

Do Prosecutor and Defendant Race Pairings Matter? Evidence from Random Assignment

CarlyWill Sloan*

June 19, 2025

Abstract

Racial disparities in criminal justice outcomes are well-documented. However, there is little evidence on whether the pairing of prosecutors and defendants contributes to these disparities. This paper tests for cross-race effects in convictions by prosecutors. I leverage conditional as-good-as-random assignment of misdemeanor cases using administrative data from New York County's Early Case Assessment Bureau. Assignment to a cross-race prosecutor increases conviction rates for property crimes by 5 percentage points (8 percent), with no strong effects for other crimes. Results appear driven by reduced use of dismissals—particularly adjournments in contemplation of dismissal, a discretionary form of leniency controlled by prosecutors.

*Sloan: United States Military Academy at West Point, carlywill.sloan@westpoint.edu

Acknowledgements: I am grateful for valuable feedback from Adam Bestenbostel, Laura Dague, Jennifer Doleac, Keanan Gleason, Mark Hoekstra, Jonathan Meer, Abigail Peralta, Brittany Street, May Tangvatcharapong, and Chelsea Temple. I also thank participants at the Southern Economic Association, the Association for Public Policy Analysis and Management, the Western Economic Association, the Society of Labor Economists, the Transatlantic Workshop on the Economics of Crime, and numerous seminar audiences not listed here. I am especially appreciative of the New York County District Attorney's Office for answering my questions and allowing me to tour their facilities. I have undoubtedly benefited from the insights of others not mentioned here—please forgive any omissions. Any remaining errors are my own. All opinions expressed in this manuscript are those of the authors and do not represent the opinions of the United States Military Academy (USMA), the United States Army, or the Department of Defense. DOD Assurance # A20133 HHS Assurance # FWA00007405

1 Introduction

Racial disparities in U.S. criminal justice outcomes are both persistent and well-documented. Black Americans are over three times as likely as non-Black Americans to have a criminal record or to have been incarcerated (Shannon et al., 2017). Public perception reflects these gaps: many believe the system treats Black and Hispanic Americans unfairly (Rasmussen Reports, 2014; Pew Research Center, 2019). A growing body of research has used quasi-random assignment to estimate racial bias among police, judges, and juries (Agan, 2024). However, comparatively little is known about the role of prosecutor-defendant racial pairings, despite widespread agreement that prosecutors possess the greatest discretion within the criminal legal system (Luna and Wade, 2015; Pfaff, 2017; Sklansky, 2018; Stith, 2008). Prosecutors make decisions about charges, plea offers, dismissals, and trial strategies, and legal scholars have identified prosecutorial discretion as a potential driver of racial disparities (Foster v. Chatman, 2016; State v. Monday, 2011; Pfaff, 2017; Rehavi and Starr, 2014; Smith and Levinson, 2011).

This paper estimates the causal effect of cross-race prosecutor-defendant pairings on misdemeanor conviction outcomes. I exploit a setting in New York County (Manhattan), where prosecutors are assigned cases through a rotational system based on case timestamps. Cases are screened by the Early Case Assessment Bureau (ECAB), and during each shift, administrators assign prosecutors the earliest timestamped case. Prosecutors cannot preview or select cases and must return for the next in sequence once a case is completed. Because assignment is based solely on arrival time—and enforced independently of prosecutor or defendant characteristics—this process yields as-good-as-random assignment conditional on screening date.

Under this system, prosecutor race is orthogonal to both observable and unobservable characteristics of the defendant and the case. I validate this by showing that prosecutor race is uncorrelated with observable defendant demographics and case features. I then estimate the effect of race pairings on conviction by interacting prosecutor and defendant

race, differencing out each actor's main effect. This empirical strategy follows a growing literature that uses racial pairings to detect bias (e.g., Anbarci and Lee, 2014; Antonovics and Knight, 2009; Anwar and Fang, 2006; Parsons et al., 2011; Price and Wolfers, 2010; West, 2018). Unlike prior studies, however, there is no concern here that prosecutors are selecting into cases.

I use administrative data collected by the Vera Institute (Kutateladze, 2017) from the New York County District Attorney's Office, which prosecutes all criminal cases in Manhattan—over 100,000 annually—in a jurisdiction of more than 1.6 million people (City of New York, 2015). The office is one of the largest in the country and is widely regarded as progressive in orientation. It has implemented several criminal justice reforms and formal equity initiatives (Manhattan District Attorney, 2018b).

I find significant cross-race effects on conviction outcomes for property crimes, but not for drug, person, or other offenses. Specifically, Black defendants charged with property crimes are convicted at a rate 5 percentage points higher when assigned to a white prosecutor rather than a Black one (65 percent vs. 61 percent). White defendants, by contrast, show similar conviction rates regardless of prosecutor race. This gap represents an 8 percent increase in conviction for Black defendants and explains roughly 40 percent of the Black–white conviction disparity in property crime cases. The result is robust to controls and corrections for multiple inference. Effects are especially pronounced among first-time defendants and appear driven by reduced use of adjournments in contemplation of dismissal (ACDs)—a non-conviction outcome where charges are dropped after a waiting period, and one that lies almost entirely within prosecutorial discretion.

While the data do not reveal why effects concentrate in property crimes, prior research suggests plausible mechanisms. Disparities in even a single high-volume, discretionary offense category carry important policy implications, making it critical to understand these patterns. One likely explanation is that drug cases, which often involve physical evidence and standardized plea practices, constrain prosecutorial discretion. Property crimes, by contrast, may rely more on subjective judgment, especially when evidence is limited. Al-

though I lack direct measures of evidentiary strength, studies show physical evidence is far more common in drug cases than in property crimes (Peterson et al., 2010).

This paper contributes to a growing literature on prosecutorial discretion, racial disparities, and institutional behavior. Recent work has examined whether prosecutors mitigate or exacerbate inequities introduced earlier in the justice process. Some studies find that prosecutors alter racial gaps in charging or sentencing (Harrington and Shaffer, 2025; Jordan, 2024), while others show outcomes respond to institutional constraints and political incentives. For example, Yang (2016) finds that judicial vacancies and pension eligibility increase dismissals; Arora (2019) and Krumholz (2019) show that electing Republican or nonwhite District Attorneys affects sentencing severity. Other work documents disparities using selection-on-observables designs (Kutateladze et al., 2012; Kutateladze and Andiloro, 2014). Complementing this work, Didwania (2022) shows that federal prosecutors exhibit own-gender favoritism in charging and sentencing, though her study focuses on gender rather than race, uses federal felony cases, and relies on courthouse fixed effects rather than random assignment.

This study builds most directly on Rehavi and Starr (2014), who finds that prosecutors help explain racial gaps in federal sentencing, and on Tuttle (2019), who shows that prosecutors increase drug weights to trigger harsher mandatory minimums for minority defendants. I extend this work in several ways. First, I exploit a setting with as-good-as-random case assignment, enabling stronger causal identification. Second, I shift the focus to state-level misdemeanors—a context where most criminal adjudication occurs and discretion is often greatest. Third, I examine racial pairings directly, capturing interpersonal effects missed in aggregate analyses. Fourth, I find that effects are concentrated in property crimes, suggesting bias may be most acute in ambiguous cases. Finally, I analyze a broader range of prosecutorial decisions to better characterize how discretion is exercised.

