The Role of Race in the Legal Representation of Low-Income Defendants

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Abstract

Racial disparities have been well-documented at every stage of the criminal justice system, as have the roles of police, prosecutors, judges, and juries in driving those disparities. In contrast, little is known about the role of race in the defense of low-income defendants by court-appointed attorneys. This is important since 80 percent of criminal defendants rely on assigned counsel for legal defense. Exploiting the quasi-random assignment of court-appointed attorneys to cases in Travis County, Texas, we test whether defense attorneys secure better deals for same-race defendants using a difference-in-differences approach. Results indicate that while Black and White attorneys are similarly effective at securing dismissals for White defendants, Black attorneys are *less* effective than White attorneys at securing dismissals for Black defendants. Specifically, Black defendants who are represented by White rather than Black attorneys are 20-22 percent more likely to have their charges dismissed. Moreover, estimates of attorney (shrunken) individual effects suggest that results are driven by the entire sample of White attorneys rather than just a handful of them.

Keywords: Race, discrimination, criminal justice, law and economics, indigent defense, legal representation

JEL codes: H76, J15, K14, K42

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1 Introduction

Racial disparities in the criminal justice system are well-documented. For example, Black individuals are almost four times more likely to be arrested for marijuana possession relative to White individuals (Union 2013). Moreover, 33 percent of Black adult males have a felony conviction, compared to only 12.8 percent of the total adult male population (Shannon et al. 2017). Importantly, some empirical evidence suggests that these disparities are driven in part by racial discrimination by other agents in the system, such as police officers (e.g., Goncalves and Mello 2021; West 2018; Horrace and Rohlin 2016), prosecutors (e.g., Tuttle 2019), and judges (e.g., Arnold et al. 2018; Alesina and La Ferrara 2014). However, little is known about the role of race in the indigent defense system, even though 80 percent of criminal defendants rely on appointed counsel for legal defense (ACLU, 2013).

Certainly, the availability of legal defense is invaluable; attorneys can significantly affect case outcomes through several channels such as challenging charges and negotiating plea deals. Perhaps most importantly, attorneys advise defendants on whether to accept a given plea deal. However, there are widespread concerns that indigent defendants are not receiving quality legal counsel, as required under the U.S. Constitution (e.g., Backus and Marcus 2018; Tucker v. State of Idaho, 2017 [1]; Hurrell-Harring v. State of New York, 2010 [2]). Moreover, the scope for differential treatment on the basis of race among court-appointed attorneys is high, given high caseloads may lead attorneys to necessarily prioritize some cases over others (Oppel and Patel 2019). While there is some anecdotal evidence that court-appointed attorneys might be racially biased (Clair 2021; Adachi 2016), empirical evidence on the role of race in the defense of low-income defendants is scarce.

In this paper, we ask whether attorneys secure better or worse outcomes for different-race defendants. We do so by exploiting the quasi-random assignment of attorneys to approximately 15,000 misdemeanor cases in Travis County, Texas. Court-appointed attorneys are

[1] Tucker v. State, 162 Idaho 11, 394 P.3d 54 (Idaho 2017)

[2] Hurrell-Harring v. State of New York, 75 A.D.3d 667, 2010 N.Y. Slip Op. 5815, 905 N.Y.S.2d 334 (N.Y. App. Div. 2010)

assigned to indigent cases using a "wheel" system, in which their names are listed alphabetically. Once a case is filed, the responsible authority assigns the first available (and eligible) attorney on the list. This implies that conditional on the filing date and court fixed effects, attorney assignment to cases is as-good-as-random. We use a difference-in-differences approach to test whether White attorneys are less likely to earn a dismissal for Black versus White defendants, relative to the dismissal rate of Black attorneys for Black versus White defendants. In doing so, we use the same method as that used in previous research to examine the impact of race in other contexts (e.g., Hoekstra and Sloan 2022; Anwar et al. 2012; Price and Wolfers 2010).

Our main difference-in-differences results show that while Black and White attorneys are similarly effective at securing dismissals for White defendants, Black attorneys are less effective than White attorneys at securing dismissals for Black defendants. Specifically, if Black defendants are represented by White attorneys rather than Black attorneys, the likelihood that their case gets dismissed increases by 20-22 percent. Moreover, we find evidence that the likelihood of incarceration decreases by 16-27 percent for Black defendants who are represented by a White attorney relative to a Black attorney. These effects are highly significant and are robust to controlling for case characteristics and interaction terms of case characteristics and attorney race. We then estimate the distribution of attorney (shrunken) individual effects, and we show that the results are due to a shift in the overall distribution of White attorneys, which means that they are driven by the entire sample of White attorneys rather than just a few.

Importantly, we show that the effect of a different-race attorney on case outcomes is not driven by other non-race (defendant or attorney) characteristics that matter for the defendant-attorney race pairing. For instance, the different-race effects could, in theory, be due to White attorneys being better skilled at representing crimes that are more likely to be committed by Black defendants. However, we show that the results are robust to controlling for the interaction terms of defendant race and attorney characteristics (including years

of experience and law school ranking) and to controlling for interaction terms of attorney race and case characteristics (including dummy variables for crime type, day of the week, defendant criminal history, sex, and age). This suggests that effects are not due to non-race disparate impact, at least to the extent that non-race factors are correlated with observed characteristics.

While we do not find evidence of a change in attorney effort, we argue that the different-race effects are driven by a change in the attorney's behavior, the prosecutor's behavior, or both. While it is possible that Black attorneys might exhibit racial bias against Black defendants, the results over time suggest that that might not be the case. The different-race effects are more pronounced in more recent years, which is consistent with the hypothesis that the recent racial justice movement may have impacted how attorneys behave toward Black defendants.

The contribution of this paper is to provide the first estimates of the impact of race among court-appointed attorneys using the quasi-random variation in attorney assignment to cases. In doing so, this paper contributes to at least two strands of the literature. First, it complements the literature on racial bias and the impact of race in the criminal justice system. Hoekstra and Sloan 2022 report that White officers scale up force more than Black officers when responding to calls in Black versus White neighborhoods, while Fryer Jr 2019 shows that police officers are more likely to use non-lethal force against minorities. Additional research shows that officers exhibit racial bias when issuing traffic citations and making traffic stops (e.g., Goncalves and Mello 2021; Ba et al. 2021; West 2018; Horrace and Rohlin 2016; Antonovics and Knight 2009), which contrasts with findings from earlier literature (e.g., Knowles et al. 2001). At later stages, other papers have shown mixed evidence of the impact of race in prosecution (e.g., Sloan n.d.; Tuttle 2019; Rehavi and Starr 2014), among judges (e.g. Arnold et al. 2022; Arnold et al. 2018; Gazal-Ayal and Sulitzeanu-Kenan 2010), in parole board decisions (e.g., Anwar and Fang 2015; Mechoulan and Sahuguet 2015), and among jurors (e.g. Anwar et al. 2022; Flanagan 2018; Anwar et al. 2012).

In addition, this study contributes to the economic literature on the quality of defense attorneys in the indigent defense system. For example, Agan et al. 2021 provide suggestive evidence that the compensation structure adopted by the court leads to disparate outcomes between indigent and non-indigent cases. Unlike Hoag 2021, who argues for the expansion of the Sixth Amendment right to *counsel of choice*, they show that being assigned a lawyer who looks like a "better match" does not lead to better case outcomes, which is consistent with our findings. In a recent paper, Shem-Tov 2022 shows that public defenders achieve better outcomes relative to court-appointed attorneys when representing indigent defense cases. In general, evidence shows that defendants with assigned counsel end up with worse outcomes relative to other forms of counsel (e.g., Iyengar 2007; Cohen 2014; Roach 2014).

The results of this paper have important policy implications. Our difference-in-differences estimates suggest that in contrast to other settings, being assigned to a different-race attorney leads to better outcomes. This is particularly relevant to designing defendant-attorney matching mechanisms since our findings indicate that a same-race attorney does not translate to better outcomes.

