The Effect of Jury Selection on Convictions by Race

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November 19, 2024

Abstract

We use the abolition of peremptory strikes in Arizona in 2022, which eliminated attorneys' ability to strike prospective jurors without cause, to estimate how jury selection affects differential conviction rates by race. Comparing to New Mexico, we find the differential conviction rates of Hispanic and non-Hispanic defendants in Maricopa County drops by around 13 percentage points immediately after the removal of peremptory strikes. We find no evidence of changes in selection into trial and we find suggestive evidence that the effect is driven by shrinking the ethnicity conviction gap for the judges for whom it was previously the largest.

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1 Introduction

> "The decision today will not end the racial discrimination that peremptories inject into the jury-selection process. That goal can be accomplished only by eliminating peremptory challenges entirely" - Marshall, 1986.

Racial discrimination in criminal justice proceedings in the U.S. has long been hypothesized to be affected, among many other things, by the jury-selection process—and specifically by attorneys' discretionary peremptory strikes. While it is illegal to strike jurors on the basis of race, a right established by the landmark 1986 Supreme Court decision *Batson v. Kentucky*, there has been a long debate about whether this status quo legal protection is effective at reducing racial differences in jury trial outcomes.

It is estimated that one in three Americans have a criminal record with at least one arrest, charge, or conviction (NCSL, 2023). In Maricopa County, Arizona only 1.25% of criminal charges are tried before a jury. Given that a jury trial is the outside option for any plea bargain negotiation, however, changes or disparities in this outside option would also affect the 70% of charges that end in pleas. Jury selection is an essential part of the trial process and comprises several stages of removing prospective jurors for various reasons. Determining whether, and if so measuring the extent to which, peremptory strikes specifically introduce further racial differences in conviction rates has remained an empirical challenge until now.

In this paper, we take advantage of a sudden policy change in Arizona taking effect on January 1st, 2022. The Arizona Supreme Court amended the state rules of criminal and civil procedure to eliminate peremptory strikes. This was the first state-wide instance of peremptory strike abolition, and provides an ideal setting to estimate their effect on racial differences in conviction rates. We leverage this sudden policy variation in order to causally identify peremptory strikes' effect on racial differences in conviction probabilities. Peremptory strikes' role in racial disparities is a particularly actionable area of the criminal justice system—because strike procedures are procedural rules, they differ from other instances of racial difference which can only be changed by affecting the opinions and biases of decision-makers.

This policy change's effect on conviction probabilities by race is theoretically ambiguous. First, both defense attorneys and prosecutors are able to strike jurors. Second, the *Batson* protocol was specifically designed to remedy racial bias introduced by peremptories. This ambiguity is helpful to motivate our main empirical assumption, which is that the unobservable "quality" of defendants' cases that go to trial by race is constant over time. This allows us to point-identify the causal effect of changes in racial differences in conviction rates as a result of peremptory strikes. We consider a simple selection model regarding the decision to go to trial that suggests that if minority defendants expect to be less likely to be convicted under this new legal regime, more unobservably weaker cases will go to trial. This would lead our estimates to be a lower bound on the true magnitude of the effect.

Our data consists of the full set of criminal trials in Maricopa County, Arizona from January

1994 to June 2024. Maricopa County is home to more than 60% of Arizona's population, and has a publicly accessible criminal docket which we scraped. We additionally observe the universe of criminal trials in state district courts in New Mexico between January 2015 and October 2024, a state where peremptories remain. Both sets of data are particularly rich, given that we are able to observe the defendant's name, their charges and resolution, as well as the identity of the defense attorney, the prosecutor, and the judge. In Maricopa County, we additionally observe the month of birth of the defendant. Given the 30 year scope of our data we are able to construct a close approximation of their entire criminal record, where relevant. We do not observe any information about seated jurors, or the pool of potential jurors, commonly referred to as the venire.

Our research design exploits the sudden elimination of peremptory strikes in Arizona in 2022. We estimate the difference in differences between Hispanic and non-Hispanic defendants[1] after the policy change by implicitly comparing the outcomes of trials for the *same* charges in the *same* years where the defendants are of *different* ethnicities. We are additionally able to estimate a triple-differences specification where we compare the difference in differences between Hispanic and non-Hispanic defendants in Maricopa County, AZ and New Mexico.

We find evidence of substantial racial differences that are introduced to the criminal justice system vis-à-vis peremptory strikes. After their removal, the difference in conviction rates between Hispanic and non-Hispanic defendants drops by between 13 and 17 percentage points across a variety of specifications. In the two and a half years after the removal of peremptory strikes, we see no discernible change in the racial difference in likelihood of pleading guilty versus choosing to go to trial before a jury. Furthermore, we find no evidence of any associated change in racial differences in conviction rates for cases that go to a bench trial in front of a judge but no jury.

Taking as given the overall direction of the effect, there are a variety of auxiliary predictions that help to build confidence in, and explain, the main result. First, racial differences in the outcomes of bench trials—trials with only a judge and no jury—should be unchanged. We find exactly that. Second, we would expect that to the extent that some judges[2] produce larger differences in conviction rates by race, these differences are at least partially driven by differences in racially differential usage of peremptory strikes. We find that the largest drop in differential likelihood of conviction comes from the judges who presided over trials where Hispanic defendants were previously convicted at the highest rates.

Our findings contribute to a literature about the effects of jury composition on case outcomes. Existing studies focus on analyzing how the characteristics of the pool of potential jurors who respond to the summons affect racial and other differences in conviction probabilities (Anwar et al., 2012; Rose et al., 2018; Anwar et al., 2022) among others. Our study instead focuses on how the procedural rules within a case regarding jury selection affect racial differences in conviction probabilities. The effects we see may be caused by differences in seated jurors' race, but they also

[1]The primary racial minority group in Maricopa County, AZ is Hispanic people, making up 31% of the population; 7% of residents are Black.

[2]Judges refers to the bundled effect of the individual judge, the prosecutors that most commonly appear before the judge, and the venire demographics at the courtroom where the judge sits.

could be caused by other differences in jurors' characteristics—unobserved to the econometrician—that lead to racial differences in conviction likelihoods.

More broadly, we contribute to a growing literature decomposing and explaining racial differences in outcomes within the criminal justice system(Gelman et al., 2007; Rehavi and Starr, 2014; Arnold et al., 2018, 2022). Our paper uses a policy change that affects the exercise of racial bias, which allows for the use of standard difference in differences estimators.

In this paper, we describe the policy environment in Section 2, the data in Section 3, the estimation strategy and results in Section 4, and Section 5 concludes.

2 Background

2.1 Jury selection

In the United States, criminal defendants charged with felonies and serious misdemeanors have a constitutionally protected right to trial by an impartial jury.. In 2019 there were $125,222$ jury trials in state courts, of which nearly 60% were criminal trials (Hannaford-Agor and Moffett, 2023). In Maricopa County, only 1.25% of all criminal charges are tried by a jury, making this a small minority of all cases. In the event that a case *does* go to trial, jurors must be summoned to the courthouse. It is estimated that more than 37 million individuals were summoned nationwide in 2019, of which more than 16 million were qualified and available for jury service. Of these 16 million, fewer than than 1.3 million were ultimately sworn in as a juror (Hannaford-Agor and Moffett, 2023).

The 37 million summoned jurors must, unless they are exempt, appear at the courthouse on their assigned date for jury selection, also known as voir dire. Of this eligible pool, those who *actually* show up make up the pool from which the jurors are chosen, also called the venire. Depending on the specific state's rules, the attorneys and judge question potential jurors to ensure that they are fit to form an impartial jury. Some jurors are removed due to their inability to serve on a jury (such as language barriers or medical conditions) or are excused or deferred due to hardship or preexisting commitments. Other jurors are removed "for cause" by the judge if they cannot be a fair or impartial, for example because of their stated biases or their unwillingness to impose certain punishments (Anwar et al., 2012). It is estimated, from states with 1-step jury selection mechanisms, that 44% of the original summons remain at this point (Hannaford-Agor and Moffett, 2023). In addition to these "for cause" strikes, in all states other than Arizona, attorneys may peremptorily strike prospective jurors. Peremptories differ from "for cause" strikes in not requiring a justification[3] and being limited in number—the median state allows 6 peremptory strikes per side in a non-capital felony case NCSC (2024).

