

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

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Abstract

This paper tests for discrimination against immigrants in pretrial detentions in Chile using a decade of nationwide administrative records. Immigrant defendants are 8.5 percentage points less likely to be released relative to Chilean defendants with similar proxies for pretrial misconduct potential. Our estimates suggest that discrimination stems from an informational problem because judges do not observe criminal records in origin countries, with stereotypes and taste-based discrimination playing a role in the informational problem's resolution. Discrimination is larger for drug offenses and has increased over time after a recent immigration wave.

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“When Mexico sends its people, they’re not sending their best. (...). They’re bringing drugs. They’re bringing crime. They’re rapists. And some, I assume, are good people.”

Donald Trump, presidential announcement speech, June 16, 2015.

1 Introduction

Increasing flows of international migrants have induced several policy challenges for destination countries in recent years (UN, 2013, 2019) including, among others, the mitigation of concerns about discrimination (Heath et al., 2013). Labor market, crime, and welfare concerns can trigger negative attitudes towards immigrants, especially when immigrants are relatively low-skilled (Mayda et al., 2022; Moriconi et al., 2022) and when cultural prejudice and misperceptions regarding foreign born and minority populations are commonplace (Dustmann and Preston, 2007; Hangartner et al., 2019; Grigorieff et al., 2020; Alesina et al., 2021, 2023; Ajzenman et al., 2022, 2023; Bursztyn et al., 2023). Characterizing discrimination patterns against immigrants is therefore a policy priority to improve the possibilities of integrating immigrants into destination countries.

This paper conducts an empirical analysis of discrimination against immigrant defendants in the criminal justice system, in particular, in the dictation of pretrial detention. Pretrial detention decisions are particularly relevant for two main reasons. First, and despite the fact that the literature has failed to find a robust link between immigration and crime (Butcher and Piehl, 1998; Bianchi et al., 2012; Bell et al., 2013; Hines and Peri, 2019; Pinotti and Rozo, 2022; Ajzenman et al., 2023), one of the prevalent narratives that underpins negative attitudes towards immigration is its potential effect on crime (Fasani et al., 2019; Bursztyn et al., 2021). Thus, if there is discrimination against immigrants, it should be especially salient in institutions that are concerned with that rubric. Second, the pretrial detention process has been shown to be permeated by discriminatory practices (Arnold et al., 2018, 2022; Grau and Vergara, 2023) and to have a significant impact on the labor market opportunities, conviction probabilities, and the participation in public assistance programs for defendants (Heaton et al., 2017; Leslie and Pope, 2017; Dobbie et al., 2018; Dobbie and Yang, 2021a,b; Grau et al., 2021; Stevenson and Mayson, 2021). These costs are particularly harmful for immigrant populations given the challenges they face relating to integration in general and entry into the labor market in particular (Åslund et al., 2014; Dustmann et al., 2017; Brell et al., 2020; Egger et al., 2022; Foged et al., 2022).

We study this scenario in Chile where the pretrial detention process functions in a similar manner to the US – that is, judges decide on the pretrial detention status of defendants using limited

information and in a limited time period with a mandate to not detain defendants unless misconduct potential (pretrial recidivism or failure to appear in court, or both) is considerable. Unlike in the US, however, the Chilean system is uniform across all localities and does not permit monetary bail. Importantly, as we discuss in Section 2, the Chilean legislation makes no distinction between natives and immigrants regarding pretrial detentions. Regarding the importance of immigration, since approximately 2013 Chile has become an important destination for immigrants from other Latin American countries. This change in immigration flows has generated hostile discourses in the public sphere and misperceptions regarding immigrants and immigration in the general population.

We conduct the empirical analysis using nationwide administrative records for the period 2008–2017. Our database covers 95% of Chile’s criminal prosecutions and includes detailed information on defendants and prosecutions. Despite having less severe criminal records upon prosecution and lower pretrial misconduct rates when released, immigrant defendants are more likely to be detained pretrial relative to defendants who are Chilean nationals. We formally explore whether this descriptive fact constitutes some form of aggregate discrimination by estimating observational benchmark regressions – that is, by testing if defendants with similar proxies for misconduct potential face, on average, different pretrial release rates because of their immigration status, an exercise similar in spirit to [Gelman et al. \(2007\)](#), [Abrams et al. \(2012\)](#), and [Rehavi and Starr \(2014\)](#). The main result shows that, conditional on type of crime, criminal records, judge leniency, attorney quality, and court-by-time fixed effects, immigrant defendants are 8.5 percentage points (pp) more likely to be detained pretrial relative to Chilean defendants, a difference that represents 53% of the unconditional baseline pretrial detention rate.

We perform two sets of diagnostics that suggest that our main estimates are not driven by systematic differences in unobserved misconduct potential (i.e., omitted variable bias, OVB). First, we implement the method set out in [Oster \(2019\)](#) for assessing the degree of selection on unobservables using observed covariates. We conclude that, if anything, our observational benchmark regression underestimates discrimination against immigrants. Specifically, we show that the discrimination estimate is attenuated (although not reversed) only if we impose the nonstandard assumption that unobservables are negatively correlated with the control variables – that is, if defendants with observables that induce lower release rates are also endowed with unobservables that work in the opposite direction. This is consistent with the fact that immigrants have, on average, less severe criminal records. In fact, and contrary to the common finding in observational benchmark regressions, we find that the release disparity between Chilean defendants and immigrant defendants is around 20% smaller (6.7 pp) when control variables are omitted.

The second diagnostic is a novel test that uses observed pretrial misconduct rates of released defendants to assess the degree of OVB in the release equation. The intuition is as follows: OVB arises if there are unobservables that correlate with immigration status and predict misconduct potential. Then, we can use observed pretrial misconduct of released defendants to test whether the included control variables are sufficient to account for systematic differences in pretrial misconduct rates between immigrant defendants and Chilean defendants. Because the sample of released defendants is selected, we estimate different selection models using assigned judges and attorneys as the excluded variables and show that, conditional on type of crime, criminal records, and time-varying court characteristics, immigration status has no predictive power on observed pretrial misconduct. This exercise suggests that the set of controls included in the benchmark regression is sufficient to account for systematic risk differences between Chilean defendants and immigrant defendants.¹

After establishing the robustness of the reduced-form discrimination estimate, we perform different heterogeneity analyses to obtain additional insights about the aggregate pattern. We first explore whether discrimination against immigrants stems from an informational problem. The criminal records of immigrant defendants are likely to be censored because judges cannot observe the records kept in the country of origin. Then, judges may process the information contained in criminal records differently depending on whether the defendant is an immigrant or not. To test this hypothesis, we estimate our main specification separately for samples of defendants with and without previous prosecutions in Chile. We find that the conditional release disparities are 1.6 pp and 13.2 pp for defendants with and without previous prosecutions, respectively. This implies that, unlike Chilean defendants, immigrant defendants are not rewarded for having clean criminal records. In fact, results suggest that judges systematically impute nonzero criminal histories for all immigrant defendants. This evidence – which is consistent with the empirical literature on Ban-the-Box policies (Agan and Starr, 2018; Doleac and Hansen, 2020; Raphael, 2021) – suggests that the informational problem seems to be the main driver behind the discrimination patterns.

The fact that discrimination stems from an informational problem does not necessarily mean that the aggregate estimate can be rationalized by accurate statistical discrimination. On one hand, the informational problem can be circumvented based on stereotypes (biased beliefs). On the other hand, the informational problem may provide room for judges to exercise preferences

¹The second test also suggests that, while time-varying court characteristics matter for the discrimination point estimate, they do little to correct for OVB and, therefore, they may induce included variable bias (IVB) in the main estimations. In other words, rather than controlling for differences in unobserved misconduct potential, court-by-time variation seems to mediate discrimination through the systematic assignment of immigrant defendants to less lenient courts. Under this interpretation, the baseline discrimination estimate of 8.5 pp constitutes a conservative bound given that excluding the court-by-time fixed effects increases the release disparity gap to 10.2 pp.

against immigrants (taste-based discrimination). To obtain further insights, we estimate observational outcome tests (Knowles et al., 2001; Grau and Vergara, 2023) that, when rejected, provide evidence of a nonseparately identified combination of stereotypes and taste-based discrimination (Hull, 2021). The outcome test is rejected suggesting that the discrimination pattern, although stemming from an informational problem, cannot be rationalized by accurate statistical discrimination. The combination of stereotypes and taste-based discrimination seems to play an important role in explaining the aggregate estimates of discrimination against immigrants.²

We also explore whether or not the patterns of discrimination vary according to crime category. We find that discrimination is particularly severe for drug offenses: conditional release disparities are around four times larger than the baseline benchmark estimate. Statistically and economically significant differences are also found for thefts and robberies and other property crimes. Other violent crimes like homicides and sexual offenses show no significant differences, although the number of immigrants that are charged with these types of crimes is too small to confidently detect an effect. Crime categories that are associated with less hostile discourses in the public sphere (such as white collar crimes or crimes against privacy) also display no significant release disparities. This heterogeneity is consistent with the discourse that links immigration flows to drug trafficking and other common property crimes playing a role in affecting preferences and stereotypes.

Finally, we explore the evolution of discrimination patterns during the recent wave of immigration to Chile. Immigration inflows in Chile grew considerably and became more racially diverse after 2012. We find that benchmark estimates are 52% larger in the 2013–2017 period relative to the 2008–2012 period. We also find that discrimination estimates are remarkably similar across different groups of immigrants, suggesting that the increase over time is driven by an intensification of discrimination rather than a composition effect. This finding is consistent with stereotypes and taste-based discrimination playing a role in rationalizing the results because, under accurate statistical discrimination, this result would imply an implausible dramatic change over time in the underlying risk distribution. While we do not establish a causal relationship, we conjecture that this trend is related to the crime-related narrative response to the immigration wave lead by political authorities and the media (Ajzenman and Dominguez, 2023; Djourelouva, 2023).

²The conclusion is robust across outcome definitions; that is, the outcome test is rejected when using nonappearance in court, pretrial recidivism, or any pretrial misconduct as an outcome. This alleviates concerns about immigrants being more likely to fail to appear in court because of the possibility of returning to their origin countries.

2 Setting and Data

This section describes the Chilean context and institutional setting. We start by characterizing immigration in Chile, and discussing its magnitude, trends, composition, and attitudes toward immigration among Chilean citizens. We then describe the pretrial detention system. Finally, we describe the data used in the empirical analysis and provide descriptive statistics.

2.1 Immigration in Chile

Immigration in Chile has changed dramatically since 2013. According to the national census, immigrants represented around 1.5% of the Chilean population in 2002 and in 2012 this figure was 2%. In 2017, however, the share of immigrants in the population increased to 4.5%, and projections from the National Institute of Statistics suggest that the number reached 7.8% in 2019. In terms of gross inflows, both the volume and composition have changed considerably. Between 2008 and 2012 the inflow of immigrants was fairly stable at around 100,000 individuals per year. Peruvians accounted for the largest share of immigrants arriving each year (roughly 50% of new arrivals) and Argentinians, Bolivians, and Colombians collectively represented around 25% of inflows. The annual inflow of immigrants grew to around 350,000 individuals in 2017. In addition, the composition changed substantially. For the 2008–2012 period, Haitian and Venezuelan immigrants accounted for around 2% of the arrivals; however, for the 2013–2017 period they represented almost 20% of the arrivals. Inflows of Colombian nationals also increased significantly after 2013. Their share among new arrivals to Chile grew from 10% in the period 2008–2012 to 19% in the period 2013–2017.