2 Institutional Background

2.1 The indigent defense system

In 1963, the constitutional right to counsel was established in the US. Under the Sixth Amendment, the courts must provide a lawyer for defendants who cannot afford one (i.e. indigent). There are three main models of indigent defense: assigned counsel, contract-based defender, and public defender. In the assigned counsel model (also called court-appointed attorneys), private defense attorneys sign up to be considered for indigent defense through a court-maintained list, called the wheel, and they get paid an hourly rate or a flat fee per case, depending on the jurisdiction. The contract-based defender is similar in nature, in the sense that the attorneys are not state employees. The difference is that they sign a contract

to represent a predetermined number of cases for a given amount of money. Finally, public defenders are full-time state employees who receive a monthly salary to represent indigent defendants. Based on a survey conducted by the Bureau of Justice Statistics in 2013, the most common state-administered indigent defense model is public defenders (Strong 2016).

In Texas, the Constitution guarantees that any indigent defendant is entitled to the appointment of counsel in any case that may result in punishment by confinement. Even though it is not required by federal law, Texas guarantees appointment at any stage of a criminal case.[3] Assigned counsel is the most commonly used model in Texas.[4] Less than 20 percent of Texas counties rely on public defenders. In counties where both assigned counsel and public defenders are available, judges choose which type of counsel to assign. Up until the year 2021, Travis County had relied on assigned counsel for both felony and misdemeanor cases. In 2021, they established a public defender's office to assist private attorneys.[5] Defense attorneys, conditional on meeting certain requirements, voluntarily sign up to be listed on a certain wheel.[6]

2.2 Case assignment in Travis County

Once a defendant is arrested, they are interviewed by the pretrial services as soon as possible in order to determine the bond status and acquire indigency information. A defendant automatically qualifies for indigent representation if they receive any government assistance, such as food stamps, Medicaid, temporary assistance for needy families (TANF), social security assistance, or public housing. Otherwise, they are deemed indigent if their financial situation reveals so compared to federal poverty guidelines (when considering their income, expenditures, number of dependents in the household, etc. ...). For example, a defendant is

[3] www.sixthamendment.org

[4] It is the default assignment process as per state law unless the court employs an alternative method.

[5] The office will handle 30 percent of indigent defendants by 2024.

[6] An attorney who wants to be listed on the wheel for the indigent defense should file an application, which will be examined by the review committee. The court-appointed counsel must meet specific criteria, including possessing a valid license to practice law, being a resident of Travis County or adjoining counties, and demonstrating substantial experience in the field of criminal law. The requirements for experience may vary based on the type of panel involved.

considered indigent if their net household income does not exceed 125 percent of the Poverty Guidelines provided by the United States Department of Health and Human Services, and the value of their assets does not exceed $2,500, according to the standards and procedures for the appointment of counsel in Texas. A defendant is also presumed to be indigent if they are currently serving a sentence in a correctional facility or a mental health institution.

Once indigency is determined, the defendant is asked whether they are interested in indigent defense counsel. If yes, their application is forwarded to the appointing authority, which reviews their case and appoints counsel. In Travis County, a managed assigned counsel program was established in 2015 to ensure that appointments are impartially allocated among eligible counsel.[7]

Appointments are made using a rotation system (the wheel system) following an alphabetical listing of the names of eligible attorneys. The eligibility of an attorney is determined based on their caseload when a case is filed, crime type, special needs, and language. For example, certain attorneys only qualify to represent misdemeanor cases. Attorneys can also sign up to represent defendants with special needs including (1) mental health-related cases and (2) cases where the defendant is non-English speaking. Thus, conditional on the filing date, crime type, and whether it is a mental health-related case or a Spanish-speaking defendant, attorney assignment is as-good-as-random.

Importantly, there are only two characteristics of cases that are used to assign attorneys to cases which we do not directly observe. The first is whether the individual only speaks Spanish. To address that potential issue, we include only White and Black defendants in the sample. This excludes the sample of Hispanic defendants who might be nonrandomly assigned to attorneys. The second is whether it is a mental health-related case. To address this, we condition on the court as a proxy for whether it is a mental health-related case. While cases are randomly assigned to courtrooms in Travis County, only a handful of courts specialize in mental health as well. Hence, by controlling for month-by-year-by-court fixed

[7]Before 2015, judges were responsible for assigning counsel using the wheel system.

effects, attorney assignments should be as-good-as-random.[8]

3 Data

We use administrative data from the county clerk's office in Travis County, Texas, which is the fifth most populous county in Texas that includes Austin, the capital of Texas. Our data consist of all misdemeanor charges that were filed between 2013 and 2022. In addition, we obtained attorney assignment data (i.e. the wheel data) from the Travis County Criminal Court Administrator's Office that allow us to observe attorney assignment per case.

Between 2013 and 2022, 132,337 misdemeanor charges (129,679 unique cases) were filed in Travis County, out of which 40 percent were assigned a court-appointed attorney, according to the wheel data. The court records allow us to observe the charge description (for example, theft, assault, etc. ...), defendant information, including race as recorded by law enforcement, filing date, the court that handled the case, disposition (whether the charge was dismissed or not), and sentencing information.

The wheel data allow us to observe case assignments, in addition to the state bar ID and full name of each attorney. However, neither the county nor the State Bar of Texas records the race of the attorneys. Thus, we manually searched for the 400 attorneys in our sample online, through the State Bar website or other platforms (such as law firm websites, LinkedIn, etc. ...) and used their images and their last names (for Hispanic attorneys) to identify their race. Using this methodology, we created a dataset that shows the name and state bar ID of each attorney, in addition to their race as shown online. We were able to identify the race of 88 percent (352) of the attorneys in our sample. Importantly, conditional on observing attorney race, 5 percent of the attorneys in our sample are Black.

For the majority of the charges, we observe one assigned attorney from the filing date until the case disposition date (91 percent). In some instances, however, we observe more

[8]For example, county court #9 supervises the Mental Health Docket, and county court #8 supervises the Special Reduction Docket. See: `https://www.traviscountytx.gov/courts/criminal/county`

than one attorney (8 percent have two assigned attorneys, and 1 percent have more than two). Based on conversations with the county, attorneys can be replaced, though very rarely, in cases such as an attorney-client conflict or an attorney leaving the practice. In our main analysis, we consider the first attorney who should be quasi-randomly assigned by the wheel. For robustness, we show that the results are not sensitive to dropping cases where we observe multiple attorneys. Moreover, prior to 2015, judges had the authority to overturn the wheel decision and non-randomly assign an attorney of their choice to cases. The wheel data allow us to observe whether an attorney on a given case was assigned by the wheel or the judge. To avoid selection bias, we drop the cases where the attorney is assigned by the judge, which consist of 11 percent of our sample.

While the court data show the defendant race, they do not distinguish between non-Hispanic and Hispanic White defendants.[9] Thus, in addition to the court-reported defendant race, we use the R-package *predictrace* in order to identify Hispanic defendants using their last names.[10] We identify and drop Hispanic defendants (40 percent) and defendants of other races (1 percent).

Moreover, according to conversations with the county, we learned that misdemeanor assault charges are more likely to be coupled with higher-level, felony charges, which some court-appointed attorneys on the misdemeanor panel are not eligible to represent. This means that including these charges in our sample will cause the non-random assignment of attorneys to cases. Hence, we drop these charges from our sample (they correspond to 12 percent of total charges).

In order to test for a different-race effect, we need to observe both, the race of the attorney and the race of the defendant for each charge. Hence, we drop the charges for which the attorney race is missing, and we also drop Hispanic attorneys since we are interested in Black and White attorney-defendant matches. This leaves us with 15,479 misdemeanor

[9]According to conversations with the county, the observed race is recorded by law enforcement at the time of arrest who do not ask the arrestee to report their own race.

[10]This package predicts the race of a last name or first name using U.S. Census data and the Social Security Administration data.

charges for the difference-in-differences analysis. The summary statistics for this sample are reported in Table 1. A more detailed discussion of the sample size is presented in subsection 9.1.

We report the summary statistics for our main sample in Table 1. As shown in Column (1), the most common type of misdemeanor charge is driving while intoxicated (22 percent of the sample). The remaining are drug-related (15 percent), property (9 percent), invalid license (10 percent), weapon (2 percent), or other (42 percent). Other misdemeanor charges include less common types of crimes, such as criminal trespass, evading arrest/detention, obstruction of highway, and violating protective orders. Only 24 percent of the defendants are female, 32 percent are Black, and the average age of a defendant is 34. An average defendant has 1 previous charge, which indicates that many defendants in our sample are repeat offenders.