While it is illegal to peremptorily strike jurors on the basis of their race, there is extensive evidence that this still occurs. Prosecutors are more likely to strike Black prospective jurors and defense attorneys are more likely to strike white prospective jurors (Turner, 1996; Rose et

[3]Unless a *Batson* challenge is made. Further discussion of this rule can be found in Section 2.2

al., 2018; Baldus et al., 2001; Sommers and Norton, 2007; Diamond et al., 2009). It appears, however, that these peremptory strikes largely cancel one another out in terms of jury racial composition(Diamond et al., 2009). Still, as noted in Anwar et al. (2012), it is unclear whether other, potentially unobservable attributes of the seated jury are altered as a result of peremptory strikes.

Every criminal jury must have at least six jurors; the average seated jury size is around 10 individuals (Hannaford-Agor and Moffett, 2023). Yet over 30 times as many individuals are summoned as are seated. This ratio is partially attributable to uncertainty over peremptory strikes and partially attributable to known and uncertain aspects of the "for cause" strikes and other reasons why jurors may not appear.

2.2 Legal background

It has long been unlawful to strike prospective jurors based on a juror's race[4]. The contemporary standard for evaluating whether a juror has been improperly struck based on their race was established in the landmark 1986 Supreme Court case *Batson v. Kentucky* (Powell, 1986). Batson laid out a procedure for raising, investigating, and adjudicating the claim that a party was using peremptory strikes in a racially discriminatory manner in a criminal case. The test involves three steps. First, a defendant must make a *prima facie* case of "discriminatory jury selection by 'the totality of the relevant facts.'" Next, after a defendant has made this showing, the burden shifts to the prosecutor to "come forward with a neutral explanation" for challenging jurors. Third, the trial court must determine if the defendant has established purposeful discrimination Souter 2005. *Batson* dealt specifically with a prosecutor striking Black jurors in a case with a Black defendant, but it has since been expanded to cover any party striking jurors of any race if the strike was because of that juror's race. Batson claims are initially litigated during jury selection, but if a court denies a claim and a party later wins the claim in appellate or post-conviction proceedings, it can lead to the conviction being reversed. Still, it has long been doubted whether *Batson*—or any courtroom procedure—can actually achieve the constitutional necessity of eliminating racism in jury strikes or eliminate the overall effects of racism in the jury selection process. In *Batson* itself, Justice Thurgood Marshall wrote in a concurrence that "the decision today will not end the racial discrimination that peremptories inject into the jury-selection process. That goal can be accomplished only by eliminating peremptory challenges entirely."

In a 2005 case involving *Batson* issues, Justice Stephen Breyer wrote in a concurrence that he "believe[s] it necessary to reconsider *Batson*'s test and the peremptory challenge system as a whole" (Breyer, 2005). Breyer noted that:

> Miller-El marshaled extensive evidence of racial bias. But despite the strength of his claim, Miller-El's challenge has resulted in 17 years of largely unsuccessful and protracted litigation—including 8 different judicial proceedings and 8 different judicial

[4]See, e.g., *Swain v. Alabama*, 380 U.S. 202, 203-05 (1965). It is also unlawful to strike jurors based on their sex. See *J. E. B. v. Alabama ex rel. T. B.*, 511 U.S. 127 (1994).

> opinions, and involving 23 judges, of whom 6 found the *Batson* standard violated and 16 the contrary. The complexity of this process reflects the difficulty of finding a legal test that will objectively measure the inherently subjective reasons that underlie use of a peremptory challenge. (Breyer, 2005)

Part of the difficulty in establishing racial bias in jury selection lies in *Batson*'s three-part test itself. At the first step, "litigants remain free to misuse peremptory challenges as long as the strikes fall below the prima facie threshold level." At the second step, "prosecutors need only tender a neutral reason [for their strike], not a 'persuasive, or even plausible' one." And at the third step, "*Batson* asks judges to engage in the awkward, sometime hopeless, task of second-guessing a prosecutor's instinctive judgment—the underlying basis for which may be invisible even to the prosecutor exercising the challenge" (Breyer, 2005). In addition to *Batson*'s apparent failure to fulfill its purpose of protecting jurors from race-based strikes, it was never designed to protect defendants from other ways racism can play a role in jury selection.[5]

2.3 Policy change

In 2021, two Arizona Court of Appeal judges petitioned the state Supreme Court to amend the state rules of civil and criminal procedure to eliminate peremptory strikes (Swann and McMurdie, 2001). Citing pervasive racial disparities in jury strikes, the lengthy litigation that surrounded jury selection in many criminal cases, and the lack of a meaningful remedy to deter and discover racially disparate strikes, the judges urged the court to eliminate peremptories altogether. In August 2021, the state Supreme Court ordered that peremptory strikes be eliminated from criminal and civil trials state-wide.[6] The new rules would be applied to any trials where the first day of jury selection took place after January 1, 2022. The change was widely covered in the popular press (Corley, 2021; Felton, 2021; Millhiser, 2021; Kanu, 2021). Attorneys in the Maricopa County County Attorney's office described the change as "com[ing] as a complete surprise" (Kanu, 2022).

3 Data

Maricopa County. We scraped the whole of the Maricopa County Criminal Docket, which is available online in this manner for all cases from 1994 until now. Maricopa County contains over 4.5 million residents, and more than 60% of the population of the state of Arizona. In the data period, there are $825,750$ unique criminal cases, $508,463$[7] unique defendants, and $1,889,267$ unique criminal charges. For each charge, we are able to observe a rich set of information, including the specific charge, sex of the defendant, the names of the judge and attorneys, and, most importantly,

[5] For example, instances where prosecutors strike white jurors who might be more sympathetic to defendants of color or strike people who have personal experiences with the criminal justice system.

[6] In the Matter of Rules 18.4 and 18.5, Rules of Criminal Procedure and Rule 47(E), of the Arizona Rules of Civil Procedure, No. R-21-0020 (Ariz. Sup. Ct. August 30, 2021). This change does not affect federal trials that take place within Arizona. Federal courts have their own rules for peremptory strikes that cannot be dictated by the states.

[7] As identified by their first and last name and month of birth, which could lead to a small undercounting.

the resolution of the case—be it pleading guilty, mistrial, jury conviction, bench conviction, or any other outcome. Furthermore, since we observe 30 years of data in such an expansive county, for the most recent defendants we can come close to observing their full criminal record in the county. We do not directly observe defendants' race; instead, we match last names to the Census to infer race. As of 2010, Maricopa County was 53% white non-Hispanic, 31% Hispanic, and only 7% Black Census (2010). We primarily focus on determining whether defendants are Hispanic. The vast majority of charges do not go to jury trial. In Figure 4 we present the total number of jury trials observed in each year of our sample, and indicate whether the defendant was convicted.

New Mexico. Additionally, we scraped the whole of the New Mexico Criminal Docket, which is available online in this manner, for all charges beginning in 2012; we subset to just dispositions from 2015 or later. New Mexico has more than 2.1 million residents, and has many demographic similarities to Maricopa County. In the data period in question, there are $105,054$ unique criminal cases, at least $79,651$ unique defendants[8], and $1,586,683$ unique criminal charges of which $35,907$ went to a jury trial. As in the Maricopa County data, we observe the specific charge, the resolution, and timeline of the case, as well as the identities of the judge and attorneys. Due to the comparative difficulties with scraping this data and identifying individuals, we are unable to observe each defendant's full criminal record. As in Maricopa County, the primary non-white group are Hispanic individuals, making up nearly 48% of the state's population.

