate must equal those that we could not generally estimate.² This basic idea is analogous to the key idea in Olley and Pakes’s (1996, henceforth, OP) study of production-function estimation. One methodological contribution of our paper is thus to extend OP’s method to a different context, in both substantive and functional-form terms.³

The informational equivalence result just discussed is useful once we add the additional structure provided by a standard Type I Extreme Value assumption on the heterogeneity in defendants’ latent utilities that neither we nor judges observe. Along with x , u , and the defendant’s bail level, the realized value of such “totally unobservable” heterogeneity terms determines which of three options a defendant selects: (i) not making bail, (ii) making bail and appearing in court as ordered, or (iii) making bail and failing to appear. Judges must set bail when they know only the probability of each defendant’s choice, i.e., without knowing the value of the heterogeneity terms just discussed. Given the Type I Extreme Value assumption, choice probabilities have the usual multinomial logit form conditional on bail, x , and u . That is important because partial derivatives of choice probabilities that have the multinomial logit form can be written as simple functions of the probabilities themselves. All of the data needed for the auxiliary regression described above thus involve conditional choice probabilities that can be estimated nonparametrically based on the one-to-one conditional relationship between u and the optimal bail level.

A nice feature of the econometric framework is that the judge’s preference parameters enter the auxiliary projection equation nonlinearly and redundantly. As a result, these parameters are overidentified. This allows us to test for model mis-specification.

To estimate the model, we use data on five large counties for 2000 and 2002 from the State Courts Processing Statistics (SCPS) data set, which we discuss below. Our empirical results suggest that the model performs quite well in describing the data, in the sense that simple overidentification tests do not reject the model’s composite predictions. We find that there is consistent, though often imprecisely estimated, evidence of discrimination against

²More formally, there are two sets of conditional choice probability functions. The first set involves conditioning on the level of bail and x , while the second set involves conditioning as well on u . Our approach is based on the fact that the two sets of functions intersect at the optimal bail level, even though they are not otherwise generally equal.

³Other work using related ideas includes Newey, Powell and Vella (1999) and Hotz and Miller (1993).

blacks in bail setting, especially for Dallas county; Los Angeles pretty clearly can be absolved of any charge of discriminatory bail setting.

Using figures from Abrams and Rohlfs (2010) to put our estimates in practical context, we find that the magnitude of discrimination against blacks implied by our point estimates is substantial. Back-of-the-envelope calculations based on our point estimates suggest that 82 days of black defendants' lost freedom is typically valued several thousand dollars less than white defendants' in several counties.

The remainder of the paper proceeds as follows. In section 2, we discuss existing literature and provide some institutional detail concerning bail policy. In section 3, we describe the SCPS data we use. In section 4, we present the basic theoretical model of judge and defendant behavior. We put some econometric structure on this model and discuss our estimation approach in section 5, and we report estimates in section 6. In section 7, we discuss the relationship of our model both to Ayres and Waldfogel's (1994) and the recent literature on racial profiling and outcome tests in the context of roadside motorist searches. We also discuss briefly the importance of having an economic model to test for discrimination. Finally, we conclude in section 8.

2 Bail Policy and Previous Literature

Historically, the sole purpose of pretrial detention was to ensure future court appearances. Judges decided bail levels primarily as a function of the offense charge, rather than the characteristics of individual defendants. Such a system causes poor defendants to be much less likely than wealthier ones to make bail.

In 1961, the Manhattan Bail Project was created in New York City to establish consistency in pretrial decisions while making pretrial outcomes less dependent on economic status. The results led to legislation that increased pre-trial release (Ares, Rankin and Sturz (1963)). The federal Bail Reform Act of 1966 laid out a set of standards for federal judges to follow when making pretrial release decisions for defendants in federal court. For the first time, this act established a prioritized list of options that a judge must follow, starting with release on recognizance.

In 1968, the American Bar Association published a set of standards elaborating on the Federal Bail Reform Act and adding two additional guidelines. First, potential danger to the community was established as a factor that should be considered by judges. Second, the ABA recommended eliminating surety bail, in which a third party assumes responsibility for the defendant's bail in return for a fee, due to a long history of perceived abuses. In 1970, the District of Columbia was the first jurisdiction to require judges to take into account the potential threat to the community along with flight risk when making their pretrial release decisions. Almost twenty years later, in 1984, the Federal Bail Reform Act was amended for the first time to allow judges to consider danger to the community and preventative detention when making pretrial release decisions (Clark and Henry (1997)).

There is now virtual consensus that preventing flight and protecting the public are the primary goals of judges making pre-trial release decisions (Demuth (2003)). That said, statutes and procedural rules governing bail setting leave substantial room for judicial discretion. For example, Florida Rule of Criminal Procedure Rule 3.131(b) states in part that

(2) The judge shall at the defendant's first appearance consider all available relevant factors to determine what form of release is necessary to assure the defendant's appearance. If a monetary bail is required, the judge shall determine the amount....

(3) In determining whether to release a defendant on bail or other conditions, and what that bail or those conditions may be, the court may consider the nature and circumstances of the offense charged and the penalty provided by law; the weight of the evidence against the defendant; the defendant's family ties, length of residence in the community, employment history, financial resources, need for substance abuse evaluation and/or treatment, and mental condition; the defendant's past and present conduct, including any record of convictions, previous flight to avoid prosecution, or failure to appear at court proceedings; the nature and probability of danger that the defendant's release poses to the community; the source of funds used to post bail; whether the defendant is already on release pending resolution of another criminal proceeding or is on probation, parole, or other release pending completion of sentence; and any other facts the court considers relevant.

Rule 3.131. Pretrial Release, Fla. R. Crim. P. Rule 3.131 (emphasis added).

Bail statutes for the other states whose counties we study allow similar discretion.⁴

The pretrial process has received relatively little research attention from social scientists. Criminological research that does exist suggests that pretrial detention may have substantive welfare effects on defendants. Work by Goldkamp (1979) suggests that jailed defendants he studied were less able to build an adequate defense and therefore received more severe punishments. Goldkamp also found that the stigma of being in jail affected a case's outcome, especially if a jury trial was involved. Irwin (1985) determined that any incarceration had negative effects on family and community ties, including employment, and ultimately stigmatized the defendant further. Clark and Henry (1997) state that defendants who were detained before trial were more likely to plead guilty, were convicted more often, and were more likely to receive a prison sentence than defendants who were released before trial. These concerns have recently received widespread media attention as part of the series on bail produced by National Public Radio, from which we quote above.

Recent social science evidence suggests that bail burdens may weigh more heavily on disadvantaged racial and ethnic groups than on whites. Using administrative data from the State Courts Processing Statistics, Demuth (2003) finds that black and Hispanic defendants are about 20 percent more likely to be denied bail than whites. Black and Hispanic defendants are also more than twice as likely to be held on bail than are whites, even after adjusting for case characteristics.

Of course, these racial differences by themselves do not prove the existence of racial discrimination. Administrative data do not usually have information on drug use, employment or ties to community which may be considered relevant by the bail-setting judge (Smith, Wish and Jarjoura (1989)). Moreover, judges can observe defendants' in-court demeanor, whereas researchers usually cannot. Such unobserved factors may be associated with race, helping to explain the observed differences, and perhaps even totally explaining them.⁵

Bail has also received relatively little attention from economists. Perhaps its earliest

⁴For example, Cal.Penal Code 1275(a) provides that "In setting, reducing, or denying bail, the judge or magistrate shall take into consideration the protection of the public, the seriousness of the offense charged, the previous criminal record of the defendant, and the probability of his or her appearing at trial or hearing of the case." Statutes for Illinois and Texas are available at 725 ILCS 5/110-5 and Texas C.C.P. Art. 17.15.

⁵For example, some prior research using databases with richer measures of community ties and employment has found no race effects on pretrial release decisions (e.g., see Albonetti, Hauser, Hagan and Nagel (1989)).

treatment in the economics literature was by Landes (1973), who models bail setting as an optimizing procedure. More recently, Abrams and Rohlfs (2010) use data from the 1981 Philadelphia Bail Experiment to estimate key parameters and calibrate a model of optimal bail setting.⁶ These parameters include the costs of jailing a defendant until trial, the defendant’s value of lost freedom when jailed, and the social costs of any bad acts the defendant would commit while on release. Abrams and Rohlfs find that increasing a defendant’s bail level reduces his probabilities of pre-trial release and of failing to appear or being rearrested while on release.

To our knowledge, Ayres and Waldfogel (1994) are the only other authors to study racial discrimination in bail setting through the lens of an economic model. Because Ayres and Waldfogel lack data on FTA, they develop a clever, market-based test for racial discrimination. They assume that judges seek to ensure appearance at trial with a minimum probability and show that this objective function implies equal failure to appear rates across defendants. Using market data on bail bonds prices in New Haven, Connecticut, they find that blacks pay lower bond prices per dollar of bail. Under the assumption that the bail bonds market is competitive, this suggests the bail bonds market undoes judicial bias in bail setting: judges in the Ayres & Waldfogel sample hold blacks to a higher appearance-probability standard, which makes it cheaper to provide supply a dollar of bail-bond protection to blacks than to whites.⁷ We discuss theirs and related work in more detail in section 7.

3 Data

3.1 Basic data facts

The State Courts Processing Statistics (SCPS) dataset has been assembled biennially by the Bureau of Justice Statistics (BJS) since 1990. It is intended to represent the population of all

⁶See Abrams and Rohlfs (2010) or Goldkamp and Gottfredson (1985) for details on this experiment. Colbert, Paternoster and Bushway (2002) conduct another experiment, involving representation by counsel in Baltimore. They find that being represented by counsel reduces bail levels, with no resulting impact on failure to appear.

⁷Helland and Tabarrok (2004) also consider issues related to bail. Their paper concerns the relative efficacy of public and private methods for finding those who have already failed to appear, which is a different topic from ours. Miles (2005) also considers re-capture of fugitives.

felony cases brought in May in the 75 largest counties in the United States. The SCPS dataset follows cases from filing to disposition, or for one year—whichever occurs first.⁸ It contains detailed information on the pretrial processing of felony defendants, including whether they are rearrested or fail to appear for a court hearing. It also contains important information on demographic, case, and contextual factors which may affect pretrial outcomes. In the analysis below, we use information on age at indictment, the offense category of the most serious charge the defendant faces, previous criminal history (including arrest and incarceration), and whether the person was under criminal justice supervision (on bail, probation, or parole) at the time of arrest.

SCPS sampling is done in two stages. In the first stage, the 75 largest counties in the U.S. are divided into four strata based on population size, and 40 counties are chosen from these strata. In the second stage, the BJS chooses days in May, with more days chosen for smaller counties than larger ones. For the smallest counties, the BJS selects 20 business days, and for the largest counties, it selects only 5, at random. For each day in each selected county, the sample includes all felony defendants indicted on that day, and data are collected over the succeeding 12 months or until the case is disposed of.

The SCPS dataset includes weights that can be used to account for representativeness and inference issues that arise due to this multi-stage sampling process. However, we do not use these weights, for reasons we explain momentarily. We wish to estimate separate parameters of interest for each county we consider. After detailed work with the data, we found that many county-year cells in the SCPS had very small samples. Since parameter estimates for small counties would be very noisy, we exclude them. The five counties we do use are: Broward, Florida (which includes Ft. Lauderdale); Cook, Illinois (Chicago); Dallas, Texas; Harris, Texas (Houston); and Los Angeles, California. All are in the largest stratum, which means that they were selected with certainty each year of the survey, and had cases from five days during May selected.⁹

⁸Murder cases are followed for up to two years, but we do not include any of these cases in our analysis.

⁹Prior to 2000, Broward was in the second largest stratum, which means it had had .55 chance of being selected for the survey, and cases from 10 days were selected. This is one reason we do not use data from 1998 or earlier. Another involves data issues with Los Angeles and Cook counties for 1998. Many observations from Los Angeles have missing data on race for 1998, so that our estimation sample would necessarily include almost no 1998 Los Angeles observations. In addition, Cook county has a substantially lower share of potential 1998 observations, by comparison to Dallas and Harris, than it does for 2000 and 2002. This

Pretrial policies vary widely across these counties. Illinois is one of four states that ban commercial bail bonds companies, so bail is handled directly by the court in Cook County. On the other hand, Texas historically has encouraged the use of commercial bail bondsmen. In Harris County, groups backed by bail bonds groups tried to get rid of non-bail pretrial release programs in the mid-1990s; these efforts were ultimately unsuccessful. A similar attempt was successful in Broward County, but not until 2009, after our data were collected (Sullivan (2010)).

To select our sample, we used the following algorithm. We first dropped all observations involving a pre-2000 indictment. Next, we kept only cases in which the defendant's race was recorded as either black or white, and we dropped all cases with a defendant reported to be of Hispanic background. We then kept only those cases in which the defendant was assigned positive bail. We dropped all cases in which the most serious charge was murder, since defendants in murder cases are followed twice as long by the SCPS as are other defendants; since there are comparatively few murders in the sample, this selection choice likely makes little difference. For the remainder of the paper, we refer to the most serious charged offense as simply the charged offense; note, though, that defendants can be charged with multiple offenses. We divide defendants into those charged with violent, property, drug, and public order offenses. These categories have the following constituent offenses:

- **Violent crimes:** rape, robbery, assault, “other violent.”
- **Property crimes:** burglary, larceny-theft, motor vehicle, theft, forgery, fraud, and “other property.”
- **Drug crimes:** drug sales, “other drug.”
- **Public order crimes:** weapons, driving-related, “other public order.”

Table 1 reports some basic summary statistics for the data we use in the analysis below. The first two columns of the table report descriptive statistics for blacks and whites charged with a violent crime. Average bail for blacks charged with violent crimes is just under

raises questions about sample comparability across these years. These issues, together with Broward's change in stratum, have convinced us to drop 1998 altogether.

\$36,000, about \$7,000 above the average bail assigned to whites charged with these crimes. One consequence of the higher average bail facing blacks charged with violent offenses is that only 40% of them made bail, by comparison to 53% of whites charged with violent offenses.

A naive approach to testing for discrimination would take the \$7,000 difference in bail for defendants charged with violent offenses as evidence of anti-black bias. However, blacks and whites accused of violent crimes differ in many ways. For example, the “Any prior FTA” row shows that 32% of the blacks in the sample have previously failed to appear in court, by comparison to only 21% of whites. Additionally, 26% of blacks in the violent-offense category could not have previously failed to appear, meaning that they were not previously ordered to appear in court; among whites, this figure is 33%. Blacks charged with violent crimes also were more likely than whites to have been arrested previously (74% versus 67%), and more of them had been arrested at least ten times (22% versus 16%). They were also more likely to have previously served time in prison (24% versus 18%, though roughly the same share of blacks and whites had previously served time in jail—45% versus 44%), and they were also twice as likely to have an active criminal justice status at the time of charging. In addition, blacks facing a violent-offense charge were less likely to be represented by a private attorney, with black-white differences in the share represented by public defenders accounting for most of this difference.