As outcome variables, we focus on case dismissal as a measure of case disposition, in addition to sentencing outcomes. As shown in Column (1), the average rate of dismissal for the entire sample is 49 percent. The dismissal rate is higher for Black defendants relative to White defendants (54 percent vs 47 percent). Next, we consider whether the defendant was sentenced to jail, probation, or received a fine only. Thirty percent of the defendants are sentenced to jail, while 13 percent receive a probation sentence, and less than 1 percent receive a fine only.[11]

The most notable difference between Black and White defendants is the charge type. As can be seen from columns (2) and (3), White defendants are much more likely to commit DWIs relative to Black defendants (28 percent versus 10 percent). On average, Black defendants are slightly more likely to be charged with drug offenses (17 percent versus 13 percent), and they have slightly more prior charges (1.4 versus 1.1). Finally, although Black defendants are more likely to have their cases dismissed (54 percent versus 47 percent), they are more likely to be sentenced to jail (33 percent versus 29 percent) and less likely to receive

[11]Probation includes deferred adjudication and community supervision.

a probation sentence (8 percent versus 16 percent).

In addition, we supplement the data described above with information from the State Bar of Texas. Using their website, we obtain each attorney's graduation date, licensing date, and the name of the law school they graduated from. In addition, we link these data with law school rankings from the US news website. On average, a given attorney has 17 years of experience, and they graduated from a school that ranks in the 70s (Table A1).

Importantly, our sample and data allow us to be substantively more precise than other studies using a similar approach to examine racial disparities in the different stages of legal proceedings (e.g., Anwar et al. 2012, Flanagan 2018, Hoekstra and Street 2021). [12]

4 Empirical Strategy

The main challenge in testing whether defense attorneys secure better case outcomes for same-race defendants among defense lawyers is the non-random selection of cases across attorneys. However, the wheel system in Travis County allows us to isolate an as-good-as-random variation in case assignment. As discussed in section 2, conditional on month-by-year-by-court fixed effects, attorney assignment to cases is quasi-random. In order to estimate a different-race effect, we use a difference-in-differences approach, where we compare case outcomes for Black and White defendants across Black and White attorneys.

Intuitively, we begin by comparing case outcomes (e.g., dismissal rate) across Black and White defendants for White attorneys. If Black defendants are observed with lower dismissal rates compared to White defendants, that difference could represent the impact of race among the defense lawyers. However, it could also represent the difference in underlying levels of "dismissibility" across defendants. To distinguish between these two potential interpretations, we compare that difference to the difference in dismissal rates across Black and White defendants for Black attorneys. If there is no "different-race effect", the expected

[12] While the minimum detectable effects of those three studies range from 23 percent to 78 percent, our minimum detectable effect is 17.31 percent, which is 24 percent smaller than the next most powerful study.

difference in dismissal rates across Black and White defendants should be similar across Black and White attorneys.

Formally, we estimate the following equation:

$$\begin{aligned} Y_c =& \alpha_0 + \alpha_1 \cdot WhiteAttorney_c + \alpha_2 \cdot BlackDefendant_c + \\ & \alpha_3 \cdot WhiteAttorney \cdot BlackDefendant_c + MonthYearCourt_c + X_c + u_c, \end{aligned} \tag{1}$$

where Y_c is the outcome of interest, $WhiteAttorney_c$ is an indicator variable that takes the value 1 if the attorney is White, $BlackDefendant_c$ is an indicator variable equals 1 if the defendant is Black, and $WhiteAttorney * BlackDefendant_c$ is an indicator variable that takes the value 1 if the attorney is White and the defendant is Black, and zero otherwise. In order to achieve quasi-random assignment, our baseline specification includes month-by-year-by-court fixed effects ($MonthYearCourt_c$). The main coefficient of interest is α_3, which represents the effect of having a different-race attorney on the probability of a case dismissal and on sentencing outcomes. Finally, X_c is a vector of charge level characteristics, including dummy variables for crime type, day of the week, zip code fixed effects, defendant age, sex, and the number of previous charges.

In all regressions, standard errors are two-way clustered at the attorney and defendant level to account for correlations within cases that are handled by the same attorney. Note that the analysis data are at the charge level. Since a single case can include multiple charges that are handled by the same attorney, we assign a probability weight for each observation, which is equal to the inverse of the number of charges per case. Nonetheless, in our sample, 98 percent of the cases have one charge only.

The identifying assumption behind this approach is that in the absence of a different-race effect, differences in dismissal rates and sentencing outcomes for Black and White defendants should be the same for Black attorneys and White attorneys. This assumption relies on the quasi-random assignment of court-appointed attorneys to cases. We begin by showing that

the attorney's race is not correlated with case characteristics as supporting evidence of quasi-random assignment using three approaches. First, we estimate the F-statistic to examine whether case characteristics jointly predict attorney race. Specifically, we regress a dummy for attorney race on dummy variables for crime types, defendant age, race, sex, day of the week, and a measure of criminal history. We do that unconditionally, controlling for month-by-year fixed effects, and controlling for month-by-year-by-court fixed effects.

Second, we use case characteristics to predict the outcome variables, then we test whether attorney race is correlated with the predicted values. The intuition behind this approach is that if cases are indeed quasi-randomly assigned, then the underlying dismissal rate of a given case, as predicted by case characteristics, should not be correlated with the race of the attorney. First, we predict the outcome variables (in this case, probability of dismissal, probability of being sentenced to jail, and probability of being on probation) for each case using case characteristics, such as crime type (drug, assault, property, driving while intoxicated, invalid license, weapon), defendant race, sex, age, day of the week, and number of previous arrests. Next, we compare the predicted outcomes for cases assigned to White attorneys to the predicted outcomes for cases assigned to Black attorneys. If cases are indeed quasi-randomly assigned, then the predicted dismissal rate and predicted sentencing outcomes should not be statistically different across attorneys of different races.

Finally, we estimate the correlation between attorney race and case characteristics by regressing the dependent variables on attorney race, each in a separate regression. We do that unconditionally, controlling for month-by-year fixed effects, and then using our baseline specification, which is controlling for month-by-year-by-court fixed effects.

In addition to these tests, we show that our main estimates are robust to controlling for case characteristics, which is consistent with the quasi-random assignment of attorneys to cases. One possible threat to the validity of this difference-in-differences approach is that attorneys of one race could be more effective at representing certain charges that are more likely to be committed by individuals of a certain race. For example, White attorneys could

be better at representing drug-related offenses, which are disproportionately committed by Black defendants. This fact could potentially be driving any nonzero difference-in-differences estimate. In order to show that any nonzero effect is due to race rather than other factors that are correlated with race, we control for interaction terms of all case characteristics with attorney race. If it is true that other race-related factors are driving a cross-race effect, then once we include the interaction terms, our main estimate α_3 should become insignificant.

5 Results

5.1 Quasi-random assignment of attorneys to cases

First, we provide empirical evidence of quasi-random assignment of cases to attorneys. We show that attorney race is not correlated with case characteristics in three ways.

We begin by showing that attorney race is not correlated with the predicted outcomes. In order to estimate the predicted values, we regress the outcome variables (probability of a case being dismissed, probability of receiving a jail sentence, and probability of receiving a probation sentence) on all case characteristics. Case characteristics include dummy variables for the charge type, day-of-the-week fixed effects, defendant sex, age, and the number of previous charges, in addition to month-by-year-by-court fixed effects. Then, we use the estimated coefficients to predict the outcome variables. Next, we estimate the effect of attorney race on the predicted outcomes, and we report the results in Table 2. Table 2 shows the correlation between attorney race and predicted outcomes. Even without conditioning for the minimum set of controls required to achieve quasi-random assignment of attorneys to cases, we show that predicted outcomes are not correlated with attorney race (Panel A), and the results are unchanged when we condition on month-by-year fixed effects (Panel B). In Panel C, we show the correlation between predicted outcomes and attorney race when controlling for month-by-year-by-court fixed effects, which is our baseline specification. Again, the results show that attorney race is not correlated with any of the predicted outcomes.

We use two other methods to provide more supportive evidence that attorney assignment is quasi-random. In addition to using predicted outcomes, we show the results of the F-test in Table B1, where we regress attorney race on case characteristics to test whether the latter jointly predict the attorney's race. We begin by showing the results without controlling for any fixed effects, as shown in Column (1). Column (1) shows that some case characteristics are marginally statistically significant. However, the F-test, with a p-value of 0.429, shows that these characteristics do not jointly predict the race of the attorney. We then control for month-by-year fixed effects. Column (2) shows that conditional on month-by-year fixed effects, only one of the case characteristics (day of the week) is marginally statistically significant, and the F-statistic is 0.6, with a p-value of 0.799. Again, this means that case characteristics do not jointly predict the race of the attorney. Finally, we report the results using our preferred specification, which includes month-by-year-by-court fixed effects. Column (3) shows that none of the coefficients are statistically significant, and the p-value of the F-statistic is 0.859.