Last Names. We use the data from Imai et al. (2022) to match each defendant's last name to their likelihood of belonging to a particular ethnic group in a method similar to the procedure in Diamond et al. (2019). This data uses 2020 Census self-reports of ethnicity by last name to report the conditional probability of being a member of an ethnic group conditional on last name. We define a defendant as having a Hispanic-sounding last name if people with their last name are more likely than not to be Hispanic. This distribution is quite bi-modal, with around 93% of defendants having a last name with either $\leq 25\%$ or $\geq 75\%$ likelihood of being Hispanic.

4 Estimation Strategy and Results

4.1 Main specification

We are interested in estimating a triple difference in differences specification, comparing the changes in the relative conviction rates between Hispanic and non-Hispanic defendants in Maricopa County, AZ and New Mexico, noting that peremptory strikes were abolished in January 2022 in Arizona. We may be concerned about some shock around 2022 to the comparative likelihood of conviction of Hispanic defendants that is unrelated to peremptory strikes. To the extent that we believe this could be a common shock within the Southwestern border states, given their similar cultures, geographic locations, and demographics, New Mexico is an appropriate control in a triple differences

[8] As we do not observe birthdate, these are unique first, middle, and last name combinations.

specification. Formally, we are estimating the following regression.

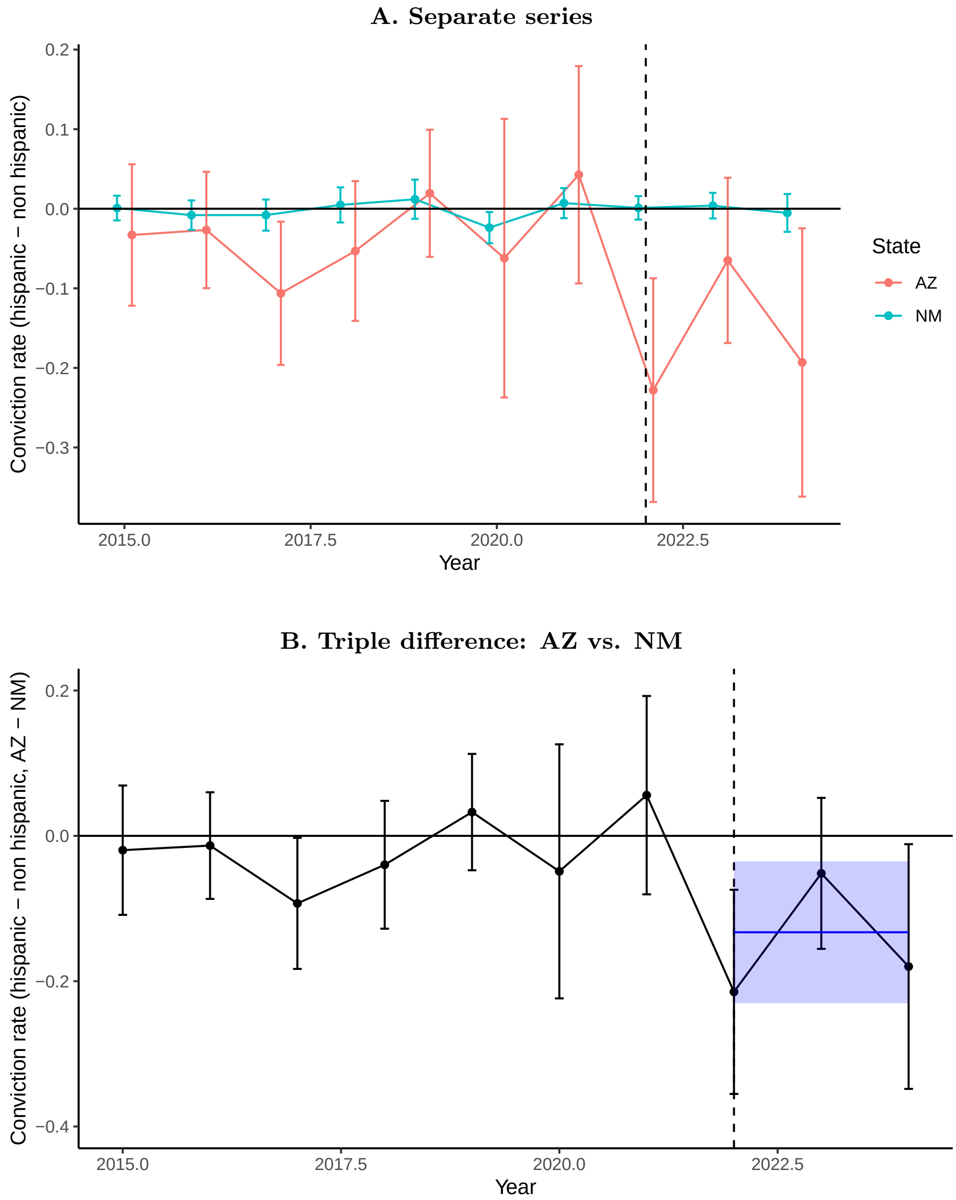
$$Y_{i,r,t,c,s,k} = \sum_{t=2012}^{2024} \tau_{t,s} \cdot \mathbb{I}\{r = h\} + \gamma_{c,t,s} + \phi_{t,s} + \xi_{i,r,t,c,s,k} + \varepsilon_{i,r,t,c,s,k}, \tag{1}$$

where $Y_{i,r,t}$ is 1 if defendant i, or race r, at time t is convicted for charge type c, and 0 otherwise. τ specifies the relationship between whether the defendant is Hispanic and their likelihood of conviction, and are the coefficients of interest. Additionally, $\phi_{t,s}$ are year by state k fixed effects, and $\gamma_{c,t,s}$ are crime by year by state fixed effects. This is able to strip out variation between different crimes' relative probabilities of conviction, as well as any differences in ways that crimes were defined, prosecuted, or charged that were race-agnostic in any time period. The coefficient of interest is $\tau_{t,AZ} - \tau_{t,NM}$ or the difference between Arizona comparative conviction rates and New Mexico comparative conviction rates. Finally, there is an associated "vertical quality" $\xi_{i,c,t}$ of the defendant's case, which is a structural unobservable. This could include the strength of the evidence in the defendant's favor, the defendant's appeal to a jury, or a variety of other factors. Throughout, we will assume that the mean ξ between Hispanic and non-Hispanic defendants is unchanged after 2022 in order to point identify a treatment effect. We note if more unobservably worse Hispanic defendants bring their case to trial after the law change, or $\mathbb{E}_{t\geq 2022}[\xi_{i,r,t,c}|r = h] - \mathbb{E}_{t\geq 2022}[\xi_{i,r,t,c}|r \neq h] < \mathbb{E}_{t<2022}[\xi_{i,r,t,c}|r = h] - \mathbb{E}_{t<2022}[\xi_{i,r,t,c}|r \neq h]$, then the τ from Equation 1 will be an underestimate of the true effect. We discuss this assumption in more detail in Section 4.2.

We present the main comparison specification in Figure 1, with the pooled difference under different specifications available in Table 1. We estimate Equation 1 first as a linear probability model, finding a 13.3 percentage point drop in the relative conviction rates of Hispanic and non-Hispanic defendants in Maricopa County after the abolition of peremptory strikes. In Figure 1 Panel (A) we observe a flat difference in relative conviction rates between Hispanic and non-Hispanic defendants in New Mexico. All of the change in the relative rates is borne out by the relative decrease in likelihood of conviction upon impact of the law change in Maricopa County, Arizona. This effect size is nearly identical to the effect size in Column (5) of Table 1 without a control state–but with richer controls, including age of defendant, their criminal history, the identity of their judge and attorney, and their sex.