As with violent offenses, blacks charged in the other three offense-type categories face higher bail levels than do whites. These differences are especially large for drug crimes (about \$13,000) and public order crimes (more than \$10,000). Blacks charged in these three categories are also much less likely than whites to make bail. As with those charged with violent crimes, blacks in the other three categories uniformly have more extensive criminal-justice backgrounds than whites, as measured by the variables considered in Table 1.

3.2 Regression-adjusted differences

These differences in criminal background suggest that simple differences in average bail by racial group are problematic measures of discrimination: it is possible that whites with the same characteristics as blacks in the sample would receive the same bail. A conventional approach to adjusting for characteristics is to estimate the black-white difference in average

bail by estimating a linear regression model in which background characteristics enter as covariates. We report the coefficient on a dummy variable indicating that the defendant is black in the second row of Table 1. For violent crimes, this coefficient is roughly -\$140, suggesting that blacks' bail is slightly lower than whites after regression adjustment, which obviously does not support the hypothesis of discrimination against blacks.^{10, 11}

Such a simple approach to controlling for defendant differences is of limited use, for two reasons. First, even if the coefficient on the race dummy could otherwise be interpreted as the causal difference in average bail due to being black rather than white, there is the problem of unobserved heterogeneity. If judges observe heterogeneity in defendants that researchers cannot, and if this heterogeneity varies systematically with race conditional on the other covariates, then the race-dummy coefficient cannot be interpreted as the causal effect of defendant's race on bail.¹² Second, without unobserved heterogeneity, plausible behavioral models might not yield a simple linear regression model for bail. The simple behavioral model we analyze below generally does not, for example.¹³ Another approach would be to use more flexible matching methods. However, under our behavioral model, this method fails if blacks and whites with the same observable characteristics do not fail to appear with the same probability.¹⁴ For these reasons, we do not pursue either simple regression estimators or matching estimators further.

¹⁰There is no support for the hypothesis that blacks are favored, either, as the estimate's p-value against the two-sided null of no effect is 0.95.

¹¹In general, regression-adjusted differences are much smaller than raw differences. The conditional black-white difference in average bail amounts is significantly different from zero only in the case of drug crimes. While these findings make for interesting summary statistics, as argued above, they are not dispositive of the discrimination issue.

¹²This problem could be solved in principle via random assignment of defendant's race, but this solution seems impractical. Defendant's demeanor in court is one important source of heterogeneity observed by judges but not researchers. How would one assign race randomly conditional on such behavior? Audit studies based on written case files would have the related problem of lacking the realism of in-court events. Heckman and Siegelman (1993) discuss other issues related to audit studies.

¹³Interestingly, it can be shown that the judicial objective assumption that Ayres and Waldfogel (1994) assume, when married to our reduced form model of defendant's behavior, yields an optimal bail function that is affine in the heterogeneity term u and a potentially nonlinear function, g_2 , of the x characteristics observed by both judges and researchers. If u is assumed to be distributed identically across race and g_2 has the same constant term for both races, then a partial regression coefficient approach can be shown to identify a measure of discrimination in bail setting. However, the racial homogeneity assumptions on u and g_2 are both quite strong, and our method does not require them.

¹⁴Note that differences in the FTA probability could be due either to unobserved heterogeneity or to racial differences in the function mapping characteristics into FTA probabilities. Both these sources of difference in the FTA regression function violate the conditional independence function required by matching estimators.

3.3 Conventional outcome tests

The “FTA, given made bail” row of Table 1 reports the fraction of defendants who failed to appear as ordered, given that they made bail (notice that this means the risk set is endogenous to whether a defendant makes bail). Because we have data on this variable, we can use Ayres and Waldfogel (1994) outcome test without requiring any of the bail-bond information they cleverly use. Table 1 shows that 10% of blacks who make bail fail to appear, by comparison to 14% of whites. While this difference is in the direction of anti-black bias according to AW’s test, it is not statistically significant: the standard error of the 3.8 percentage-point difference is 4.7 percentage points. Thus, among those charged with violent offenses, we would fail to reject the null hypothesis of no discrimination against blacks using AW’s outcome test. Interestingly, among those charged with drug offenses, blacks actually FTA more often than whites—25% by comparison to 20%. The AW outcome test would thus conclude that for those accused of drug crimes, blacks are favored in bail-setting relative to whites. However, this difference is again statistically insignificant.¹⁵ We discuss the relationship between our approach and conventional outcome tests in more detail in section 7.

4 Model

4.1 Basic setup

We assume the judge’s objective is to minimize social costs, defined momentarily, and we consider the stage of the case when her only choice variable is b , the amount of bail that she sets. Let $\theta = (x, u)$ be a signal concerning the defendant. We assume that the judge can observe this entire signal, while researchers generally can observe only x . We define the conditional probability that the defendant makes bail, given the bail amount b , as $M(b, \theta)$. For each θ , we assume there exists some b^{\max} such that this probability is strictly decreasing for all $b \in [0, b^{\max}]$, with $M(0, \theta) = 1$, $M(b, \theta) \in (0, 1)$ for all $b \in (0, b^{\max})$ and $M(b, \theta) = 0$

¹⁵Even accepting this method for testing discrimination, though, the results just described are for a sample of defendants pooled across several different counties. Judges in different counties might be differently prejudiced, so we do our main analysis separately by county below.

for all $b \geq b^{\max}$. Since defendant's wealth is finite, it is always mathematically feasible for the judge to choose a bail level sufficiently great that the defendant cannot make bail.

If the defendant does not make bail, then the state will incur pecuniary jailing cost P ; this cost does not vary with the amount of bail the judge chooses. In addition, the defendant will lose his freedom. We denote with V the value the judge places on the defendant's lost freedom.¹⁶ We define $J = P + V$ as the judge's perceived total jailing cost, and we assume that P and V each are (1) unaffected by the level of bail the judge sets, and (2) constant within race-county-offense-type cells.¹⁷ In addition, we assume that P does not vary across race given the county and offense category. Thus while Cook and Dallas counties might face different pecuniary jailing costs for defendants charged with a given offense, and while Cook might face different jailing costs for defendants charged with violent and property crimes, no county's pecuniary jailing cost varies across defendants charged with the same type of crime.

We model race-based discrimination as occurring when a judge values defendants' lost freedom differently as a direct function of defendants' races. This means that a judge who values blacks' lost freedom less than whites' has $V^{\text{blacks}} < V^{\text{whites}}$, and vice-versa. Given the definitions and assumptions above, racially unbiased judges will have the same J for defendants charged with the same type of offense, regardless of race. By contrast, racially biased judges will have lower J for the type of defendants against whom they are biased. Intuitively, a judge who discriminates against blacks in bail setting treats them as if the costs of jailing them are lower than the costs of jailing whites, other things equal.

We define $S(b, \theta)$ as the expected social costs due to bad acts that a type- θ defendant will commit if released on bail. We assume that the conditional-on-release social costs function can be written

¹⁶For an excellent model and estimation of the value of freedom, see Abrams and Rohlfs (2010).

¹⁷A more complex and ambitious model would allow the costs of lost freedom to include the opportunity cost of defendants' lost earnings, which would vary across defendants. We ignore this issue for tractability, and also because we do not have earnings data.

$$\begin{aligned}
S(b, \theta) &= c_1 F(b, \theta) + c_0 [1 - F(b, \theta)] \\
&= c_0 + (c_1 - c_0) F(b, \theta),
\end{aligned} \tag{1}$$

where $F(b, \theta)$ is the probability that a type- θ defendant fails to appear given bail level b , c_1 is the expected social cost of bad acts for a released defendant who fails to appear, and c_0 is the corresponding expected social cost for a defendant who does appear.

The expected cost c_0 will be positive if some defendants would both show up for trial and also commit crimes while awaiting trial. In our sample, the fraction of released defendants who are ever rearrested pending trial is roughly 6 percent among those who do not fail to appear, which suggests that c_0 is nonzero. By comparison, roughly 20 percent of those who do fail to appear are ever arrested.¹⁸ Thus, it is reasonable to believe that c_1 is considerably greater than c_0 .

In the econometric model below, J , c_0 , and c_1 are not separately identified. However, we will be able to identify $R \equiv \frac{J-c_0}{c_1-c_0}$. This is sufficient to test for racial discrimination provided that P , c_0 , and c_1 do not vary across race within county-offense type cells. In this case, the black-white difference in R satisfies

$$\Delta R = \frac{1}{c_1 - c_0} (V^{\text{blacks}} - V^{\text{whites}}),$$

which will be negative when judges value blacks' freedom less than whites. If we also know c_1 and c_0 , then we can estimate the black-white difference ΔV using $(c_1 - c_0)\Delta R$. Finally, if we know P , c_0 , and c_1 , then we can also estimate the levels of V^{blacks} and V^{whites} . Below we use rough estimates of these three parameters to estimate the V parameters. We stress that even if one does not accept our specific estimates of P , c_0 , and c_1 , we can still estimate the sign of ΔR , which is a meaningful test of discrimination, as long as P , c_0 , and c_1 are

¹⁸For whites in our sample, the shares ever rearrested are 5.48 percent (out of 347 who do not FTA) and 19.75 percent (out of 81 who do FTA). For blacks in our sample, the shares are 6.59 percent (out of 516 who do not FTA) and 22.22 percent (out of 117 who do FTA).

constant across race.

To make palatable the various assumptions necessary to interpret R meaningfully, we estimate separate values of R for each county-offense category cell. Since we have two racial groups, five counties, and four offense categories, this means we estimate 40 values of R in total.

4.2 The optimal level of bail

The judge has three options: (i) release the defendant on his own recognizance, i.e., set bail equal to zero, (ii) set bail at or above b^{\max} so as to prevent release,¹⁹ and (iii) set bail somewhere in the middle, allowing the defendant probability $M(b, \theta) \in (0, 1)$ of obtaining his release. Because they are the only ones we study empirically, we focus here only on interior-optimum cases. This focus poses no selection problems, provided that our econometric model is otherwise specified correctly.

Suppressing the θ parameter, the judge's decision problem is to

$$\begin{aligned} \min Q(b) &\equiv M(b)\{F(b)c_1 + [1 - F(b)]c_0\} + [1 - M(b)]J \\ &= M(b)[(c_1 - c_0)F(b) - (J - c_0)] + J. \end{aligned}$$

Since $c_1 > c_0$ and J does not depend on b , we can focus attention on the increasing affine transformation of $Q(b)$ given by

$$q(b) \equiv M(b)[F(b) - R], \tag{2}$$

where we recall that $R \equiv \frac{J - c_0}{c_1 - c_0}$. Differentiating with respect to b yields the first-order condition

¹⁹We think of denying bail as setting $b = \infty$. The statutes discussed above generally suggest that judges should not do so without good reason, however. For example, Texas's statute states that "The power to require bail is not to be so used as to make it an instrument of oppression." Texas C.C.P. Art. 17.15.

$$M_b(b^*)[F(b^*) - R] + M(b^*)F_b(b^*) = 0. \quad (3)$$

We assumed above that $M_b < 0$. Because the defendant will lose his bail when he fails to appear, we also assume that $F_b < 0$ for all b . Given that $M(\bullet)$ is interior to $[0, 1]$ for an interior solution, the second term in (3) must be negative. Therefore, the first term must be positive, so $F(b^*) < R$ at the optimal bail level. We now turn to our econometric specification.

5 Econometric Specification

5.1 The basic econometric model

Let $1(\bullet)$ be the indicator function, and let $D_m = 1(\text{defendant makes bail})$ and $D_f = 1(\text{defendant fails to appear})$. Because the defendant cannot fail to appear if he does not make bail, there are only three possible outcomes to consider:

0. A defendant does not make bail: $D_m = 0$.
1. He makes bail and does not fail to appear: $D_m = 1$ and $D_f = 0$.
2. He makes bail and fails to appear: $D_m = 1$ and $D_f = 1$.

Where appropriate, we will use subscripts “0” to refer to the first event, “1” to refer to the second, and “2” to refer to the third. Additionally, we define the discrete dependent variable D that takes on values in $\{0, 1, 2\}$ given each outcome, so that

0. $\Pr(D = 0) = \Pr(D_m = 0)$
1. $\Pr(D = 1) = \Pr(D_m = 1 \text{ and } D_f = 0)$
2. $\Pr(D = 2) = \Pr(D_m = 1 \text{ and } D_f = 1)$

We assume that for a defendant with bail level b and characteristics (x, u) , latent utility in each of the three states just defined can be written as

$$\begin{aligned}
 V_0 &= \gamma_0 \ln b + g_0(x) + \epsilon_0 \\
 V_1 &= \epsilon_1 \\
 V_2 &= \gamma_2 b + g_2(x) + u + \epsilon_2.
 \end{aligned}
 \tag{4}$$

Thus, the event that $D = 0$ is the event that $V_0 > \max[V_1, V_2]$; the event that $D = 1$ is the event that $V_1 > \max[V_0, V_2]$; and the event that $D = 2$ is the event that $V_2 > \max[V_0, V_1]$. In (4), we have normalized to zero the systematic part of utility in the $D = 1$ case (defendant makes bail and doesn't FTA). As usual, such a normalization is required since only the difference of parameters across equations is generally identified in multinomial choice settings. We have also used general notation to allow maximum flexibility in the way the covariate vector x enters V_0 and V_2 . Conventional approaches involve intrinsically linear parameterizations of the functions g_0 and g_2 . Given that we estimate choice probabilities nonparametrically, however, we do not need to impose functional forms on g_0 and g_2 .

Since higher bail levels increase the probability of not making bail, we expect but do not impose that $\gamma_0 > 0$. The bail level enters V_0 in log form in order to satisfy the intuitive requirement that defendants should make bail with probability one when $b = 0$, in which case the latent utility of not making bail is $-\infty$. Defendants will not always fail to appear when bail is 0, though, so we assume that bail enters linearly into V_2 , the made-bail/didn't-FTA choice's utility. Since higher bail should serve as a deterrent to FTA among those who do make bail, we expect but do not impose that $\gamma_2 < 0$. We assume that u has mean zero in the population, which is innocuous given the presence of the nonparametric function g_2 .²⁰

Finally, consider ϵ_0 , ϵ_1 , and ϵ_2 , which are the components of indirect utility that are "totally unobserved," in that neither we nor the judge can observe them. We assume that

²⁰Another point concerns our exclusion of u from V_0 . This exclusion is not just a normalization, as it would be if u appeared in V_1 as well as V_2 . We believe that we could address this concern by allowing for at least some function of u to enter V_0 . We have not proved the model's identification under such an extension. We note in addition that our empirical method would be unaffected by this extension, i.e., it would simply extend the domain of models for which our empirical work would be appropriate.

these terms are distributed jointly as Type I Extreme Value. Under this assumption, the model has the usual multinomial logit (MNL) structure, conditional on b , x and u . As we discuss below, this MNL assumption is critical to our approach, even when we estimate choice probabilities nonparametrically. Note though, that from the perspective of researchers, the presence of heterogeneity in u means that the model does not have a multinomial logit form.