Finally, we show that attorney race is not correlated with case characteristics, by regressing each characteristic, separately, on each attorney race. The results are reported in appendix Table B2. Each column represents a separate regression equation. Panel (A) shows the results unconditional on any time or court fixed effects, Panel (B) shows the results using month-by-year fixed effects, and Panel (C) shows the results using our baseline specification, which is using month-by-year-by-court fixed effects. The results are in line with the results outlined above, and they provide more supportive evidence that conditional on month-by-year-by-court fixed effects, attorney assignment to cases is quasi-random.

Now, we turn to showing the effect of being assigned a different-race attorney on case outcomes using the difference-in-differences approach.

5.2 Different-race effect

5.2.1 Case outcomes

We begin by showing the dismissal rates for White and Black attorneys by defendant race (Table 3). As can be seen from the table, White attorneys have a higher likelihood of earning a dismissal, regardless of the race of the defendant. For White defendants, White attorneys have a dismissal rate of 47 percent, while Black attorneys have a dismissal rate of 45 percent. While this difference could be due to a race-effect, it could also be a function of other characteristics correlated with attorney-race, but are not race itself. For instance, White attorneys might be better-skilled relative to Black attorneys, so they are more successful at securing dismissals for their defendants relative to Black attorneys, irrespective of defendant race. For the latter to be the only explanation for this difference in dismissal rates across White and Black attorneys for White defendants, we should observe the same difference when considering the dismissal rates for Black defendants.

We then consider the difference in dismissal rates across attorneys but for Black defendants, which will be our second difference. In the absence of a "different-race" effect, the difference in dismissal rates for Black defendants across attorney races should be equivalent to the difference in dismissal rates for White defendants across attorney races. However, the difference in dismissal rates across White and Black attorneys for Black defendants is 0.125 percentage points, which is larger than the difference in dismissal rates across White and Black attorneys for White defendants, 0.014 percentage points. Since we know that cases are quasi-randomly assigned to attorneys, this difference is evidence of a different-race effect.

We formally estimate the different-race effect on case disposition using Equation 1, and we report the results in Table 4. Column (1) represents the difference-in-differences estimates using our baseline specification, which only controls for month-by-year-by-court fixed effects. The results show that there is a different-race effect of almost 10 percentage points, and it is statistically significant at the 1 percent level. That is, relative to the

outcome mean of 0.49, an attorney is 20 percent more likely to earn a dismissal when they are representing a different-race defendant. We have previously shown that an attorney's race is not correlated with case characteristics, which means that controlling for case characteristics should not affect our estimates besides improving precision. Indeed, when we control for case characteristics, including crime type, day of the week, defendant sex, age, and criminal history, our estimate is unchanged (column 2).

These difference-in-differences estimates suggest that a defendant receives more favorable outcomes when represented by an attorney of a different race since that increases the likelihood that their case gets dismissed. There are several possible explanations for these different-race effects, which we will discuss extensively in section 6. However, the different-race effect could be driven by other factors that are correlated with defendant race, but are not race itself. For instance, perhaps White attorneys are better at representing drug-related crimes, which are more likely to be committed by Black defendants. To address this concern, we control for interactions of attorney race with all case characteristics, including crime type, day of the week, defendant sex, age, and criminal history. If this is true, then interacting case characteristics with attorney race and controlling for them should cause the cross-race effect, or α_3, to go to zero. When we control for the interaction terms, our coefficient of interest does not change. Specifically, we estimate a different-race effect of 11 percentage points (22 percent) (Column (3) of Table 4). To the extent that these case characteristics are good proxies for case type, these results provide supportive evidence that differences in case types across White and Black defendants cannot be the main drivers of the different-race effect.

Similarly, White attorneys might be better quality attorneys, in a way that matters for representing Black defendants vs White defendants. For instance, White attorneys might have more years of experience, which makes them better at defending Black individuals than their Black counterparts. To address this, we interact attorney's years of experience and law school ranking, as proxies for experience and quality, with defendant race. However, the

results remain unchanged (Column 4).

Next, we examine whether having a different-race attorney affects sentencing outcomes. The data allow us to observe whether a defendant was sentenced to jail, probation, or was released on a fine only. We report the results in Table 5, where Panels (A) and (B) show the effect on the likelihood of receiving a jail sentence and a probation sentence, respectively, and Panel (C) shows the effect on receiving a fine only.

Using our baseline specification, Column (1) of Panel (A) shows that having a different-race attorney causes an 8 percentage point decrease in the likelihood of being sentenced to jail, which is statistically significant at the 10 percent level. This estimate is robust to controlling for case characteristics and interaction terms of case characteristics and attorney race. For instance, Column (3) in Panel (A) shows that having a different-race attorney causes a 5 percentage point decrease in the likelihood of receiving a jail sentence, which is a 17 percent decrease relative to the control mean. In contrast, having a different-race attorney does not impact the likelihood of receiving a probation sentence or a fine only, as shown in Panels (B) and (C), respectively.

As mentioned earlier, the voucher data show that 9 percent of the charges are assigned to more than one attorney. Based on conversations with the county, this can be due to reasons such as an attorney-client conflict or an attorney leaving the practice of defense. Thus far, we have focused on the first attorney assigned to a given charge, as identified by the assignment date. As a robustness check, we drop the sample of charges that had more than one attorney and estimate the difference-in-differences model using the cases that had one court-appointed attorney from the filing date until the disposition date.

We report the results in Table B5. Column (1) shows the different-race effect for dismissal rates. As can be seen, the results are similar to what we show in Table 4. Specifically, we estimate a different-race effect of 9 percentage points on the probability of earning a dismissal, which is statistically significant at the 1 percent level. Compared to the outcome mean, assigning a different-race attorney increases the likelihood of dismissing a case by 17

percent. This suggests that our main estimates are not driven by a subset of charges where the main attorney, for which quasi-random assignment holds, was replaced.

Column (2) shows that a different-race attorney decreases the likelihood of receiving a jail sentence by 7 percentage points (23 percent). However, the estimate is not statistically significant at conventional levels. That being said, this is possibly due to a loss in precision when restricting the sample to cases with only one attorney. Compared to the outcome mean, the lower confidence interval is 51 percent, which means that we cannot reject meaningful decreases in the likelihood of receiving a jail sentence. As for receiving a probation sentence or fine only, the results remain unchanged (Columns 3 and 4).

Our results contradict findings from other research. Contrary to agents such as police officers (e.g., Hoekstra and Sloan 2022) or juries (e.g., Anwar et al. 2012), we find that different-race attorneys secure better outcomes for their defendants. In section section 6, we present an extensive discussion of the possible mechanisms behind these effects.

5.2.2 Individual attorney effects

So far, we have shown that on average, attorneys are more likely to earn favorable outcomes for different-race defendants. What is not clear though is whether these effects are driven by the entire sample of attorneys or just a handful of attorneys in the tails. In order to address this question, we estimate attorney (shrunken) random effects and plot the distributions for both Black and White attorneys. We estimate the random effects separately by defendant race to account for differences across racial groups.

First, we regress the probability of case dismissal on month-by-year-by-court fixed effects and case characteristics. Second, we save the residuals and use the Stata command mixed to estimate a random effects model and compute individual attorney random effects. Finally, we graph the residuals using a kernel density plot for Black and White attorneys by defendant race (Figure 2).

Comparing the distributions by defendant race, panel (a) in Figure 2 shows that con-

ditional on the defendant being White, an average Black attorney is similar to an average White attorney, but there are more White attorneys in both tails than there are Black. However, when we compare the distributions conditional on the defendant being Black (panel (b)), the figure results show that the distribution of White attorneys is shifted to the right relative to that of Black attorneys. This indicates that when handling Black defendant cases, White attorneys are generally better, i.e., they are more likely to earn a dismissal for a Black defendant relative to Black attorneys. These results suggest that the different-race effects are driven by White attorneys being more effective at representing Black defendants relative to Black attorneys. Since the entire distribution of White attorney random effects shifts to the right, it indicates that our difference-in-differences results in Table 4 are driven by the entire distribution of White attorneys, rather than just a handful of them.