These findings emerge in a jurisdiction that has explicitly pursued racial equity reforms. The New York County District Attorney’s Office has declined to prosecute many low-level infractions and introduced alternatives to incarceration for drug offenses (Cyrus Vance For

District Attorney, 2017; Manhattan District Attorney, 2018a). It also maintains a diversity committee and employs a chief diversity officer. If disparities persist in this relatively progressive office, they may be larger elsewhere, where oversight is weaker or commitments to reform are less robust.

These disparities carry broader consequences. Most prosecutors are white and most defendants Black, making race-mismatched pairings both frequent and consequential. The effects are especially pronounced for first-time defendants charged with misdemeanors, which make up the vast majority of criminal cases—about 13.2 million annually in the U.S. (Stevenson and Mayson, 2018a). These often represent a person’s first or only formal contact with the legal system. Early prosecutorial decisions—particularly the choice between dismissal and conviction—can affect long-term employment and future justice involvement. Prior work documents lasting labor market penalties of criminal records (Agan et al., 2022; Finlay, 2008; Mueller-Smith and T. Schnepel, 2021; Pager, 2003); recent evidence suggests even retroactive expungement rarely erases these effects (Agan et al., 2024).

These results underscore the importance of targeting reforms to high-discretion settings. Although I find no cross-race effects in most offense categories, disparities in a common and discretionary charge type suggest that equity-focused interventions may be most effective when tailored to specific case contexts. Improving demographic alignment between prosecutors and defendants—or increasing oversight of discretionary decisions—may offer a path forward. At the same time, identifying the institutional and interpersonal conditions under which these effects emerge remains an important area for future research.

2 Background and Data

2.1 Case Assignment and the Prosecutor’s Role in New York County

Evaluating prosecutor race effects is challenging due to nonrandom case assignment. But in New York County, misdemeanor cases are assigned without prosecutorial discretion.

After arrest, police record charges and prior history. Less serious offenses (e.g., infractions or violations) often result in a summons, with no prosecutor assigned. Misdemeanor and felony cases are sent to the Early Case Assessment Bureau (ECAB), though felonies are screened by senior prosecutors and excluded from this study.

In contrast, misdemeanor cases are assigned quasi-randomly. During each ECAB shift, administrators assign prosecutors the earliest open case based on timestamp. I observed this process directly, and it has been independently validated. Assignment is unrelated to prosecutor or defendant characteristics. ECAB operates 24/7, and prosecutors receive new cases only after completing the previous. Prior studies using these data did not isolate race pairings or restrict to randomly assigned cases (Kutateladze and Andiloro, 2014).

Once in ECAB, misdemeanor cases are screened based on severity and logistics: fare evasion goes through Expedited Arrest Processing (EAP); minor cases are handled via Desk Appearance Tickets (DATs); more serious cases are reviewed in person; some are screened by phone to reduce processing time.¹ First-year prosecutors rotate through ECAB and other units before specializing. Nearly all felony prosecutors begin their careers here.

Once assigned, prosecutors shape key outcomes: they may decline prosecution, adjust charges, recommend detention, offer a plea deal, or seek dismissal. These decisions often determine whether a defendant is convicted. Declinations are rare, likely due to the DA–NYPD relationship, and typically occur only when evidence is insufficient or the charge is no longer prosecuted (Kutateladze and Andiloro, 2014).

Prosecutors may also adjust charge severity at screening.² For example, a Class B misdemeanor (up to 90 days) can be elevated to Class A (up to one year) (New York State, 2018). Severity affects detention, plea deals, and sentencing norms (Frederick and Steimen, 2012a).

Prosecutors structure plea deals, which may include reduced charges or recommended sentences. Judges approve them, but prosecutors drive the terms. Convictions result from

¹EAP involved rapid screening soon after arrest, often without full documentation. DATs were screened by paralegals and scheduled for later arraignment.

²Prosecutors are trained to evaluate charges beyond the arrest report, incorporating history and statutory criteria.

pleas or trials. Alternatively, prosecutors may offer an adjournment in contemplation of dismissal (ACD), which drops charges after 6–12 months if the defendant avoids new arrests. ACDs are not convictions or admissions of guilt.³ They are rarely reopened—of 36,411 ACDs in 2010–2011, only 1% were re-calendared—and require prosecutor approval.⁴

Dismissals can be initiated by judges, defense attorneys, or prosecutors—who may unilaterally dismiss misdemeanors (Kutateladze and Andiloro, 2014). The most common reason, failure to prosecute on time, accounts for 34% of dismissals. Prosecutorial prioritization influences which cases are dropped. Trials are extremely rare (0.03%), so trial and acquittal outcomes are included in the conviction measure. The DA’s Office uses vertical prosecution: in 57% of misdemeanor cases, the ECAB prosecutor sees the case through. In others, cases are reassigned post-arraignment, allowing for intent-to-treat analysis.

2.2 Data

I use administrative data from the New York County District Attorney’s Office, which prosecutes all crimes in Manhattan. The dataset, compiled by the Vera Institute and housed by the National Archive of Criminal Justice Data, covers all misdemeanor cases screened by the Early Case Assessment Bureau (ECAB) in 2010–2011 (Kutateladze, 2017).

My analysis focuses on cases originally charged as misdemeanors.⁵ The unit of analysis is the case. I limit the main sample to Black and white prosecutors and defendants, who together account for the vast majority of observed race pairings and provide sufficient sample size for subgroup analysis.⁶ This focus also aligns with longstanding research and public attention to Black–white disparities in the criminal justice system.⁷

Police record perceived defendant race in NYPD arrest reports, which are included in

³New York Criminal Procedure §170.55

⁴Defense attorneys may request ACDs, but only prosecutors authorize them. Marijuana ACDs are a statutory exception.

⁵Cases that are up- or down-charged remain in the dataset.

⁶Less than 1.5% of cases have a Hispanic ADA.

⁷Cross-race results for other groups are directionally similar but smaller. See Table A.4. Estimates for Hispanic and white defendants and prosecutors are not statistically distinguishable from zero (Table A.5).

ECAB case files.⁸ Prosecutor race is reported by the DA's Office. Race is missing for 1.6% of defendants and 4.9% of prosecutors. I also lack date of birth for 20 defendants, gender for 166, and arrest ZIP code for 94. The analysis sample includes only cases with complete characteristics. These missing values likely stem from clerical errors and are unrelated to race or outcomes. In Section A.2, I show results are robust to including these cases and to alternative assumptions about missing data.

The dataset includes defendant and prosecutor race, plus case details: arrest, screening, and sentencing charges; crime type; prior arrest, conviction, and incarceration history; gender and age. Prosecutor gender and race are also reported. I observe the full set of dispositions (trial conviction or acquittal, plea, dismissal, decline, or ACD) and each case's screening date. As-good-as-random assignment only requires conditioning on screening date. Section 4.1 shows prosecutor race is uncorrelated with case or defendant characteristics.