5.2 Estimation

The only unconventional aspect of (4) is the presence of the term u in the specification of V_2 . This term represents any defendant-specific heterogeneity that can be observed by bail-setting judges but not by us. For example, judges can observe a defendant’s behavior in the courtroom during the hearing, which we cannot. Observable unruliness in court might be a good signal that a defendant is unlikely to appear in court except when he faces a high bail level. Good behavior plausibly has the opposite signal value. Other aspects of the defendant’s background that we cannot observe might also be observable to the judge; examples include community ties, having a job, and so on.

Given that judges choose bail according to the first order condition (3), observed bail will be a function of all the other elements of the model: (γ_0, γ_2) , the vector x ,²¹ the functions g_0 and g_2 , and the unobservable heterogeneity term u . First consider the case when $u = 0$ for all observations, so that judges observe nothing more than we do. Then optimal bail may be written as $b = b^*(x; \gamma_0, \gamma_2, g_0, g_2)$. The fact that judges choose the level of bail to minimize expected social costs would have no bearing on estimation via maximum likelihood in this

²¹The variables that we include in x in our empirical work are based on the background variables summarized in Table 1. We discuss these variables in detail in appendix C. We note here that we do not include information about attorney type in the estimation. While we would like to do so, we found that observations with missing attorney-type differed systematically from those with non-missing data. For example, those missing attorney-type data are much more likely to come from Broward than from other counties; are 14 percentage points more likely to FTA for blacks, but only 3 points more likely for whites (these figures are conditional on having made bail); are 30 points more likely to make bail for whites and 36 points more likely to make bail for blacks; and have lower mean bail amounts by more than \$4,000 for whites, and more than \$7,000 for blacks. Perhaps as a consequence, we found that our results were sensitive to restricting estimation to those observations with non-missing attorney-type data (whether or not we used attorney type as an x variable). Under the circumstances, we believe the best approach is to disregard attorney-type data. We note, however, that estimates using the sample of those with nonmissing attorney-type data yielded lower magnitudes for R , though most were still negative. We note as well that we became aware of the availability of attorney-type data only after circulating earlier versions of this paper that contained the estimation sample and x choice on which we focus in the paper’s present version.

case, provided that we and the judge both know g_0 and g_2 . Intuitively, in this case we can account for all features of the problem that judges do. More formally, when we conditioned on x in forming the likelihood function, we would implicitly also be conditioning on bail, since optimal bail depends only on x and fixed parameters of the model given $\text{Variance}(u) = 0$. Conditioning on observed bail thus would add no further information and so could not cause any problems for estimation.

With heterogeneity in u , the optimal bail function now depends on both x and u , i.e., $b = b^*(u, x; \gamma_0, \gamma_2, g_0, g_2)$. So conditioning on x is no longer sufficient to condition on b . If we observed u , we could condition on it as well in forming the likelihood, and we could use maximum likelihood estimation, given parameterizations of g_0 and g_2 . This avenue is blocked since we cannot observe u , though, and the likelihood function will be mis-specified if we ignore u . Moreover, since bail is chosen optimally as a function of x and u , there will be dependence in the joint distribution of b , x and u . The structure of this dependence will be functionally unknown even given knowledge of g_0 and g_2 . This makes otherwise attractive methods like Mroz's (1999) discrete-factor approach problematic, and possibly intractable.

An alternative approach would be to specify a distribution for u and use numerical methods to simulate the likelihood. One could then use grid-search methods to calculate the optimal bail directly for each observation as a function of R and then choose R either to minimize a criterion function or to match moments of the data. An advantage of this class of approaches is that it would allow us to drop the Type I Extreme Value assumption on the joint distribution of $(\epsilon_0, \epsilon_1, \epsilon_2)$ given (x, u) . However, this approach would also require us to specify a specific conditional distribution of u , given x . We regard mis-specification of this latter distribution as a greater concern than mis-specification of defendant preferences.

Our approach is motivated by the fact—which we prove in appendix A—that the level of optimal bail is monotonically increasing in the heterogeneity term u under the Type I Extreme Value assumption. Intuitively, a defendant with higher u has higher utility of failing to appear and is thus more likely to FTA at a given bail level. A rational judge might therefore respond to a ceteris paribus increase in u by increasing the level of bail. This result must be proved, though, because when the judge increases the bail level, probability shifts from the event that a defendant makes bail and does not FTA, to the event that he does

not make bail in the first place. This increase in the probability of not making bail brings higher expected jailing costs along with the increase in bail. The judge’s optimal response to an increase in u thus involves costs on both sides of the ledger.

Given the multinomial logit functional form that we assume, it can be shown that, given x , every optimal bail level maps to a unique value of u . More precisely, suppressing notation related to γ_0 , γ_2 , g_0 , and g_2 , let $b^*(u; x)$ be the optimal bail level given (x, u) . In Lemma 1 of appendix A, we show the unique-mapping result that there exists a function h such that $u = h(b^*(u; x); x)$.

If we could find a closed form representation of the function h known up to a finite parameter vector, then we could use a standard control-function approach, substituting $h(b; x)$ for u and then estimating the parameters of the defendant’s multinomial choice decision using maximum likelihood.²² However, inspection of the first order condition, as well as some fruitless attempts on our part, suggests that this approach is unlikely to work in general.

Instead, we use the unique-mapping lemma. This result implies that at the optimal bail level b^* , conditioning on (x, b^*) alone is equivalent to conditioning on (x, b^*, u) . To see this, observe that the information set $\{x, b^*\}$ is sufficient for the information set $\{x, b^*, h(b^*; x)\}$, given $(\gamma_0, \gamma_2, g_0, g_2)$. Therefore, knowing $\{x, b^*\}$ is as good as knowing $\{x, b^*, u\}$.²³

Define $m(b, x) \equiv \Pr[D_m = 1|x, b]$ and $f(b, x) \equiv \Pr[D_m = 1, D_f = 1|x, b]$. These functions differ from their capital-letter counterparts in that the lower-case functions do not involve conditioning on u . By the unique-mapping lemma, however, for each $d \in \{0, 1, 2\}$, the conditional probability that the defendant chooses $D = d$ at the judge’s optimum, given (x, b^*) , satisfies the following ignorability condition:

$$\begin{aligned} \Pr [D = d|x, b^*] &= \Pr [D = d|x, b^*, h(b^*; x)] \\ &= \Pr [D = d|x, b^*, u]. \end{aligned} \tag{5}$$

²²For example, if we happened to know that $h(x, b; \gamma_0, \gamma_2, g_0, g_2)$ were a quadratic function of x and b , then we could simply enter a fully interacted second-order polynomial in these variables as regressors and then estimate the model by maximum likelihood. The resulting reduced form parameter estimates could then be used to back out estimates of $(\gamma_0, \gamma_2, g_0, g_2)$.

²³Note that this result does not generally hold for non-optimal levels of bail: if b' is not the optimal bail level given (x, u) , then $u \neq h(b'; x)$, so $\{x, b', u\}$ contains more information than $\{x, b'\}$.

As a result, it follows that

$$m(b^*, x) = M(b^*(u; x), x, u) \quad \text{and} \quad f(b^*, x) = F(b^*(u; x), x, u). \quad (6)$$

Given knowledge of the joint distribution of (b, x) , the functions $m(b, x)$ and $f(b, x)$ are nonparametrically identified, even without knowing anything about u or its distribution. Under the assumption that judges choose bail optimally, the relevant joint distribution involves the optimal bail level, i.e., (b^*, x) . From observable information, then, we can identify $m(b^*, x)$ and $f(b^*, x)$. Thus, under the assumption that the judge chooses bail optimally, $M(b^*(u; x), x, u)$ and $F(b^*(u; x), x, u)$ are nonparametrically identified without any knowledge of u or its distribution. We establish this fact formally in Theorem 1, stated and proved in Appendix A.²⁴

The next step in demonstrating identification of R is to show how to rewrite the judge's first-order condition entirely in terms of the optimal bail level and the conditional choice probabilities $m(b^*, x)$ and $f(b^*, x)$. This is possible using the well known fact that derivatives of multinomial logit conditional choice probabilities are fully determined by the choice probabilities themselves. Observe that for any (b, x, u) the conditional probability of not making bail is

$$M(b, x, u) = \Pr[D_m = 1|x, b, u] = \frac{1 + \exp[\gamma_2 b + g_2(x) + u]}{1 + \exp[\gamma_0 \ln b + g_0(x)] + \exp[\gamma_2 b + g_2(x) + u]}. \quad (7)$$

The probability of failing to appear given that a defendant makes bail, $\Pr[D_f = 1|x, b, u, D_m = 1]$, may be written

²⁴For theoretical completeness, we also establish identification of (γ_0, γ_2) , as well as point identification of the function $g_0(x)$. The function $g_2(x)$ is not identified, although the sum $g_2(x) + u$ is. In addition, it is possible to identify differences in u across two observations having the same value of x . We do not use these identification results in our empirical work, however.

$$\begin{aligned}
F(b, x, u) &= \Pr[D_f = 1 | x, b, u, D_m = 1] \\
&= \frac{\Pr[D_m = 1 \text{ and } D_f = 1 | x, b, u]}{\Pr[D_m = 1 \text{ and } D_f = 0 | x, b, u] + \Pr[D_m = 1 \text{ and } D_f = 1 | x, b, u]} \\
&= \frac{\exp[\gamma_2 b + g_2(x) + u]}{1 + \exp[\gamma_2 b + g_2(x) + u]}. \tag{8}
\end{aligned}$$

Differentiating (7) and (8) partially with respect to the bail level and engaging in a large amount of tedious algebra, it can be shown that for any (b, x, u) ,

$$M_b(x, b, u) = \left[\gamma_2 F(b, x, u) - \frac{\gamma_0}{b} \right] [1 - M(b, x, u)] M(b, x, u), \tag{9}$$

and

$$F_b(b, x, u) = \gamma_2 F(b, x, u) [1 - F(b, x, u)]. \tag{10}$$

By the ignorability condition (5), all conditioning on u is extraneous in the probabilities on the left hand side of (7) and (8) at the optimal bail level $b^*(u; x)$. Thus, we can replace the probabilities $M(b^*, x, u)$ and $F(b^*, x, u)$ in (7), (8), (9) and (10) with $m(b^*, x)$ and $f(b^*, x, u)$. Fixing x and u and writing $m^* = m(b^*, x)$ and $f^* = f(b^*, x)$, it follows that (9) and (10) can be re-written at their optimal values as²⁵

$$M_b(b^*, x, u) = \left(\gamma_2 f^* - \frac{\gamma_0}{b^*} \right) (1 - m^*) m^* \tag{11}$$

$$F_b(b^*, x, u) = \gamma_2 f^* (1 - f^*), \tag{12}$$

Plugging these first derivatives' values into the first-order condition (3), we now have as the necessary condition for optimality that

²⁵For notational ease, we leave implicit the empirically important fact that m^* and f^* vary with x .

$$m^* \gamma_2 f^* (1 - f^*) + \left[\gamma_2 f^* - \frac{\gamma_0}{b^*} \right] (1 - m^*) m^* \{f^* - R\} = 0. \quad (13)$$

This form shows that for given values of γ_0 , γ_2 , and R , the first-order condition can be written in closed form as a function of the optimal bail level and the identified conditional choice probabilities m^* and f^* . Dividing (13) by γ_2 , moving terms to the right-hand side, and regrouping then yields

$$m^* f^* (1 - m f^*) = \left[\frac{(1 - m^*) m^* f^*}{b^*} \right] \frac{\gamma_0}{\gamma_2} + \left[(1 - m^*) m^* f^* \right] R + \left[\frac{(1 - m^*) m^*}{b^*} \right] \left(-\frac{\gamma_0}{\gamma_2} \times R \right). \quad (14)$$

Equation (14) can then be rewritten as the auxiliary relationship

$$L = W_1 \delta_1 + W_2 \delta_2 + W_3 \delta_3, \quad (15)$$

where the auxiliary variables L , W_1 , W_2 , and W_3 are given by

$$\begin{aligned} L &= m^* f^* (1 - m f^*) \\ W_1 &= \left[\frac{(1 - m^*) m^* f^*}{b^*} \right], \\ W_2 &= [(1 - m^*) m^* f^*] \\ W_3 &= \left[\frac{(1 - m^*) m^*}{b^*} \right], \end{aligned}$$

and the auxiliary coefficients are given by

$$\delta_1 = \frac{\gamma_0}{\gamma_2}, \quad \delta_2 = R, \quad \delta_3 = -\frac{\gamma_0}{\gamma_2} \times R. \quad (16)$$

Letting subscript i index individual observations, assume momentarily that for observed (b_i, x_i) and unobserved u_i , we observe the true probabilities $F_i = F(b_i, x_i, u_i)$ and $M_i = M(b_i, x_i, u_i)$, regardless of whether bail is set optimally. Since each of these probabilities depends only on defendant behavior, knowing these values does not itself tell us anything about whether judges do choose bail in the way we have assumed. In other words, there is no guarantee that the observed bail level b_i is the solution to (3). If it isn't, then as we discussed above, (14) won't generally hold when we replace f_i^* and m_i^* , the optimal values given (x_i, u_i) , with their observed counterparts F_i and M_i . Given that judges actually do choose bail optimally, though, we have seen that $F_i = f_i^*$ and $M_i = m_i^*$. In that case, R could be estimated by calculating δ_2 , the coefficient on W_2 , via least-squares estimation of equation (15).

In addition, from (16) we see that when the model is correctly specified, so that the probabilities F_i and M_i are optimal, we have three functions of the two unknowns R and $-\gamma_0/\gamma_2$. Thus with knowledge of the true probabilities F_i and M_i , we could test whether judge behavior is consistent with optimal bail setting given the objective function we have assumed. To test the null hypothesis of correct specification, we would use estimates of the delta coefficients to evaluate whether $H_0 : \delta_1 \times \delta_2 + \delta_3 = 0$ can be rejected given the estimated coefficients $\widehat{\delta}_1$, $\widehat{\delta}_2$, and $\widehat{\delta}_3$, together with their estimated sampling distributions.