For robustness, we estimate attorney fixed effects and we plot them in Figure B1. The figures show that attorney fixed effects are pretty similar to the estimated shrunken random effects estimated previously.

5.2.3 Heterogeneous effects

First, we estimate the different-race effect by crime type. Previous research argues that the evidence for some crime types, such as assault, is more scarce, leaving more room for discretion (e.g., Spohn and Holleran 2001). Thus, one might expect that the results are driven by the types of crimes for which this is true.

We use the charge description to classify crimes into five main categories: drug-related, property, traffic-related, and other.[13] For each crime category, we estimate the difference-in-differences model separately using Equation 1, and we report the results in Table B6. In all regressions, we control for month-by-year-by-court fixed effects and case characteristics, and

[13]Drug-related include charges such as possession of marijuana or possession of a controlled substance. Property crimes include thefts of any kind (property, service, organized retail theft, etc. ...), attempted thefts, and burglaries. Traffic-related include driving without a valid license, driving while intoxicated, and speeding. Finally, other crimes less common offenses such as failure to appear in court, criminal mischief, criminal trespass, evading arrest/detention, obstructing highway passageway, weapon-related offenses, resisting arrest or search, violating bond or protective order, etc. ...

we two-way cluster the standard errors at the attorney and defendant level.

As the table shows, we estimate a statistically insignificant effect of a different-race attorney on our outcomes of interest for each crime type. That being said, dividing the sample by crime type leads to a substantial decrease in the sample size and statistical power, which leads our estimates to be highly imprecise. For example, looking at property-related crimes in Column (3), the estimate in Panel (A) shows that there is a 14 percentage points increase in the likelihood of earning a dismissal as a result of a different-race attorney, but the estimate is highly imprecise. In particular, relative to the control mean, we cannot reject increases that are less than 80 percent.[14] This provides suggestive evidence that the results are driven by both other-misdemeanor charges and property-related crimes.

Second, we estimate the different-race effect by year. In our paper, we find that different-race attorneys earn more dismissals and less jail sentences for their defendants, which means that having a different-race attorney is better for case outcomes. In order to better understand what drives these estimates, we estimate the effects over time to examine whether they coincide with any temporal factors that could help explain them.

We begin by estimating Equation 1 for each year, and we report the difference-in-differences coefficients over time in Figure 1. In the figure, we report the coefficient for each estimation, in addition to the 95 percent confidence intervals. The results show that in earlier years, the different-race effect is not statistically different from zero. This is true for every year until 2018, which means that our main estimates are driven by years after 2018. For example, the difference-in-differences estimate was about +0.25 in the year 2019, which is highly statistically significant. The estimate is statistically insignificant and imprecise in 2020, and we believe this could be due to COVID-19. Similarly, the estimate is highly imprecise for cases filed in 2022, and that could be due to the low number of charges filed in that year (around 700). One possible explanation for why the results are driven by later years is the heightened concerns over racial discrimination in the US, especially within the

[14]The upper 95 percent confidence interval is an 80 percent increase relative to the control mean of 0.466.

criminal justice system. These concerns instigated social and political movements, such as the Black Lives Matter movement that gained momentum after Michael Brown's killing by the police in 2014. However, the evidence is merely suggestive that this is true, especially since the estimates are insignificant for the years 2020 and 2022.

Hence, we have shown that our main results are driven by less common crime types, such as criminal trespassing and evading an arrest, and by property-related crimes. In addition, they are driven by more recent years, possibly due to nationwide political changes. In the next section, we turn to the results using our new proposed method to test for absolute racial bias.

6 Mechanisms

In the previous section, we show that contrary to other agents in the criminal justice system, for example, police (e.g., Hoekstra and Sloan 2022), attorneys achieve better outcomes for their different-race defendants. This is primarily driven by White attorneys being more effective in terms of securing dismissals for their Black defendants. In this section, we provide an extensive discussion of the possible mechanisms behind these estimates. A case outcome depends on several factors, such as the amount of evidence presented, procedural errors, rights violations, witnesses, and testimonies, all of which can be influenced by both, prosecutors and defense attorneys. For instance, an attorney can challenge the reliability of the prosecution's testimonial evidence as a defense strategy. On the other hand, prosecutors and law enforcement control the evidence presented in a given case.

Let's begin by considering the attorneys' behavior in response to the race of their defendant. One possible explanation for the different-race effect is that White attorneys are more skilled at defending the types of crimes that are more likely to be committed by Black defendants relative to Black attorneys. However, we show in Column (3) of Table 4 that the results remain robust after controlling for interaction terms of attorney race and case

characteristics. We also show that the results are robust to controlling for interaction terms of defendant race and attorney characteristics, including years of experience and law school ranking (Column (4) of Table 4). To the extent that case characteristics are good proxies for case type, and to the extent that attorney characteristics are good proxies for attorney quality, these results suggest that the differences in cases across Black and White defendants and the difference in skills across Black and White attorneys cannot explain the different-race effect.

Another possible explanation is racial discrimination in legal representation by attorneys *against* members of the same group. Conversely, this also implies favoritism towards defendants of a different race. While empirical evidence shows that more often than not, individuals exhibit racial discrimination against individuals of a different group, the opposite can sometimes be true, such as in the context of juvenile judges (Depew et al. 2017). This can be due to treating in-group individuals more harshly when they violate social norms.

While we cannot directly test for racial discrimination, one way through which it could manifest itself is through the amount of effort an attorney exerts when representing a certain case. If an attorney is discriminatory against one race over the other, they might differentially allocate their time and effort depending on the defendant's race. We use two measures as proxies for attorney effort: case length and compensation amount. The payment structure incentivizes attorneys to dispose of cases swiftly to get assigned more cases and earn more (e.g., Anderson and Heaton 2012). Thus, an increase in the number of days until disposition may signal an increase in attorney efforts (Agan et al. 2021). This could also be reflected in the amount of money an attorney receives for each case. While attorneys receive a flat fee per case, they receive additional payments for other additional actions they take (for instance, appealing, trial, jail visit, etc..). While we do not observe the specific actions taken by each attorney, we do observe the total payment amount they receive for each case, which we use as a second proxy for attorney effort.

We estimate the different-race effect on days until disposition using Equation 1, and

we report the results in Table B3. We find that having a different-race attorney does not increase the time until disposition. For example, Column (1) shows that a different-race attorney causes the time spent on a given case to increase by about 4 days. However, this increase is economically small and statistically insignificant. The results are robust to controlling for case characteristics (column 2) and to controlling for interaction terms of case characteristics with attorney race (column 3). In addition, we estimate the effect of having a different-race attorney on the compensation amount the attorney receives per case. The results are reported in Table B4. The coefficients are small compared to the mean of the compensation, 194.3, and statistically insignificant across the specifications. Thus, the results in Table B3 and Table B4 suggest that the different-race effect cannot be explained by attorneys exerting more effort when the defendant is of a different race.

However, we cannot entirely rule out that the results are driven by White attorneys "favoring" Black defendants for two reasons. First, anecdotally, some White attorneys adjust their behavior to counteract potential biases that exist elsewhere in the system. According to conversations with a public defender from a different county, attorneys sometimes use "racial bias" as a defense strategy to dismiss charges pressed against Black defendants. It's also believed that claims of racial bias might be seen as more credible when presented by a White lawyer compared to a Black one. If this is true, it can lead to a higher dismissal rate for Black defendants represented by White attorneys. Second, our difference-in-differences effects over time show that the results are positive and significant in more recent years, which loosely coincide with national political movements against racial injustice. Although descriptive, this can be suggestive evidence that White attorneys exhibit favoritism towards Black defendants for the sake of political correctness, or perhaps to avoid accusations of racial discrimination against Black defendants.

Finally, a different-race attorney may improve case outcomes by affecting how others behave towards the defendant. A mismatch of an attorney and a defendant race could affect how prosecutors and/or judges perceive a case. For example, judges may be more

sympathetic towards Black defendants who are represented by White attorneys and thus, are more lenient towards them. We argue that judges' behavior is less of a concern in our setting since most of these charges get resolved without a trial. While we do not observe whether the cases in our sample go to trial or not, other research shows that most misdemeanor charges are resolved without trial (Mayson and Stevenson 2020).