These results are robust to a variety of alternative specifications. In Column (2) of Table 1 we make our baseline restriction to Statutes charged at least 4 times in that year more stringent, limiting to Statutes charged at least 10 times in that year–finding an 17.3 percentage point decrease in the relative likelihood of conviction for Hispanic defendants. We find a qualitatively similar effect size using a logistic regression in Column (3) of Table 1, finding a −.88 drop in the untransformed logit function in probability of conviction of Hispanic defendants relative to non-Hispanic defendants. In Column (4) of Table 1 we assess a different definition of racial minority. We compare defendants with Hispanic-sounding last names *or* Black-sounding first names to a control group of defendants with white-sounding first *and* last names and find a 15.1 percentage point drop in the minority groups' likelihood of conviction in a jury trial. Finally, given that changes in jury selection ought to only affect the outcome of cases that go to trial that *have a jury* we report the effect on effect on the likelihood of conviction in bench trials, which are trials decided by a judge, in Column (6) of Table 1. There are relatively few bench trials, but we find a statistically insignificant small positive effect on the differential likelihood of conviction for Hispanic defendants in bench trials.

Figure 1: Effect on relative conviction rate of Hispanic defendants: compared to New Mexico



Note: Hispanic defendants are identified by their last names. All regressions contain crime × year × state and county × year fixed effects.We normalize the y-axis to make the average τ between 2018-2021 zero. The pooled effect is estimated comparing 2018 – 2021 and 2022 – 2024. We subset to charged statutes with ≥ 4 charges in that year.

4.2 Effects on selection

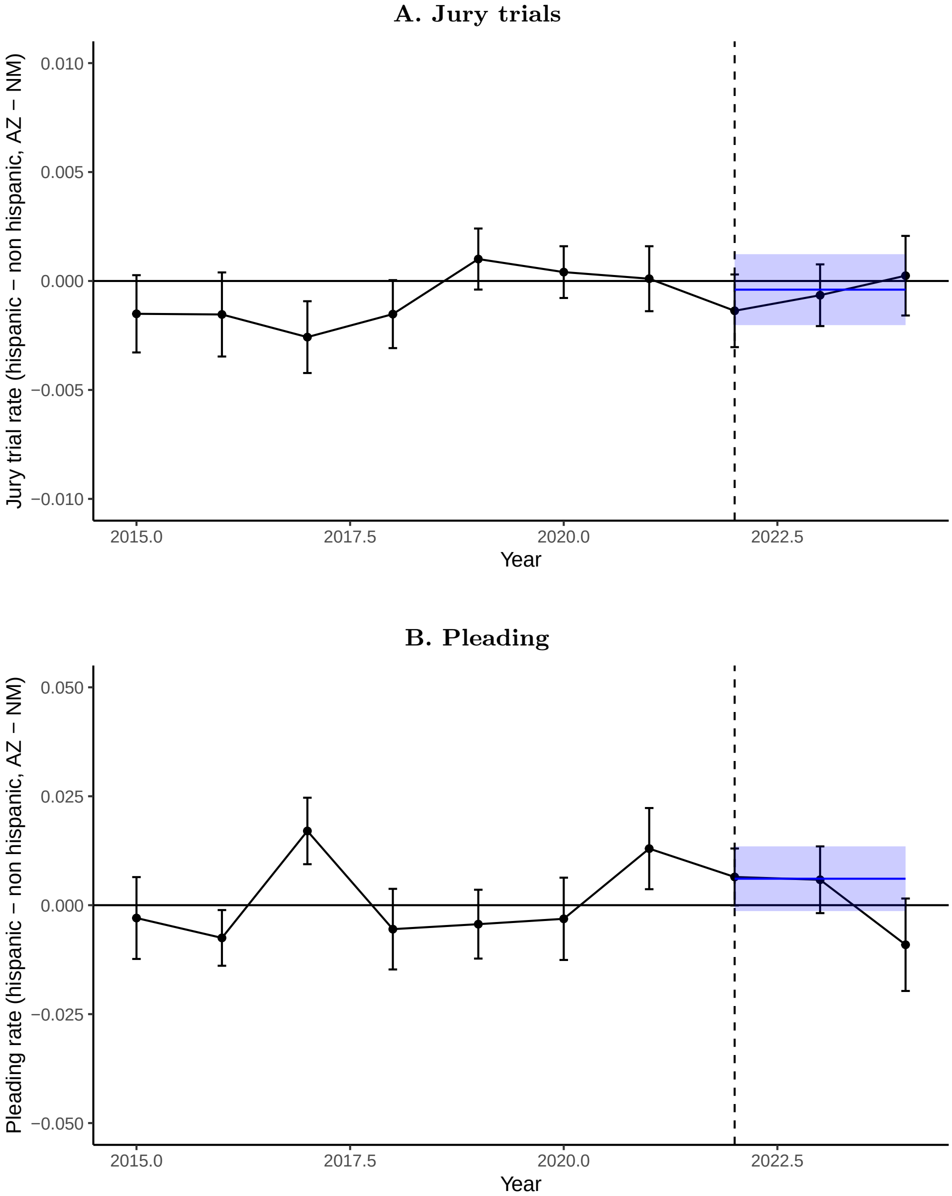
In order to interpret our results in Section 4.1 as the causal effect of changes in laws regarding peremptory strikes, it is important to consider how they may also affect margins of selection. We consider the following toy model. The defendant (considering the defendant and their defense attorney as a joint decision maker) observes her $X_{i,t}$ and $\xi_{i,r,t,c}$ and can assess her likelihood of conviction conditional on going to a jury trial, which we will denote as $p_{i,r,t,c} = \tau_{t,r} + \beta X_{i,t} + \gamma_{c,t} + \xi_{i,r,t}$. We will consider the cost of trial for crime c to be C_c, and the benefit of avoiding conviction to be B_c. We note very simply that a risk-neutral agent will go to trial when $p_{i,r,t,c}(1 - B_c) \geq C_c$[9].

We note then that when τ_t is lower, there are more Hispanic agents with lower $\xi_{i,r,t}$ (and "worse" $X_{i,t}$) who clear this cutoff rule. Indicating that in a full-information state we would expect for more observably (and unobservably) worse cases with Hispanic defendants that go to jury trial. There are a variety of reasons why we may expect for this not to happen in the short run, first and foremost because this is, to our knowledge, the first empirical study of the differential effect on convictions after the elimination of peremptory strikes. The effect τ_t was theoretically ambiguous since the law change also removed the ability for defense attorneys to use peremptory strikes.

In Figure 2 we present two evaluations of this selection margin. We estimate the same regression as in Equation 1, now with the differential likelihood of a jury trial and of any plea (in non-jury trials) in panels A and B respectively. We find no statistically significant effect on either outcome, perhaps suggesting relatively limited selection in the short run. At baseline, 1.25% of cases in Maricopa County, AZ and 2.26% of cases in New Mexico go to jury trials. In Maricopa County, 70% of cases plea, compared to 55% of cases in New Mexico. The estimated effects on selection in Figure 2are both statistically insignificant and qualitatively small. Furthermore, in Panel (B) of Figure 2 we can rule out there being a substantial decrease in gap in pleading rates for non-jury trials when comparing Hispanic and non-Hispanic defendants. Such a change would be consistent with an increase in relative leniency for Hispanic defendants outside of the context of jury trials. We find no supporting evidence of changes mirroring the change in Figure 1 in cases that are *not* tried before a jury.

[9]This abstracts away from idiosyncratic differences, but suggests a mechanism by which changes in the likelihood of winning a jury trial may make the marginal defendant have a "worse" case.

Figure 2: Effect on selection margins for Hispanic defendants



Note: Hispanic defendants are identified by their last names. All regressions contain crime × year × state and county × year fixed effects.We normalize the y-axis to make the average τ between 2018-2021 zero. The pooled effect is estimated comparing 2018 – 2021 and 2022 – 2024. We subset to charged statutes with ≥ 4 charges in that year.