In practice, we do not know the true values of F_i and M_i . Provided that we can estimate them consistently, though, we can still estimate the sample analog of (15),

$$\widehat{L} = \widehat{W}_1 \delta_1 + \widehat{W}_2 \delta_2 + \widehat{W}_3 \delta_3 + v, \quad (17)$$

where hats denote estimated quantities and we replace M_i and F_i with their observable-at-the-optimum counterparts m_i and f_i . Thus, for example, the empirical analog of W_1 for observation i is $\widehat{W}_{1i} = b_i^{-1}[1 - \widehat{m}(b_i, x_i)]\widehat{m}(b_i, x_i)\widehat{f}(b_i, x_i)$. The residual v is due to estimation

error of \widehat{F}_i and \widehat{M}_i .²⁶

To estimate the conditional choice probabilities $m(b_i, x_i)$ and $f(b_i, x_i)$, we use non-parametric, generalized product kernel regression methods proposed by Hall, Racine and Li (2004) and implemented in the **R** software environment via the **np** package by Hayfield and Racine (2008). We discuss relevant details of the nonparametric estimation in Appendix C. For the main text, we simply take as given that we have consistent estimates \widehat{m}_i and \widehat{f}_i of the conditional probabilities $m(b_i, x_i)$ and $f(b_i, x_i)$ for each observation i . To account for sampling variation due to the estimated nature of the variables in auxiliary equation (17), we use the bootstrap, using nonparametric Monte Carlo re-sampling.²⁷

Before we turn to our econometric results, we emphasize the critical identifying role of our assumption that the unobserved heterogeneity term u is scalar. As discussed, our approach is robust to unobserved heterogeneity because the heterogeneity adds no information at the optimal bail level. If there were multiple heterogeneity dimensions, then this robustness would no longer hold, because bail optimality would no longer be sufficient to hold constant all unobserved heterogeneity. Mathematically, the problem is that we have access to only a single first-order condition to pin down unobserved heterogeneity. This is an important limitation of our approach (one that is shared by Olley and Pakes’s (1996)). Our primary defense against the possibility of multi-dimensional unobserved heterogeneity is the fact that our empirical results do not reject overidentifying restrictions, as discussed below (the Olley and Pakes (1996) model also allows for overidentifying testing).

²⁶Estimates of the δ parameters based on (17) are consistent provided that \widehat{f}_i and \widehat{m}_i are consistent for F_i and M_i . Note that this is true even though functions of b_i , F_i , and M_i appear on both sides of equation (15). We make no claim to estimating any type of partial or causal effect of (W_1, W_2, W_3) on L , whatever that would even mean in this context. We are simply using the fact that, at the optimum under correct model specification, these variables must be related as discussed above. Only the linear projection relationship between these functions of the data are needed, and this relationship is precisely what can be estimated consistently using least-squares estimation of (17).

²⁷On each resample, we recalculate the estimated conditional choice probabilities and use these new estimates to re-estimate (17). We do not re-estimate the bandwidths used to generate kernel density estimates, due to the associated computational burden of re-doing cross-validation for bandwidth selection many times.

6 Econometric Results

We first report results computed while pooling over offense categories, within counties. This is not our main focus, as we want to allow separate values of c_0 and c_1 by county-offense type cells. However, these results allow us to explain the main features of the empirical results with only ten race-by-county cells, simplifying the exposition. We then turn to our main focus and discuss results for county-by-offense type cells.

6.1 Results pooled within county

The top three rows of Table 2 report estimates of the three δ parameters by county and race. These estimates come from OLS estimation of (17), with the estimates of the auxiliary variables constructed using nonparametric estimates of the choice probabilities, as discussed above. Estimates of δ_1 are negative in seven of the ten county-race cells, as predicted by the model, though most are not significant using conventional critical values.²⁸

Under correct specification of the model, δ_2 equals R , defined above. Estimates of this parameter are positive in all ten cases, as predicted, and are highly significantly different from zero in all cases.²⁹ Even though our main focus is on black-white differences in R , we defer substantive discussion of these estimates, because R is overidentified in (17), as we discuss in detail below. The third row of Table 2 reports our estimates of δ_3 from OLS estimation of (17). Nine of these ten estimates are positive, as expected, and most are significantly different from zero using conventional critical values.

The fourth row in Table 2 reports estimates of the statistic $T \equiv \delta_1\delta_2 + \delta_3$. If the model is correctly specified and judges determine bail optimally given their preferences concerning value of defendants' freedom and the social cost of an FTA, then T should equal zero up to sampling error. The county- and race-specific estimates of T show that in general, this statistic is imprecisely estimated and is by itself generally insignificantly different from zero. The following two rows of the table report estimated p-values testing the null hypothesis

²⁸While the scale of δ_1 is difficult to interpret, we note that the estimates display relatively great variation across county-race cells. Thus it appears that the lack of statistical significance in these estimates is due to large sampling variance, rather than to persistent small estimates.

²⁹All these point estimates are also greater than one, which implies $F(b^*) < R$, as the model predicts; see the discussion in section 4.2 on this point.

that T equals zero for the given county-race combination. The first set of p -values, labeled “Conventional,” are computed in the usual way, ignoring the fact that we have multiple estimates. The lowest of these estimated p -values is 0.026, for blacks in Broward county. Taken by itself, this estimate suggests that the model’s restrictions are rejected.

However, because we have 10 county-race cells, it is important to use a test that accounts for multiple comparisons. One common and simple approach to adjusting test size to account for multiplicity of comparisons is to use the Bonferroni correction. In our context, this means dividing the desired familywise significance level by 10. The lowest p -value in the table is 0.026, for blacks in Broward county. These results suggest that any significance level below 0.26, we should not reject the null hypothesis for any county-race combination.

The Bonferroni correction does come with a loss of power. As an alternative, we use the bootstrap distribution of our coefficient estimates to estimate uniform critical values that can be used directly in place of the Bonferroni tests. Let $s = \min_{\{c,r\}}(T^{cr})$ be the minimum estimated value of the 10 test statistics, indexed by c for county and r for race. Similarly, let $S = \max_{\{c,r\}}(T^{cr})$ be the maximum estimated value of the 10 test statistics. Given a test-statistic estimate T^{cr} , we estimate the uniform, equal-tailed p -value for testing the null hypothesis $T^{cr} = 0$ against the two-sided alternative $T^{cr} \neq 0$ as either $Pr(S < T^{cr})$, when $T^{cr} < 0$, or $Pr(S > T^{cr})$, when $T^{cr} > 0$; we use the empirical bootstrap distribution of S to evaluate these probabilities.³⁰ Estimated uniform p -values appear in the final row of Table 2. The lowest such estimate, again for Broward blacks, is 0.18. This result indicates that there is little reason for concern that the model is inconsistent with the data.

Next we consider black-white differences in estimated R values. As noted above, we could simply use our estimates of δ_2 , since this parameter equals R under correct specification. Because $\delta_1\delta_2 + \delta_3 = 0$ under correct specification, though, we also have $\delta_3/\delta_1 = R$ under correct specification. Thus, two distinct estimates of R will be implied by any set of estimates of (17) from a finite sample.

³⁰It is important to impose the null hypothesis where possible in using bootstrap tests. To do so, we simply subtract, from each bootstrap replication of T^{cr} , the mean of the bootstrapped value of this statistic. To deal with scale differences in the sampling distributions of T^{cr} for various county-race cells, we also divide each statistic by our bootstrap estimate of its standard deviation. This scaling leaves all draws with marginal standard deviation one while preserving the dependence structure across county-race cells. We re-center and re-scale the real-data realization of T^{cr} in the same way for purposes of computing p -values.

Given that the specification tests reported in Table 2 do not reject our model’s constraint that $T = \delta_1\delta_2 + \delta_3 = 0$, we re-estimate the auxiliary regression model with this constraint imposed. Such constrained estimation allows us to obtain a unique estimate of R for each county-race cell. We impose the constraint by using nonlinear least squares (NLS) to estimate the model

$$\widehat{L} = \widehat{W}_1 R + \widehat{W}_2 \frac{\gamma_0}{\gamma_2} + \widehat{W}_3 \left(-\frac{\gamma_0}{\gamma_2} \times R \right) + v, \quad (18)$$

where the parameters to be estimated are R and $\frac{\gamma_0}{\gamma_2}$.

We report these constrained NLS estimates in Table 3. All 20 estimates reported in the table have the expected sign. Of the ten estimates of γ_0/γ_2 , eight are statistically significantly different from zero. All ten county-by-race estimates of R are statistically significant at any conventional level using uniform tests. These results suggest that the model’s predictions given optimal bail setting are largely borne out by the data.

Of course, our primary interest is in testing for racial animus on the part of judges, via estimates of R . Consider Table 3’s estimates for R of 1.27 and 2.30 for blacks and whites in Broward county, respectively. Interpreted through our model’s lens, this estimate tells us that Broward judges set bail as if $(J - c_0)/(c_1 - c_0) = 1.27$ for blacks, and $(J - c_0)/(c_1 - c_0) = 2.30$ for whites.

While there are obvious problems with these estimates, most notably that they ignore variation in c_0 and c_1 by offense type, we continue to discuss them for expositional reasons. To put these numbers in perspective, we adopt figures used in Abrams and Rohlfs (2010, henceforth, AR) to measure components of R ; doing so lets us back out an implied estimate of the value of lost freedom, V . Recalling that $J = P + V$, solving for V yields

$$V = (c_1 - c_0)R + c_0 - P. \quad (19)$$

AR use a figure of \$106 per day as the pecuniary cost of incarcerating a prisoner;³¹ this

³¹AR report a figure of \$9,500 for 90 days as the midpoint of those reviewed by Levitt (1996). Like the

figure corresponds to the daily value of our parameter P . We use an estimate of 82 days to estimate the number of days a defendant would spend incarcerated as a result of not making bail.³² Thus for our parameter P , we use $\$106$ per day \times 82 days, or a total of $\$8,692$.

Next we estimate c_0 and c_1 .³³ Following the discussion in AR’s section VI.D, we assume that the average social cost associated with each rearrest is $\$45,000$.³⁴ As noted in footnote 18 on page 14 above, the frequency of rearrest among defendants in our sample who do not FTA is roughly 6 percent, while the frequency of rearrest among those who do FTA is roughly 20 percent. Thus, we use estimates $c_0 = 0.06 \times \$45,000 = \$2,700$ and $c_1 = 0.20 \times \$45,000 = \$9,000$. As a result, we have $V = \$6,300R + (\$2,700 - \$8,692) = \$6,300R - \$5,992$. Our estimated R value of 1.27 for Broward blacks thus implies $V = \$2,009$. By contrast, our estimated R value of 2.30 for Broward whites implies $V = \$8,498$. Taken at face value, these point estimates would suggest that judges in Broward value 82 days of white defendants’ freedom $\$6,489$ more than they value 82 days of black defendants’ freedom, or roughly $\$80$

other dollar amounts Abrams and Rohlfs use, this figure is expressed in 2003 dollars.

³²The SCPS reports the number of days from arrest to adjudication. We found very large differences in this variable’s distribution according to whether a defendant makes bail. For example, among those who do not make bail, the median number of days between arrest and adjudication was 40, while it was 133 among the 1,497 defendants in our sample who do make bail. We use only those who did not make bail to approximate the expected number of days a defendant would spend incarcerated while awaiting trial if he did not make bail. To estimate the mean number of days between arrest and adjudication for these defendants, we need to account for the fact that the SCPS contains follow-up information for only about a year. Four observations in our sample of those who are held over for trial have a duration listed as “more than one year,” while an additional 47 observations have arrest-to-adjudication duration listed as pending. If we assume that these 51 right-censored cases closed at 366 days, the average number of days from arrest to adjudication would be 78.8. If we instead account for right-censoring by assuming that arrest-to-adjudication duration is distributed exponentially and use maximum likelihood estimation to estimate the exponential parameter, the result is -4.40. An exponential distribution with this parameter has mean $\exp(4.40) = 81.5$, so we use a mean duration of 82 days. This calculation ignores the fact that a small amount of time elapses between arrest and bail hearings, while a larger amount of time may pass before a defendant is able to gather bail or bond money. However, the average time between arrest (not bail hearing) and pre-trial release among those who do make bail is only about 9 days. Since only a bit more than 40 percent of our sample makes bail, the net effect of these biases is likely only a few days on average, so we ignore these issues.

³³In estimating c_1 , we ignore any social costs related to finding the defendant. We also ignore the fact that defiance of a valid judicial order to appear for trial is costly in the sense that it demonstrates disrespect for the criminal justice system and the courts. We ignore these aspects of c_1 partly because they would be difficult to measure. In addition, ignoring such costs is conservative in estimating the black-white difference in implied value of freedom, because this difference equals $(c_1 - c_0)$ times the black-white difference in R .

³⁴This calculation has two components. First, AR calculate using others’ cost estimates that in their sample, the average social cost of a crime that results in a rearrest is $\$5,790$. Second, AR note that results in the literature imply that “crimes resulting in arrest account for roughly 13% of the total cost of criminal victimizations.” Dividing $\$5,790$ by 0.13 yields $\$44,538$, which we round up to $\$45,000$.

more per day.^{35,36}

Next, we consider estimates for the black-white difference in R for all five counties. We report these estimates in the top row of Table 4. The difference for Broward is -1.03, reflecting the just-discussed Broward R estimates of 1.27 for blacks and 2.30 for whites. The estimated standard error of 0.50 yields an asymptotic z -ratio of 2.06. The associated conventional p -value is 0.020 for testing the null hypothesis of zero difference in R against to the one-sided alternative of anti-black discrimination. However, we again must adjust the p -value to account for the fact that we have estimated the black-white difference in R for five counties. To do so, we repeated the uniform p -value procedure described above, calculating the minimum value of ΔR^c over the five counties, indexed by c , for both the actual data and our 999 bootstrap replications. Our p -value estimate is given by the fraction of bootstrap replications having a more negative minimum value of ΔR^c than the minimum value observed for the real data. The resulting p -value for Broward county is 0.108, considerably higher than the conventional level.

Black-white differences in R estimates are negative for the other counties but Los Angeles. For Cook and Harris counties, we cannot reject the null hypothesis of zero difference. For Dallas, though, the estimated black-white difference in R of -0.75 has an asymptotic z -ratio of 2.34. The corresponding uniform p -value is 0.054, suggesting significant evidence of discrimination against blacks in Dallas based on our pooled estimates.

In generating the estimates just discussed, we assumed that R is constant across all offense types. This assumption might be plausible for J alone, if jailing-security costs do not vary with offense type. But, as we discussed above, we wish to allow judges' perceived social costs of FTA, c_0 and c_1 , to vary with the type of offenses with which a defendant is

³⁵The estimate of $V = \$2,009$ for blacks is roughly twice AR's estimated average value of freedom of $V = \$1,050$. The estimate for whites is much higher than their point estimate. This difference may stem from the very different estimation approach AR take. In addition, AR's estimates of social costs of releasing a defendant pending trial are imprecise, as are some of their estimates of V when AR allow them to differ by recommended bail level, which should map roughly into offense type category. Thus, the usefulness of comparing our results to theirs may be limited.

³⁶Ideally, we would use county-race-specific cross-FTA differences in rearrest-rate figures here, and county-offense-type-specific figures below. However, because only about 40 percent of our sample makes bail, the number of rearrested defendants is sufficiently small that this approach would yield little more than noise. Thus while our estimates of R are robust to variation in the parameters c_1 and c_0 across county and offense type, for purposes of doing back-of-the-envelope estimates of V , we will assume that these parameters always take the values discussed in the text just above.

charged. We now turn to estimates that allow R to vary with offense type.