However, it is still possible for prosecutors to contribute to the different-race effect in two ways: prosecutors can be more sympathetic towards Black defendants who are represented by White attorneys, so they prosecute a Black defendant's case less harshly when they are represented by a White attorney. On the contrary, if prosecutors exhibit racial bias against Black individuals in general, they might dedicate more effort to prosecute a case that is brought up against a Black defendant who is also represented by a Black attorney. We find the first possibility to be more plausible than the second one. If prosecutors are racially biased against Black individuals, they should affect case outcomes of Black defendants equally, regardless of the race of their attorney. Second, the fact that the results are driven by more recent years suggests that prosecutors may act in the same manner as defense attorneys, in the sense that they are motivated by political correctness and are more concerned about being accused of racism.

In sum, we argue that the effect of a different-race attorney on case outcomes cannot be driven by other (defendant or attorney) characteristics that matter for the defendant-attorney race pairing. Specifically, they are not driven by White attorneys being better at representing crimes that are more likely to be committed by Black defendants. While we do not find evidence of a change in attorney effort, we argue that the results are driven by a *positive* change in White attorneys' behavior, prosecutors' behavior, or both, towards Black defendants. While it is possible that Black attorneys might exhibit racial bias against Black defendants, the results over time suggest that that might not be the case. The different-race effects are more pronounced in more recent years, which could be explained by the national movement against racial injustice that could impact the way agents behave towards Black

individuals in the criminal justice system.

7 Conclusion

In this paper, we use the quasi-random assignment of court-appointed attorneys to misdemeanor cases to test whether defense attorneys secure better deals for same-race defendants. Using approximately 15,000 misdemeanor cases from Travis County, our difference-in-differences estimates show that attorneys achieve better outcomes for different-race defendants, in contrast with what others have found in different contexts, such as policing (Hoekstra and Sloan 2022). Specifically, we show that a different-race attorney causes a 20-22 percent increase in the likelihood of a case dismissal and a 16-27 percent decrease in the likelihood of incarceration. Estimating attorney random effects, we show that these results are due to White attorneys being more effective at securing dismissals for Black defendants relative to Black attorneys. In addition, the random effects suggest that this is driven by the entire sample of White attorneys, rather than a handful of them.

To understand the drivers of these different-race effects, we perform a battery of tests. First, we rule out the possibility that other observed characteristics that are correlated with race, but not race itself, drive these results. We do so by controlling for the interaction terms of case characteristics (crime type, date, defendant sex, age, and criminal history) with attorney race and by controlling for interaction terms of attorney characteristics (law school ranking and years of experience) with defendant race. We show that controlling for these interaction terms does not affect our coefficient estimates. Second, we show that the results are not driven by a change in attorney effort, as proxied by case length and compensation amount received per case. Third, we show that the results are more pronounced in more recent years, which corroborates anecdotal evidence that White attorneys or prosecutors adjust their behavior to counteract potential biases elsewhere in the system, or are simply motivated by the avoidance of accusations of racial bias.

In light of the existing racial disparities in the criminal justice system, our results have important policy implications. In terms of designing defendant-attorney matching mechanisms, assigning an attorney of the same race does not necessarily translate into better outcomes. More broadly, our results suggest that putting significant weight on extralegal factors, such as race, may be less effective than other considerations with respect to improving outcomes.

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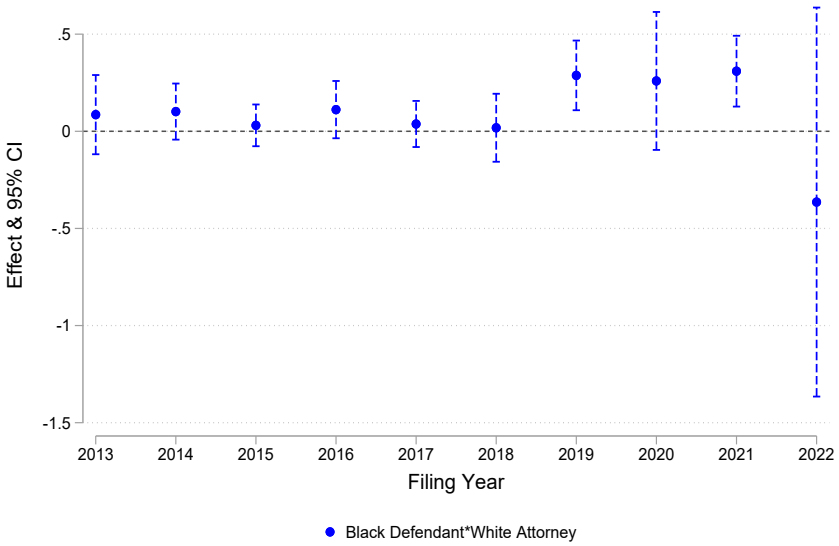
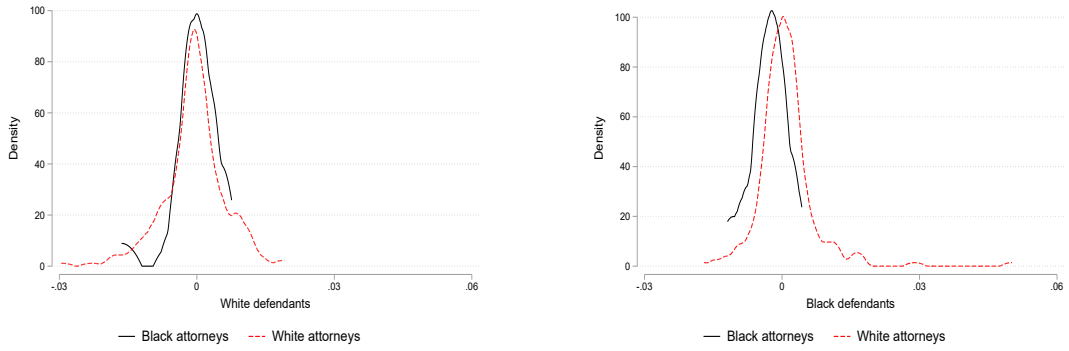
8 Tables and figures

Figure 1: The effect of a different-race attorney over time



Notes: This figure shows the difference-in-differences estimate by year. We estimate Equation 1 for each year separately, and we report the coefficients with their 95% confidence intervals. The standard errors are two-way clustered at the attorney and defendant level. Year 2022 includes 34 charges that were filed in 2023.

Figure 2: Attorney effects by defendant race



(a) White defendants

(b) Black defendants

Notes: These figures show the distribution of individual attorney random effects by attorney race and by defendant race. Panel (a) shows the random effects for attorneys conditional on a White defendant, while Panel (b) shows the random effects for attorneys conditional on a Black defendant. The random effects are calculated by regressing our main outcome variable (dismissal) on a set of case characteristics and fixed effects (charge type, day of the week, defendant sex, age, zipcode, previous arrests, and month-by-year-by-court fixed effects). We then save the residualized case dismissal and calculate constant random effects using the Stata command *mixed*. Each panel shows results from a separate regression.

Table 1: Summary statistics

	(1) All defendants	(2) Black defendants	(3) White defendants
Outcomes			
Dismissed	0.492	0.543	0.467
Probation	0.132	0.0756	0.159
Jail	0.300	0.331	0.285
Fine only	0.000452	0.000600	0.000382
Defendant characteristics			
Black	0.323	1	0
Female	0.237	0.207	0.251
Age(years)	34.37	34.57	34.27
Previous charges	1.190	1.381	1.099
Case characteristics			
White attorney	0.952	0.950	0.953
Drug	0.145	0.172	0.132
Property	0.0926	0.0896	0.0941
DWI	0.222	0.0984	0.282
Invalid license	0.102	0.120	0.0934
Weapon	0.0197	0.0240	0.0177
Other misd.	0.418	0.496	0.381
Observations	15479	5001	10478

Standard deviations in parentheses

Notes: This table shows the means for our outcome variables, defendant characteristics, as well as case characteristics. This sample consists of cases for which we observe the attorney's race. The data are at the charge level.