My main outcome is a case-level indicator for conviction: if a defendant is convicted on any charge, the case is coded as a conviction. This includes dismissed cases. Convictions result from either a plea deal or a trial. Cases are coded as not convicted if declined, dismissed, or ending in acquittal. Nearly all convictions (99.9%) result from pleas.⁹

I also examine other prosecutor-influenced outcomes that may affect case resolution: declined prosecution (the case is dropped at ECAB), dismissal (by judge or prosecutor), and adjournment in contemplation of dismissal (ACD), which ends the case in 6–12 months if there are no new arrests. These outcomes lead directly to no conviction. I also track charge increases (raising severity pre-disposition) and pretrial detention (detained after arraignment), which may affect outcomes indirectly.

Crime types follow New York law, as defined by the original researchers.¹⁰ The three main categories are drug, property, and person crimes; all others are labeled “other.”¹¹ While I don't observe the exact statute per case, common examples include marijuana

⁸Some case files include defendant photos.

⁹Of 208 trial cases, 119 end in conviction.

¹⁰Kutateladze et al. (2012): person offenses (§120–135), property (§140–165), drugs (§220–221).

¹¹Most “other” crimes involve custody (PL §205), weapons (PL §265), or public order (PL §240).

possession for drug crimes, petit larceny (under \$1,000) for property, and third-degree assault for person crimes (Kutateladze and Andiloro, 2014; Chauhan et al., 2014). Drug crimes make up 24% of the sample, property 39%, person 7%, and other 30%.

Table A.1 shows summary statistics for 75,666 cases. The average defendant has over four prior arrests and convictions, though nearly half have no prior arrests. About 20% of cases are dismissed, 20% end in ACD, and 60% result in conviction. Nearly all convictions (99.9%) come from plea deals. Most defendants (81%) are male, with an average age of 35. Female prosecutors make up 59% of the sample. Half of defendants have at least one prior arrest, and 45% have at least one prior conviction. Black defendants comprise 75% of the sample; Black prosecutors handle 14% of cases. Case combinations include: 3% white defendants with Black prosecutors, 10% Black defendants with Black prosecutors, 65% Black defendants with white prosecutors, and 21% white defendants with white prosecutors. There are 90 Black prosecutors and 533 white prosecutors.¹²

Table 1 reports conviction rates by race pairings. White defendants face similar conviction rates across Black and white prosecutors (48.6% vs. 50.5%). For Black defendants, conviction rates are higher overall—59.4% with Black prosecutors and 63.5% with white prosecutors. The Black–white conviction gap is 2.3 percentage points larger for white prosecutors. In property crime cases (Table 1), white defendants again have similar conviction rates across prosecutor race (50.2% with both). For Black defendants, conviction rates are 60.6% with Black prosecutors and 65.5% with white prosecutors. The Black–white conviction gap is 4.9 percentage points larger for white prosecutors.

These raw comparisons do not account for selection into screening days and thus do not perfectly reflect the research design. However, because prosecutors are assigned without case-level discretion and both Black and white prosecutors see Black and white defendants at similar rates (Table A.1), the tables preview the main results.

¹²Race information is missing for a small set of cases. I address this in A.2.

3 Methods

Random assignment of cases to prosecutors offers a strong setting to estimate the effect of prosecutor race on conviction. I use a generalized difference-in-differences model to measure the impact of cross-race pairings. Comparing conviction rates between Black and white defendants could conflate racial bias with differences in underlying case strength or evidence. Instead, I compare the Black–white conviction gap across prosecutor race: how much more likely Black defendants are to be convicted than white defendants when prosecuted by a white attorney versus a Black attorney. If prosecutor race does not matter, the gap should remain constant.¹³ I estimate the following:

$$\begin{aligned} Conviction_c = & \beta_0 + \beta_1 I(BlackDefendant)_c + \beta_2 I(WhiteProsecutor)_c + \\ & \beta_3 I(BlackDefendant * WhiteProsecutor)_c + X_c + ScreeningDate_c + \epsilon_c, \quad (1) \end{aligned}$$

where *Conviction* is a binary variable equal to 1 if the defendant is convicted, and 0 otherwise. *BlackDefendant* equals 1 for Black defendants, and *WhiteProsecutor* equals 1 for white prosecutors. β_1 captures differences in conviction probability by defendant race, while β_2 captures differences by prosecutor race. The coefficient of interest, β_3 , on the interaction term, measures the effect of being assigned a cross-race prosecutor. Specifically, the interaction coefficient captures the sum of the leniency shown by white prosecutors, and by Black prosecutors. For simplicity, I refer to this as the cross-race effect.¹⁴ X_c includes case-level controls: defendant age, gender, number of arrest charges, total arrest counts, prior arrests, felony arrests, prior convictions, felony convictions, prior jail and prison sentences, and prior non-incarcerative sentences. I also include indicators for A, B, and U misdemeanors; drug, property, person, and other crimes; screening method (EAP, DAT, in-person, phone); arrest ZIP code; and prosecutor gender (i.e., the characteristics

¹³However, a null estimate does not imply racial equity.

¹⁴This paper focuses on the effect of cross-race prosecutors. However, I also estimate the effect of prosecutor race alone. Results are shown in Table A.6. Here I regress conviction on indicators for prosecutor race. I find that white prosecutors increase the probability of conviction by 1.75 percentage points (2.9% and not statistically significant) overall.

shown in Table 2). All specifications include fixed effects for *ScreeningDate*.

I cluster standard errors at the prosecutor level to account for within-prosecutor correlation. To adjust for multiple hypothesis testing, I compute false discovery rate (FDR) q -values following Anderson (2008), correcting for four crime subgroups (property, drug, person, other). These q -values serve as adjusted two-sided p -values and account for the increased likelihood of extreme estimates when testing multiple outcomes.

Additionally, I compute randomization inference p -values. Specifically, I randomly reassign defendant race (assuming 75% of defendants are Black) and re-estimate β_3 from Equation (1) in 10,000 replications. This process generates an empirical distribution under the null. The two-sided p -value is the share of simulated coefficients more extreme (in absolute value) than the observed estimate.

Intuitively, the difference-in-differences compares the conviction gap between Black and white defendants across Black and white prosecutors. The model allows Black defendants to be more or less likely to be convicted than white defendants and permits average conviction rates to differ across prosecutor race. The identifying assumption is that the Black–white conviction gap would be the same regardless of prosecutor race in the absence of cross-race effects. This assumption could fail if prosecutor race is correlated with case quality. For example, if Black prosecutors only accept strong cases for white defendants but accept all cases for Black defendants, observed effects may reflect selection rather than race pairing. I address this concern by leveraging the random assignment of prosecutors conditional on screening date and show empirically that prosecutor race is uncorrelated with observed defendant and case characteristics.

Finally, I examine whether estimated effects reflect defendant race or characteristics correlated with race. For example, if Black prosecutors have higher conviction rates for drug crimes, and white defendants are more likely to be charged with drug offenses, this could produce a cross-race effect unrelated to bias. While controlling for such interactions may “over-correct” depending on one’s definition of bias, I test robustness by interacting all case and defendant characteristics with prosecutor race. If estimates change, some of

the effect may reflect prosecutors' responses to case features correlated with race, rather than race itself.