6.2 Estimating R separately by county and offense type

In this section, we re-calculate our estimates allowing R to vary by category of most serious offense, while holding constant $\delta_2 = \gamma_0/\gamma_2$.³⁷ To do so without imposing the constraint that $\delta_1\delta_2 + \delta_3 = 0$, we simply re-estimate (17) allowing separate values of δ_1 and δ_3 for each offense category. That is, we interact \widehat{W}_1 and \widehat{W}_3 from (18) with a full set of dummies indicating offense category. With no constant included in the estimation, the resulting coefficients represent offense category-specific δ_1 and δ_3 estimates for each county-race cell. We can use these category-specific estimates together with the category-invariant estimate of δ_2 to construct category-specific test statistics analogous to T for each of the four categories we consider: drug, violent, property, and public order offenses.

As with the pooled estimates above, we calculated a series of both conventional and uniform p-values for model specification testing. The conventional p-values are based on χ^2 tests of the null hypothesis that both $T^{\text{black}} = 0$ and $T^{\text{white}} = 0$ hold, against the two-sided alternative that these equalities do not hold. All but one of these estimated p-values exceeded conventional significance levels. The exception, for those charged with violent offenses in Broward county, yielded a conventional p-value of 0.06. Since there are twenty county-offense cells, the conventional p-values are surely downward-biased. After estimating uniform p-values based on a method similar to that described above, we found that all p-values exceeded conventional significance levels. Thus, there is little reason for concern that the model with interactions is mis-specified.

The foregoing results suggest that the data are consistent with optimal bail setting given our model. Thus, we now consider offense category-specific estimates of the black-white difference in R calculated when we impose the correct-specification constraint that $\delta_1\delta_2 + \delta_3 = 0$. As above, we impose this constraint by re-estimating a version of (18) with nonlinear least squares. We estimate separate R and γ_0/γ_2 values for each offense category by interacting \widehat{W}_1 and \widehat{W}_3 with four offense category dummies, as in the unconstrained interaction just

³⁷The ratio γ_0/γ_2 involves defendant preferences rather than judicial ones. While we do not allow this ratio to differ by offense type, we do allow it to vary by race and county.

described; we estimate five versions of this model, one for each county.

We report the black-white differences in the resulting category-specific R estimates in Table 5. We will discuss the point estimates first, deferring momentarily our discussion of statistical significance. Of the 20 estimates, 18 are negative. Only for Los Angeles are judges' estimated levels of R substantially similar across race, with two being positive, two negative, and none exceeding 0.17 in magnitude.

Our point estimates for Dallas county indicate that judges there set bail levels for blacks in such a way as to systematically and substantially undervalue the costs of holding them over relative to the costs for whites. The estimated differences in R are -0.78 for drug offenses, -0.80 for violent offenses, -0.65 for property offenses, and -1.22 for public order offenses. These are substantial differences, whether measured relative to the costs of incarceration discussed above, or relative to the level of estimated R values for Dallas blacks, which are 1.56 for drug offenses, 1.50 for violent offenses, 1.66 for property offenses, and 1.59 for public order offenses.

Thus, the racial difference in R ranges from roughly 40% of the R level in the case of property offenses to more than 75% for public order offenses. To put these differences in dollar terms using the calculations from section 6.1, recall that our calibration implies approximately $V = \$6,300R - \$5,992$ over a period of 82 days. A black-white difference in R of -0.8 thus implies that judges value 82 days of blacks' freedom \$5,100 less than they value whites, or roughly \$62 per day. The most plausible non-discriminatory justification for a race differential in V is that blacks typically would earn less than whites in the legal labor market, so that their opportunity cost of being in jail is lower. Even for a full-time worker, though, \$62 per day is nearly \$8 per hour, which plausibly exceeds the difference in hourly earnings that white felony defendants could earn relative to black felony defendants on average. We thus view point estimates of these magnitudes as suggesting the possibility of substantial discrimination against black defendants.

Finally, we consider statistical significance of the county-by-offense type R estimates in Table 5. Several of these estimates have z -ratios that would be considered significant using conventional critical values. For example, three of the four Dallas estimates have z -ratios above 2, with the other exceeding 1.7; two of the Broward z -ratios exceed 1.65, as does one

of the Harris t -ratios. While any one of these results would usually be considered significant, we must again take account of the fact that we have 20 estimates of R .

To do so, we again use the bootstrap distribution to calculate uniform critical values for testing the null hypothesis that any one estimate is zero against the one-sided alternative of discrimination against blacks. The estimated critical value for a level-0.10 version of this test is -2.68. In other words, given that we are taking 20 draws, under the null we cannot consider an individual estimate of R significant at the 10% level unless its z -ratio is negative and exceeds 2.68 in magnitude. The z -ratio with the largest magnitude in Table 5 is Dallas's estimate for public order crimes, which has an estimate of -2.44. This corresponds to a uniform p -value of roughly 0.15, based on our bootstrap distribution. Thus, we cannot conclude that any estimate is individually significant.

The estimates in Table 5 all involve some generated-variable information in common. Thus, it is possible that they are jointly significantly different from zero, even if no estimate is individually significant. A simple χ^2 test of the null that the estimates are jointly zero yielded a p -value of 0.41, far from significant. However, this test relies heavily on the appropriateness of the normality approximation for our estimates. The auxiliary-projection estimator we use is in fact asymptotically normal, and given its parametric second step, we conjecture that it converges at rate $n^{-1/2}$. However, the estimator is based on underlying nonparametric estimators involving many variables, so it would be unsurprising for the estimator's convergence to occur slowly in practice. Indeed, casual inspection of the marginal distributions of our county-by-offense type estimates of R suggests that a number of them are skewed and heavy-tailed.

To address the role of non-normality, we again used the bootstrap. To construct a test of the null hypothesis that all estimates of R are zero, we first impose this null on the bootstrap distribution by centering each bootstrap estimate of R^{cr} around the mean of the bootstrap distribution for that estimate and dividing by the bootstrap estimate of the standard deviation of R^{cr} . Next, we tabulated the number of the 20 scaled/re-centered estimates that are negative. Our result using the real data is that 18 of the estimates are negative. This result lies far in the tail of the bootstrap distribution of tabulated re-centered estimates: in the bootstrap distribution, we see 15 or fewer negative estimates 94 percent of

the time, we see 18 negative estimates only 0.8 percent of the time, and we never see 19 or 20 negative estimate.

On this latter basis, then, there is strong evidence against the null hypothesis that there is no discrimination against blacks based on our county-by-offense type estimates. Clearly, though, our estimates within county-offense-type cells are relatively imprecise.

6.3 Summary of results

We can sum up our econometric results with three key conclusions. First, that estimates of γ_0/γ_2 and R from constrained auxiliary specifications have the signs predicted by our model, which provides additional support for our approach. Second, specification tests from our auxiliary projection relationships provide little evidence against the composite null hypothesis that each of the following simultaneously holds:

1. our model captures defendant behavior;
2. choice probabilities have a multinomial logit structure;
3. judges know $(x, u, \gamma_0, \gamma_2, g_0(x), g_2(x))$ for each defendant;
4. judges set bail optimally given this knowledge.

Third, given the model, the point estimates suggest that there is substantial evidence of bias against blacks in bail setting. Point estimates are generally largest in Broward and Dallas counties. This conclusion hold both when we pool across offense types and when we allow R to differ across offense type within county. On the other hand, Los Angeles appears to treat blacks and whites very similarly in terms of the summary measure R . Finally, our estimates are somewhat imprecise, especially those that allow variation in R by both county and offense type. We emphasize that this conclusion arises in large part because of our use of inference methods that take into account the multiple-testing that is typically ignored in econometric research.

We conclude this section by addressing the possibility that judges behave in a way that is different from our model, but which is statistically indistinguishable from the way a judge

would behave under this composite null hypothesis. This is possible since we cannot practically characterize the power of our specification tests against all alternative models. We acknowledge that we have no way to rule out such a possibility. However, our estimates are still informative about racial discrimination in an important, counterfactual sense. Recall that our estimates of the conditional choice probabilities are nonparametric and do not require optimality of bail setting. An alternative view of our method, then, is that it tells us whether and how much actual bail setting involves discrimination relative to optimal bail setting. That is, whatever it is that judges are actually doing, one might still accept our model as a reasonable description of what judges would do if they behaved in a socially optimal way. Given the Type I Extreme Value assumption and our functional form assumptions on how bail affects defendants' behavior, then, our estimates can be interpreted as consistent estimates of the R value that judges' behavior implicitly reveals. This interpretation is of more than academic interest, because it is entirely possible that judges (i) do not wish to discriminate, but (ii) they nevertheless set bail using heuristics that have discriminatory effect.

7 Our Model and the Outcome-Test Literature

The idea of using outcomes to test for discrimination in other contexts has a long history. For example, Becker (1957) discusses the fact that wage discrimination requires paying different wages for given marginal product, which means that cross-group wage differences are not sufficient information to conclude that there is taste-based discrimination. In this context, the group-level average of workers' individual ratios of marginal productivity-to-wage would serve as an outcome measure.³⁸ Another application is to use mortgage profitability as an outcome measure to test for discrimination in credit markets, as Becker (1993, page 389) discusses.³⁹

More recently, Knowles, Persico and Todd (2001, henceforth, KPT) have used the out-

³⁸Data on marginal productivity are quite difficult to collect; see Hellerstein and Neumark (1998) for an attempt to use plant-level productivity measures to assess labor market discrimination.

³⁹Berkovec, Canner, Gabriel and Hannan (1994) use data on mortgage default rates among minority and non-minority borrowers for this purpose. For critiques of such work, see Yinger (1996) and Ayres (2005).

come test idea to test for whether police discriminate against racial minorities when deciding whether to search motorists stopped on the highway. KPT assume that police seek to maximize the number of “hits,” i.e., searches that turn up contraband. Under this assumption, white and black “marginal searchees” must have the same probability of carrying contraband.

We now link this result to the idea behind Ayres and Waldfogel’s (1994, henceforth, AW) paper on bail and racial discrimination. AW assume that a judge chooses a defendant’s bail level in order to ensure that the conditional probability of FTA is no greater than some pre-specified constant, p , given that the defendant is released. Suppose momentarily that AW judges must either release a defendant or hold him until trial. Such judges will release a defendant if and only if $F(0, u) \leq p$.⁴⁰ Thus, if judges use the same value of p for blacks and whites, black and white “marginal releasees” will each have probability p of FTA. If judges discriminate against, say, blacks, then $p^{\text{black}} < p^{\text{whites}}$, so that the marginal black defendant faces a lower FTA probability.

In both the motorist-search and binary pretrial release-detain situations, however, there is an inframarginality problem. Even though marginal searchees and releasees have the same outcome value—contraband hit rates or FTA probability—when there is no racial discrimination, the average values of these outcomes need not be equal across race. For example, suppose that either the structure of the function F or the distribution of u differs across race in the binary release-or-detain case. Then the integral, over the distribution of u , of $F(0, u)$ will generally differ across race, even though all marginal releasees have the same FTA probability, regardless of race. Thus, equal FTA probabilities for marginal releasees is insufficient for equal average FTA probabilities. In general, only the average FTA probability is identified, so the inframarginality problem wrecks the usefulness of comparing FTA rates across race.

There are different routes to solving the inframarginality problem. KPT do so by hypothesizing that motorists optimally choose whether to carry contraband, taking as given the probability that police will search them when they are stopped. KPT show that in Nash equilibrium, contraband-carry rates will be equal across all motorists who are searched with probability interior to $(0, 1)$. As a result, the inframarginality problem does not exist in

⁴⁰We assume away variation in x for purposes of the discussion in this section.

Nash equilibrium.⁴¹

The inframarginality problem also disappears in the bail context under AW's assumption on the judicial objective function. An AW judge's optimal choice of bail $b^{\text{AW}}(u)$ for a defendant with heterogeneity term u must satisfy $F(b, u) \leq p$. For some defendants, $F(0, u) < p$, and an AW judge will release these defendants with zero bail. For others, $\lim_{b \rightarrow \infty} F(b, u) > p$, so that no finite bail will convince such defendants to show up for trial with the desired probability.⁴² An AW judge will deny bail to such defendants. For all other defendants, there must be a positive and finite value $b^{\text{AW}}(u)$ such that $F(b^{\text{AW}}(u), u) = p$. Thus, given AW's assumption on judge behavior, all defendants who receive a positive and finite bail level have the same probability of FTA at the optimal bail level, except to the extent that there is groupwise discrimination, in which case FTA probabilities will be equal within, but not across, groups.

As a result, there is no inframarginality problem when judges can choose bail continuously and behave according to the objective function AW assume. This fact helped motivate Ayres's (2001) argument that the inframarginality problem may generally be surmountable when the decision variable is continuous, because then "every defendant [is] marginal" Ayres (2001, pp. 409-10). This claim can be read in either of two ways.

The first reading of Ayres's conjecture is that outcome tests based on easily observed level variables like FTA are generally appropriate in any case with a continuous decision variable. Such a result would open up a wide array of settings to more robust testing for the

⁴¹Several papers have suggested plausible violations of KPT's assumptions, under which the inframarginality problem reappears. For example, Antonovics and Knight (2004) show that if there is trooper heterogeneity in search costs, then "the test that KPT employ no longer distinguishes between preference-based discrimination and statistical discrimination" (page 10). Additionally, both Anwar and Fang (2006) and Antonovics and Knight (2004) have pointed out that the inframarginality problem reappears if there is systematic heterogeneity in how troopers behave as a function of troopers' own race. Antonovics and Knight, Anwar and Fang, and Bjerck (forthcoming) all note that the same problem arises when, as Antonovics and Knight put it, "officers can observe some characteristic . . . that is correlated with the likelihood that the motorist is guilty, but that . . . is unknown to the motorist at the time that he or she decides to traffic drugs" (page 10). Dharmapala and Ross (2004) provide examples in which limits on troopers' ability to monitor motorists, as well as differences in offense severity, can lead to equilibria in which some types of motorists always carry contraband, and they show that the link between average and marginal hit rates breaks down in such cases. All of these authors, and Close and Mason (forthcoming) as well, propose alternative tests of discrimination. Finally, Brock, Cooley, Durlauf and Navarro (2010) nest KPT's assumed preference structure in a broader class for which KPT's outcome test is generally uninformative.

⁴²This result cannot happen in our model, however, since the latent utility of FTA converges to zero as b goes to infinity.

presence discrimination. By rewriting the version of the judge’s first-order condition in (3), we see that at the optimal bail level,

$$F(b^*, x, u) = R - \frac{M(b^*, x, u)F_b(b^*, x, u)}{M_b(b^*, x, u)}, \quad (20)$$

where we suppress the (x, u) argument of the function b^* . Since R is a constant parameter within racial groups, the FTA probability at the optimal bail level can be constant across u only if the second term on the right hand side of (20) is constant across u . We prove in appendix B that when u has a full-support continuous distribution, conditional on x , this term cannot generally be constant across u in our model. Thus, the first version of the Ayres conjecture does not hold when judges have the objective function we assume, which means it does not generally hold.