Table 2: The effect of attorney race on predicted outcomes

	(1) Predicted dismissal	(2) Predicted jail	(3) Predicted probation
Panel A: Unconditional			
White attorney	0.0173	-0.0238	-0.00731
	(0.0138)	(0.0222)	(0.00496)
Observations	15479	15479	15479
Panel B: Month-year FE			
White attorney	0.00209	-0.000652	0.00144
	(0.00554)	(0.00811)	(0.00413)
Observations	15479	15479	15479
Panel C: Month-year-court FE			
White attorney	0.00128	-0.000438	0.000966
	(0.00523)	(0.00755)	(0.00439)
Observations	15479	15479	15479
Outcome Mean	0.491	0.300	0.132

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the correlation between predicted outcomes and attorney race. For each outcome, we use case characteristics, including crime type, day of the week, month and year, defendant's race, sex, and age to predict the likelihood of case dismissal, prison, and probation. We then estimate the effect of attorney race on each predicted outcome. Each column represents an outcome, and we report the unconditional results in Panel (A), while we include month-by-year fixed effects and month-by-year-by-court fixed effects in Panels (B) and (C), respectively. In all regressions, standard errors are two-way clustered at the attorney and defendant level.

Table 3: Average dismissal rates by attorney race and defendant race

	(1) White attorneys	(2) Black attorneys
White defendant	0.468	0.454
Black defendant	0.549	0.424

Notes: This table shows a simple 2x2 representation of the difference-in-differences approach. Each cell represents the average dismissal rate for a specific group. For example, column (1) and row (1) show the rate of dismissal for White attorneys conditional on the defendants being White.

Table 4: The effect of a different-race attorney on case dismissal

	(1)	(2)	(3)	(4)
Outcome: Dismissed				
White attorney×Black defendant	0.0967***	0.0987***	0.108***	0.111***
	(0.0237)	(0.0255)	(0.0302)	(0.0331)
Observations	15479	15479	15479	14880
Outcome Mean	0.490	0.490	0.490	0.491
Defendant race indicator	Y	Y	Y	Y
Month-year-court FE	Y	Y	Y	Y
Attorney FE	Y	Y	Y	Y
Case Characteristics	N	Y	Y	Y
Attorney race*Case characteristics	N	N	Y	Y
Defendant race*Attorney characteristics	N	N	N	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the difference-in-differences estimates for the different-race effect on dismissal using our Equation 1. In all three columns, we include month-by-year-by-court fixed effects, in addition to an indicator for defendant race and attorney fixed effects. Column (1) shows the results using our baseline specification. In column (2), we control for case characteristics, including dummy variables for charge type, day of the week, defendant characteristics including age, sex, and number of previous charges. In column (3), we control for case and defendant characteristics, in addition to interaction terms for attorney race with case characteristics. In all regressions, standard errors are two-way clustered at the attorney and defendant level.

Table 5: The effect of a different-race attorney on sentencing outcomes

	(1)	(2)	(3)
Panel A: Jail			
White attorney×Black defendant	-0.0815*	-0.0714*	-0.0506*
	(0.0424)	(0.0399)	(0.0298)
Observations	15479	15479	15479
Outcome Mean	0.301	0.301	0.301
Panel B: Probation			
White attorney×Black defendant	-0.0127	-0.0232	-0.0185
	(0.0191)	(0.0160)	(0.0177)
Observations	15479	15479	15479
Outcome Mean	0.132	0.132	0.132
Panel C: Fine only			
White attorney×Black defendant	-0.0000360	-0.0000561	0.000135
	(0.000703)	(0.000707)	(0.000625)
Observations	15479	15479	15479
Outcome Mean	0.000458	0.000458	0.000458
Defendant race indicator	Y	Y	Y
Month-year-court FE	Y	Y	Y
Attorney FE	Y	Y	Y
Case Characteristics	N	Y	Y
Interactions	N	N	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the difference-in-differences estimates for the different-race effect on sentencing outcomes using Equation 1. Each panel represents the effect on a separate outcome; panel (A) shows the effect on being sentenced to jail, panel (B) shows the effect on receiving a probation sentence, and panel (C) shows the effect on receiving a fine only. Each column is a separate regression. In all three columns, we include month-by-year-by-court fixed effects, in addition to an indicator for defendant race and attorney fixed effects. Column (1) shows the results using our baseline specification. In column (2), we control for case characteristics, including dummy variables for charge type, day of the week, defendant characteristics including age, sex, and number of previous charges. In column (3), we control for case and defendant characteristics, in addition to interaction terms for attorney race with case characteristics. In all regressions, standard errors are two-way clustered at the attorney and defendant level.

9 Online Appendix

9.1 Appendix A: Data

We use misdemeanor cases that were filed in Travis County, Texas for the years 2013-2022, a total of 131,166 charges (129,679 unique cases). Here, we show how the data restrictions that we perform as explained in section 3 affect the sample size. Note that the numbers here reflect the number of individual charges rather than individual cases since the data are at the charge-level.

- To obtain charges that were represented by court-appointed attorneys, we match the misdemeanor records to the wheel data (that shows the court-appointed attorney). Total number of indigent charges from 2013-2022: 52,685 charges (51,979 unique cases).
- Dropping cases where the attorney was non-randomly assigned by a judge (11%): 46,682 charges.
- Dropping assault charges (12%): 41,259 charges.
- Dropping Hispanic defendants or defendants of other races (Asian, Middle Eastern, etc. ...) (40%): 24,849 charges.
- Dropping cases with missing defendant race (<1%): 24,706 charges
- Dropping observations where attorney race is missing or attorney is not Black nor White (37%): 15,479 charges

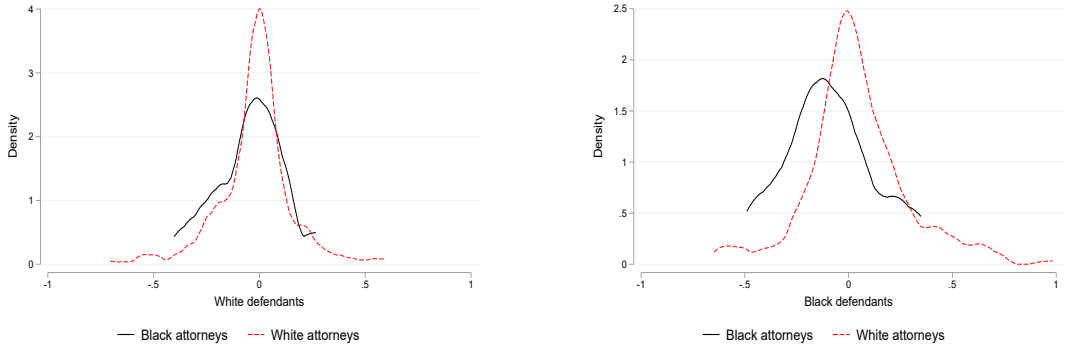
Table A1: Attorney characteristics

White attorney	0.921
Black attorney	0.0792
Law school ranking	75.32
Experience (years)	17.07
Observations	240

Notes: This table shows the summary statistics for attorney characteristics in the main sample.

9.2 Appendix B: Additional results

Figure B1: Attorney fixed effects



(a) White defendants (b) Black defendants

Notes: These figures show the distribution of the individual attorney fixed effects by defendant race (Panels (a) and (b)). To estimate attorney fixed effects, we regress case dismissal on a set of case characteristics (including dummy variables for charge type, day of the week, defendant sex, age, number of previous charges, and month-by-year-by-court fixed effects) and attorney fixed effects and save the attorney fixed effects (using the Stata command reghdfe). Each panel shows the estimates from a separate regression.

Table B1: The effect of case characteristics on attorney race

	(1) White attorney	(2) White attorney	(3) White attorney
Drug	-0.00830*	-0.000654	-0.00231
	(0.00469)	(0.00380)	(0.00431)
Property	-0.00998	-0.00422	-0.00332
	(0.00685)	(0.00760)	(0.00841)
DWI	-0.00558	-0.00385	-0.00447
	(0.00377)	(0.00348)	(0.00344)
Invalid license	-0.0158**	-0.00626	-0.00709
	(0.00687)	(0.00658)	(0.00631)
Weapon	0.0151	0.0120	0.00946
	(0.0122)	(0.0102)	(0.0114)
Female	0.00422	0.00444	0.00364
	(0.00369)	(0.00365)	(0.00383)
Age(years)	-0.000159	-0.000233	-0.000271
	(0.000338)	(0.000353)	(0.000355)
Black	-0.00394	-0.00422	-0.00392
	(0.00486)	(0.00476)	(0.00475)
Day of week	0.00197*	0.00206*	0.00183
	(0.00117)	(0.00118)	(0.00123)
Previous charges	0.00168*	0.000779	0.000669
	(0.000918)	(0.00126)	(0.00124)
N	15479	15479	15479
Outcome Mean	0.952	0.952	0.952
F-stat	1.018	0.616	0.543
P-value	0.429	0.799	0.859
Unconditional	Y	N	N
Month-year FE	N	Y	N
Month-year-court FE	N	N	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the results from F-tests that show the effect of case characteristics on attorney race. Each column is a separate regression equation, where we regress a dummy variable equals to one if the attorney is White and zero otherwise, on dummy variables for charge type, defendant age, sex, race, a measure of criminal history, and day of the week. Column (1) shows the results without controlling for any date or court fixed effects, column (2) shows the effects using month-by-year fixed effects, and column (3) shows the effects using our baseline specification, which includes month-year-court fixed effects. For each set of results, we report the joint F-statistic including the p-value. In all regressions, standard errors are two-way clustered at the attorney and defendant level.