The recent immigration wave has not gone unnoticed by Chile’s population, politicians, and media. Similar to the inhabitants of other countries (Hopkins et al., 2019; Jørgensen and Osmundsen, 2022; Alesina et al., 2023), Chileans overestimate the size of the immigrant population. Surveys show that in 2019 Chileans thought that immigrants represented 33% of the total population, which compares unfavorably to the administrative estimate of 7.8% (Ajzenman et al., 2023). Additional survey evidence shows that *increase in crime* is the most prominent concern related to immigration among Chile’s population (Espacio Público, 2018), despite the fact that immigrants in Chile are less likely to be engaged in criminal activity (Blanco et al., 2020). Ajzenman et al. (2023) examine the relationship between crime and immigration in Chile and report that these perceptions are not related to changes in victimization rates, even though the concerns were severe enough to induce increases in preventive behavioral responses, such as investments in home security. The authors show that the media –measured by local radio density– plays an important role in the divergence between

perceptions and crime rates. More recently, [Ajzenman and Dominguez \(2023\)](#) show that the media may also exacerbate stereotypes by disproportionately increasing the number of crime-related news segments after an alleged perpetrator of a homicide was an immigrant.

Public attitudes toward immigrants can also be inferred from recent political campaigns. Immigration, which was not a public issue before the recent increase in immigration inflows, has become a prominent topic in the national political debate, with an increasing number of politicians framing their public discourse in the context of immigration regulation. For example, inspired by Donald Trump, the far-right candidate in the last presidential election, José Kast, proposed to build a ditch on Chile’s border with Bolivia. In most cases, a link between crime and immigration is proffered as the most important reason for stopping or reducing the arrival of new immigrants. For example, when the National Congress of Chile passed President Piñera’s migration project in 2019, he described the initiative as a “(...) great step forward to continue putting our house in order regarding matters of migration, and thus better combat illegal immigration and the evils such as delinquency, drug trafficking, and organized crime entering Chile” (Twitter, January 16, 2019). These types of statements, that display either implicit or explicit anti-immigrant sentiment, have been accompanied by instances of misinformation that appear to be designed to stigmatize immigrants or immigration. For example, in 2018, a widely shared social media post claimed that former President Bachelet received USD 3,000 from the UN for each Haitian immigrant who entered Chile. The increase in negative attitudes towards immigrants has contributed to Chile’s political climate becoming more polarized and an increase in the relative popularity of far-right nationalist groups with conservative-leaning voters.

2.2 Pretrial Detentions in Chile

The procedure to define pretrial detention for people arrested in Chile works as follows: During the 24 hours after the initial detention, there is an arraignment hearing in which a detention judge must decide whether the defendant will be incarcerated during the criminal investigation. Monetary bail is not an option in the Chilean system. Following the legal principle of the presumption of innocence, judges should not incarcerate defendants unless there is a clear danger of escape (i.e., a high probability of failing to appear in court), the defendant represents a significant threat to society (i.e., a high probability of committing another crime during the investigation), or the imprisonment of the defendant aids the investigation of the criminal case. As described in [Grau et al. \(2021\)](#), pretrial detention became more frequent between 2007 (17,891 cases) and 2018 (34,815 cases). In the same period, the proportion of cases where pretrial detention is imposed has increased from

7.3% to 9.6%, and pretrial detainees as a share of total prisoners rose from 21.9% to 36%.

Pretrial detention hearings are brief (lasting about 15 minutes) and judges have to make a decision whether or not to detain the defendant at the end of the hearing. Judges are assigned in a quasi-random fashion at the court-by-time level ([Grau and Vergara, 2023](#)): In every court, at the beginning of each month, judges are assigned to different time slots to lead arraignment hearings for no reason other than splitting this duty among the judges in the particular court. The assignment of public attorneys to each case is also quasi-random at the court-by-time level.

In Chile, the principle of equality and non-discrimination – which applies to the criminal justice system in general and pretrial detentions in particular – is guaranteed for every *person* (rather than *citizen* or *national*) independently of their citizenship status. This characteristic most likely follows the fact that, until recently, the country has received low levels of immigration flows, so there was no need to accommodate the common legislation for the immigrant population. The recent change in immigration patterns has motivated a judicial public discussion for updating immigration laws that does not affect our period of analysis.

To be more specific, the first article of the Constitution establishes that “every *person* is born free and equal on dignity and rights” (Art 1, own translation). Also, there are several aspects of the Chilean legislation that ensure that immigrants must be treated without distinction from Chileans in the criminal justice system. For example, the Constitution establishes that “every *person* has the right to legal defense in the manner indicated by law and no authority or individual may prevent, restrict, or disturb the proper intervention of the lawyer if required” (Article 19.3, own translation). Similarly, international law – through agreements that Chile has signed and ratified – reaffirms the principle of equality and non-discrimination in this setting. For example, Article 8.2. of the American Convention on Human Rights (IACHR) states the inalienable right to be assisted by a defender, and Article 14.2 of the International Covenant on Civil and Political Rights recognizes the right “to be present at the trial and to defend herself personally or be assisted by a defense attorney of her choice, to be informed, if she does not have a defense attorney, of her right to have one, and, whenever the interest of justice requires it, to be appointed a public defender free of charge if she lacks sufficient means to pay for it”. Article 18.3.d of the International Convention on the Protection of the Rights of Migrant Workers and Members of Their Families also confirms the above. In summary, the Chilean system mandates judges to treat native and immigrant defendants equally when dictating pretrial detention.

2.3 Data

We use administrative records from the Public Defender’s Office (PDO). The PDO is a centralized public service under the oversight of the Ministry of Justice. It offers criminal defense services to all individuals accused of or charged with a crime; as such, it ensures the right to a defense by a lawyer and due process in criminal trials. Our estimation sample covers more than 95% of the criminal cases for the period between 2008 and 2017 and contains detailed case and defendant characteristics, including the nationalities of the defendants. We define immigrant defendants as defendants who are nationals of any other country than Chile. In addition, we can identify the judges assigned to each case at the beginning of the criminal process (i.e., when pretrial detention decisions take place), as well as the public attorney assigned to the arraignment hearing.

To build the estimation sample, we consider all detention hearings for adult defendants who were arrested between 2008 and 2017 and defended by the PDO. We exclude hearings due to legal summons because the information set available to the judge may be different in those cases. To be able to focus on arraignment hearings in which pretrial detention is a plausible outcome, we only consider types of crimes that have at least a 5% probability of pretrial detention. For the same reason, when defendants are accused of more than one crime during the same arraignment hearing, we only retain the information related to the most severe crime (with severity measured as the probability of pretrial detention), although we record the number of crimes that are imputed in each case regardless of their severity. A more detailed description of the data, the sample restrictions, and the variables is presented in [Appendix B](#).

Table 1 shows descriptive statistics for the estimation sample. This table provides four stylized facts that motivate our research question. First, relative to Chilean defendants, immigrants are more likely to be detained before the trial. The pretrial release rate for immigrant defendants and Chilean defendants is 77% and 84%, respectively. Second, the severity and the number of imputed crimes of current cases are similar for immigrant defendants and Chilean defendants. Third, immigrant defendants have less severe criminal records. Chilean defendants are more likely to have previous prosecutions, convictions, and instances of pretrial misconduct. The criminal records of immigrants may be censored because judges cannot observe the criminal histories of immigrant defendants before they arrive in Chile. Therefore, how judges interpret immigrants’ (lack of) criminal records could play a role in explaining the release disparities. Fourth, once released, immigrants are less likely to be engaged in any kind of pretrial misconduct, both in terms of nonappearance in court and, especially, in pretrial recidivism.

3 Conceptual Framework

This section formally defines and discusses the notion of discrimination we use throughout the paper and motivates the empirical exercise of Section 4.

3.1 Setup

Let i index defendants and j index judges, with judges assigned to defendants according to the mapping $j(i)$. Let $Y_i^* \in [0, 1]$ be the pretrial misconduct probability of defendant i if released, which is not observed by the judge.³ Defendants are also characterized by I_i which is a binary variable that takes value 1 if the defendant is an immigrant. Let R_{ij} be a binary variable that takes value 1 if defendant i is pretrial released by judge j .

While the structure of the judge-level release decision is left unrestricted, the institutional framework mandates judges to base the release decision on Y_i^* . Concretely, the principle of the presumption of innocence and the documented impact of pretrial detentions on different outcomes imply that only defendants with a *high* probability of pretrial misconduct should be detained. Because the decision is discretionary and judges potentially have heterogeneous preferences for leniency, the implicit release thresholds are likely to be heterogeneous across judges.

Defining discrimination In this paper, we focus on an aggregate notion of discrimination. The definition explores whether, on average, there are release disparities between immigrant and native defendants that have the same probability of pretrial misconduct. As we discuss below, there are several potential structural drivers of release disparities that can affect this high-level definition.

DEFINITION (DISCRIMINATION): *Judge-level discrimination, \mathbf{d}_j , is defined as*

$$\mathbf{d}_j = \mathbb{E}[d_j(Y_i^*)] = \mathbb{E}[\mathbb{E}[R_{ij}|I_i = 1, Y_i^*] - \mathbb{E}[R_{ij}|I_i = 0, Y_i^*]], \quad (1)$$

where the outer expectation integrates over Y_i^* . System-level discrimination, \mathbf{D} , is the (case-weighted) average across judges, $\mathbf{D} = \mathbb{E}[\mathbf{d}_j]$.

This definition closely follows the definition outlined in [Arnold et al. \(2022\)](#). Under this discrimination definition, there is system-wide discrimination against immigrants if $\mathbf{D} < 0$. The inner object, $d_j(Y_i^*)$, is the judge-specific gap in release rates between immigrant and native defen-

³One way of motivating Y_i^* is by assuming that the (ex-post) observed pretrial misconduct outcome if released, $Y_i \in \{0, 1\}$, is a binary variable generated by a Bernoulli random coefficients model with parameter Y_i^* .

dants conditional on misconduct potential Y_i^* and, therefore, does not impose restrictions on the group-specific distributions. A negative value of $d_j(Y_i^*)$ implies that, conditional on the risk level, immigrant defendants are pretrial detained at higher rates than native defendants. The aggregate measure of discrimination averages over the distribution of misconduct potential and across judges.

Sources of release disparities and interpretation Models of judge decision-making suggest that a defendant’s nationality may affect the release decision through different channels (Hull, 2021; Arnold et al., 2022; Canay et al., 2022; Grau and Vergara, 2023). First, judges may use I_i (or other variables that correlate with I_i) to predict (or interpret signals of) Y_i^* if distributions vary with I_i . This channel corresponds to the standard notion of (accurate) statistical discrimination (Aigner and Cain, 1977). Second, if statistical discrimination is inaccurate and the degree of inaccuracy varies with I_i , this induces a differential prediction bias to the decision. We refer to this as biased beliefs or stereotypes (Bordalo et al., 2016; Bohren et al., 2021). Third, judges may base their release thresholds on I_i (or other variables that correlate with I_i) because of animus, giving form to the standard notion of taste-based discrimination or discriminatory preferences (Becker, 1957, 1993). Fourth, a systematic assignment of immigrant defendants to less lenient courts or judges may contribute to the conditional release disparity, which under Bohren et al. (2023) framework can be framed as institutional discrimination. Fifth, judges may simply deviate from the legal mandate and make their release decisions based on alternative objective functions that can induce a differential impact on immigrant defendants, similar to the notion of omitted payoff bias defined in the literature of algorithmic decision-making (Kleinberg et al., 2019).⁴

The notion of discrimination we adopt in this paper potentially incorporates all five sources of release disparities mentioned above. A possibly complex combination of these drivers of release disparities may yield a reduced-form effect of I_i on R_{ij} even within defendants with equal Y_i^* . As emphasized in Yang and Dobbie (2020) and Arnold et al. (2022), all sources of disparities are problematic through the lens of legal principles similar to the Equal Protection Clause in the US Constitution, which suggests that this reduced form effect has normative content regardless of the specific sources. Intuitively, the composite effect of all sources may lead defendants with equal misconduct potential but different immigration status to face different pretrial detention rates.