Consider now a second, more optimistic, reading of Ayres’s conjecture. As with the first one, the second reading holds that continuous decision variables eliminate the inframarginality problem. However, the second reading accepts that using outcome analysis to test for discrimination generally involves complicated outcomes whose estimation may require functional form assumptions. For example, our approach in this paper can be seen as a generalized form of outcome analysis. We estimate the probability of making bail and of FTA non-parametrically and then use assumptions concerning (i) functional form and (ii) optimal behavior to justify (iii) regarding these probabilities as $M(b^*, x, u)$ and $F(b^*, x, u)$ and (iv) expressing the derivatives $M_b(b^*, x, u)$ and $F_b(b^*, x, u)$ in terms only of the non-parametric conditional probability estimates. As a consequence, we are able to estimate all elements of (20), including R , up to a finite-dimensional parameter vector.⁴³

Thus, like the simple approach in AW or KPT, our approach uses outcome information together with behavioral assumptions to identify a parameter of interest. The only point of difference between our approach and AW’s, besides the fact that we assume different judicial objective functions, is that we must estimate outcome information, whereas AW (and KPT) can use raw data. Thus, the second reading gives broad credence to the Ayres conjecture. If

⁴³Indeed, it is not difficult to write our judge’s problem in notation that represents a (slight) extension of Persico’s (2009) model, in which simple outcome tests generally exist.

one is willing to both assume the form of a decision-maker’s objective function and willing to assume enough behavioral structure, then the presence of continuous decision variables seems likely to eliminate inframarginality problems.

An additional question concerns which objective function is more appropriate in the present context, AW’s or ours. Of course, this is at least partly an empirical question. There is some empirical support for our model in that it passes the overidentification tests we use above. AW’s objective function is also testable in principle, because it requires FTA rates to be the same for all defendants of a given race, even when there is racial discrimination.

To test this assumption, we estimated an ordinary least squares model relating actual FTA among defendants who made bail to the same covariates that enter our nonparametric estimators discussed in appendix C. Each model included a race dummy and a full set of county-by-offense type dummies to allow AW judges to set different levels of FTA by county-offense type cells, and we allowed the covariates to have separate coefficients for blacks and whites. We estimated standard errors robust to heteroskedasticity. Under the null hypothesis that judges follow the AW objective function, the coefficients on all covariates, other than the ones involving county, race, and offense type dummies, should be zero.⁴⁴ The F -statistic testing that all the relevant coefficients are zero has a p-value of 0.0359.

Thus, empirical evidence suggests that in our sample, judges are not choosing bail to equalize FTA across defendants, even within race. Given that our own model is not rejected by the data, we think the evidence favors our approach, at least for our data. In other samples representing other judges, AW’s objective function might be the right one; of course, some third objective function might also. On this point, we note that Brock et al. (2010) have shown persuasively that any method to testing for discrimination using observational data will require substantive assumptions on decision makers’ objectives.

⁴⁴Even though the dependent variable is binary, this model is correctly specified under the null hypothesis that judges follow AW’s objective function. This is true because (a) the conditional FTA probability given the optimal bail level and x must be unrelated to either b or x under the null, and thus (b) the dummy variables that appear in the null linear model saturate it fully.

8 Summary

We consider a model of optimal bail setting in which judges choose bail levels to trade off various costs. On one side are the costs of jailing defendants who do not make bail. These costs include both the pecuniary costs of running jails and the like, and the value judges place on freedom lost by (potentially innocent) defendants. On the other side are the social costs expected to be imposed by those defendants who make bail and are released pending trial. The optimal bail level is an implicit function of the defendant's characteristics, including x , which both we and the judge observe, and u , which only the judge observes. The presence of heterogeneity that we cannot observe invalidates conventional cross-race comparisons of average bail levels, even conditional on x .

We develop an approach to testing for discrimination based on estimation of the judge's first-order condition in an auxiliary regression. There are two key steps in making this estimation method practical. First, when there is a one-to-one mapping between optimal bail and the unobserved heterogeneity term u , conditional on x , one need not know a defendant's u value to consistently estimate his conditional choice probabilities at the optimal bail level, given (b, x, u) . This result allows us to leave the distribution of u entirely unrestricted, even conditional on x .

Second, because partial derivatives of defendants' conditional choice probabilities can be written in terms of the levels of these probabilities under a multinomial logit structure, we do not need to directly estimate the partial derivatives of conditional choice probabilities with respect to bail. Instead, we are able to estimate these partial derivatives using nonparametric estimates of the levels of the conditional probabilities.

We then recover the key parameter of interest, R . We do so by estimating the first-order condition after re-writing it in terms of auxiliary variables that depend only on the level of bail and the conditional choice probabilities. This estimation approach can be regarded as an extension of Olley and Pakes's (1996) basic idea to a new setting.

Our point estimates suggest that judges value freedom less for blacks than whites in Broward, Cook, Dallas, and Harris counties. Statistical significance of the estimates that yield this conclusion depends somewhat on the specification and approach to inference we

use. As a general matter, the point estimates are substantial but imprecise given that we account for multiple testing. Focusing only on the estimates' magnitudes, back-of-the-envelope estimates suggest that judges value lost freedom substantially less for blacks than whites, with \$60-\$80 per day being a reasonable estimate. For a defendant held until trial for a typical period of nearly three months, this range amounts to several thousand dollars' difference.

These findings suggest the possibility of substantial bias against blacks in bail setting. This result is important for multiple reasons. First, it is evidence of substantial judicial bias against blacks, which is of per se concern. Second, bail affects utility directly by affecting the probability that a defendant will lose his freedom for the potentially lengthy pre-trial period. Discriminatorily higher bails thus will make black defendants worse off in this sense. Finally, being held over might also affect the probability of conviction. Our results are therefore of substantial legal policy interest.

A Identification of Parameters

The results in the main text require the identification only of R and the ratio γ_0/γ_2 . For completeness, in this appendix we provide a formal proof of sufficient conditions for identification of these two quantities as well as for γ_0 , γ_2 , $g_0(x)$, and $g_2(x) + u$. In the case of $g_0(x)$ and $g_2(x) + u$, the identification is pointwise. We note also that differences in u are identified for two defendants with the same x .

A 1 (Partially Linear Multinomial Choice Structure). *The indirect utilities received by a defendant who chooses $(D_m, D_f) \in \{(0, 0), (0, 1), (1, 1)\}$ are given by (4).*

A 2 (Multinomial Logit Structure). *The residuals $(\epsilon_0, \epsilon_1, \epsilon_2)$ are jointly distributed according to a Type I Extreme Value distribution.*

A 3 (Gamma Signs). $\gamma_0 > 0 > \gamma_2$.

A 4 (Judge Information). *For any given defendant, judges know x , γ_0 , γ_2 , g_0 , g_2 , and u .*

A 5 (Optimal Bail Setting). *Given the information in Assumption 4, judges set bail to minimize the social cost function $q(b, x, u)$.*

Theorem 1 (Identification). *Suppose assumptions 1-5 hold. Then for any defendant for whom the optimal level of bail given (x, u) is positive and finite:*

1. *the parameters γ_0 , γ_2 , and $R = \frac{J-c_0}{c_1-c_0}$ are identified given knowledge of the joint population distribution of (D_m, D_f, b^*, x) , where b^* is the optimal bail level given (x, u) ;*

2. the value $g_0(x)$ is point-identified at any x in the support of the population;
3. the sum $g_2(x) + u$ is identified at any (x, u) in the support of the population;
4. for given x , differences in u are identified.

Proof of Theorem 1. We begin by stating and proving a lemma concerning the relationship between b and $(x, u, \gamma_0, \gamma_2, g_0, g_2)$ under the assumptions listed in the hypothesis of this theorem; we will suppress the notation concerning $(\gamma_0, \gamma_2, g_0, g_2)$ for exposition. Define $e_0 = \exp[\gamma_0 \ln b + g_0(x)]$ and $e_2 = \exp[\gamma_2 b + g_2(x)]$, so that the probability of not making bail given (b^*, x, u) is $1 - M(b, x, u) = \Pr(D_m = 0|b, x, u) = e_0/(1 + e_0 + e_2)$; the probability of making bail and appearing is $M(b, x, u)[1 - F(b, x, u)] = \Pr(D_m = 1, D_f = 0|b, x, u) = 1/(1 + e_0 + e_2)$, and the probability of making bail and failing to appear is $M(b, x, u)F(b, x, u) = \Pr(D_m = 1, D_f = 1|b, x, u) = e_2/(1 + e_0 + e_2)$. We now state Lemma 1.

Lemma 1. *Suppose that assumptions 1-5 all hold. If the optimal bail level is finite and positive for a defendant with given (x, \underline{u}) , then:*

1. *there exists a continuously differentiable function b^* on some neighborhood of (x, \underline{u}) such that $b^*(\underline{u}; x) = \underline{b}$;*
2. *\underline{u} is the unique value of u such that $\underline{b} = b^*(\underline{u}; x)$ is optimal.*

Proof of Lemma 1. To establish item 1, it is enough to observe that all functions involved in the first-order condition for optimal bail are continuous, and that for a positive and finite optimal bail level, the first-order condition holds with equality.

To establish item 2, we begin by showing that if $q_b(\underline{b}, x, u) = 0$, then $q_{bu}(\underline{b}, x, u) < 0$. Suppressing the (b, x, u) argument of q , M , and F , we redefine $q = M(F - R)$, which is just (2). Thus we have $q_u = M_u(F - R) + MF_u$. Using the multinomial logit functional forms for M and F conditional on (b, x, u) , it can be shown that $M_u = (1 - M)MF$ and $F_u = F(1 - F)$. Thus, $q_u = MF[(1 - M)(F - R) + 1 - F]$, or $q_u = MF[1 - R - M(F - R)]$. Differentiating with respect to b , we have

$$q_{bu} = \{M_b F + F M_b\} [(1 - M)(F - R) + 1 - F] - MF \frac{\partial M(F - R)}{\partial b} \quad (21)$$

$$= \{M_b F + F M_b\} [1 - R - M(F - R)] - MF q_b. \quad (22)$$

The factor in braces is always negative, since M_b and F_b are each negative while F and M are each positive. When $q_b = 0$, the second term on the right hand side of (22) is zero. Thus, q_{bu} is negative when $q_b = 0$ if and only if the factor in square brackets is positive, which we now establish is always true when $q_b = 0$. To do so, we use the fact that since $q_b = M_b(F - R) + MF_b$, $q_b = 0$ implies that $F - R = -MF_b/M_b$. It can be shown that $F_b = \gamma_2 F(1 - F)$ and $M_b = M(1 - M)[\gamma_2 F - \gamma_0/b]$. Plugging these functions into the bracketed term in (22) yields

$$(1 - M)(F - R) + 1 - F = (1 - M) \times \frac{M\gamma_2 F(1 - F)}{M(1 - M)[\gamma_2 F - \gamma_0/b]} + 1 - F \quad (23)$$

$$= (1 - F) \left[\frac{\gamma_2 F}{\gamma_2 F - \frac{\gamma_0}{b}} - 1 \right] \quad (24)$$

$$= \frac{1 - F}{\gamma_2 F - \frac{\gamma_0}{b}} \left[\frac{\gamma_0}{b} \right], \quad (25)$$

which is negative since the denominator of the leading factor is negative and the other two terms are positive. This establishes that if $q_b = 0$, we must have $q_{ub} < 0$. By symmetry of differentiation, this establishes $q_{bu} < 0$.

Next we establish that there can be at most one value of u for which a given bail level is optimal, given x . One reasonable approach to do so would be to observe that by the implicit function theorem, it is locally true at any minimum of q that $\partial b^*/\partial u = -q_{bu}/q_{bb}$. Since $q_{bb} < 0$ at any local minimum, and since we have just shown that $q_{bu} < 0$ at any stationarity point, it follows that $\partial b^*/\partial u > 0$ holds locally. Again using the implicit function theorem, there must exist a continuously differentiable function h such that, local to $(b^*(u; x); x)$, we have that $u = h(b^*(u; x); x)$, with $\partial h/\partial b = (\partial b^*/\partial u)^{-1}$. Thus, h is increasing wherever b^* is.

This argument does not suffice, however, because we cannot establish general conditions for the global convexity of q in b (in fact, we have been able to find parameter values for which it is non-convex and even has both an interior local minimum and an interior local maximum). Without further results, it would be theoretically possible for the optimal bail level to jump discretely between small changes in u . If the jump were downward, then item 2 of the lemma would not hold, i.e., each optimal bail level would not map to a unique u .

To establish uniqueness, we proceed by contradiction. First, observe that $q_b(b, x, u)$ is continuous in u given (b, x) . Next, suppose that for $\bar{u} > \underline{u}$ we have $\underline{b} = b^*(\underline{u}; x) = b^*(\bar{u}; x)$, so that the same bail level is locally optimal for both \underline{u} and \bar{u} . For \underline{b} to be an interior optimum given (x, \bar{u}) , we must have $q_b(\underline{b}, x, \bar{u}) = 0$. We showed above that this implies that $q_{bu}(\underline{b}, x, \bar{u}) < 0$. Therefore, there exists a small enough $\bar{\epsilon} > 0$ such that $q_b(\underline{b}, x, \bar{u} - \bar{\epsilon}) > 0$ as well as a small enough $\underline{\epsilon} > 0$ such that (i) $q_b(\underline{b}, x, \underline{u} + \underline{\epsilon}) < 0$, and (ii) $\underline{u} + \underline{\epsilon} < \bar{u} - \bar{\epsilon}$. By the intermediate value theorem, there must then exist a $u^{\text{mid}} \in (\underline{u} + \underline{\epsilon}, \bar{u} - \bar{\epsilon})$ such that $q_b(\underline{b}, x, u^{\text{mid}}) = 0$. In addition, $q_{bu}(\underline{b}, x, u^{\text{mid}}) > 0$ must also hold at this point, or it will be impossible to “get to” $(\underline{b}, x, \bar{u})$ by increasing u . But we have seen that $q_{bu} < 0$ always holds when $q_b = 0$, so this is a contradiction. Therefore, for fixed, positive, and finite \underline{b} , there cannot be more than one u value satisfying $\underline{b} = b^*(u; x)$, i.e., for which \underline{b} is optimal. We have thus established item 2 of the lemma. \square

We next state and prove another lemma, which makes Lemma 1 useful for identification purposes.

Lemma 2. *Suppose that assumptions 1-4 hold, and suppose that the joint distribution of (D_m, D_f, b^*, x) is known, where the optimal bail level b^* given (x, u) is finite and positive. Then for fixed $(\gamma_0, \gamma_2, g_0, g_2)$, at the optimal bail level, $\Pr[D_m = d_m, D_f = d_f | b^*, x, u] = \Pr[D_m = d_m, D_f = d_f | b^*, x]$. Furthermore, $\Pr[D_m = d_m, D_f = d_f | b^*, x, u]$ is identified.*

Proof of Lemma 2. By Lemma 1, there exists a unique function h such that $u = h(b^*; x)$. Thus $\Pr[D_m = d_m, D_f = d_f | b^*, x, u] = \Pr[D_m = d_m, D_f = d_f | b^*, x, h(b^*; x)]$. For fixed $(\gamma_0, \gamma_2, g_0, g_2)$, the latter conditional probability involves no more information than (b^*, x) . Identification of $\Pr[D_m = d_m, D_f = d_f | b^*, x, u]$ follows because the joint population distribution function for (D_m, D_f, b^*, x) is known by hypothesis, proving the lemma. \square

We now prove the theorem. Writing (15) as $L = W'\delta$, where $W = (W_1, W_2, W_3)'$, it is clear that

$$\delta = (E[WW'])^{-1} E(WL), \quad (26)$$

so δ is identified if the expectations on the right hand side of (26) are. Each element of the auxiliary data (L, W_1, W_2, W_3) involves only functions of b^* , m^* , and f^* . The expectation of any function involving these objects is identified, by Lemma 2 and the fact that the joint population distribution of (D_m, D_f, b^*, x) is known by hypothesis. Therefore, the vector δ is identified. This immediately establishes identification of γ_0/γ_2 , and R , since they equal δ_1 and δ_2 , respectively. Because $\delta_3 = -\delta_1\delta_2$ under the assumptions of the theorem, it provides no further identifying information, and we must look elsewhere to separately identify γ_0 and γ_2 .