Table B2: The correlation between attorney race and case characteristics

	(1) Drug	(2) Property	(3) DWI	(4) Invalid license	(5) Weapon	(6) Other misd.	(7) Black	(8) White	(9) Female	(10) Age(years)	(11) Previous charges
Panel A: Unconditional											
White attorney	-0.0138	-0.00900	-0.00741	-0.0260*	0.00794	0.0483***	-0.0150	0.0150	0.0132	-0.346	0.289*
	(0.00932)	(0.0110)	(0.0133)	(0.0153)	(0.00503)	(0.0152)	(0.0215)	(0.0215)	(0.0146)	(1.036)	(0.154)
Observations	15479	15479	15479	15479	15479	15479	15479	15479	15479	15479	15479
Outcome Mean	0.145	0.0926	0.222	0.102	0.0197	0.418	0.323	0.677	0.237	34.37	1.190
Panel B: Month-year FE											
White attorney	0.00279	-0.00232	-0.0106	-0.0102	0.00528	0.0151	-0.0169	0.0169	0.0169	-0.686	0.108
	(0.00982)	(0.0143)	(0.0119)	(0.0139)	(0.00392)	(0.0184)	(0.0207)	(0.0207)	(0.0151)	(1.087)	(0.216)
Observations	15479	15479	15479	15479	15479	15479	15479	15479	15479	15479	15479
Outcome Mean	0.144	0.0923	0.224	0.103	0.0195	0.417	0.321	0.679	0.237	34.41	1.192
Panel C: Month-year-court FE											
White attorney	-0.000542	0.000235	-0.0106	-0.0113	0.00448	0.0177	-0.0156	0.0156	0.0145	-0.801	0.0899
	(0.0109)	(0.0154)	(0.0115)	(0.0134)	(0.00451)	(0.0193)	(0.0205)	(0.0205)	(0.0155)	(1.083)	(0.215)
Observations	15479	15479	15479	15479	15479	15479	15479	15479	15479	15479	15479
Outcome Mean	0.144	0.0923	0.224	0.103	0.0195	0.417	0.321	0.679	0.237	34.41	1.192

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the effect of attorney race on case characteristics. Each column is a separate regression, with the case characteristic being the dependent variable. We report the results without controlling for date and court-fixed effects in Panel (A). In Panel (B), we add month-by-year fixed effects, while in Panel (C), we add month-by-year-by-court fixed effects, which is our baseline specification. In all regressions, standard errors are two-way clustered at the attorney and defendant level.

Table B3: The effect of a different-race attorney on days until disposition

	(1)	(2)	(3)
Outcome: Days until disposition			
White attorney×Black defendant	6.329	-3.364	2.354
	(21.05)	(20.54)	(21.19)
Observations	13938	13938	13938
Outcome Mean	366.1	366.1	366.1
Defendant race indicator	Y	Y	Y
Month-year-court FE	Y	Y	Y
Attorney FE	Y	Y	Y
Case Characteristics	N	Y	Y
Interactions	N	N	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the difference-in-differences estimates for the different-race effect on days until disposition using Equation 1. The outcome variable, days until disposition, reflects the number of days between the disposition date and the filing date of each charge. In all three columns, we include month-by-year-by-court fixed effects. Column (1) shows the results using our baseline specification. In column (2), we control for case characteristics, including dummy variables for charge type, day of the week, and defendant characteristics including age, sex, and number of previous charges. In column (3), we control for case and defendant characteristics, in addition to interaction terms of attorney race with case characteristics. In all regressions, standard errors are two-way clustered at the attorney and defendant level.

Table B4: The effect of a different-race attorney on compensation amount per case

	(1)	(2)	(3)
Outcome: Compensation amount per case			
White attorney×Black defendant	-2.954	-3.663	1.303
	(10.96)	(10.78)	(8.201)
Observations	15479	15479	15479
Outcome Mean	194.3	194.3	194.3
Defendant race indicator	Y	Y	Y
Month-year-court FE	Y	Y	Y
Attorney FE	Y	Y	Y
Case Characteristics	N	Y	Y
Interactions	N	N	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the difference-in-differences estimates for the different-race effect on the compensation amount an attorney receives per case using Equation 1. The outcome variable, compensation, reflects the total amount of compensation an attorney receives per case. In all three columns, we include month-by-year-by-court fixed effects. Column (1) shows the results using our baseline specification. In column (2), we control for case characteristics, including dummy variables for charge type, day of the week, and defendant characteristics including age, sex, and number of previous charges. In column (3), we control for case and defendant characteristics, in addition to interaction terms of attorney race with case characteristics. In all regressions, standard errors are two-way clustered at the attorney and defendant level.

Table B5: The effect of a different race attorney on case outcomes – cases with only one attorney

	(1) Dismiss	(2) Jail	(3) Probation	(4) Fine
White attorney×Black defendant	0.0853***	-0.0704	-0.0154	0.00000550
	(0.0251)	(0.0438)	(0.0179)	(0.000778)
Observations	14166	14166	14166	14166
Outcome Mean	0.490	0.306	0.126	0.000500
Defendant race indicator	Y	Y	Y	Y
Month-year-court FE	Y	Y	Y	Y
Attorney FE	Y	Y	Y	Y
Case Characteristics	Y	Y	Y	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the difference-in-differences estimates for the different-race effect on case outcomes, dropping cases where we observe multiple court-appointed attorneys, which account for 9 percent of the sample. Each column represents an outcome. In all regressions, we control for month-by-year-by-court fixed effects, an indicator for a defendant's race, attorney fixed effects, and case characteristics. Case characteristics include dummy variables for charge type, day of the week, defendant's sex, age, and number of previous charges. Standard errors are two-way clustered at the attorney and defendant level.

Table B6: The effect of a different-race attorney by crime type

	(1) Drug	(2) Property	(3) Traffic	(4) Other
Outcome: Dismissed				
White attorney×Black defendant	-0.0371	0.142	-0.0340	0.0729
	(0.0887)	(0.116)	(0.0606)	(0.0496)
Observations	2241	1434	5031	6773
Outcome Mean	0.793	0.466	0.394	0.468
Outcome: Jail				
White attorney×Black defendant	0.0445	-0.0701	-0.0445	-0.0188
	(0.0672)	(0.124)	(0.0448)	(0.0659)
Observations	2241	1434	5031	6773
Outcome Mean	0.180	0.326	0.257	0.368
Defendant race indicator	Y	Y	Y	Y
Month-year-court FE	Y	Y	Y	Y
Attorney FE	Y	Y	Y	Y
Case Characteristics	Y	Y	Y	Y

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the difference-in-differences estimates for the different-race effect on case outcomes conditioning on crime type. Violent crimes include weapon-related offenses. Drug crimes include possession of marijuana or any controlled substance. Property crimes include any kind of theft or attempted theft (for example, organized retail theft, theft from a person, etc. ...) and burglaries. Traffic-related crimes include driving while intoxicated and driving with an invalid license. Finally, other misdemeanors include criminal trespass, evading arrest/detention, obstruction of highway passageway, violating protective orders, failure to appear in court, other less common crimes (e.g., illegal dumping, false statement, indecent exposure, etc... .), and weapon-related offenses. Note that we drop assault charges for the reason mentioned in Section 3. In all regressions, we control for month-by-year-by-court fixed effects, an indicator for a defendant's race, attorney fixed effects, and case characteristics. Case characteristics include dummy variables for charge type, day of the week, defendant's sex, age, and number of previous charges. Standard errors are two-way clustered at the attorney and defendant level.