The previous discussion implies that our definition of discrimination seeks to identify a disparate

⁴One source of discrimination not accounted by our definition is the notion of systemic discrimination outlined in Bohren et al. (2023). Our definition conditions on Y_i^* but the generating process of Y_i^* could be affected by discrimination in previous stages, eventually leading to disparities in its distribution that could be interpreted as discrimination. Our analysis below abstracts from this channel.

impact (rather than disparate treatment) notion of discrimination (Arnold et al., 2022; Rose, 2022). The disparate impact doctrine focuses on release disparities among defendants with equal misconduct potential but different group membership. Under this perspective, different structural models that rationalize similar release gaps conditional on misconduct potential are normatively equivalent. On the other hand, the disparate treatment doctrine focuses on the actual release decision, understanding as discrimination explicit judge-level practices against a particular group. This implies that the normative implication of release disparities among defendants with equal misconduct potential is different if they follow explicit discriminatory practices against group membership or if, for example, are driven by discriminatory practices against other characteristics that correlate with group membership. Identifying notions of discrimination consistent with the disparate treatment doctrine requires additional structural restrictions on individual judges’ decisions in terms of their information sets and the actual determinants of their prediction problem and release thresholds, and should be built over analogs of equation (1) that condition on additional defendant characteristics.

3.2 Empirical test

One fundamental challenge when testing for discrimination following our definition above is that Y_i^* is most likely not observable by the econometrician; therefore, a regression of R_{ij} on I_i controlling for Y_i^* is not implementable. On the other hand, a regression that simply omits Y_i^* is potentially subject to standard omitted variable bias (OVB) if its distribution varies with I_i . Arnold et al. (2022) propose a weighting scheme that equalizes group-specific distributions of Y_i^* under which omissions of Y_i^* from the (weighted) regression of R_{ij} on I_i do not induce OVB and, therefore, identifies \mathbf{D} . Unfortunately, for our period of analysis, the immigrant population who interacted with the criminal justice system is small, which prevents us from implementing their methodology.⁵ As an alternative, we implement observational benchmark regressions similar in spirit to Gelman et al. (2007), Abrams et al. (2012), and Rehavi and Starr (2014). Formally, we assume that Y_i^* can be expressed as a function of observed variables, X_i , so a regression of R_{ij} on I_i controlling by the (correctly specified) function of X_i , say $g(X_i)$, identifies \mathbf{D} .

In Section 4 we discuss our choice of X_i and provide diagnostics to assess its performance in

⁵The Arnold et al. (2022) method combines quasi-random assignment of judges, observational misconduct data on released defendants’ behavior, and extrapolation techniques. The extrapolation step requires several judges to handle several cases that involve defendants who are immigrants. Our limitation is, therefore, statistical power. Specifically, the extrapolation technique used in Arnold et al. (2022) is based on regressing average pretrial misconduct rates of released immigrant defendants against judge leniency to infer the average pretrial misconduct rate of a supremely lenient judge using an identification at infinity argument. For this technique to work properly several judges are required in order to have enough support, in addition to several cases per judge so that pretrial misconduct rates can be precisely estimated. Both requirements are not met in our immigrant subsample.

approximating Y_i^* . This is central to the analysis because the extent to which this is a plausible approach to recover \mathbf{D} fundamentally depends on the vector X_i . More specifically, as noted by [Arnold et al. \(2022\)](#), observational benchmark regressions can suffer both from included variable bias (IVB) and OVB. IVB arises when some of the included variables in X_i mediate discrimination against, in this case, immigrant defendants in a disparate impact sense. For example, if there is discrimination based on place of living and immigrants are overrepresented in a discriminated locality, then controlling for place of living will attenuate the estimated disparity. In other words, IVB makes the discrimination estimate to move from a disparate impact to a disparate treatment notion of discrimination. On the other hand, OVB arises when the vector X_i does not fully account for group differences in misconduct potential and, therefore, the estimated coefficient reflects differences in unobservables rather than discriminatory forces. This empirical challenge generates a tension between over- and under-controlling. We deal with this tension by choosing the smallest set of variables that successfully eliminates OVB. Section 4 presents different tests that reject OVB given our choice of X_i and develops sensitivity analyses that flag potential sources of IVB in our setting. In particular, variables that do little to attenuate OVB concerns and significantly affect the discrimination estimate are more likely to contaminate the result with IVB.

4 Empirical Analysis

This section presents the empirical strategy and the main results. We estimate substantial discrimination against immigrant defendants in pretrial detention decisions. We discuss and implement different tests for assessing the degree of OVB and conclude that unobservables are unlikely to explain the results. If anything, unobserved differences in misconduct potential are likely to exert downward bias on the discrimination estimates.

4.1 Benchmark Regressions

Empirical strategy Discrimination, as defined in Section 3, can be identified by *benchmark regressions* – that is, by reduced-form regressions of $R_{ij(i)} (\equiv R_i)$ on I_i and $g(X_i)$.⁶ We assume that g follows a linear probability model in X_i – that is, $\mathbb{E}[Y_i^*|X_i] = X_i'\alpha_X$. Important for the discussion on OVB, we further define $X_i = [X_i^o \ X_i^u]$, where o accounts for variables that are observed by the econometrician and u accounts for variables that are unobserved by the econometrician. The

⁶The coefficient of I_i of a regression of R_i on I_i controlling for $g(X_i)$ may differ from \mathbf{D} since the regression weights are possibly different from the marginal distribution of Y_i^* . This is a caveat of the results below.

observational benchmark test therefore consists of regressions of the following type:

$$R_i = \alpha_0 + \alpha_D I_i + X_i^{o'} \alpha_{X_o} + \varepsilon_i, \quad (2)$$

where $\varepsilon_i = X_i^{u'} \alpha_{X_u} + \xi_i$ is the error term. We distinguish between two sources of unobserved heterogeneity that affect the release decision. First, $X_i^{u'} \alpha_{X_u}$ accounts for unobservables that predict misconduct potential. The vector X_i that accurately predicts Y_i^* may be extensive and may therefore contain information that is not observed by the econometrician. Second, ξ_i accounts for unobservables that do not predict misconduct potential conditional on X_i but may affect the release decision. For example, ξ_i may contain variables that judges use to discriminate on top of immigration status but that are not correlated with Y_i^* after controlling for X_i .

Let $\hat{\alpha}_D$ be the ordinary least squares (OLS) estimator of (2). The main identification assumption for $\mathbb{E}[\hat{\alpha}_D | I_i, X_i^o] = \mathbf{D}$ is $\mathbb{C}(I_i, X_i^u) = 0$. That is to say, the unobservables that predict misconduct potential, X_i^u , are not correlated with immigration status, I_i . This follows from our definition of discrimination because discrimination is defined as average release disparities conditional on misconduct potential. If I_i and X_i^u are correlated, then $\hat{\alpha}_D$ will capture both discrimination and unobserved differences in misconduct potential and will therefore be contaminated by standard OVB. Note, however, that correlation between I_i and ξ_i is allowed by our definition of discrimination given our focus on a disparate impact notion of discrimination so its presence is, therefore, not problematic for the identification of \mathbf{D} . After presenting the main results, we discuss and implement diagnostics to assess the pervasiveness of potential OVB in our main benchmark estimations.

We estimate equation (2) using OLS and include the following sets of controls in the vector X_i^o . First, we include individual-level characteristics related to the criminal history and the current case. These include an indicator for whether the individual has previous prosecutions, the number of previous prosecutions, the severity of the last prosecution (measured as the average pretrial detention rate of the type of crime), an indicator for whether the individual was engaged in pretrial misconduct during a previous prosecution, an indicator for whether the individual has been convicted of a crime in the past, type of crime fixed effects for the most severe imputed crime in the current prosecution, and the number of imputed crimes in the current prosecution. These variables are expected to proxy for misconduct potential.⁷ Second, to improve precision, we include

⁷Arnold et al. (2022) show that discrimination estimates are biased when criminal history and type of crime variables are included in regressions that already control for misconduct potential through an IVB mechanism, which implies that these variables are imperfect proxies for misconduct potential. This can be explained if, for example, the distributions of types of crime differ by immigration status and judges are engaged in crime-specific discrimination patterns, or if there is discrimination against immigrants in previous stages of the judicial process that is manifest in differences in the criminal records. Note, however, that Arnold et al. (2022) estimated observational release disparities

measures of judge leniency and public attorney quality and their squares, measured as residualized (against court-by-year fixed effects) leave out release rates, as in [Dobbie et al. \(2018\)](#). Third, we include court-by-year fixed effects that can potentially account for unobserved shocks that correlate with misconduct potential. Standard errors are clustered at the court-by-year level.

Results Table 2 presents the results. Each column represents a different regression and reports the estimated benchmark coefficient, $\hat{\alpha}_D$, with its corresponding standard error. In the absence of OVB, $\hat{\alpha}_D$ is a valid measure of discrimination, with negative values accounting for discrimination against immigrant defendants. Column 1 shows that the raw (uncontrolled) release rate for immigrants is 6.7 pp lower than the release rate for defendants who are Chilean nationals. The difference is quantitatively important: it represents 8% of the unconditional average release rate or almost 42% of the unconditional average detention rate.⁸

If there are differences in the distribution of pretrial misconduct probabilities between immigrant defendants and Chilean defendants, this result could be explained by OVB. Columns 2, 3, and 4 explore how the gap changes when adding the specific sets of controls previously discussed. When adding the individual controls (Column 2), the release rate gap increases to 10.2 pp. Under the assumption that criminal records and the severity of the current prosecution are positively correlated with misconduct potential, this suggests that the raw gap cannot be explained by differences in latent risk. In fact, the result conjectures that ignoring differences in misconduct potential leads to an underestimation of discrimination. When adding the (residualized) controls for the assigned judge and attorney (Column 3), the release disparity remains unchanged. This is consistent with judges and attorneys being quasi-randomly assigned at the court-by-year level. When adding court-by-year fixed effects (Column 4), the release rate gap decreases to 3 pp. There are two potential explanations: The first explanation is that court-by-time fixed effects mediate discrimination against immigrant defendants if they are systematically assigned to more severe courts (i.e., IVB through institutional discrimination). Table A.I of Appendix A shows that, in fact, courts with less lenient judges have larger shares of immigrant defendants.⁹ The second explanation is that

that control for type of crime and criminal records are close in magnitude to the weighted benchmark regressions that recover the unbiased estimate, which suggests that this set of controls do a reasonably good job when approximating potential misconduct. Concretely, they document unconditional gaps of 7.2 pp that are reduced to 5.2 pp when including controls and fixed effects. The bias-corrected weighted regressions with no controls report discrimination estimates that range from 4.2 pp to 5.4 pp, depending on the specific extrapolation technique.

⁸Under [Bohren et al. \(2023\)](#) notation, assuming that $Y^0 = 0$ (i.e., a case where there are no initial qualification differences between Chilean and immigrants) would imply that the raw (uncontrolled) release rate disparity is a valid measure of discrimination that also internalizes systemic components.

⁹For this result we proceed as follows. To avoid confounding court severity with discrimination, we compute release rates at the judge level only based on cases with national defendants. Then, we compute the case-weighted

the 3.3 pp decrease in the release gap captures unobserved heterogeneity in misconduct potential that varies at the court-by-year level. Below we present suggestive evidence in favor of the first interpretation. Finally, when adding the three sets of controls (Column 5), the gap in release rates between immigrant defendants and Chilean defendants is 8.5 pp. This represents 10.1% of the unconditional average release rate or 53.1% of the unconditional average detention rate.