4 Results

4.1 Conditional Exogeneity of Prosecutor Race

This section begins by establishing that prosecutor race is not correlated with confounding factors, conditional on screening date fixed effects. This expectation is grounded in the case assignment process at the New York County District Attorney's Office, which I substantiate with empirical evidence. Specifically, I regress defendant and case characteristics—determined prior to case assignment—on prosecutor race, clustering standard errors at the prosecutor level and including screening date fixed effects. While these characteristics are highly predictive of a defendant's case outcome (see column 2 of Table 2), they show no significant correlation with prosecutor race (see column 3). Among the 23 estimated coefficients, only one is significant at conventional levels, consistent with chance. Most effects are near zero; for example, Black prosecutors are just 0.5 percentage points more likely to be assigned Black defendants.

To further assess the random assignment of cases, I conducted additional robustness checks. First, regressing prosecutor race on all characteristics (with screening date fixed effects) yields a joint F-test p-value of 0.740. Results are similar when restricting to property crimes (0.458), testing gender balance (0.255)¹⁵, and using screening week fixed effects instead of day (0.899). Next, I performed 45,713 t-tests for every combination of case and defendant characteristics, omitting screening dates with no variation in prosecutor or defendant traits. After adjusting for multiple testing (Benjamini–Hochberg), only 1,101 (2.4%) are significant at the 10% level.

Finally, I test whether predicted conviction probabilities vary by prosecutor race. I estimate these probabilities for each defendant using all observable characteristics (from Table

¹⁵For gender balance, I regress all case and defendant characteristics on an indicator for female prosecutor (including screening date fixed effects) and report the joint F-test p-value.

2), including arrest ZIP fixed effects but excluding prosecutor race. As shown in Table 1 (Panels C and D), predicted conviction probabilities are nearly identical across prosecutor race. For White defendants: 51.4% with white prosecutors vs. 52.0% with Black prosecutors ($p = 0.8280$). For Black defendants: 62.6% vs. 62.2% ($p = 0.3322$). Results are similar for property crimes. Together, these tests—spanning balance regressions, joint F-tests, t-tests, and predicted conviction models—consistently support the claim that prosecutor race is conditionally random within screening dates.

5 Cross-Race Prosecutor Effects

5.1 Cross-Race Effects on Conviction

Next, I present results examining cross-race effects across crime types (Figure 1).¹⁶ For the full sample, the baseline coefficient on *BlackDefendant * WhiteProsecutor* is 0.021, significant at the 10 percent level—implying a 2.1 percentage point (3.5%) increase in conviction under cross-race prosecution. The estimate remains similar but loses conventional significance when adding controls for defendant and case characteristics (from Table 2, arrest ZIP code, and prosecutor gender), and changes little with prosecutor fixed effects. The most saturated model interacts all case characteristics with prosecutor race to address potential confounding—e.g., if Black defendants more often face drug charges and white prosecutors are tougher on drugs. The coefficient remains similar at 0.019 ($p < 0.10$). While these results point to potential cross-race effects, we interpret them cautiously and place more weight on the crime-type-specific analysis below.

Crime type classifications are based on the original coding by the administrative data providers and not constructed by the author. Different types of crimes are often handled uniquely based on their quality of evidence (Frederick and Stemen, 2012b; Ratledge et al., 1982). In general, property crimes tend to have less physical evidence than other types of crimes (Peterson et al., 2010), potentially allowing more room for prosecutorial discretion.

¹⁶See also Table A.2.

Earlier research also suggests racial disparities may differ by crime type (e.g., Albonetti, 1997; Mustard, 2000).

Results in Figure 1 and Table A.3 show strong cross-race effects specifically for property crimes. The baseline estimate of 0.048 indicates that cross-race prosecutor assignment increases conviction by about 5 percentage points (8 percent) for property crimes. This effect remains robust and significant at the 1-percent level across all specifications - with case controls, prosecutor fixed effects, and prosecutor-characteristic interactions. In contrast, coefficients for drug, person, and other offenses are close to zero across specifications. Using the combined sample of non-property crimes, I can rule out increases larger than 1.3 percent¹⁷, and these estimates are statistically different from the property crime effects at conventional levels.

The magnitude of cross-race effects also varies substantially by defendant history. Results by prior arrests and convictions (Table 4) show that the largest disparities are concentrated among defendants with no prior contact with the system. Among those with no prior convictions, being assigned a cross-race prosecutor increases the probability of conviction by nearly 7 percentage points (17% $p < .01$). For defendants with prior records, the estimated effects are close to zero and not statistically significant. These patterns suggest that prosecutorial discretion may have the greatest impact on first-time or low-contact defendants, for whom a single case outcome may be especially consequential.

To assess whether these effects reflect systemic disparities or are driven by a few outlier prosecutors, I estimate a random-effects model at the individual prosecutor level. Using the `mixed` command in Stata, I compute shrunken cross-race estimates for each prosecutor, controlling for screening date, defendant demographics, criminal history, case characteristics, and prosecutor gender (Table 2). I limit the sample to prosecutors with more than 25 property crime cases—136 in total, including 22 Black prosecutors—capturing 93 percent of the original sample.

¹⁷

$$\frac{-0.00568 + 0.012 \times 1.96}{0.588} = \frac{0.01788}{0.588} \approx 0.0304$$

Figure A.1 shows the distribution of shrunk cross-race effects by prosecutor and defendant race. Among white defendants, Black prosecutors are associated with slightly higher conviction probabilities. Among Black defendants, the distribution shifts more clearly under white prosecutors. These patterns suggest that the property crime results in Figure

Because I report results for multiple types of crimes, I also compute false discovery rate (FDR) adjusted q -values for the estimates presented in Figure 1 (see Table A.3), using the method proposed by Anderson (2008). The FDR q -values, which can be interpreted as adjusted p -values, remain significant at the 5-percent level or less for all property crime specifications. Additionally, I conduct randomization inference by randomly reassigning defendant race and estimating cross-race effects for 10,000 replications. The resulting p -values confirm significant effects for property crimes ($p=0.009$) but not for drug ($p=0.653$), person ($p=0.651$), or other crimes ($p=0.665$). The distribution of these estimates is reported in Figure A.2. These results indicate that the effects I find are unlikely to be due to chance. Combined, these findings demonstrate robust cross-race effects for property crimes only.¹⁸

5.2 Sentencing and Mechanism Results

Table 3 examines additional outcomes through which cross-race effects might operate: sentencing decisions and case processing mechanisms. For misdemeanors, convictions typically have greater long-term consequences than the formal punishments imposed. This is because even misdemeanor convictions create criminal records that can affect employment, housing, and other opportunities and punishments are relatively mild. Nevertheless, examining sentencing helps provide a complete picture of disparate treatment. For misdemeanor property crimes, the most common sentence is a conditional discharge with community service. When incarceration occurs, sentences are typically short - the median is 15 days, with only 10 percent exceeding 90 days. Among convicted defendants, 36 per-

¹⁸It is also natural to ask whether cross-race effects vary with prosecutor experience, as some evidence suggests prosecutors are harshest early in their careers (e.g., Wright and Levine, 2014). While I do not observe tenure directly, results by number of previously assigned cases (Figures A.3a and A.3b) show no clear pattern, suggesting that early-career prosecutors are not driving the results.

cent receive conditional release (primarily community service), 34 percent receive jail or prison time, 28 percent get time served, and 2 percent are fined.¹⁹

Panel A shows no significant cross-race effects on sentencing outcomes for the full sample. For property crimes (Panel B), I find no evidence that cross-race prosecutors affect conditional discharge, fines, or incarceration length. However, they increase the probability of any time by 1 percentage point (111%, $p < 0.1$). The results suggest that prosecutor-defendant pairings play a more influential role in determining conviction outcomes than in shaping post-conviction sentences.