4.3 Effects by judge

If there is pre-period heterogeneity across judges in their degree of racial bias, we would expect a larger treatment effect on the judges whose courtrooms are home to more racially disparate outcomes. There are a variety of reasons why there may be heterogeneity along this dimension: demographics of the venire called to that courthouse, willingness of the prosecutors most frequently assigned to that courtroom to make racially biased peremptory strikes, judges' willingness to grant defense *Batson* motions, and more.

A common feature of these theories is that we would expect to see that the effect of removing peremptory strikes on the differential propensity to convict Hispanic defendants would be largest amongst the judges whose courtrooms ex-ante yielded the most biased outcomes. In order to assess this, we estimate the following first-stage regression to recover judge by time differential conviction rates between Hispanic and non-Hispanic defendants

$$Y_{i,j,r,t,c} = \sum_{t \in B} \tau_{t,j} \cdot \mathbb{I}\{r = h\} + \tau_t \cdot \mathbb{I}\{r = h\} + \beta X_{i,t} + \gamma_{c,t} + \xi_{i,j,r,t,c} + \varepsilon_{i,j,r,t,c}, \tag{2}$$

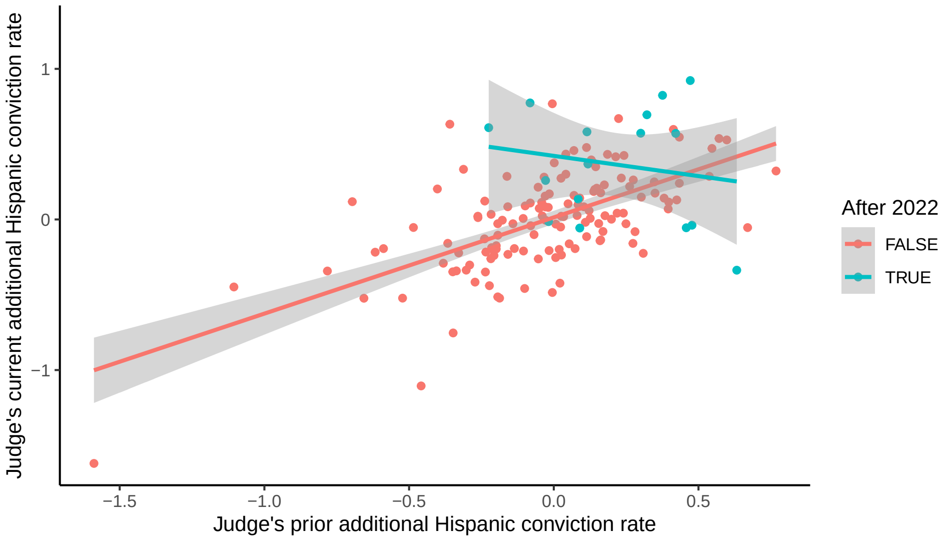
where all notations are as in Equation 1, with B being 4-year time bins starting from 1994, up until 2022, and j is the judge—with $\tau_{t,j}$ being the judge-by-time differential conviction rate—differencing out the common component τ_t.

We are interested in the serial correlation of $\tau_{t,j}$, and how this changes after 2022. We model this as an AR(1) process where there is some persistence parameter α, as well as a differential effect of persistence after 2022, δ.

$$\tau_{t,j} = \alpha \tau_{t-1,j} \times \mathbb{I}\{t < 2022\} + \delta \tau_{t-1,j} \times \mathbb{I}\{t = 2022\}. \tag{3}$$

We present the results graphically in Figure 3. We can see a clear deviation from previous trends after 2022, wherein the change in slope is statistically significant. Previously, there was a serial persistence coefficient (α) of around 0.69. In 2022 and beyond, this value becomes negative where the previously more differentially high-propensity to convict Hispanic defendant judges are indeed slightly less likely.

Figure 3: Serial correlation of conviction rates over time by judge



Note: Each observation is a judge for which we have an estimate of $\tau_{t.j}$ and $\tau_{t-1,j}$. On the x-axis, we present a judge's additional likelihood of conviction for hispanic defendants in the previous time period. On the y-axis, we present a judge's additional likelihood of conviction for hispanic defendants in the current time period. Prior to the removal of peremptory strikes, there was substantial serial correlation–with a slope of 0.68 (0.07). This serial correlation becomes slightly negative, but insignificant when comparing the pre-2022 τ_j to the post-2022 τ_j–with a slope of -0.27 (0.29).

5 Conclusion

In this paper, we test whether peremptory strikes in jury selection have racially disparate impacts on defendants in criminal trials. We use the sudden removal of this procedural component of trials to assess their effect on trial outcomes. We find evidence that there are substantial racial differences introduced into the criminal justice system by the capacity of attorneys to peremptively strike jurors. We find that after the removal of peremptory strikes, Hispanic defendants in Maricopa County, AZ go from being around 8 percentage points *more* likely than non-Hispanic defendants to be convicted to around 6 percentage points *less* likely than non-Hispanic defendants to be convicted at jury trials.

The racially differential effects of peremptory challenges occur despite the status quo rule that prosecutors may not discriminate against jurors by virtue of their race. Without more information on the composition of juries, we are not able to say whether *Batson* challenges, or the ability to argue that a strike was racially motivated, are insufficient to protect the rights of potential jurors. Perhaps more importantly, however, we find that peremptory strikes allow attorneys to endogenously affect jury composition in a manner that makes it *less* sympathetic to defendants who are racial minorities than a jury that is selected by random chance and for-cause strikes.

In any case, this provides evidence that *Batson* challenges are insufficient to eliminate unconstitutional racial bias from jury selection. The sample of jurors that are selected by random chance and for-cause strikes are approximately 13 percentage points *less* likely to convict Hispanic defendants than a jury selected with peremptory strikes.

While fewer than 1 in 80 criminal charges results in a jury trial, the effects of bias injected through this procedure could be expected to guide other disparate outcomes in the criminal justice system. Under a simple model of bargaining between defendants and prosecutors, plea deals are disciplined by the "outside option" of disagreement—a jury trial. Thus, differential treatment of different racial groups due to bias from jury selection may also affect the outcomes of defendants whose cases do not go to trial.

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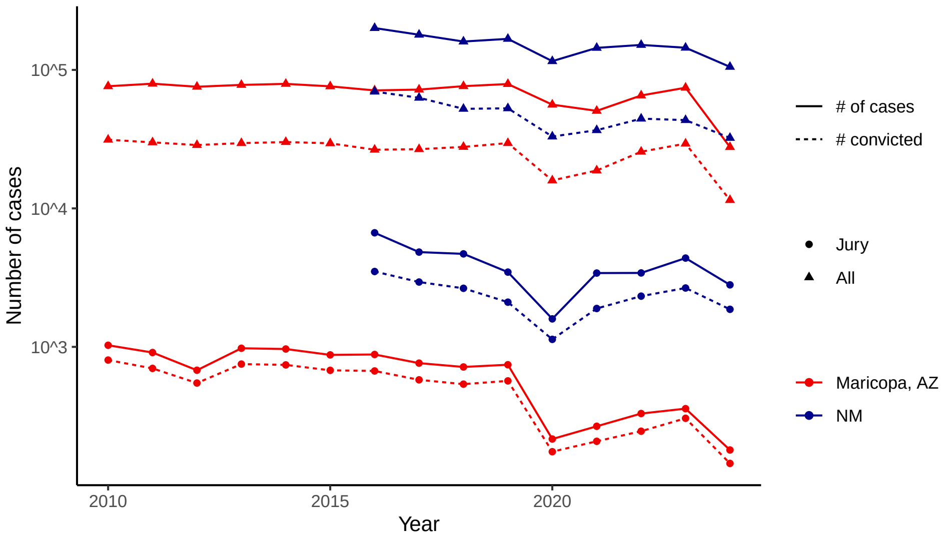
A Appendix figures and tables

Figure 4: Jury trials and outcomes over time



Note: Plotting series for the number of jury trials in both Maricopa County, AZ and NM–as well as the number of convictions from jury trials over time. Since 2014 the number of jury cases per year in Maricopa County has been steadily declining. In both geographies the number of jury trials in 2020 was substantially lower–as a result of the COVID-19 pandemic, but this dip was more pronounced in Maricopa County. In both series, the 2024 data contains only trials up until May 22.