Next, observe that the multinomial logit structure implies that at b^* ,

$$\rho(b^*, x) \equiv \ln \left(\frac{\Pr[D_m = 0 | b^*, x]}{\Pr[D_m = 1, D_f = 0 | b^*, x]} \right) = \gamma_0 \ln b^* + g_0(x), \quad (27)$$

with $\rho(b^*, x)$ being identified since each conditional probability in the parenthetical is identified, by Lemma 2. The conditional expectation given x of both sides of (27) is

$$E[\rho(b^*, x) | x] = \gamma_0 E[\ln b^* | x] + g_0(x), \quad (28)$$

since $g_0(x)$ depends only on x . Observe that both expectations are identified, by Lemma 2. Now eliminate $g_0(x)$ by subtracting (28) from (27):

$$\rho(b^*, x) - E[\rho(b^*, x) | x] = \gamma_0 (\ln b^* - E[\ln b^* | x]), \quad (29)$$

which eliminates $g_0(x)$. Multiplying both sides of (29) by $(\ln b^* - E[\ln b^* | x])$, taking expectations over the joint distribution of (b^*, x) , and rearranging then yields

$$\gamma_0 = \frac{E_{\{b^*, x\}} [(\rho(b^*, x) - E[\rho(b^*, x) | x]) (\ln b^* - E[\ln b^* | x])]}{E_{\{b^*, x\}} [(\ln b^* - E[\ln b^* | x])^2]}, \quad (30)$$

proving that γ_0 is identified, since the expectations on the right hand side of (30) are identified, by Lemma 2. Since we have previously shown identification of the ratio γ_0/γ_2 , identi-

fication of γ_0 is also sufficient for identification of γ_2 . This establishes item 1 of Theorem 1. Given γ_0 , it is trivial to see from (28) that $g_0(x)$ is point-wise identified for given x , which establishes item 2 of the theorem.

Finally, to prove identification of $g_2(x) + u$ for given (x, u) , again use the model's multinomial logit structure to see that

$$\ln\left(\frac{F^*}{1 - F^*}\right) - \gamma_2 b^* = g_2(x) + u. \quad (31)$$

Since all elements of the left hand side are identified, the sum $g_2(x) + u$ is as well, establishing item 3 of the theorem.

To establish item 4, consider data $(b^*(x, u_1), x, u_1)$ and $(b^*(x, u_2), x, u_2)$. Taking the difference of (31) across the two data vectors, $g_2(x)$ drops out, and we have

$$u_1 - u_2 = \left[\ln\left(\frac{F_1^*}{1 - F_1^*}\right) - \gamma_2 b_1^* \right] - \left[\ln\left(\frac{F_2^*}{1 - F_2^*}\right) - \gamma_2 b_2^* \right],$$

whose right hand side is identified by Lemma 2 and the identification of γ_2 . \square

B A Counterexample to the Ayres Conjecture

If Ayres's conjecture were correct, then judges' optimal bail function $b^*(\theta)$ would vary with (x, u) in such a way to ensure that $F(b^*(u; x), x, u)$ were constant across defendants within racial groups. Using (20) and suppressing the (b^*, x, u) argument, we have that at the optimum,

$$F = R + \frac{\gamma_2 F(1 - F)}{(1 - M) [\gamma_2 F - \frac{\gamma_0}{b}]}. \quad (32)$$

Under the Ayres conjecture, the FTA probability must be constant, so the right hand side of (32) constant as well. Since R and γ_2 are constant parameters, the conjecture can be correct only if the denominator of (32) is also constant. Next, observe that when the conjecture is correct, we have $\phi \equiv \ln[F/(1 - F)] = b^* \gamma_2 + g_2(x) + u$, with ϕ being constant. The optimal bail level is then $b^* = (\phi - g_2(x) - u)/\gamma_2$. For some fixed λ , denote $D(\lambda)$ as that set of defendants for whom $u = \lambda - g_2(x)$. For these defendants, optimal bail is the constant value $(\phi - \lambda)/\gamma_2$. For these defendants, then, the second term in the denominator of (32) equals $\gamma_2 F - \gamma_0 \gamma_2 / (\phi - \lambda)$, which is constant, since γ_2 and γ_0 are fixed parameters and F , ϕ , and λ all are constant by hypothesis. Thus, for the Ayres conjecture to hold in our model, $(1 - M)$ must be constant for all defendants in the set $D(\lambda)$. In our model, $M = (1 + e_2)/(1 + e_0 + e_2)$. Since e_2 must be constant for defendants in $D(\lambda)$ when the Ayres conjecture holds, $(1 - M)$ is constant if and only if $e_0 = \gamma_0 \ln b^* + g_0(x)$ is also constant. Since all defendants in $D(\lambda)$ have the same optimal bail level, this requires that all such

defendants have the same $g_0(x)$. But this last requirement does not generally hold for fixed λ . For example, consider two defendants, i and j , with

1. $g_2(x_i) \neq g_2(x_j)$;
2. $u_j = u_i + g_2(x_i) - g_2(x_j)$;
3. $u_i = \lambda - g_2(x_i)$.

Conditions 2 and 3 imply that $u_j = \lambda - g_2(x_j)$, so both defendants are in $D(\lambda)$. However, since $g_2(x_i) \neq g_2(x_j)$, we must have $x_i \neq x_j$. Since the functions g_0 and g_2 are unrestricted in our model, this means that $g_0(x_i) \neq g_0(x_j)$ is generally possible for two defendants in $D(\lambda)$, which would entail a contradiction.

This establishes that the Ayres conjecture cannot generally hold in our model, which means it cannot hold in general.

C Nonparametric Kernel Estimation

In this appendix we briefly give an overview of the kernel nonparametric estimators of $m(b, x) = Pr(D_m = 1|b, x)$ and $f(b, x) = Pr(D_f = 1|D_m = 1, b, x)$ that we use to estimate the probabilities use in the auxiliary projection equation discussed in section 5.⁴⁵ Recall that $D_m \equiv 1$ (defendant makes bail), while $D_f \equiv 1$ (defendant FTAs| $D_m = 1$), so that $D = D_m + D_f$ provided that we set $D_f = 0$ when $D_m = 0$.

We follow the convention of using capital letters to indicate random variables and lower case letters to indicate specific values they take on; we also use p as generic notation for densities. We observe that if p is the joint density of (D, B, X) , then the conditional density of D given $(B = b, X = x)$ can be written

$$p(d|b, x) = \frac{p(d, b, x)}{p(b, x)}. \quad (33)$$

Thus we can consistently estimate the conditional density of D given (B, X) if we consistently estimate both the joint density of (D, B, X) and the marginal density of (B, X) . The theory needed to estimate these functions nonparametrically is well developed. We follow the approach of using generalized product kernels suggested by Li and Racine (2007). Using general notation, let Z be a $q \times 1$ random column vector, from whose probability distribution P an i.i.d. sample $\{Z_i \in \mathbb{R}^q\}_{i=1}^n$ is sampled. Let Z_{ij} be the j^{th} row element of Z_i , and let z_j be the j^{th} row element of a fixed $q \times 1$ column vector z that lies in the support of P . A generalized product kernel estimator for the associated density $p(z)$ can be written

⁴⁵In this discussion, we use b rather than b^* to refer to the observed bail level. We do so because the non-parametric estimators of $m(b, x)$ and $f(b, x)$ are consistent regardless of whether judges choose bail optimally within the model we have specified. That is, optimal judge behavior is not necessary to estimate $Pr(D_m = 1|b, x)$ and $Pr(D_f = 1|D_m = 1, b, x)$ consistently. We emphasize, though, that these estimates are useful as deployed in the auxiliary projection equation only when bail is in fact chosen optimally, because only when bail is chosen optimally is u deterministically related to the observed (and thus optimal) bail level, given x .

$$\widehat{p}(z) = \frac{1}{n \times h_1 h_2 \dots h_q} \sum_{i=1}^n \prod_{j=1}^q k_j(Z_{ij}, z_j, h_j), \quad (34)$$

where h_j is a bandwidth parameter for the j^{th} element of the random vector Z and k_j is a kernel function for that element. Li and Racine (2007) provide conditions on $h \equiv (h_1, h_2, \dots, h_q)'$ and the set of kernel functions $\mathcal{K} \equiv \{k_1, k_2, \dots, k_q\}$ under which $\widehat{p}(z)$ is consistent for $p(z)$. A key issue is that the bandwidth parameters and kernel functions must be adapted to the types of random variables in Z . We can divide these variables into four groups:

- the dependent variable D , which is an unordered categorical variable equal to 0 for defendants who do not make bail, equal to 1 for those who make bail and do not FTA, and equal to 2 for those who make bail and do FTA;
- the bail level and the defendant's age, which we treat as continuous variables;
- a set of unordered categorical variables in x : offense category, county, year, and a variable indicating summarizing the defendant's history of prior FTAs (any, none, or inapplicable);
- a set of ordered categorical variables in x : the number of prior arrests, the number of prior prison terms, the number of prior jail terms, and whether the defendant had an active criminal case at the time of the current indictment. Also, in specifications including the private-attorney dummy, we treat it as ordered.

The kernel function used for each type of random variable must be appropriate to that type. For each random variable type just described, we use the default kernel types implemented in Hayfield and Racine's (2008) `npcdensbw` command. When the j^{th} variable is discrete and unordered, with C_j values in its support, we use the Aitchinson and Aitken kernel:

$$k_j(Z_{ij}, z_j, h_j) = \begin{cases} 1 - h_j, & \text{if } Z_{ij} = z_j, \\ \left(\frac{h_j}{C_j - 1}\right), & \text{if not.} \end{cases} \quad (35)$$

Notice that when the bandwidth equals $(C_j - 1)/C_j$, k_j will have the value $(1/C_j)$ regardless of Z_{ij} 's value. This means that the j [th] variable is treated as having uniform density for purposes of the joint-density computation. As discussed in, e.g., Li and Racine (2007), this result is a feature rather than a bug, since it allows the bandwidth-selection algorithm to endogenously treat a variable as irrelevant (the selection algorithm is constrained not to choose a value for h_j above this value).

When the j^{th} variable is discrete and ordered, we use the Wang and van Ryzin kernel with bandwidth h_j :

$$k_j(Z_{ij}, z_j, h_j) = \begin{cases} 1 - h_j, & \text{if } Z_{ij} = z_j, \\ \frac{1}{2}(1 - h_j)h_j^{|Z_{ij} - z_j|}, & \text{if not.} \end{cases} \quad (36)$$

Again, if $h_j = 0$, only observations with $Z_{ij} = z_j$ would contribute to the density estimate for z_j . In addition, when h_j equals the maximum allowed value of 1, the “density estimate” would be zero for all observations. We do encounter some instances of this situation. We interpret such cases as telling us that the variable in question is irrelevant, so we simply set $k_j(Z_{ij}, z_j, 1) = 1$, rather than 0, for all such cases.

Finally, when the j^{th} variable is continuous, we use the Gaussian kernel with bandwidth h_j :

$$k_j(Z_{ij}, z_j, h_j) = \frac{1}{\sqrt{2\pi}} \exp \left[-\frac{1}{2} \left(\frac{Z_{ij} - z_j}{h_j} \right)^2 \right]. \quad (37)$$

Given the set of kernel functions \mathcal{K} , we choose the bandwidth vector h optimally via maximum-likelihood cross-validation with the **npcdensbw** command implemented as part of Hayfield and Racine’s **np** package (Hayfield and Racine (2008)). We refer readers to Li and Racine (2007) for further details on product kernel estimation.

We estimate separate bandwidth parameters for blacks and whites (estimates appear in Appendix Table 1). We calculate the associated estimates of $m(b_i, x_i)$ and $f(b_i, x_i)$ by using these bandwidth estimates to evaluate (34) separately for $z = (d, b_i, x_i)$, $d \in \{0, 1, 2\}$. We then use (33) for the appropriate choice of d to determine the implied estimates of $m(b_i, x_i)$ and $f(b_i, x_i)$. The resulting estimates are what we use to form the generated variables L_i , W_{1i} , W_{2i} , and W_{3i} that we use in the auxiliary projection equation.

Finally, note that while we estimate separate bandwidths by race, we pool observations across county and offense type when estimating the bandwidths and implied estimates of $m(b_i, x_i)$ and $f(b_i, x_i)$. We do so to avoid excessive variability in bandwidth estimation. Because we use data-driven bandwidths, we do not believe that our data pooling will be problematic; if, for example, two counties have very different densities of other variables, then the resulting bandwidth estimates should account for that fact, up to sampling error.

References

- Abrams, David S. and Chris Rohlfs, “Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment,” Economic Inquiry, 2010.
- Albonetti, C., R. Hauser, J. Hagan, and I. Nagel, “Criminal justice decision making as a stratification process: The role of race and stratification resources in pretrial release,” Journal of Quantitative Criminology, 1989, 5, 57–82.