To understand the mechanisms driving conviction effects, I examine five key prosecutorial decisions that can influence case outcomes. Pretrial detention, which prosecutors can recommend, often increases convictions by incentivizing plea deals (Dobbie et al., 2018; Heaton et al., 2017). Charge increases can pressure defendants by raising potential penalties. Prosecutors can also directly affect outcomes through declined prosecution, case dismissal, or adjournment in contemplation of dismissal (ACD). ACDs are particularly important as they offer a path to avoiding conviction - cases are suspended for 6-12 months and dismissed (with no conviction record) if the defendant avoids new arrests. This option is especially valuable for first-time offenders since it allows them to avoid creating a criminal record entirely.

Panels C and D present these results. For the full sample, none of the mechanisms show significant effects, though the point estimates suggest cross-race prosecutors may reduce dismissals and ACDs while increasing pretrial detention and charge severity. For property crimes, the primary mechanism appears to be ACDs - cross-race prosecutors reduce the probability of an ACD by 4.76 percentage points (20%, $p < 0.01$). These effects are concentrated among defendants with no prior criminal history (Table 4). The prominence of ACD effects makes sense given that prosecutors have substantial discretion over offering ACDs, unlike other mechanisms that often involve judges or speedy trial requirements. The magnitude suggests that differential access to these non-conviction outcomes accounts for a

¹⁹Because I categorize crimes based on their *initial* arrest charges there are some cases that begin as misdemeanors but are disposed as felonies. Therefore, some individuals arrested for misdemeanors may serve prison time.

substantial portion of the overall conviction effect, indicating that prosecutors may exhibit cross-race effects through their willingness to offer this alternative to conviction.

6 Discussion and Conclusion

I find strong evidence of cross-race effects for property crimes, but not for drug, person, or other offenses. While the effect is limited to one category, it is statistically meaningful and suggests that prosecutorial discretion may interact with race in certain contexts. The absence of effects in other categories is not necessarily surprising: some charges are highly standardized or constrained by mandatory practices, while others allow more discretion. The presence of effects in a large, discretionary category like property crimes is therefore both plausible and important.

One possible explanation involves the strength of evidence. Prosecutorial discretion may be greater when evidence is weaker, allowing more space for subjective judgment or bias. Strong physical evidence, common in drug and many person crimes, could limit discretion. In contrast, property crimes often lack physical evidence, making outcomes more sensitive to perceived credibility and culpability. While I cannot directly test this mechanism, empirical studies support its plausibility. Prosecutors themselves rank evidence strength as the most important factor when deciding whether to proceed with a case (Frederick and Stemen, 2012a), and physical evidence is collected in nearly all drug cases, in 22–83% of person crimes, and in only 9–17% of property crimes (Peterson et al., 2010). Additional studies support the idea that prosecutors rely heavily on evidence quality (Spohn and Spears, 1997; Ratledge et al., 1982; Spohn and Holleran, 2001), and that weak evidence may amplify bias against minority defendants (Smith and Levinson, 2011; Kutateladze et al., 2016).

Consistent with that interpretation, I find that cross-race prosecutors are less likely to offer adjournments in contemplation of dismissal (ACDs) in property cases. Because ACDs are a discretionary form of leniency that avoid conviction, this finding implies that differ-

ences in how discretion is exercised may help explain the observed conviction gap. The effects are also driven largely by defendants with no prior criminal record, reinforcing the idea that disparities emerge most clearly in marginal cases—those without strong signals of guilt or dangerousness.

Other mechanisms may also be relevant. Person crimes often involve victims, which may constrain prosecutorial behavior through institutional norms or emotional considerations. Drug offenses are typically supported by tangible evidence and processed through standardized plea procedures. Property crimes may occupy a middle ground: serious enough to be prosecuted, yet flexible enough to allow variation in approach. My person crime sample is small, so further work is needed to assess these patterns using richer case-level data, including information on victim presence, charge severity, and resolution paths.

These findings are policy-relevant given the scale and discretionary nature of property crimes. In 2016, over 7.9 million property crimes were reported in the U.S., and more than 1.4 million arrests were made in 2014 alone (FBI: UCR, 2016; Stevenson and Mayson, 2018b). Roughly one-quarter of jail inmates are incarcerated for such offenses (Sawyer and Wagner, 2019), making them a common entry point into the justice system. The pattern holds locally: property crimes are the most frequently prosecuted misdemeanor in New York County. Because these cases are both prevalent and flexibly handled, they offer a key setting where prosecutorial discretion can shape outcomes—and contribute to racial disparities. The estimated cross-race effect I document—a 5 percentage point increase in conviction—has meaningful implications. In fact, it could explain roughly 40 percent of the Black-white conviction gap in these cases.²⁰ Even modest disparities can produce large aggregate effects in high-volume contexts, underscoring how race-matching dynamics between prosecutors

²⁰0.86 is the probability of being assigned a white prosecutor for property crimes, 0.0535 is my estimate of cross-race effects for property crimes, and 0.07 is conviction rate disparity from Table A.6. Referring to the model I present in equation (1), I estimate that the difference in conviction rates between Black and white defendants is $[(\beta_0 + \beta_1) \cdot \Pr(\text{Black Prosecutor}|\text{Black Defendant}) + (\beta_0 + \beta_1 + \beta_2 + \beta_3) \cdot \Pr(\text{White Prosecutor}|\text{Black Defendant})] - [\beta_0 \cdot \Pr(\text{Black Prosecutor}|\text{White Defendant}) + (\beta_0 + \beta_2) \cdot \Pr(\text{White Prosecutor}|\text{White Defendant})]$. Because cases are randomly assigned, $\Pr(\text{Black Prosecutor}|\text{White Defendant}) = \Pr(\text{Black Prosecutor}|\text{Black Defendant})$, and similarly $\Pr(\text{White Prosecutor}|\text{White Defendant}) = \Pr(\text{White Prosecutor}|\text{Black Defendant})$. This simplifies to $\beta_1 + \Pr(\text{White Prosecutor})\beta_3$. Therefore, $\frac{\beta_3 \cdot \Pr(\text{White Prosecutor})}{\beta_1 + \beta_3 \cdot \Pr(\text{White Prosecutor})}$ represents the share of the Black-white conviction gap explained by cross-race effects.

and defendants may influence legal outcomes in practice.

Moreover, these findings likely reflect a lower bound on disparities. The Manhattan District Attorney's Office has implemented progressive reforms, including declining to prosecute most low-level infractions and offering treatment alternatives for drug charges. It also employs a chief diversity officer and maintains formal equity initiatives (Cyrus Vance For District Attorney, 2017; Manhattan District Attorney, 2018a). If cross-race effects persist in this relatively reform-oriented setting, they may be even more pronounced elsewhere. These results are also not driven by a small set of outlier prosecutors or very early-career attorneys. The effects appear to be diffuse across prosecutors, and given that most begin their careers with misdemeanors, the potential for these disparities to extend into felony prosecution cannot be ruled out.