Table 1: Effect of Removing Peremptories on Conviction By Race

Dependent Variable:	Convicted					
Model:	(1) OLS	(2) OLS	(3) Logit	(4) OLS	(5) OLS	(6) OLS
Variables						
Hispanic × AZ × After	-0.133***	-0.173**	-0.884***		-0.138***	0.077
	(0.050)	(0.072)	(0.340)		(0.050)	(0.222)
Minority × AZ ×After				-0.151***		
				(0.049)		
Hispanic	0.000	-0.001	-0.003		0.099***	-0.002
	(0.007)	(0.007)	(0.054)		(0.028)	(0.006)
Hispanic × After	0.002	0.002	0.019			0.004
	(0.009)	(0.009)	(0.071)			(0.011)
Hispanic × AZ	0.075***	0.132***	0.474**			-0.168
	(0.029)	(0.037)	(0.186)			(0.135)
Minority				0.006		
				(0.008)		
Minority × After				-0.004		
				(0.011)		
Minority × AZ				0.051*		
				(0.028)		
log(Prior charged + 1)					0.014	
					(0.020)	
log(Prior charges + 1)					0.016	
					(0.014)	
log(Age)					0.167***	
					(0.041)	
log(# charges this case)					0.045***	
					(0.013)	
Fixed-effects						
Statute × Year	Yes	Yes	Yes	Yes	Yes	
Year × State	Yes	Yes	Yes	Yes	Yes	Yes
Statute						Yes
Sex					Yes	
Judge					Yes	
Public Defender					Yes	
Fit statistics						
Observations	23,773	21,813	15,016	21,536	1,549	7,993
Squared Correlation	0.47	0.45	0.19	0.47	0.25	0.44
Pseudo R^2	0.45	0.43	0.16	0.46	0.26	7.73
BIC	29,206.41	25,001.81	21,697.43	27,137.62	3,251.64	-945.50

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: We compare outcomes of trials between 2018-2024. Hispanic defendants are identified by their last names. Minority defendants have either Hispanic-sounding last names or Black-sounding first names. In Column (1) we present our main specification as a linear probability model. In Column (2), we subset to observations where there are at least 10 instances of that charge in a given year. In Column (3) we present our main specification as a logit model. In Column (4) we vary our treated definition, including all Minority Defendants and using as control *only* defendants with white-sounding first and last names. In Column (5) we present a difference-in-differences, not using the NM data to include a full set of controls. In Column (6) we present our main specification considering only bench trials, overseen by a judge, without Statute × Year fixed effects due to the limited sample size making that collinear with the coefficient of interest.

B Supplementary Appendix

B.1 Details about data construction

For both samples, we collected the online docket which contains rich information about each case. In both, the URLs are structured in a way that allows for systematic collection of all cases.

B.1.1 Maricopa County, Arizona

Access. There are no barriers to access for this selection, besides the possibility of being rate limited from visiting too many case pages in a short period of time.

Case numbers. The case numbers for any given year are not in numerical order, and the support is quite large. It is infeasible to visit each possible URL. However, it is possible to search by initial for all cases. The online system limits the number of results, but by searching for all combinations of first initial and first two letters of the last name all results were under this limit. These case numbers also contain the defendants' date of birth.

Cases. Each case then has a structured URL given the case number, and all information from the HTML can be retrieved easily.

Disposition. Each disposition is in common text and is standardized. Thus, it can easily be classified.

Statute. The law under which a defendant is charged is referred to as the ARS code, which are not harmonized across geographies.

B.1.2 New Mexico

Access. Users must solve a Captcha in order to be able to access cases.

Case numbers. The case numbers, within each court, for any given year are in numerical order. This case number is then prefixed by the court number.

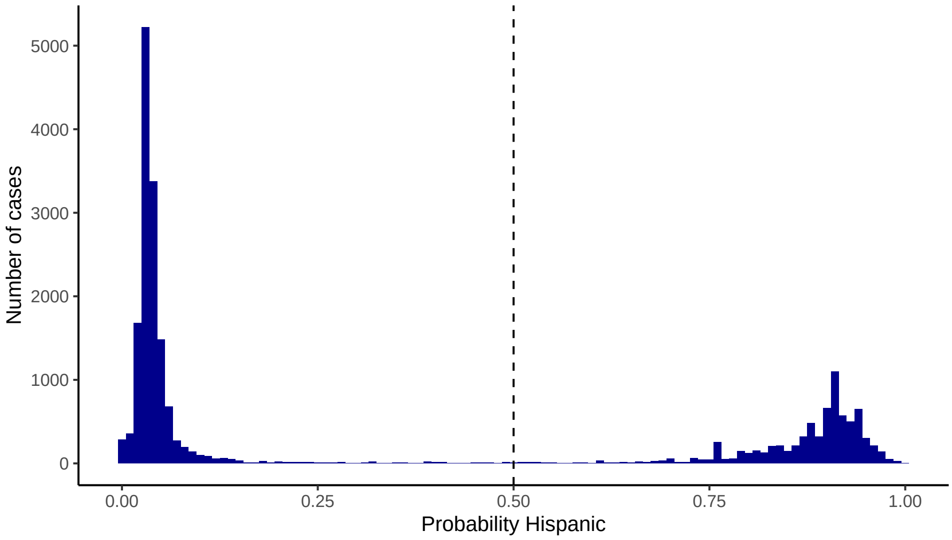
Cases. Each case then has a structured URL given the case number, and all information from the HTML can be retrieved easily.

Disposition. Each disposition is in common text and is standardized. Thus, it can easily be classified.

Statute. The law under which a defendant is charged is referred to as the statute, which is not harmonized across geographies.