4.2 Assessing Omitted Variable Bias

The main threat for interpreting the previous estimates as discrimination is OVB. If there are omitted variables that predict misconduct potential and that are correlated with immigration status, then the discrimination estimate will be biased. We conduct two sets of diagnostics to assess the extent to which OVB affects our main estimations. Both exercises suggest that unobservables that predict misconduct potential are unlikely to explain the results and, if anything, they are likely to exert downward bias on our discrimination estimates. The second diagnostic also provides elements for assessing the degree of IVB in our benchmark regressions.

Diagnostic 1: Bounds on selection on unobservables One way of assessing the role of unobservables in explaining the estimated released disparities is to follow the extension proposed by [Oster \(2019\)](#) to the method proposed in [Altonji et al. \(2005\)](#). This approach interprets observables as unobservables that the econometrician happened to observe, establishing an implicit relationship between them. Under this interpretation, and assuming some degree of correlation between X_i^o and X_i^u , the sensitivity of $\hat{\alpha}_D$ to the inclusion of observed control variables is informative about potential OVB because the bias arising from omitting observables is related to the bias induced by the unobserved variables. This allows for the computation of bounds on $\hat{\alpha}_D$ given different assumptions on the informativeness of observables about unobservables.

Based on (2), let $W_{1i} = X_i^{o'}\alpha_{X_o}$ and $W_{2i} = X_i^{u'}\alpha_{X_u}$, and define $\sigma_{jI} = \mathbb{C}(W_{ji}, I_i)$ and $\sigma_j^2 = \mathbb{V}(W_{ji})$. The intuition behind the method proposed in [Altonji et al. \(2005\)](#) and extended by [Oster \(2019\)](#) is to assume that the correlation between the vectors W_{ji} and I_i is proportional – that is,

$$\delta \frac{\sigma_{1I}}{\sigma_1^2} = \frac{\sigma_{2I}}{\sigma_2^2}, \quad (3)$$

with δ being the coefficient of proportionality. This can be rationalized by assuming a correlation structure, in a projection sense, between the vectors X_i^o and X_i^u . [Oster \(2019\)](#) assumes $\delta = 1$. That

average leniency at the court level and run regressions against the share of cases with immigrant defendants. Results show a robust negative correlation between court leniency and immigrant share.

is to say, biases from omitting observables and unobservables are of similar magnitudes. Bounds, however, can be computed under different assumptions on δ to accommodate different a priori correlation structures.¹⁰

With an argument based on auxiliary regressions, [Oster \(2019\)](#) characterizes OVB as a function of δ that naturally suggests bounds on the estimated coefficient. [Oster \(2019\)](#) also notes that the degree to which the sensitivity analysis informs us about OVB depends on the contribution of X_i to the variance of R_i , conditional on I_i . Intuitively, coefficient stability may also arise from lack of explanatory power, so the implied OVB has to be scaled by the explanatory power of the observed and unobserved covariates. This implies that the econometrician also has to make an assumption about R_{max} – that is, the hypothetical R^2 that would be obtained after estimating (2) by OLS including both X_i^o and X_i^u . Bias is less likely to be problematic if coefficient stability after the inclusion of X_i^o is accompanied by important increases in goodness of fit relative to R_{max} .

Table 3 presents the estimated bounds for the coefficient $\hat{\alpha}_D$ under different assumptions on δ and R_{max} . Each cell shows the estimated $\hat{\alpha}_D$ parameter adjusted for OVB under the assumption that selection on observables informs us about selection on unobservables. Note that larger values for δ and R_{max} imply more conservative bounds on $\hat{\alpha}_D$ because both impose a stronger role for unobservables in the release equation.

Imposing $\delta > 0$, which is the standard assumption, suggests that discrimination is underestimated: bounds are larger (in absolute value) than the fully controlled benchmark regressions displayed in Column 5 of Table 2. This follows from the fact that, on average, immigrant defendants have less severe criminal records than Chilean defendants. It also relates to the discussion on Column 2 in Table 2, which posited that, if anything, unobservables exert downward bias on the discrimination estimates. The results show that the only way in which OVB works against finding discrimination is by assuming that X_i^o and X_i^u are negatively correlated – that is, that defendants with observables that induce lower misconduct potential also have unobservables that work in the opposite direction. Although it is counterintuitive, this scenario can be explored by assuming negative values for δ . Note that, even in the most conservative (and presumably implausible) scenario in which the negative correlation is large ($\delta = -1$) and the explanatory power of omitted variables is sizable ($R_{max} = 1$), the point estimate still suggests (mild) discrimination against immigrant defendants. These results suggest that our main findings are unlikely to be driven by unobservables that explain misconduct potential and correlate with immigration status.

¹⁰ Assuming $\delta = 1$ mimics a setting in which X_i^o is a random subsample of X_i . If one assumes that the *most important* elements of X_i are included in X_i^o , then δ is likely to be smaller than one.

Diagnostic 2: Using the outcome equation to test for OVB We develop an alternative test to assess the role of unobservables in explaining the estimated release disparities. This approach uses the outcome equation (controlling for selection bias) to validate the benchmark equation. Intuitively, omitted variables that compromise the identification of \mathbf{D} have to predict pretrial misconduct potential and correlate with immigration status. Then, a regression of observed pretrial misconduct against immigration status controlling for our proxies of misconduct potential should inform, through the partial correlation between being an immigrant and pretrial misconduct, about unexplained sources of misconduct potential that correlate with immigration status.

Formally, if we assume linear models for simplifying the exposition, then the release and outcome equations can be written as

$$R_i = \alpha_0 + \alpha_D I_i + X_i^{o'} \alpha_{Xo} + X_i^{u'} \alpha_{Xu} + \xi_i, \quad (4)$$

$$PM_i = \beta_0 + \beta_D I_i + X_i^{o'} \beta_{Xo} + X_i^{u'} \beta_{Xu} + \epsilon_i, \quad (5)$$

where $PM_i \in \{0, 1\}$ is the realized pretrial misconduct if released.

In what follows, the estimated coefficients from regressions that include (I_i, X_i^o, X_i^u) are denoted by $(\hat{\alpha}_D^{ou}, \hat{\beta}_D^{ou})$, and the estimated coefficients from regressions that only include (I_i, X_i^o) are denoted by $(\hat{\alpha}_D^o, \hat{\beta}_D^o)$. Concerns about OVB in observational benchmark regressions (possibly) imply that $\mathbb{E}[\hat{\alpha}_D^{ou}|I_i, X_i] \neq \mathbb{E}[\hat{\alpha}_D^o|I_i, X_i^o]$. We argue that failing to reject $\beta_D = 0$ in an outcome regression that omits X_i^u implies that $\mathbb{E}[\hat{\alpha}_D^{ou}|I_i, X_i] = \mathbb{E}[\hat{\alpha}_D^o|I_i, X_i^o]$; thus, the outcome regression can be used as an indirect test for OVB in the release equation.

To see why, recall that, by assumption, $X_i = [X_i^o \ X_i^u]$ is an accurate proxy of misconduct potential, implying that $\beta_D = 0$: immigration status cannot predict pretrial misconduct after controlling for pretrial misconduct potential. The hypothetical regression of PM_i versus I_i and X_i yields unbiased estimates. Specifically, $\mathbb{E}[\hat{\beta}_D^{ou}|I_i, X_i] = \beta_D = 0$.¹¹ This logic does not hold in a regression that omits X_i^u because $\mathbb{E}[\hat{\beta}_D^o|I_i, X_i^o] = \beta_D + bias$, where the bias depends on β_{Xu} and the correlation between I_i and X_i^u through a standard OVB formula. Assuming $\beta_{Xu} \neq 0$ (the contrary would imply that OVB is ruled-out by assumption), nonzero estimates of $\hat{\beta}_D^o$ indicate that the correlation between I_i and X_i^u is nonzero. Intuitively, I_i becomes a good predictor for pretrial misconduct only if it correlates with elements of X_i that are not observed by the econometrician.

¹¹This requires us to assume that ϵ_i is uncorrelated with I_i , so $\mathbb{E}[\epsilon_i|I_i, X_i] = \mathbb{E}[\epsilon_i|X_i] = 0$. If we think of ϵ_i as the projection error, $Y_i^* = g(X_i) + \epsilon_i$, then the second equality is true by construction, and the first equality requires us to assume that the projection error does not vary with immigration status, which is a plausible assumption given that X_i^u is unrestricted (eventually making ϵ_i negligible) and allowed to correlate with I_i .

However, the nonzero correlation between I_i and X_i^u is exactly what induces bias when estimating discrimination in the release equation without observing X_i^u . In particular, $\mathbb{E}[\hat{\alpha}_D^{ou}|I_i, X_i] = \mathbf{D}$ but $\mathbb{E}[\hat{\alpha}_D^o|I_i, X_i^o] = \mathbf{D} + \text{bias}$, where the bias depends on α_{X^u} and, again, the correlation between I_i and X_i^u . Note that the correlation that induces OVB in the release equation is the same that induces nonzero $\hat{\beta}_D^o$ coefficients in the outcome equation.

Putting the two arguments together, we note that failing to reject $\beta_D = 0$ in a regression that omits X_i^u (i.e., when the point estimate is $\hat{\beta}_D^o$) implies $\mathbf{D} = \mathbb{E}[\hat{\alpha}_D^{ou}|I_i, X_i] = \mathbb{E}[\hat{\alpha}_D^o|I_i, X_i^o]$. Put simply, not rejecting that immigration status does not predict pretrial misconduct on top of X_i^o confirms that the vector X_i^o successfully controls for group differences in misconduct potential and, therefore, the discrimination estimates from equation (4) are not affected by standard OVB.¹² One challenge in implementing this test is that the sample of released defendants is selected because pretrial misconduct if released is not observed for detained individuals. Accordingly, we estimate selection models that use the judge and attorney controls as the excluded variables in the outcome equation based on the assumption that judges' leniency and attorneys' quality affect selection into treatment but do not affect misconduct potential.¹³ Our main specification implements a standard two-step parametric Heckit selection correction (Heckman, 1974). We also provide results using the semiparametric correction proposed by Newey (2009).^{14,15}

Table 4 presents the results using the parametric selection correction. Table A.V of Appendix A presents the results using the semiparametric selection correction. Each column represents a

¹²More conservatively put, it may be the case that the vector X^u is still important for explaining Y_i^* and that some of its elements correlate with I_i . But if correlations are in opposite directions so the net correlation between X_i^u with I_i conditional on X_i^o is zero, then the benchmark regression effectively avoids OVB in expectation.

¹³To avoid saturating the nonlinear first-stage model with the court-by-time fixed effects, we replace them with time-varying variables at the court level: number of judges, average pretrial release rate, and number of prosecutions (within a court in a given year). Estimating the fully controlled benchmark regression (Column 5 in Table 2) and replacing the court-by-time fixed effects by these time-varying court-level controls yields a point estimate of -0.084, suggesting that the time-varying variables do a good job of approximating the court-by-time fixed effects.

¹⁴The Heckit selection correction assumes that the error terms of the first and second steps are jointly normal. The semiparametric correction of Newey (2009) uses series approximations to compute control function corrections. We implement the semiparametric correction following Low and Pistaferri (2015) where the first step uses Gallant and Nychka (1987) estimator to approximate the unknown density by third-degree Hermite polynomial expansions and the second step controls for non-linear transformations of the density prediction. As in Low and Pistaferri (2015), we consider three models. Let \hat{f} denote the predicted density. The control function used in Model I is \hat{f} and its square, in Model II is $\Phi(\hat{\alpha}_0 + \hat{\alpha}_1 \hat{f})$ and its square—where Φ is the normal cumulative distribution function and $(\hat{\alpha}_0, \hat{\alpha}_1)$ are the estimated coefficients of a Probit model of *Release* on a constant and \hat{f} —, and in Model III is $\lambda(\hat{\alpha}_0 + \hat{\alpha}_1 \hat{f})$ and its square—where $\lambda(x) = \phi(x)/\Phi(x)$ is the inverse Mills ratio and ϕ the normal density.