Although I cannot fully explain why effects are concentrated in property crimes, the findings offer clear evidence that prosecutor-defendant race pairings can influence legal outcomes in discretionary settings. Using as-good-as-random assignment of misdemeanor cases in New York County, I show that cross-race pairings increase conviction rates for property offenses by approximately 5 percentage points—an 8% increase—driven largely by first-time defendants. These findings highlight how discretionary power can produce racial disparities and suggest the importance of identifying other prosecutorial contexts where bias may emerge.

References

- Agan, A. Y. (2024). Racial disparities in the criminal legal system: Shadows of doubt and beyond. *Journal of Economic Literature* 62(2), 761–806.
- Agan, A. Y., J. L. Doleac, and A. Harvey (2022). Misdemeanor prosecution. *forthcoming Quarterly Journal of Economics*.
- Agan, A. Y., A. Garin, D. K. Koustas, A. Mas, and C. Yang (2024). Can you erase the mark of a criminal record? labor market impacts of criminal record remediation.
- Albonetti, C. A. (1997). Sentencing under the Federal Sentencing Guidelines: Effects of Defendant Characteristics, Guilty Pleas, and Departures on Sentence Outcomes for Drug Offenses, 1991–1992. *Law & Society Review* 31(4), 789.
- Anbarci, N. and J. Lee (2014). Detecting racial bias in speed discounting: Evidence from speeding tickets in Boston. *International Review of Law and Economics* 38, 11–24.
- Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association* 103(484), 1481–1495.
- Antonovics, K. and B. G. Knight (2009). A New Look at Racial Profiling: Evidence from the Boston Police Department. *Review of Economics and Statistics* 91(1), 163–177.
- Anwar, S. and H. Fang (2006). An alternative test of racial prejudice in motor vehicle searches: Theory and evidence. *American Economic Review* 96(1), 127–151.
- Arora, A. (2019). Too tough on crime? The impact of prosecutor politics on incarceration. *Working Paper*.
- Chauhan, P., A. Fera, M. Welsh, E. Balazon, and E. Misshula (2014). Trends in misdemeanor arrests. *Report Presented to the Citizens Crime Commission*.
- City of New York (2015). Twenty-Five Largest U.S. Cities by Population: Elected Prosecutor Salaries. https://www1.nyc.gov/assets/quadrennial/downloads/pdf/tables/elected_prosecutor_data.pdf.
- Cyrus Vance For District Attorney (2017). Criminal justice reform. <http://www.cyvanceforda.com/page/criminal-justice-reform>.
- Didwania, S. H. (2022). Gender favoritism among criminal prosecutors. *The Journal of Law and Economics* 65(1), 77–104.
- Dobbie, W., J. Goldin, and C. S. Yang (2018). The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges†. *American Economic Review* 108(2), 201–240.
- FBI: UCR (2016). Table 1: Crime in the United States. <https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016/tables/table-1>.
- Finlay, K. (2008). Effect of employer access to criminal history data on the labor market outcomes of ex-offenders and non-offenders.

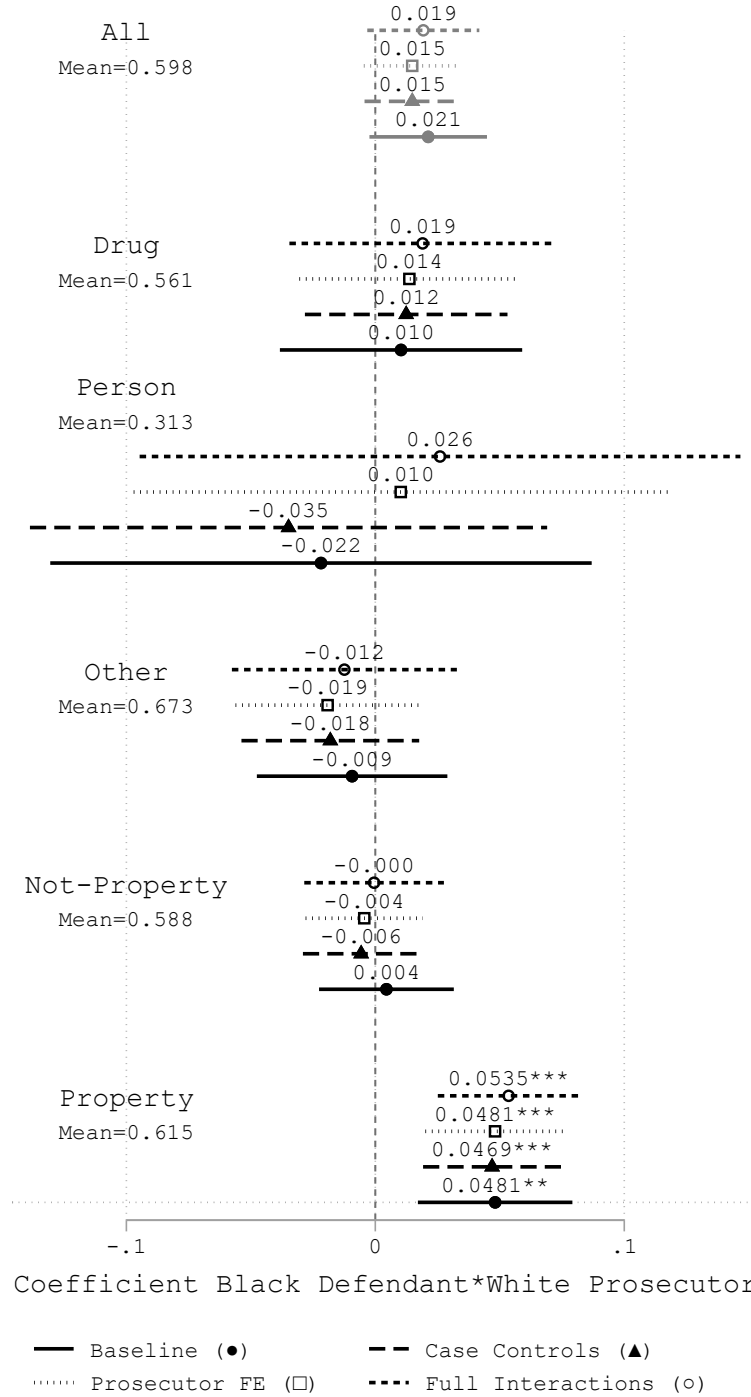
- Foster v. Chatman (2016). Volume 136. Supreme Court.
- Frederick, B. and D. Stemen (2012a). The anatomy of discretion: An analysis of prosecutorial decision making. *Final Report submitted to the National Institute of Justice*.
- Frederick, B. and D. Stemen (2012b). Anatomy of discretion: An analysis of prosecutorial decision making. *Summary Report to the National Institute of Justice* (December).
- Harrington, E. and H. Shaffer (2025). Brokers of bias in the criminal justice system: Do prosecutors compound or attenuate racial disparities introduced by police? *The Review of Economics and Statistics*.
- Heaton, P., S. Mayson, and M. Stevenson (2017). The downstream consequences of misdemeanor pretrial detention. *Stan. L. Rev.* 69.
- Jordan, A. (2024). Racial patterns in approval of felony charges. *Available at SSRN 4093128*.
- Krumholz, S. (2019). The Effect of District Attorneys on Local Criminal Justice Outcomes. *Working Paper*.
- Kutateladze, B. (2017). Case Processing in the New York County District Attorney’s Office; 2010-2011. <https://doi.org/10.3886/ICPSR34681.v1>.
- Kutateladze, B. L. and N. R. Andiloro (2014). Prosecution and racial justice in New York County—Technical report. *New York: Vera Institute of Justice*.
- Kutateladze, B. L., N. R. Andiloro, and B. D. Johnson (2016). Opening Pandora’s box: How does defendant race influence plea bargaining? *Justice Quarterly* 33(3), 398–426.
- Kutateladze, B. L., V. Lynn, and E. Liang (2012). Do Race and Ethnicity Matter in Prosecution? Technical Report June.
- Luna, E. and M. L. Wade (2015). *The Prosecutor in Transnational Perspective*. Oxford University Press.
- Manhattan District Attorney (2018a). DA Vance and Vera Institute of Justice Announce The Release of Study Examining Racial Disparities in The Manhattan Criminal Justice System. <https://www.manhattanda.org/da-vance-and-vera-/institute-justice-announce-release-study-examining-racial-disparitie/>.
- Manhattan District Attorney (2018b). Manhattan district attorney website. <https://www.manhattanda.org/>.
- Mueller-Smith, M. and K. T. Schnepel (2021). Diversion in the criminal justice system. *The Review of Economic Studies* 88(2), 883–936.
- Mustard, D. B. (2000). Racial, Ethnic and Gender Disparities in Sentencing: Evidence from the US Federal Courts.
- New York State (2018). Chapter 1: Criminal Justice System for Adults in NYS.
- Pager, D. (2003). The Mark of a Criminal Record. *American Journal of Sociology* 108(5), 937–975.