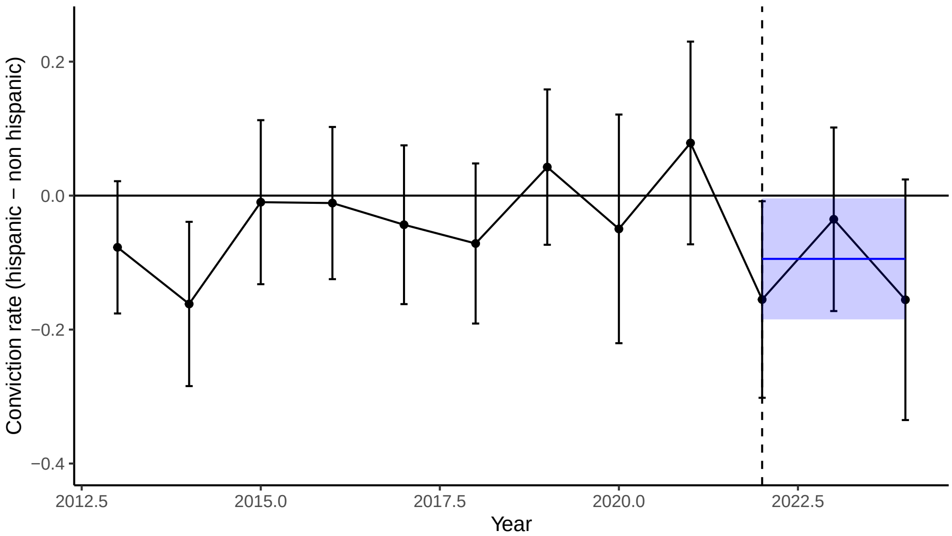
B.2 Additional Figures and Tables

Figure 5: Distribution of the probability of being Hispanic by last name



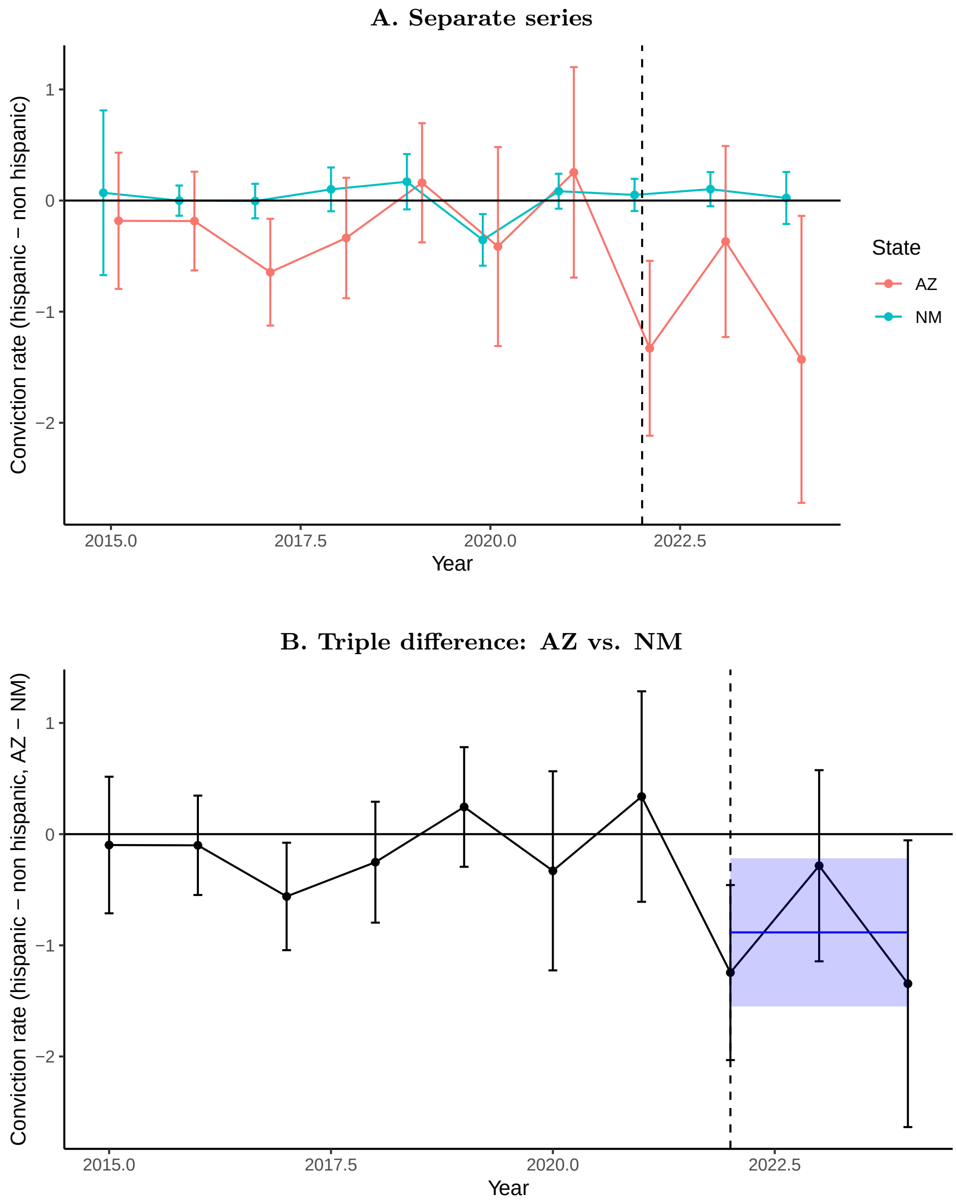
Note: Matching last names to 2010 Census implied probabilities. Dashed line at 0.5 represents our decision rule with regard to determining if a defendant is Hispanic.

Figure 6: Effect on relative conviction rate of Hispanic defendants: Full Controls



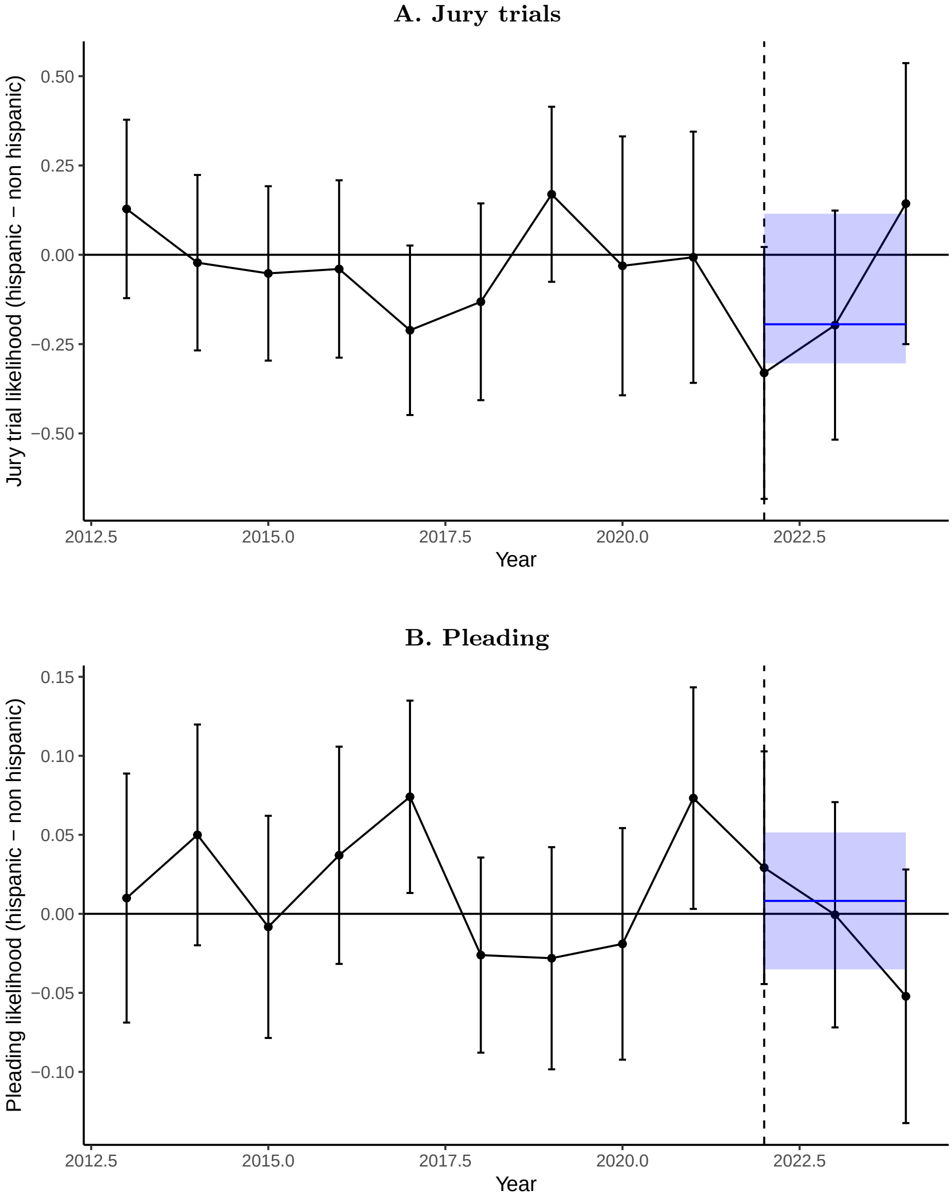
Note: Hispanic defendants are identified by their last names. Contains crime × year, judge, public defender, and sex fixed effects. Also control for log of age, prior charges, prior convictions or pleas, number of charges in this case, and age. We normalize the y-axis to make the average τ between 2018-2021 zero.