¹⁵One limitation of the Heckit selection model is that it imposes monotonicity on the leniency instrument (Kline and Walters, 2019). This assumption has received scrutiny in the related literature (Frandsen et al., 2023). In the Appendix, we show that the results hold in a specification that interacts the instruments with the observed covariates, as in Mueller-Smith (2015) and Rivera (2022), which should attenuate concerns for monotonicity violations. Also, Newey (2009) methodology does not impose monotonicity on the excluded instruments, being robust to this caveat.

different regression and reports the estimated $\hat{\beta}_D^o$ coefficient from equation (5) with its corresponding standard error. In all regressions, the selection equation includes the complete set of controls, but the included covariates in the outcome equation vary between columns. Note that the F-test for judge leniency and attorney quality (and their squares) suggests they effectively help with the selection correction. Column 1 in Table 4 shows that after controlling for selection, pretrial misconduct rates are 7.9 pp smaller for immigrant defendants. This suggests that the benchmark regression with no controls (Column 1 in Table 2) is affected by OVB. The implied OVB, however, suggests that abstracting from controls leads to an underestimation of the discrimination against immigrant defendants. Column 2 shows that including individual controls (criminal records and current case controls) reduces the pretrial misconduct rate differences to 1.4 pp. That is to say, conditioning on the individual controls reduces group variation in Y_i^* by more than 80%. This suggests that, although imperfect, criminal records and current case controls are good predictors of pretrial misconduct. By contrast, adding time-varying court controls do little to correct for unobserved differences in misconduct potential (Column 3). Taken together, we interpret this as support for our choice of X_i^o . The vector X_i^o seems to do a good job in accounting for group differences in misconduct potential, which implies that the discrimination estimates of Column 5 of Table 2 are not driven by unobserved variation in latent risk. The model with the semiparametric selection correction proposed by Newey (2009) yields the same conclusions.^{16,17}

A similar test can be implemented by comparing the role of unobservables in the selection and outcome equations using Kitagawa–Oaxaca–Blinder (KOB) decompositions (Kitagawa, 1955; Oaxaca, 1973; Blinder, 1973). Intuitively, unobservables can affect the interpretation of the estimated benchmark coefficients if they correlate with immigration status and explain misconduct potential. If unobservables are important for predicting group-specific misconduct potential when making release decisions, they should also explain differences in observed pretrial misconduct rates. Appendix C shows KOB decompositions based on the vectors of individual covariates, using both raw data and residualized data against court-by-time fixed effects. The share of variation that is

¹⁶The three models reproduce the pattern displayed in Table 4. After accounting for selection, immigrants have lower pretrial misconduct rates. These gaps are substantially reduced after including individual controls in the outcome equation. In Model I, individual controls reduce the gap by two-thirds, although the gap remains significant at almost 3 pp after including the complete set of controls. In Models II and III the individual controls completely eliminate the gap in pretrial misconduct to a nonsignificant estimate of less than a percentage point, even in the specification that excludes time-varying court characteristics.

¹⁷One concern with these results is that the source of pretrial misconduct (nonappearance in court versus pretrial recidivism) may not be weighted equally by judges and, therefore, if nonappearance in court is more frequent for immigrant defendants and is more heavily weighted by judges, then these results would not be capturing the relevant margin of risk. In Appendix A, we show that the results hold when considering both sources of pretrial misconduct separately. While risk disparities in nonappearance in court are less severe, it is still the case that the rates are significantly larger for Chilean defendants after accounting for selection, and that the disparities disappear after controlling by the individual characteristics.

not explained by observables is large in the release equation but it is essentially zero in the outcome equation. This suggests that the immigrant indicator in the release equation is effectively capturing discrimination and not differences in unobservables.

On included variable bias Our diagnostics do not explicitly deal with IVB concerns, but the second OVB diagnostic provides some insights to assess the pervasiveness of IVB. Individual controls are much more important than court-by-time variation in terms of accounting for group differences in pretrial misconduct rates. This indicates that the inclusion of court-by-time fixed effects in the benchmark regression may induce IVB in the sense that these particular fixed effects substantially impact the estimated discrimination coefficient but do little to attenuate OVB. In other words, regressions that include court-by-year fixed effects could be over-controlling and therefore inducing bias to our discrimination estimates.

IVB affects the interpretation of the sensitivity analysis. As shown in Table 2, including court-by-year fixed effects significantly reduces the discrimination estimate. The results presented in Table 4 suggest that the sensitivity of $\hat{\alpha}_D$ to the inclusion of court-by-year fixed effects is not explained by unobserved variation in misconduct potential but rather by an alternative mechanism that mediates discrimination against immigrant defendants. For example, the fact that discrimination decreases when controlling by court-by-time fixed effects may indicate that part of the discrimination against immigrant defendants is driven by their systematic allocation to courts that are more severe on average. Results displayed in Table A.I of Appendix A support this hypothesis. Therefore, from a disparate impact perspective, the relevant discrimination estimate could be the one that only includes individual controls (Column 2 of Table 2), which suggests larger levels of discrimination against immigrant defendants than the fully controlled specification. Said differently, the estimate presented in Column 5 of Table 2 may omit the role of institutional discrimination in aggregate patterns. Because this conjectured IVB attenuates the discrimination estimate, we take a conservative position by presenting the fully controlled benchmark regression as our preferred specification. It is important to keep in mind, however, that the impact of IVB may mean that the actual discrimination against immigrant defendants is even larger.

5 Dissecting the Discrimination Estimates

Section 4 documents a robust pretrial release rate disparity between immigrant defendants and Chilean defendants after controlling for variables that proxy for misconduct potential that we

interpret as evidence of discrimination against immigrant defendants. This section dissects this reduced-form result to get a more comprehensive characterization of the discrimination patterns against immigrant defendants. All results presented in this section consider the full set of controls and should therefore be compared to Column 5 in Table 2.

First, we explore some behavioral drivers of the discrimination estimate. We provide evidence that suggests that a sizable portion of the effect stems from an informational problem: immigrant criminal records are censored and this leads judges to punish immigrants with no previous prosecutions. Outcome test estimations show that the result cannot be rationalized by accurate statistical discrimination, suggesting that biased beliefs and taste-based discrimination play a role in the resolution of the informational problem. Second, we explore heterogeneities by type of crime and find that discrimination is especially large for drug-related offenses. This is consistent with a hostile discourse in the public sphere against immigrants that links immigration to drug trafficking. Finally, we explore the time trend of discrimination patterns considering the recent immigration wave discussed in Section 2. We find that discrimination has dramatically increased in recent years, and that this increase is not explained by the change in the composition of the immigrant population.

5.1 Behavioral Drivers of Discrimination

The criminal records of immigrant defendants are censored (i.e., judges cannot observe the criminal histories of immigrant defendants in their origin countries). This means that judges may interpret the information available to them differently. Similar to what has been found in the literature on Ban-the-Box policies (Agan and Starr, 2018; Doleac and Hansen, 2020; Raphael, 2021), this may be more detrimental to immigrant defendants with no criminal history in Chile because the main information contained in the criminal records is at the extensive margin. We explore whether this informational gap is in fact driving discrimination patterns against immigrant defendants.

As an indirect test of the information hypothesis, we estimate the main benchmark regressions separately for defendants with and for defendants without previous prosecutions. These estimates include all sets of controls analogous to Column 5 in Table 2. Results are displayed in Columns 1 and 2 in Table 5. Immigrant defendants with previous prosecutions are 1.6 pp less likely to be released compared to Chilean defendants with previous prosecutions; compared to a difference of 13.2 pp for defendants without previous prosecutions. To assess the magnitude of the difference between the estimations, note that the (unconditional) pretrial release rate is 12 pp larger for defendants with no previous prosecutions, which is roughly similar in magnitude to the difference in the discrimination estimate between samples. This implies that not having previous prosecutions is,

essentially, irrelevant for immigrant defendants: they are not “rewarded” for having clean criminal records. This result is consistent with judges systematically imputing nonzero criminal histories for all immigrant defendants regardless of their criminal record in Chile.

To further explore this intuition, we test whether judge experience matters for the aggregate discrimination patterns. If judges face an informational problem whose imperfect solution leads to discriminatory practices, they should gradually implement non-discriminatory solutions as they gain experience. Consequently, we should observe that more experienced judges exhibit lower levels of discrimination. To test this, we define experienced judges as those who handled more than 200 cases in the period 2008–2012 and estimate the benchmark regressions for the period 2013–2017, including the experience indicator and its interaction with the immigrant indicator. Column 3 shows that release disparities are around 35% smaller for experienced judges.

As discussed in Section 3, aggregate patterns of discrimination represent a possibly complex combination of different behavioral sources – these being, statistical discrimination, biased beliefs, and taste-based discrimination, together with institutional discrimination and omitted payoff bias – which is not possible to decompose without additional structural assumptions (Bohren et al., 2021; Hull, 2021; Arnold et al., 2022). This implies that the fact that discrimination in pretrial detention decisions stems from an informational problem does not necessarily mean that the aggregate estimate can be rationalized by accurate statistical discrimination. On the one hand, judges can circumvent the informational problem based on stereotypes. This may, for example, lead judges to “overcorrect” for censored criminal records for immigrants. On the other hand, the informational problem may provide judges with the room to exercise taste-based discrimination.

To get additional insights, we implement the outcome test (Becker, 1957, 1993) using the observational approaches of Knowles et al. (2001) and Grau and Vergara (2023).¹⁸ The outcome test is a diagnostic for differences in effective selection thresholds between groups. Differences in effective thresholds can be identified by differences in observed misconduct among marginally released defendants (Arnold et al., 2018; Hull, 2021). Knowles et al. (2001) propose using the average behavior of released defendants. Grau and Vergara (2023) refine Knowles et al. (2001) approach by performing a sample selection procedure that limits the extent of inframarginality bias.¹⁹

¹⁸The small share of the immigrant population prevents us from implementing the instrument-based approach proposed by Arnold et al. (2018).

¹⁹When risk distributions vary by group, the behavior of the average released defendant is potentially different from the behavior of the marginally released defendants. This is called inframarginality bias. Grau and Vergara (2023) deal with this problem in two stages. First, they use observational variation to identify samples with a smaller risk of inframarginality bias (“marginals”). This procedure is done by ranking released defendants according to their propensity score. Second, they perform differences in means of pretrial misconduct rates between immigrant defendants and Chilean defendants using the samples of marginal defendants. The sample of marginal defendants

Intuitively, discrimination in pretrial detention decisions may originate from judges setting stricter release thresholds for immigrant defendants, in a disparate impact sense, that imply lower release rates at equivalent true pretrial misconduct probabilities. These differences in the treatment of immigrant defendants could be driven by stereotypes or animus or both. This source of discrimination does not incorporate sources of release disparities driven by differences in group-specific misconduct potential. Differences in true pretrial misconduct potential affect how often a given defendant crosses the release threshold, but the threshold itself is not informative about them. Then, rejecting the outcome test implies that the observed behavior of marginally released defendants is inconsistent with a selection process that lacks stereotypes and animus (Hull, 2021).