- Parsons, C. A., J. Sulaeman, M. C. Yates, and D. S. Hamermesh (2011). Strike Three: Discrimination, Incentives, and Evaluation. *American Economic Review* 101(4), 1410–1435.
- Peterson, J., I. Sommers, D. Baskin, and D. Johnson (2010). The role and impact of forensic evidence in the criminal justice process. *National Institute of Justice*, 1–151.
- Pew Research Center (2019). Race in America. <https://www.pewsocialtrends.org/2019/04/09/race-in-america-2019/>.
- Pfaff, J. (2017). *Locked in: The true causes of mass incarceration—and how to achieve real reform*. Basic Books.
- Price, J. and J. Wolfers (2010). Racial discrimination among NBA referees. *Quarterly Journal of Economics* 125(4), 1859–1887.
- Rasmussen Reports (2014). Rasmussen Reports. http://www.rasmussenreports.com/public_content/archive/mood_of_america_archive/supreme_court_ratings/43_say_u_s_justice_system_unfair_to_most_americans.
- Ratledge, E. C., S. H. Turner, J. E. Jacoby, and L. R. Mellon (1982). Prosecutorial decision-making: A national study.
- Rehavi, M. M. and S. B. Starr (2014). Racial Disparity in Federal Criminal Sentences. *Journal of Political Economy* 122(6), 1320–1354.
- Sawyer, W. and P. Wagner (2019). Prison Policy Initiative Mass Incarceration: The Whole Pie 2019. <https://www.prisonpolicy.org/reports/pie2019.html>.
- Shannon, S. K., C. Uggen, J. Schnittker, M. Thompson, S. Wakefield, and M. Massoglia (2017). The Growth, Scope, and Spatial Distribution of People With Felony Records in the United States, 1948–2010. *Demography* 54(5), 1795–1818.
- Sklansky, D. A. (2018). The problems with prosecutors. *Annual Review of Criminology* 1, 451–469.
- Smith, R. J. and J. D. Levinson (2011). The impact of implicit racial bias on the exercise of prosecutorial discretion. *Seattle UL Rev.* 35, 795.
- Spohn, C. and D. Holleran (2001). Prosecuting sexual assault: A comparison of charging decisions in sexual assault cases involving strangers, acquaintances, and intimate partners. *Justice Quarterly* 18(3), 651–688.
- Spohn, C. and J. Spears (1997). The effect of offender and victim characteristics on sexual assault case processing decisions. *Justice Quarterly* 13(4), 649–679.
- State v. Monday (2011). Number No. 82736-2. Washington Supreme Court.
- Stevenson, M. and S. Mayson (2018a). The scale of misdemeanor justice. *Boston University Law Review* 98, 731–793.
- Stevenson, M. and S. Mayson (2018b). The scale of misdemeanor justice. *BUL Rev.* 98, 731.
- Stith, K. (2008). The Arc of the Pendulum: Judges, Prosecutors, and the Exercise of

- Discretion. *Yale Law Journal* 117(7), 1420–1497.
- Tuttle, C. (2019). Racial Discrimination in Federal Sentencing: Evidence from Drug Mandatory Minimums. *Working Paper*.
- West, J. (2018). Racial Bias in Police Investigations. *Working Paper*.
- Wright, R. F. and K. L. Levine (2014). The cure for young prosecutors' syndrome. *Ariz. L. Rev.* 56, 1065.
- Yang, C. S. (2016). Resource constraints and the criminal justice system: Evidence from judicial vacancies. *American Economic Journal: Economic Policy* 8(4), 289–332.

7 Tables and Figures

Figure 1: Estimates of Cross-Race Effects for Defendant Conviction



Notes: This figure plots interaction coefficients (β_3) between defendant race (Black) and prosecutor race (White) from the regression: $Conviction_c = \beta_0 + \beta_1 I(Black\ Defendant)_c + \beta_2 I(White\ Prosecutor)_c + \beta_3 I(Black\ Defendant * White\ Prosecutor)_c + \epsilon_c$. Estimates are shown by crime type. Specifications vary by included controls: baseline (race only), case controls (defendant and case characteristics see Table 2), prosecutor fixed effects, and full interactions between prosecutor race and controls. Standard errors clustered by prosecutor.

Table 1: Predicted and Actual Conviction by Prosecutor and Defendant Race

Panel A: All Crimes				
Actual Conviction Rates				
	Black Pros. Mean	White Pros. Mean		
White Defendants	0.4859	0.5045		
Black Defendants	0.5940	0.6354		
Panel B: Property Crimes				
Actual Conviction Rates				
	Black Pros. Mean	White Pros. Mean		
White Defendants	0.5016	0.5021		
Black Defendants	0.6059	0.6546		
Panel C: All Crimes				
Predicted Conviction Rates				
	Black Pros. Pred. Mean	White Pros. Pred. Mean	Difference (W-B)	p-value
White Defendants	0.5159	0.5149	-0.0010	0.8280
Black Defendants	0.6211	0.6266	0.0055	0.3311
Panel D: Property Crimes				
Predicted Conviction Rates				
	Black Pros. Pred. Mean	White Pros. Pred. Mean	Difference (W-B)	p-value
White Defendants	0.5017	0.5052	0.0035	0.6142
Black Defendants	0.6288	0.6344	0.0056	0.4537

Notes: This table presents conviction rates by prosecutor and defendant race for misdemeanor cases. Panels A and B show raw conviction rates. Panels C and D show predicted conviction rates after controlling for observable case characteristics and residualizing screening date fixed effects. The difference column shows the gap between White and Black prosecutors, with p-values from regressions clustered at the prosecutor level.