Figure 7: Effect on relative conviction rate of Hispanic defendants: compared to NM, logit



Note: Hispanic defendants are identified by their last names. All regressions contain crime × year × state and county × year fixed effects.We normalize the y-axis to make the average τ between 2018-2021 zero.

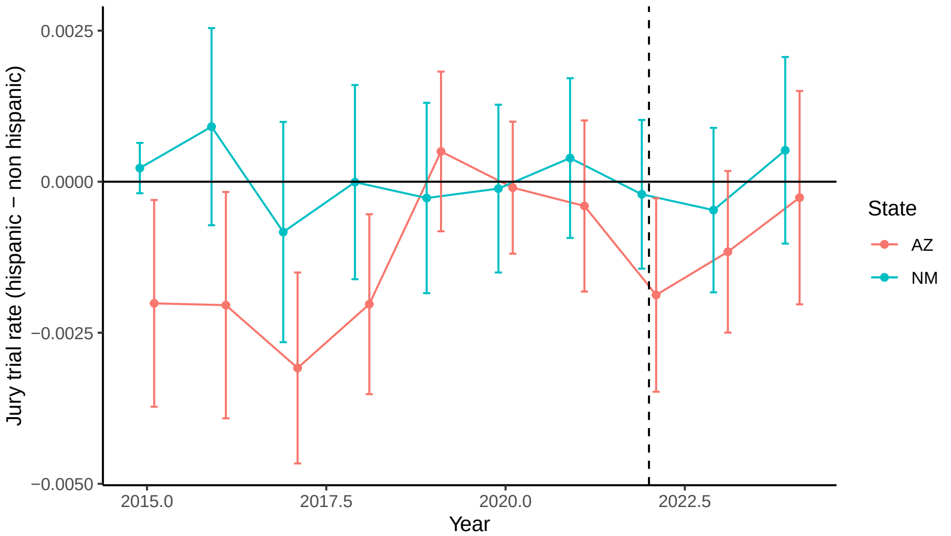
Figure 8: Effect on selection margins for Hispanic defendants: Full Controls



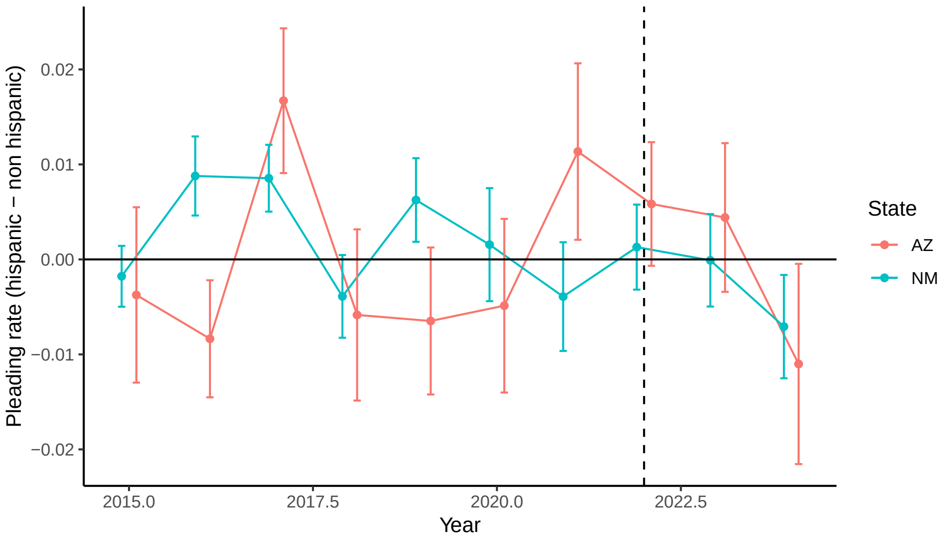
Note: Hispanic defendants are identified by their last names. All regressions contain crime × year, judge, public defender, and sex fixed effects. Also control for log of age, prior charges, prior convictions or pleas, number of charges in this case, and age. Both specifications are logistic regressions.

Figure 9: Separate series of selection margins for Hispanic defendants

A. Jury trials

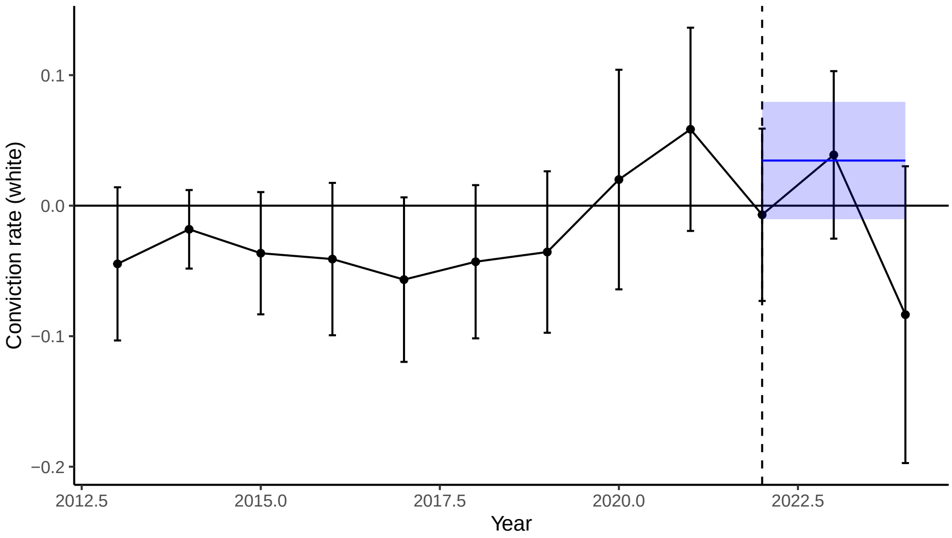


B. Pleading



Note: Hispanic defendants are identified by their last names. All regressions contain crime × year, judge, public defender, and sex fixed effects. Also control for log of age, prior charges, prior convictions or pleas, number of charges in this case, and age. Both specifications are logistic regressions.

Figure 10: Effect on white defendants



Note: White defendants are identified by their first or last names. All regressions contain crime, judge, public defender, and sex fixed effects. Also control for log of age, prior charges, prior convictions or pleas, number of charges in this case, and age. Linear probability model.

Table 2: Serial correlation in judge differences: pre and post

Dependent Variable:		$\tau_{t,j}$	
Model:	(1)	(2)	
Variables			
$\tau_{t-1,j}$	0.639***	0.687***	0.639***
(α: before 2022)	(0.073)	(0.112)	(0.073)
$\tau_{t-1,j}$	-0.907***	-0.956***	
(δ: after 2022)	(0.297)	(0.112)	
$\tau_{t-1,j}$			-0.268
(α_{2022}: after 2022)			(0.288)
Fixed-effects			
Year		Yes	
Fit statistics			
Observations	152	152	152
R^2	0.41	0.46	0.41
Within R^2		0.39	

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Each observation is a judge for whom we have an estimate of $\tau_{t.j}$ and $\tau_{t-1,j}$. Using $\tau_{t-1,j}$ estimated as in Equation (3). In Column (1) and (2) we present a model of the form $\tau_{t,j} = \alpha\tau_{t-1,j} + \delta\tau_{t-1,j} \times \mathbb{I}\{t = 2022\} + \phi_t$. In Column (3) we present the main specification $\tau_{t,j} = \alpha\tau_{t-1,j} \times \mathbb{I}\{t < 2022\} + \delta\tau_{t-1,j} \times \mathbb{I}\{t = 2022\} + \phi_t$.