Columns 4 and 5 in Table 2 show the results of the outcome test using Knowles et al. (2001) and Grau and Vergara (2023) approaches, respectively. Estimates indicate that immigrant defendants face effective release thresholds that are, on average, between 5 and 6.5 pp stricter. That is to say, immigrants are required to have, on average, smaller pretrial misconduct probabilities to qualify for pretrial release relative to Chilean defendants. Although the magnitudes between benchmark and outcome regressions are not directly comparable (Domínguez et al., 2022), this result rejects accurate statistical discrimination as the only driver of the discrimination estimates, implying that a combination of stereotypes and animus plays an important role in explaining aggregate patterns of discrimination against immigrants.^{20,21}

5.2 Heterogeneity by Type of Crime

If statistical structures, stereotypes, or judges' preferences vary with type of crime, then discrimination patterns should do as well. Exploring this particular pattern of heterogeneity is instructive because a hostile facet of the public discourse regarding immigrants links immigration with drug trafficking, thefts and robberies, and violent crimes, so discrimination should be particularly salient in pretrial detention decisions concerning these crimes.

is defined as the 10% of released defendants with larger conditional probabilities of being marginal. That is why the number of observations is smaller when using this approach. Additional details about the implementation of the outcome test proposed by Grau and Vergara (2023) are presented in Appendix D.

²⁰The average pretrial misconduct rate is larger in Column 5 (37%) relative to Column 4 (29%). This suggests that Grau and Vergara (2023) implementation of the outcome test successfully attenuates inframarginality concerns by focusing on a sample of released defendants that are, on average, riskier and, therefore, presumably more likely to be at the margin of pretrial detention.

²¹In Appendix A, we compute separate outcome tests for nonappearance in court and pretrial recidivism. For both dependent variables, the point estimate is negative, attenuating concerns about immigrants being more likely to fail to appear in court. The differences at the margin of release are larger for pretrial recidivism, which suggests that stereotypes and discriminatory preferences are particularly important for assessing pre-trial recidivism.

To investigate this, we split prosecutions between the following nine mutually exclusive categories: homicides, sexual offenses, thefts and robberies, other property crimes, drug offenses, white collar crimes and tax crimes, crimes against public trust, crimes against the freedom and privacy of people, and other crimes.²² Using these categories, we estimate the following regression:

$$R_i = \alpha_0 + \sum_{k=1}^9 \alpha_D^k I_i \cdot C_i^k + X_i^{o'} \alpha_{Xo} + \varepsilon_i, \quad (6)$$

where C_i^k is an indicator that takes the value one if the imputed crime is in category k . All other variables are defined as in equation (2); therefore, the noninteracted crime indicators are included in X_i^o . In this specification, $\hat{\alpha}_D^k$ identifies the controlled pretrial release disparities between Chilean defendants and immigrant defendants (our measure of discrimination) in crime category k .

Table 6 shows the results. Discrimination against immigrants is especially large for drug offenses – around four times larger than the baseline benchmark estimate. Significant effects are also found for thefts or robberies and property crimes. Other violent crimes like homicides and sexual offenses show no significant differences; however, as shown in Table A.VIII of Appendix A, the number of immigrants imputed for sexual offenses and homicides in the period considered is remarkably low, therefore potentially being underpowered to precisely estimate disparities in those particular categories. Crime categories that are associated with less hostile discourses in the public sphere (such as white-collar crimes and tax crimes or crimes against privacy, and for which the number of prosecuted immigrants is in fact larger) also show no significant release disparities. Overall, these results suggest that discrimination patterns are mainly driven by drug offenses, property crimes, and thefts and robberies. This is consistent with the idea that public discourses regarding immigrants play a role in the formation of stereotypes and hostile preferences.²³

5.3 Time Trend and the Immigration Wave

Recall in Section 2 it was premised that immigration flows have substantially increased in recent years, generating a hostile discourse against immigrants in the public sphere. It was also argued that the composition of the immigrant population by country of origin has changed considerably. We now examine whether these changes have been accompanied by a change in the aggregate

²²For examples of the crimes included in each category, see Appendix B.

²³One concern with these results is that the imputed drug offenses may vary between immigrant and Chilean defendants. For example, drug offenses for Chilean defendants could be more often related to drug consumption and possession rather than drug production and selling. Table A.VII of Appendix A replicates Table 6 differentiating between the two types of drug offenses, and confirms that the estimated effect is driven by drug offenses related to production and selling.

discrimination patterns across time.

Following the changes in immigration flows described in Section 2, Table 7 shows the benchmark estimates separately for the periods 2008–2012 and 2013–2017. The fully controlled discrimination estimate increased from 6.5 to 9.9 pp, a 53% increase. A potential explanation for this increase is the increase in the hostility of the public discourse that may have affected stereotypes and preferences. Alternatively, this result could be driven by both a change in the magnitude of the phenomena and a composition effect related to the country of origin of the new group of immigrants. Although Peru was the principal source of immigration flows to Chile for many years, beginning in 2013 the relative share of immigrants from Colombia, Haiti, and Venezuela increased considerably. If there is heterogeneity in discrimination patterns – that is, discrimination is more intense against the “new” group of immigrants (for example, because of more salient racial differences) – then the increase of discrimination between the two periods could be explained by a composition effect and not necessarily by an increase in the average intensity of discrimination for a given nationality.²⁴ Column 4 presents the discrimination estimate for the 2013–2017 period separately for the “new” and the “old” groups of immigrants.²⁵ We cannot reject that the discrimination estimates for both groups are equal, which suggests that the increase in discrimination is unlikely to be driven by the change in the composition of the immigrant population. These results reinforce the previous finding that stereotypes and taste-based discrimination possibly play a role in the aggregate estimates because accurate statistical discrimination would imply an implausible substantial change in the underlying risk distribution.

6 Conclusion

This paper leverages rich administrative data to test for discrimination in pretrial detention decisions against defendants who are immigrants in Chile. We find that immigrant defendants are released pretrial at substantially lower rates relative to Chilean defendants with similar criminal records, case characteristics, and court-by-time fixed effects. Several diagnostics suggest that these estimated disparities are not driven by unobserved differences in pretrial misconduct potential between Chilean defendants and immigrant defendants. We therefore interpret the results as robust evidence of discrimination against immigrant defendants in pretrial detention decisions.

²⁴Potential differences in risk between the two groups are not a concern for the dynamic results because benchmark regressions include the full set of controls.

²⁵This exercise cannot be computed for the 2008–2012 period because the “new” group, by definition, is essentially nonexistent before 2013, as the number of observations suggests.

We show that release disparities are an order of magnitude larger for defendants with no previous criminal prosecutions in Chile. We argue this suggests that discrimination is originated in an informational problem driven by censored criminal records. We show that accurate statistical discrimination cannot rationalize the release disparities, suggesting that biased beliefs and taste-based discrimination play an important role. Consistent with the hostile discourses against immigrants in the public sphere, we also find that discrimination in pretrial detention decisions is particularly severe for drug offenses, and that the intensity of discrimination has increased in recent years during which the magnitude and the composition of the immigrant population has changed considerably.

Overall, our findings suggest that discrimination against immigrant defendants in pretrial detention decisions is substantial in the Chilean context and highlight the importance of implementing effective anti-discrimination policies to better assimilate the integration of immigrants in destination countries. Several countries are enacting anti-discrimination policies (or strengthening existing ones) to foster the integration of immigrant populations (OECD, 2019). This policy challenge appears to be particularly urgent in countries where immigration is a relatively recent phenomenon and institutions are not adequately equipped to deal with unexpected shocks in this regard.

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Table 1: Descriptive Statistics

	Chilean	Immigrant
Released	0.84	0.77
Outcomes (only for released)		
Nonappearance in court	0.17	0.16
Pretrial recidivism	0.19	0.13
Pretrial misconduct	0.29	0.24
Individual Characteristics		
Male	0.88	0.87
At least one previous case	0.68	0.41
At least one previous pretrial misconduct	0.40	0.22
At least one previous conviction	0.65	0.39
No. of previous cases	4.58	2.48
Severity previous case	0.09	0.05
Severity current case	0.18	0.18
No. of imputed crimes	1.22	1.22
Court Characteristics		
Average severity (year/Court)	0.09	0.11
No. of cases (year/Court)	3,024	3,557
No. of judges (year/Court)	46	58
Observations (released)	580,403	4,900
Observations (nonreleased)	112,606	1,462

Notes: This table presents descriptive statistics for our estimation sample. The sample considers all arraignment hearings for adult defendants who were arrested between 2008 and 2017. We do not include hearings due to legal summons and only consider types of crimes with at least a 5% probability of pretrial detention. When defendants are accused of more than one crime, we retain the information related to the most severe crime (with severity measured as the probability of pretrial detention) and record the number of crimes imputed in the same case. More details about the data and the variables can be found in [Appendix B](#).

Table 2: Main Results–Benchmark Regressions: $\hat{\alpha}_D$

	Dependent Variable: Pretrial Release				
	(1)	(2)	(3)	(4)	(5)
Immigrant	-0.067 (0.014)	-0.102 (0.013)	-0.063 (0.013)	-0.030 (0.012)	-0.085 (0.012)
Mean dep. variable	0.84	0.84	0.84	0.84	0.84
Individual controls	No	Yes	No	No	Yes
Attorney and judge controls	No	No	Yes	No	Yes
Court-by-year fixed effects	No	No	No	Yes	Yes
No. of Immigrants	6,362	6,362	6,362	6,362	6,362
No. of Chileans	693,009	693,009	693,009	693,009	693,009
R-squared	0.000	0.173	0.011	0.023	0.197

Notes: This table presents the estimated benchmark coefficient, $\hat{\alpha}_D$, of equation (2) and its corresponding standard error in parentheses (clustered at the court-by-year level). The dependent variable is a binary variable that takes value 1 if the defendant was pretrial released. Each column represents a different regression that includes different sets of controls. *Individual controls* include an indicator for whether the individual has previous prosecutions, the number of previous prosecutions, the severity of the last prosecution (measured as the average pretrial detention rate of the type of crime), an indicator for whether the individual was engaged in pretrial misconduct during a previous prosecution, an indicator for whether the individual has been convicted of a crime in the past, type of crime fixed effects for the most severe imputed crime in the current prosecution (consisting of nine mutually exclusive categories), and the number of imputed crimes in the current prosecution. *Attorney and judge controls* include judge leniency and public attorney quality and their squares, measured as residualized (against court-by-year fixed effects) leave out release rates, as in Dobbie et al. (2018).

Table 3: [Oster \(2019\)](#) Test for Coefficient Stability: $\hat{\alpha}_D$

	δ					
	-1	-0.5	-0.25	0.25	0.5	1
R_{max}						
0.25	-0.080	-0.082	-0.083	-0.086	-0.087	-0.089
0.5	-0.058	-0.071	-0.078	-0.092	-0.099	-0.114
0.75	-0.038	-0.061	-0.072	-0.097	-0.111	-0.140
1	-0.020	-0.050	-0.067	-0.103	-0.124	-0.168

Notes: This table presents bounds for the estimated coefficient $\hat{\alpha}_D$ of equation (2) that correct for potential OVB by implementing the method developed by [Oster \(2019\)](#), under different assumptions on δ and R_{max} . δ is the coefficient of proportionality that accounts for the correlation between observables and unobservables. $\delta = 0$ is not included because it rules out OVB by construction. R_{max} is the R^2 of a regression that includes observables and unobservables. Each cell presents a bound on the coefficient of being immigrant on pretrial release relative to the regression that includes all sets of covariates (Column 5 of Table 2).