- Antonovics, Kate L. and Brian G. Knight, “A New Look at Racial Profiling: Evidence from the Boston Police Department,” Technical Report W10634, NBER 2004.
- Anwar, Shamena and Hanming Fang, “An Alternative Test of Racial Prejudice in Motor Vehicle Searches: Theory and Evidence,” American Economic Review, March 2006, pp. 127–151.
- Ares, C. E., A. Rankin, and H. Sturz, “The Manhattan Bail Project: An interim report on the use of pretrial parole,” New York University Law Review, 1963, 38, 67–95.
- Ayres, Ian, Pervasive Prejudice, Chicago: University of Chicago Press, 2001.
- , “Three Tests for Measuring Unjustified Disparate Impacts in Organ Transplantation: The Problem of ”Included Variable” Bias,” Perspectives in Biology and Medicine, 2005, 48 (1), S68–S87.
- and Joel Waldfogel, “A Market Test for Race Discrimination in Bail Setting,” Stanford Law Review, 1994, 46.
- Becker, Gary S., The economics of discrimination, Chicago: University of Chicago Press, 1957.
- , “Nobel Lecture: The Economic Way of Looking at Behavior,” The Journal of Political Economy, June 1993, 101 (3), 385–409.
- Berkovec, James, Glenn Canner, Stuart Gabriel, and Timothy Hannan, “Race, Redlining, and Residential Mortgage Loan Performance,” Journal of Real Estate Finance and Economics, nov 1994, 9 (3), 263–94.
- Bjerk, David, “Racial Profiling, Statistical Discrimination, and the Effect of a Colorblind Policy on the Crime Rate,” Journal of Public Economic Theory, forthcoming.
- Brock, William A., Jane Cooley, Steven N. Durlauf, and Salvador Navarro, “On The Observational Implications of Taste-Based Discrimination in Racial Profiling,” February 2010. typescript.
- Clark, John and D. Alan Henry, “The pretrial release decision,” Judicature, 1997, 81, 76–81.
- Close, Billy R. and Patrick L. Mason, “Searching for Efficient Enforcement: Officer Chars and Racially Biased Policing,” Review of Law and Economics, forthcoming.
- Colbert, D., R. Paternoster, and S. Bushway, “Do Attorneys Really Matter? The Empirical and Legal Case for the Right of Counsel at Bail,” Cardoza Law Review, 2002, 23 (15), 1719–94.
- Demuth, Stephen, “Racial and ethnic differences in pretrial release decisions and outcomes: A comparison of Hispanic, black and white felony arrestees,” Criminology, 2003, 41, 873–907.

- Dharmapala, Dhammika and Stephen L. Ross, “Racial Bias in Motor Vehicle Searches: Additional Theory and Evidence,” The B.E. Journal of Economic Analysis & Policy, 2004, 0 (1).
- Goldkamp, John S., Two Classes of Accused: A Study of Bail and Detention in American Justice, Cambridge, MA: Ballinger, 1979.
- and Michael R. Gottfredson, Policy Guidelines for Bail: An Experiment in Court Reform, Philadelphia, PA: Temple University Press, 1985.
- Hall, Peter, Jeffrey S. Racine, and Qi Li, “Cross-Validation and the Estimation of Conditional Probability Densities,” Journal of the American Statistical Association, 2004, 99, 1015–26.
- Hayfield, Tristen and Jeffrey S. Racine, “Nonparametric Econometrics: The np Package,” Journal of Statistical Software, 2008, 27 (5).
- Heckman, James J. and Peter Siegelman, “The Urban Institute Audit Studies: Their Methods and Findings,” in M. Fix and R. J. Struyk, eds., Clear and Convincing Evidence: Measurement of Discrimination in America, Washington, D.C.: Urban Institute Press, 1993, pp. 187–258.
- Helland, Eric and Alex Tabarrok, “The Fugitive: Evidence on Public versus Private Law Enforcement from Bail Jumping,” Journal of Law and Economics, 2004, 47 (1), 93–122.
- Hellerstein, Judith K. and David Neumark, “Wage Discrimination, Segregation, and Sex Differences in Wages and Productivity Within and Between Plants,” Industrial Relations, 1998, 37 (2), 232–60.
- Hotz, V. Joseph and Robert A. Miller, “Conditional Choice Probabilities and the Estimation of Dynamic Models,” Review of Economic Studies, July 1993, 60 (3), 497–529.
- Irwin, John, The Jail: Managing the Underclass in American Society, Berkeley: University of California Press, 1985.
- Knowles, John, Nicola Persico, and Petra Todd, “Racial Bias in Motor Vehicle Searches: Theory and Evidence,” Journal of Political Economy, 2001, 109, 203–29.
- Landes, William M., “The Bail System: An Economic Approach,” Journal of Legal Studies, January 1973, 2 (1), 79–105.
- Levitt, Steven D., “The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation,” The Quarterly Journal of Economics, 1996, 111 (2), 319–51.
- Li, Qi and Jeffrey S. Racine, Nonparametric Econometrics: Theory and Practice, Princeton University Press, 2007.
- Miles, Thomas J., “Estimating the Effect of America’s Most Wanted: A Duration Analysis of Wanted Fugitives,” Journal of Law and Economics, April 2005, 48 (1), 281–306.

- Newey, Whitney K., James L. Powell, and F. Vella, "Nonparametric Estimation of Triangular Simultaneous Equations Models," Econometrica, 1999, 67, 565–603.
- Olley, G. Steven and Ariel Pakes, "The dynamics of productivity in the telecommunications equipment industry," Econometrica, 1996, 64 (6), 1263–97.
- Persico, Nicola, "Racial Profiling? Detecting Bias Using Statistical Evidence," Annual Review of Economics, 2009, 1, 229–54.
- Smith, Douglas, Eric Wish, and G. Roger Jarjoura, "Drug use and pretrial misconduct in New York City," Journal of Quantitative Criminology, 1989, 5, 101–126.
- Sullivan, Laura, "Bail Burden Keeps U.S. Jails Stuffed With Inmates," National Public Radio January 21 2010. Accessed on June 7, 2010, at <http://www.npr.org/templates/story/story.php?storyId=122725771>.
- Yinger, John, "Why Default Rates Cannot Shed Light on Mortgage Discrimination," Cityscape: A Journal of Policy Development and Research, 1996, 2 (1), 25–31.

Table 1: Summary Statistics for Estimation Samples (generating time: Sun Feb 13 10:50:47 2011 from file *summary-stats.ara*.)

	Violent		Property		Drug		Public Order	
	Black	White	Black	White	Black	White	Black	White
Bail (in \$1000s)	35.9 (30.4)	29.0 (28.6)	28.3 (28.2)	22.6 (26.7)	29.0 (27.5)	16.1 (19.6)	30.3 (30.4)	19.0 (22.0)
Regression-adjusted difference	-0.14		1.39		6.37		0.63	
p-value for H_0 : no difference	0.95		0.41		0.00		0.85	
<u>Outcomes</u>								
Made bail	0.40	0.53	0.35	0.48	0.34	0.57	0.45	0.63
FTA, given made bail	0.10	0.14	0.14	0.19	0.25	0.20	0.16	0.20
<u>Background characteristics</u>								
Age in years	29.9 (10.5)	33.5 (11.0)	31.7 (10.1)	29.8 (9.4)	32.3 (10.4)	32.6 (9.5)	30.2 (9.6)	33.3 (11.5)
Any prior FTA	0.32	0.21	0.45	0.34	0.45	0.30	0.41	0.19
No possible prior FTA	0.26	0.33	0.14	0.29	0.11	0.26	0.14	0.28
Any prior arrests	0.74	0.67	0.86	0.71	0.89	0.74	0.86	0.72
At least 10 prior arrests	0.22	0.16	0.36	0.26	0.34	0.27	0.33	0.19
Any prior prison	0.24	0.18	0.36	0.22	0.43	0.21	0.34	0.23
Any prior jail	0.45	0.44	0.59	0.42	0.50	0.46	0.56	0.46
Active CJ status	0.30	0.15	0.41	0.31	0.29	0.27	0.33	0.12
<u>Attorney type</u>								
Private attorney	0.24	0.38	0.16	0.28	0.13	0.28	0.26	0.39
Public defender	0.40	0.29	0.58	0.45	0.57	0.41	0.47	0.28
Court-appointed	0.17	0.16	0.15	0.15	0.18	0.16	0.13	0.18
Data missing	0.18	0.17	0.11	0.12	0.11	0.15	0.13	0.16
<u>County and Year</u>								
Broward	0.13	0.17	0.09	0.12	0.08	0.15	0.08	0.10
Cook	0.19	0.13	0.29	0.21	0.50	0.20	0.43	0.29
Dallas	0.18	0.16	0.15	0.11	0.06	0.12	0.07	0.11
Harris	0.21	0.35	0.17	0.23	0.21	0.24	0.17	0.34
Los Angeles	0.28	0.18	0.30	0.33	0.14	0.29	0.25	0.17
Fraction from year 2000	0.54	0.49	0.53	0.51	0.57	0.55	0.41	0.34
N	277	146	418	255	853	295	143	83

Note: See text for discussion of variable definitions. Figures in parentheses are standard deviations for continuously distributed variables.

Table 2: Auxiliary Parameter Estimates From NP Estimation of Choice Probabilities and Unconstrained OLS Estimation of the Auxiliary Equation
 (generating time: Mon Feb 14 00:46:23 2011 from file *aux-results-by-race.ava*.)

	Broward		Cook		Dallas		Harris		LA	
	<u>Black</u>	<u>White</u>	<u>Black</u>	<u>White</u>	<u>Black</u>	<u>White</u>	<u>Black</u>	<u>White</u>	<u>Black</u>	<u>White</u>
Aux OLS estimates										
$\delta_1 (= \frac{\gamma_0}{\gamma_2 \times 1000} < 0)$	-1.55 (0.51)	-0.64 (1.09)	-4.97 (2.59)	1.99 (3.03)	0.26 (0.81)	3.54 (2.06)	-0.55 (0.53)	-1.43 (1.62)	-0.08 (2.20)	-7.60 (4.57)
$\delta_2 (= R > 0)$	2.00 (0.28)	2.43 (0.38)	1.59 (0.16)	1.79 (0.21)	1.58 (0.13)	2.15 (0.26)	1.41 (0.07)	1.87 (0.26)	1.24 (0.10)	1.31 (0.11)
$\delta_3 (= -\frac{\gamma_0}{\gamma_2 \times 1000} R > 0)$	0.61 (0.17)	0.21 (0.16)	1.56 (0.55)	0.31 (0.58)	0.26 (0.12)	-0.16 (0.30)	0.21 (0.06)	0.57 (0.25)	1.06 (0.47)	4.39 (1.24)
Specification test										
$T = \delta_1 \times \delta_2 + \delta_3 (= 0)$	-2.49 (1.12)	-1.33 (2.65)	-6.35 (3.53)	3.87 (4.77)	0.67 (1.19)	7.46 (4.25)	-0.56 (0.72)	-2.11 (2.81)	0.97 (2.22)	-5.54 (4.88)
Conventional p -value	0.026	0.615	0.072	0.417	0.575	0.079	0.439	0.452	0.664	0.257
Uniform p -value	0.18	1.00	0.39	1.00	1.00	0.76	1.00	1.00	1.00	1.00

Notes:

- Expressions in parentheses in far-left column are values taken on under the null hypothesis of correct model specification.
- Estimated standard errors appear in parentheses; all standard errors are based on 999 bootstrap replications (see text for bootstrap details).
- Conventional p -value is from usual two-sided test, based on asymptotic normality, against the column-by-column null hypothesis that $\delta_1 \times \delta_2 + \delta_3 = 0$, with no correction for multiple testing.
- Uniform p -value is from two-sided test against the same null hypothesis, using bootstrap distribution to correct for multiple testing (see text for details).

Table 3: Auxiliary Estimates of $\frac{\gamma_0}{\gamma_2}$ and R Based on NP Estimation of Choice Probabilities and Constrained (NLS) Estimation of the Auxiliary Equation

(generating time: Mon Feb 14 00:46:23 2011 from file *aux-results-by-race.ara.*)

	Broward		Cook		Dallas		Harris		LA	
	Black	White	Black	White	Black	White	Black	White	Black	White
$\frac{\gamma_0}{\gamma_2 \times 1000} (< 0)$	-0.40 (0.15)	-0.06 (0.04)	-0.05 (0.42)	-0.35 (0.14)	-0.21 (0.07)	-0.15 (0.07)	-0.12 (0.03)	-0.21 (0.31)	-1.01 (0.34)	-2.78 (0.77)
$R(> 0)$	1.27 (0.30)	2.30 (0.38)	1.63 (0.15)	1.87 (0.21)	1.61 (0.13)	2.36 (0.29)	1.38 (0.07)	1.79 (0.29)	1.26 (0.09)	1.20 (0.08)

Notes:

- Expressions in parentheses in far-left column are values taken on under the null hypothesis of correct model specification.
- Estimated standard errors appear in parentheses; all standard errors are based on 999 bootstrap replications (see text for bootstrap details).
- All estimates of R are highly significantly different from zero based on uniform tests that use the bootstrap distribution to account for multiple testing.

Table 4: Estimated Black-White Difference in R , Pooled Over Crime Type, Based on NP Estimation of Choice Probabilities and Constrained NLS Estimation of the Auxiliary Equation
 (generating time: Mon Feb 14 00:46:24 2011 from file *aux-dif-results.ara*.)

	Broward	Cook	Dallas	Harris	LA
R	-1.03 (0.50)	-0.24 (0.26)	-0.75 (0.32)	-0.41 (0.30)	0.05 (0.12)
Test of $H_0 : R = 0$ against $H_a : R < 0$, using					
Conventional p -value	0.020	0.172	0.009	0.089	0.324
Uniform p -value	0.108	0.596	0.054	0.348	0.993
Test of $H_0 : T^b = T^w = 0$, using:					
Conventional p -value	0.075	0.142	0.183	0.559	0.478
Uniform p -value	0.511	0.770	0.785	0.998	0.972

Notes:

- Estimated standard errors appear in parentheses; all standard errors are based on 999 bootstrap replications; see text for bootstrap details.
- Uniform p -value is one-sided and based on bootstrap distribution.
- p -value testing $T^b = T^w = 0$ is conventional value based on unconstrained OLS estimates reported above, with no correction for multiple testing.

Table 5: Estimated Black-White Difference in R by Crime Type Based on Nonparametric Estimation of Choice Probabilities and Constrained (NLS) Estimation of the Auxiliary Equation
 (generating time: Mon Feb 14 00:46:24 2011 from file *aux-dif-results.ara.*)

	Broward	Cook	Dallas	Harris	LA
By offense type					
Drug offenses only	-0.51 (0.68)	-0.57 (0.37)	-0.78 (0.36)	-0.33 (0.34)	0.08 (0.16)
Violent offenses only	-0.65 (0.59)	-0.52 (0.44)	-0.80 (0.36)	-0.19 (0.35)	-0.08 (0.17)
Property offenses only	-0.81 (0.49)	-0.21 (0.32)	-0.65 (0.38)	-0.38 (0.34)	0.17 (0.19)
Public order offenses only	-1.44 (0.81)	-0.11 (0.43)	-1.22 (0.50)	-0.68 (0.37)	-0.14 (0.19)

Notes:

- Estimated standard errors appear in parentheses; all standard errors are based on 999 bootstrap replications; see text for bootstrap details.

Appendix Table 1: Bandwidth results

	Upper Bound	Black Sample	White Sample
<i>Unordered categorical variables:</i>			
Dependent variable	0.6667	0.0181	0.0247
Most serious offense	0.9333	0.7366	0.8658
County	0.8000	0.2421	0.4997
Year	0.5000	0.2737	0.1364
Prior FTAs	0.6667	0.6667	0.2733
<i>Ordered categorical variables:</i>			
Prior arrests	1.0000	0.8368	1.0000
Prior prison terms	1.0000	0.4650	0.3049
Prior jail terms	1.0000	0.8011	0.7860
Criminal justice status	1.0000	0.9450	0.1240
<i>Continuous variables:</i>			
Bail	∞	8859.5520	9874.7580
Age	∞	21.4947	6982694.0000

See appendix on non-parametric estimation for more details.