Table 4: Parametric Selection Model for Assessing OVB: $\hat{\beta}_D^o$

	Dependent Variable: Pretrial Misconduct			
	(1)	(2)	(3)	(4)
Immigrant	-0.079 (0.007)	-0.014 (0.006)	-0.091 (0.007)	-0.012 (0.006)
Mean dep. variable	0.29	0.29	0.29	0.29
Individual controls	No	Yes	No	Yes
Court-by-year controls	No	No	Yes	Yes
No. of Immigrants	4,900	4,900	4,900	4,900
No. of Chileans	580,407	580,407	580,407	580,407
F-test for excluded variables	948.9	948.9	948.9	948.9

Notes: This table presents the results from the two-step parametric sample selection model (Heckman, 1974) with different sets of controls, where the dependent variable is any pretrial misconduct. We report the point estimate for the immigrant indicator (i.e., the coefficient $\hat{\beta}_D^o$) of equation (5) and its standard error. Both sets of controls (*individual controls* and *court-by-year controls*) are always included in the selection equation, where the dependent variable is pretrial release, but the columns vary in their inclusion in the outcome equation. Judge and attorney controls are defined as in Table 2 and are excluded from the outcome equation. The last row presents the joint F-test for judge and attorney (excluded) controls in the selection equation. *Individual controls* are defined as in Table 2. To avoid saturating the nonlinear first-stage with court-by-year fixed effects they are replaced in the regressions by court-by-year time varying covariates – namely, the average number of judges, the average pretrial release rate, and the number of prosecutions (within a court in a given year).

Table 5: Behavioral Drivers of Discrimination and the Role of Information

Dependent variables	Benchmark Test (Pretrial Release)			Outcome Test (Pretrial Misconduct)	
	Previous Prosecution			KPT (4)	GV (5)
	Yes (1)	No (2)	(3)		
Immigrant	-0.016 (0.007)	-0.132 (0.015)	-0.133 (0.023)	-0.050 (0.009)	-0.065 (0.017)
Immigrant x experienced judge			0.049 (0.022)		
Mean dep. variable	0.80	0.92	0.82	0.29	0.37
No. of Immigrants	2,630	3,732	4,187	4,900	843
No. of Chileans	472,637	220,372	357,521	580,403	57,688
R-squared	0.19	0.17	0.21		

Notes: Columns 1 to 3 present the estimated benchmark coefficient, $\hat{\alpha}_D$, of the extensions of equation (2) and its corresponding standard error (clustered at the court-by-year level). The dependent variable is pretrial release. Each column represents a different regression and includes all sets of controls analogous to Column 5 in Table 2 (see Table 2 notes for details). Columns 1 and 2 present $\hat{\alpha}_D$ separately for defendants with and for defendants without previous prosecutions. Column 3 presents $\hat{\alpha}_D$ for the period 2013–2017 and includes an interaction between I_i and an indicator that takes the value one if the case was handled by an experienced judge (i.e., judges that handled 200 or more cases in the 2008–2012 period). Columns 4 and 5 present the outcome test estimations using Knowles et al. (2001) (KPT) and Grau and Vergara (2023) (GV) approaches, respectively. The dependent variable is any pretrial misconduct. KPT approach consists on an OLS regression of pretrial misconduct on the immigrant indicator for the complete sample of released defendants. GV performs a sample selection procedure that limits the extent of inframarginality bias in the KPT implementation. Details of the outcome test implementation of GV can be found in Appendix D.

Table 6: Discrimination by Type of Crime: $\hat{\alpha}_D^k$

	Coeff.	SE
Homicide	-0.032	(0.066)
Sexual offense	0.000	(0.033)
Theft or robbery	-0.036	(0.018)
Other property crime	-0.051	(0.014)
Drug offense	-0.353	(0.034)
White collar or tax crime	-0.016	(0.019)
Crime against public trust	0.029	(0.015)
Crime against people's freedom and privacy	-0.002	(0.006)
Other crimes	0.013	(0.020)
Mean dep. variable	0.84	
No. of Immigrants	6,362	
No. of Chileans	693,009	
R-squared	0.198	

Notes: This table presents the estimated benchmark coefficients by type of crime, $\hat{\alpha}_D^k$, of equation (6) and its corresponding standard error in parentheses (clustered at the court-by-year level). The dependent variable is a binary variable that takes value 1 if the defendant was pretrial released. The regression includes all sets of controls analogous to Column 5 in Table 2 (see Table 2 notes for details).

Table 7: Discrimination in Different Periods and Group Heterogeneity: $\hat{\alpha}_D$

	Dependent Variable: Pretrial Release			
	2008-2012		2013-2017	
	(1)	(2)	(3)	(4)
Immigrant	-0.065 (0.016)		-0.099 (0.016)	
Immigrant (old group)		-0.062 (0.017)		-0.097 (0.015)
Immigrant (new group)				-0.104 (0.022)
Mean dep. variable	0.85	0.85	0.82	0.82
No. of Immigrants (old group)	1,956	1,956	2,873	2,873
No. of Immigrants (new group)	219	0	1,314	1,314
No. of Chileans	335,488	335,488	357,521	357,521
R-squared	0.178	0.177	0.213	0.213

Notes: This table presents the estimated benchmark coefficient, $\hat{\alpha}_D$, of extensions for equation (2) and its corresponding standard error (clustered at the court-by-year level). The dependent variable is a binary variable that takes value 1 if the defendant was pretrial released. Each column represents a different regression and includes all sets of controls analogous to Column 5 in Table 2 (see Table 2 notes for details). Columns 1 and 2 use data from the period 2008–2012, and Columns 3 and 4 use data from the period 2013–2017. In columns 2 and 4 the immigrant indicator is defined separately depending on the nationality of the immigrant. *New group* includes immigrants from Colombia, Haiti, and Venezuela. *Old group* considers immigrants from all the other countries.

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

Online Appendix

Patricio Domínguez, Nicolás Grau, Damián Vergara

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A Additional Figures and Tables

Table A.I: Correlation Between Court Leniency and Immigrant Share

	(1)	(2)	(3)	(4)
Share Immigrant	-0.416 (0.114)	-0.411 (0.112)	-0.979 (0.119)	-0.967 (0.119)
Residualized against type of crime	No	Yes	No	Yes
Weighted by number of cases	No	No	Yes	Yes
Observations	1451	1451	1451	1451
R-squared	0.025	0.025	0.167	0.164

Notes: This table presents results from a regression of the average judge leniency at the court-by-year level against the share of cases at the court-by-year level where the defendant is an immigrant. To avoid confounding the results with discrimination, judge leniency is computed as the mean pretrial release rate among the cases where defendants are nationals. Then, court leniency is defined as the case-weighted judge leniency at the court-by-year level. To control for potential selection into crime severity among courts, Columns (2) and (4) consider a measure of individual judge-leniency where the release rate is residualized against indicator variables of type of crime. Columns (1) and (2) consider unweighted regressions at the court-by-year level, while Columns (3) and (4) consider regressions weighted by the number of cases.

Table A.II: Parametric Selection Model for Assessing OVB: $\hat{\beta}_D^o$
(Table 4 with Different Dep. Variable)

	Dependent Variable: Nonappearance in Court			
	(1)	(2)	(3)	(4)
Immigrant	-0.027 (0.005)	-0.008 (0.005)	-0.046 (0.005)	-0.006 (0.005)
Mean dep. variable	0.17	0.17	0.17	0.17
Individual controls	No	Yes	No	Yes
Court-by-year controls	No	No	Yes	Yes
No. of Immigrants	4,900	4,900	4,900	4,900
No. of Chileans	580,407	580,407	580,407	580,407
F-test for excluded variables	948.9	948.9	948.9	948.9

Notes: This table presents the results from the two-step parametric sample selection model (Heckman, 1974) with different sets of controls, where the dependent variable is nonappearance in court. We report the point estimate for the immigrant indicator (i.e., the coefficient $\hat{\beta}_D^o$) of equation (5) and its standard error. Both sets of controls (*individual controls* and *court-by-year controls*) are always included in the selection equation, where the dependent variable is pretrial release, but the columns vary in their inclusion in the outcome equation. Judge and attorney controls are defined as in Table 2 and are excluded from the outcome equation. The last row presents the joint F-test for judge and attorney (excluded) controls in the selection equation. *Individual controls* are defined as in Table 2. To avoid saturating the nonlinear first-stage with court-by-year fixed effects they are replaced in the regressions by court-by-year time varying covariates – namely, the average number of judges, the average pretrial release rate, and the number of prosecutions (within a court in a given year).

Table A.III: Parametric Selection Model for Assessing OVB: $\hat{\beta}_D^o$
(Table 4 with Different Dep. Variable)

	Dependent Variable: Pretrial Recidivism			
	(1)	(2)	(3)	(4)
Immigrant	-0.083 (0.006)	-0.010 (0.005)	-0.082 (0.006)	-0.013 (0.005)
Mean dep. variable	0.19	0.19	0.19	0.19
Individual controls	No	Yes	No	Yes
Court-by-year controls	No	No	Yes	Yes
No. of Immigrants	4,900	4,900	4,900	4,900
No. of Chileans	580,407	580,407	580,407	580,407
F-test for excluded variables	948.9	948.9	948.9	948.9

Notes: This table presents the results from the two-step parametric sample selection model (Heckman, 1974) with different sets of controls, where the dependent variable is pretrial recidivism. We report the point estimate for the immigrant indicator (i.e., the coefficient $\hat{\beta}_D^o$) of equation (5) and its standard error. Both sets of controls (*individual controls* and *court-by-year controls*) are always included in the selection equation, where the dependent variable is pretrial release, but the columns vary in their inclusion in the outcome equation. Judge and attorney controls are defined as in Table 2 and are excluded from the outcome equation. The last row presents the joint F-test for judge and attorney (excluded) controls in the selection equation. *Individual controls* are defined as in Table 2. To avoid saturating the nonlinear first-stage with court-by-year fixed effects they are replaced in the regressions by court-by-year time varying covariates – namely, the average number of judges, the average pretrial release rate, and the number of prosecutions (within a court in a given year).

Table A.IV: Parametric Selection Model for Assessing OVB: $\hat{\beta}_D^o$
(Table 4 with Interacted First Stage)

	Dependent Variable: Pretrial Misconduct			
	(1)	(2)	(3)	(4)
Immigrant	-0.079 (0.007)	-0.020 (0.006)	-0.091 (0.007)	-0.011 (0.006)
Mean dep. variable	0.29	0.29	0.29	0.29
Individual controls	No	Yes	No	Yes
Court-by-year controls	No	No	Yes	Yes
No. of Immigrants	4,900	4,900	4,900	4,900
No. of Chileans	580,407	580,407	580,407	580,407
F-test for excluded variables	109.2	109.2	109.2	109.2

Notes: This table presents the results from the two-step parametric sample selection model (Heckman, 1974) with different sets of controls, where the dependent variable is any pretrial misconduct. We report the point estimate for the immigrant indicator (i.e., the coefficient $\hat{\beta}_D^o$) of equation (5) and its standard error. Both sets of controls (*individual controls* and *court-by-year controls*) are always included in the selection equation, where the dependent variable is pretrial release, but the columns vary in their inclusion in the outcome equation. Judge and attorney controls are defined as in Table 2, are interacted with the complete set of controls in the first stage as in Mueller-Smith (2015) and Rivera (2022), and are excluded from the outcome equation. The last row presents the joint F-test for judge and attorney (excluded) controls in the selection equation. *Individual controls* are defined as in Table 2. To avoid saturating the nonlinear first-stage with court-by-year fixed effects they are replaced in the regressions by court-by-year time varying covariates – namely, the average number of judges, the average pretrial release rate, and the number of prosecutions (within a court in a given year).

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

	Dependent Variable: Pretrial Misconduct			
	(1)	(2)	(3)	(4)
Model I: Immigrant	-0.094 (0.0067)	-0.029 (0.0063)	-0.104 (0.0067)	-0.028 (0.0062)
Model II: Immigrant	-0.084 (0.0066)	-0.005 (0.0061)	-0.096 (0.0066)	-0.007 (0.0061)
Model III: Immigrant	-0.083 (0.0066)	-0.006 (0.0061)	-0.095 (0.0066)	-0.007 (0.0061)
Mean dep. variable	0.29	0.29	0.29	0.29
Individual controls	No	Yes	No	Yes
Court-by-year characteristics	No	No	Yes	Yes
No. of Immigrants	4,900	4,900	4,900	4,900
No. of Chileans	580,407	580,407	580,407	580,407

Notes: This table presents the results of the OVB test proposed in Section 4 using the semiparametric correction of Newey (2009) that uses series approximations to compute control function corrections. We implement the semiparametric correction following Low and Pistaferri (2015) where the first step uses Gallant and Nychka (1987) estimator to approximate the unknown density by third degree Hermite polynomial expansions and the second step controls for non-linear transformations of the density prediction. As in Low and Pistaferri (2015), we consider three models. Let \hat{f} denote the predicted density. The control function used in Model I is \hat{f} and its square, in Model II is $\Phi(\hat{\alpha}_0 + \hat{\alpha}_1 \hat{f})$ and its square –where Φ is the normal cumulative distribution function and $(\hat{\alpha}_0, \hat{\alpha}_1)$ are the estimated coefficients of a Probit model of *Release* on a constant and \hat{f} –, and in Model III is $\lambda(\hat{\alpha}_0 + \hat{\alpha}_1 \hat{f})$ and its square –where $\lambda(x) = \phi(x)/\Phi(x)$ is the inverse Mills ratio and ϕ the normal density. We report the point estimate for the immigrant indicator (i.e., the coefficient $\hat{\beta}_D^o$) of equation (5) and its standard error. Standard errors are computed using bootstrap with 500 repetitions to account for the fact that the density is estimated in the first stage. Both sets of controls (*individual controls* and *court-by-year controls*) are always included in the selection equation, but the columns vary in their inclusion in the outcome equation. Judge and attorney controls are defined as in Table 2 and are excluded from the outcome equation. *Individual controls* are defined as in Table 2. To avoid saturating the nonlinear first-stage with court-by-year fixed effects they are replaced in the regressions by court-by-year time varying covariates – namely, the average number of judges, the average pretrial release rate, and the number of prosecutions (within a court in a given year).

Table A.VI: Outcome Test With Different Misconduct Definitions

	Any Pretrial Misconduct		Nonappearance in Court		Pretrial Recidivism	
	KPT (1)	GV (2)	KPT (3)	GV (4)	KPT (5)	GV (6)
Immigrant	-0.050 (0.009)	-0.065 (0.017)	-0.017 (0.008)	-0.010 (0.013)	-0.056 (0.005)	-0.085 (0.015)
Mean dep. variable	0.29	0.37	0.17	0.17	0.19	0.29
No. of Immigrants	4,900	843	4,900	843	4,900	843
No. of Chileans	580,403	57,688	580,403	57,688	580,403	57,688

Notes: This table presents the outcome test estimations for different notions of pretrial misconduct using [Knowles et al. \(2001\)](#) (KPT) and [Grau and Vergara \(2023\)](#) (GV) approaches, respectively. Columns 1 and 2 replicate the results presented in Table 5 and use any pretrial misconduct as a dependent variable. Columns 3 and 4 use nonappearance in court as a dependent variable. Columns 5 and 6 use pretrial recidivism as a dependent variable. KPT approach consists on an OLS regression of pretrial misconduct on the immigrant indicator for the complete sample of released defendants. GV performs a sample selection procedure that limits the extent of inframarginality bias in the KPT implementation. Details of the outcome test implementation of GV can be found in Appendix D.

Table A.VII: Discrimination by Type of Crime: $\hat{\alpha}_D^k$ (Table 6 Splitting Drug Offenses)

	Coeff.	SE
Homicide	-0.032	(0.066)
Sexual offense	-0.000	(0.033)
Theft or robbery	-0.036	(0.018)
Other property crime	-0.051	(0.014)
Drug offense (production / selling)	-0.353	(0.034)
Drug offense (consumption / possession)	-0.010	(0.058)
White collar or tax crime	-0.016	(0.019)
Crime against public trust	0.028	(0.015)
Crime against people's freedom and privacy	-0.002	(0.006)
Other crimes	0.012	(0.020)
Mean dep. variable	0.84	
No. of Immigrants	6,362	
No. of Chileans	693,009	
R-squared	0.199	

Notes: This table presents the estimated benchmark coefficients by type of crime, $\hat{\alpha}_D^k$, of equation (6) and its corresponding standard error in parentheses (clustered at the court-by-year level). The dependent variable is a binary variable that takes value 1 if the defendant was pretrial released. The regression includes all sets of controls analogous to Column 5 in Table 2 (see Table 2 notes for details).

Table A.VIII: Crime Type Distribution

	Chilean		Immigrant	
	%	N	%	N
Homicide	0.01	6,384	0.01	69
Sexual offense	0.02	12,275	0.03	188
Theft or robbery	0.26	180,085	0.17	1,104
Other property crime	0.18	126,336	0.16	1,038
Drug offense	0.12	85,936	0.21	1,306
White-collar or tax crime	0.02	11,948	0.02	142
Crime against public trust	0.06	44,031	0.07	464
Crime against people's freedom and privacy	0.29	198,834	0.29	1,816
Other crimes	0.04	27,180	0.04	235

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

B Data appendix

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

B.1 Sources

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

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

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

B.2 Estimation sample

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

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

Sample restrictions We then make the following sample restrictions:

- We exclude hearings due to legal summons (1,233,909 observations). We do this because the information set available to the judges is likely to be different.
- We drop cases involving juvenile defendants (254,243 observations). We do this because the juvenile criminal justice system works differently, and the mandated selection rule and the preventive measures differ between systems (see [Cortés et al., 2020](#) for details).
- We drop cases where the defendant hires a private attorney as their exclusive defender (103,092 observations). We do this because we do not observe the result of the arraignment hearing (and what happens after in the prosecution) in these cases.
- We drop cases that are longer than two years in duration (55,495 observations).
- For defendants that are accused of more than one crime in a given case and the records provide multiple observations, we consider the most severe crime (see below for the severity definition). In this step we drop 200,412 observations. To be clear, we do not drop defendants, only cases. We do this to have at most one case/defendant pair per day of arraignment hearing. In this step, we also record the number of crimes that are imputed in every case.
- We drop cases where the detention judge ID is missing (66,975 observations).
- We drop the types of crime with a likelihood of pretrial detention that is less than 5% (942,672 observations). We do this because we want to study the decisions of judges in cases where pretrial detention is a plausible outcome.
- We drop cases handled by judges that see less than 10 cases in the whole time period (2,849 observations). We also resolve to only consider cases in which the assigned public attorney has defended at least 10 cases previously. It was not necessary to drop any data because of this restriction.

After all these adjustments the sample size is 699,371, consistent with the figure in Table 1.

B.3 Variables

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

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

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

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

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

C Kitawaga-Oaxaca-Blinder decompositions

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

$$\Delta_R = \bar{X}_1' \beta_1^R - \bar{X}_0' \beta_0^R = (\bar{X}_1 - \bar{X}_0)' \beta_1^R + \bar{X}_0' (\beta_1^R - \beta_0^R). \quad (\text{C.I})$$

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

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

$$\Delta_{PM} = \bar{X}_1' \beta_1^P - \bar{X}_0' \beta_0^P = (\bar{X}_1 - \bar{X}_0)' \beta_1^P + \bar{X}_0' (\beta_1^P - \beta_0^P). \quad (\text{C.II})$$

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

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

Table C.I: Kitawaga-Oaxaca-Blinder Decomposition

	Release		Pretrial Misconduct	
	Raw	Residualized	Raw	Residualized
	(1)	(2)	(3)	(4)
Total Difference	0.067 (0.011)	0.030 (0.011)	0.050 (0.009)	0.076 (0.007)
Explained: Due to difference in characteristics	-0.035 (0.003)	-0.054 (0.003)	0.064 (0.003)	0.075 (0.003)
Unexplained: Due to differences in coefficients	0.103 (0.011)	0.083 (0.010)	-0.014 (0.008)	0.001 (0.003)
No. of Immigrants	6,362	6,362	4,900	4,900
No. of Chileans	693,009	693,009	580,403	580,403

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

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

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

D Outcome test

This appendix describes the outcome test, specifically the observational implementation proposed by [Grau and Vergara \(2023\)](#), and provides suggestive evidence that the proposed identification argument is valid in our setting.

Outcome test The outcome test identifies a combination of biased beliefs and taste-based discrimination using observed outcomes of marginally selected individuals. Formal proofs are provided in [Arnold et al. \(2018\)](#), [Grau and Vergara \(2023\)](#), and [Hull \(2021\)](#). In what follows, the intuition for the outcome test is presented.

The release decision can be conceptualized as follows:

$$R_{ij(i)} = 1 \left\{ p(I_i, Z_i) + b_{j(i)}(I_i, Z_i) \leq t_{j(i)}(I_i, Z_i) \right\}, \quad (\text{D.I})$$

where Z_i are defendant characteristics (other than nationality), p is the “true” conditional pretrial misconduct probability, $b_{j(i)}$ is the bias in the predicted probability made by judge $j(i)$, and $t_{j(i)}$ is the release threshold set by (“leniency of”) judge $j(i)$. The outcome test examines whether the effective thresholds, $t_{j(i)}(I_i, Z_i) - b_{j(i)}(I_i, Z_i)$, are systematically different between immigrant defendants and Chilean defendants. Put formally, it tests whether

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

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

The insight produced by the outcome test is that although $t_{j(i)}(I_i, Z_i) - b_{j(i)}(I_i, Z_i)$ is not observed, for defendants that were released on a borderline decision $p(I_i, Z_i) = t_{j(i)}(I_i, Z_i) -$

$b_{j(i)}(I_i, Z_i)$, and therefore the average misconduct rates of marginally released defendants identify $t_{j(i)}(I_i, Z_i) - b_{j(i)}(I_i, Z_i)$. Then, the outcome test is reduced to a difference in means that tests whether misconduct rates are different between marginally released immigrant defendants and marginally released Chilean defendants.

The main identification challenge, then, is to identify marginal individuals. [Arnold et al. \(2018\)](#) provide a quasi-experimental approach that relies on the quasi-random assignment of judges. That approach is not applicable in our setting given the small share of immigrant defendants (the required instrument is underpowered). [Knowles et al. \(2001\)](#) suggests using the average behavior of released defendants to proxy for the behavior of the marginally released defendants, at the risk of being affected by the inframarginality bias. [Grau and Vergara \(2023\)](#) propose an observational approach that attenuates the scope for inframarginality bias and does not require instruments, which is the one implemented in this paper.

P-BOT The prediction-based outcome test (P-BOT) proposed by [Grau and Vergara \(2023\)](#) uses the propensity score to identify samples of released defendants that are less likely to be affected by the inframarginality bias. The implementation of the outcome test then proceeds as follows. First, we estimate the propensity score and compute the predicted values. Second, we rank released defendants according to their predicted release probabilities and define as marginal the released defendants at the bottom of the distribution. Third, we implement differences in means for pre-trial misconduct rates between immigrant defendants and Chilean defendants who were marginally released. We estimate the propensity score using the same set of observables included in the main benchmark estimations: the immigrant indicator, individual controls, judge and attorney controls, and court-by-time fixed effects. Then, we rank released defendants according to the predicted values and define the bottom 10% of the distribution as marginals. Standard errors are bootstrapped considering that the sample selection rule is based on an estimated value.