Table 2: Correlation Between Case Characteristics and Prosecutor Race

Variable	Mean (Std. Dev.)	β Conviction	β Black Prosecutor
Black Defendant	0.754 (0.431)	0.095*** (0.004)	0.005 (0.006)
Defendant Age	34.573 (12.896)	6.864*** (0.125)	0.009 (0.209)
Defendant Male	0.813 (0.390)	0.090*** (0.004)	-0.004 (0.006)
Prior Arrests	4.218 (9.732)	4.544*** (0.094)	-0.073 (0.182)
Prior Felony Arrests	0.820 (2.057)	0.763*** (0.020)	0.001 (0.037)
Prior Convictions	4.452 (10.348)	5.478*** (0.080)	-0.150 (0.189)
Prior Felony Convictions	0.221 (0.593)	0.195*** (0.004)	0.001 (0.007)
Prior Jail Sentences	2.014 (5.922)	2.736*** (0.043)	-0.080 (0.094)
Prior Incarcerations	0.142 (0.484)	0.139*** (0.003)	0.001 (0.005)
Prior Non-Incaracerative Sentences	2.277 (5.129)	2.593*** (0.041)	-0.070 (0.099)
Number of Charges	1.686 (0.792)	0.167*** (0.007)	-0.027* (0.014)
Total Arrest Counts	1.754 (1.370)	0.200*** (0.010)	-0.020 (0.016)
A Misdemeanor	0.626 (0.484)	-0.006 (0.006)	-0.006 (0.007)
B Misdemeanor	0.219 (0.414)	-0.110*** (0.006)	0.006 (0.007)
U Misdemeanor	0.155 (0.362)	0.116*** (0.003)	-0.001 (0.005)
Drug Crime	0.239 (0.426)	-0.032*** (0.006)	0.006 (0.011)
Property Crime	0.394 (0.489)	0.011** (0.005)	0.006 (0.015)
Person Crime	0.069 (0.254)	-0.079*** (0.004)	-0.005 (0.007)
Other Crimes	0.298 (0.457)	0.100*** (0.004)	-0.007 (0.009)
Screen EAP	0.175 (0.380)	0.076*** (0.004)	0.004 (0.016)
Screen DAT	0.294 (0.456)	-0.155*** (0.005)	0.008 (0.019)
Screen In Person	0.030 (0.172)	-0.017*** (0.002)	-0.003 (0.003)
Screen Phone	0.500 (0.500)	0.095*** (0.006)	-0.009 (0.026)
<i>F-test Statistics:</i>			
Joint F-test p-value: 0.740		Joint F-test (Property): 0.458	
Joint F-test (Week FE): 0.899		Joint F-test (Gender): 0.255	
T-tests: 1101/45713 Failures			

Standard errors in parentheses for coefficients, standard deviations for means

Notes: This table examines the relationship between case/defendant characteristics and prosecutor assignment. Column (1) shows means and standard deviations. Columns (2) and (3) report regression coefficients where each characteristic is regressed on *Conviction* and *Black Prosecutor* respectively, with screening date fixed effects. F-tests at bottom assess joint significance of characteristics in predicting prosecutor race for: all cases, property crimes only, using week fixed effects, and testing prosecutor gender balance. Standard errors clustered by prosecutor.

Table 3: Sentencing and Mechanism Results

	Conditional Discharge	Any Fine	Time Served	Jail or Prison Sentence	IHS Incarceration Length
Panel A: Sentencing Outcomes All Crimes					
Black Def.* White Pros.	0.0127 (0.00973)	0.00215 (0.00616)	0.00723 (0.00756)	-0.00277 (0.00783)	0.00545 (0.0330)
Observations	75331	75331	75331	75331	73353
Outcome Mean	0.187	0.101	0.151	0.160	0.606
	Conditional Discharge	Any Fine	Time Served	Jail or Prison Sentence	IHS Incarceration Length
Panel B: Sentencing Outcomes Property Crimes					
Black Def.* White Pros.	0.0225 (0.0169)	0.0130* (0.00706)	0.0113 (0.0133)	-0.00469 (0.0140)	-0.0130 (0.0553)
Observations	29773	29773	29773	29773	28993
Outcome Mean	0.221	0.0117	0.174	0.212	0.795
	Pretrial Detention	Charges Increased	Declined Prosecution	Case Dismissed	Adjournment in Contemplation of Dismissal
Panel C: Mechanism Outcomes All Crimes					
Black Def.* White Pros.	0.00579 (0.00740)	0.00349 (0.00587)	0.000363 (0.00138)	-0.00612 (0.00873)	-0.0100 (0.0126)
Observations	75666	75666	75666	75666	75666
Outcome Mean	0.0935	0.0523	0.00333	0.201	0.196
	Pretrial Detention	Charges Increased	Declined Prosecution	Case Dismissed	Adjournment in Contemplation of Dismissal
Panel D: Mechanism Outcomes Property Crimes					
Black Def.* White Pros.	0.00922 (0.0130)	0.00691 (0.00889)	0.00108 (0.00349)	0.000318 (0.0108)	-0.0476** (0.0192)
Observations	29815	29815	29815	29815	29815
Outcome Mean	0.0894	0.0738	0.00845	0.138	0.238
Pros. & Def. Race Indicators	Y	Y	Y	Y	Y
Case-Level Controls	Y	Y	Y	Y	Y
Interactions	Y	Y	Y	Y	Y
Prosecutor FE	Y	Y	Y	Y	Y

Notes: This table examines the effect of racial matches between prosecutors and defendants on case outcomes using the specification: $Outcome_c = \beta_0 + \beta_1 I(\text{Black Defendant})_c + \beta_2 I(\text{White Prosecutor})_c + \beta_3 I(\text{Black Defendant} \times \text{White Prosecutor})_c + X_c + \text{Screening Date}_c + \epsilon_c$. Each column displays the estimate of β_3 , representing the interaction between defendant race and prosecutor race. Panels A and B report sentencing outcomes for all crimes and property crimes, with average incarceration length of 208 days across the sample. Panels C and D show case processing mechanisms, including Adjournment in Contemplation of Dismissal (ACD) – a case resolution where prosecution is temporarily suspended, with potential full dismissal if the defendant meets specified conditions (e.g., no new arrests for 6-12 months). All specifications include screening date fixed effects, case-level controls, and prosecutor-race interaction terms, with standard errors clustered at the prosecutor level.

Table 4: Estimates of Cross-Race Effects in Defendant Conviction and ACDs by Criminal History

	(1) No Prior Arrests	(2) No Prior Convictions	(3) Prior Arrests	(4) Prior Convictions
Panel A: All Crimes				
<i>Outcome: Conviction</i>				
Black Def.*White Prosecutor	0.0160 (0.0122)	0.0139 (0.0119)	0.0141 (0.0190)	0.0127 (0.0193)
Observations	37859	41589	37807	34077
Outcome Mean	0.458	0.432	0.739	0.801
Panel B: Property Crimes				
<i>Outcome: Conviction</i>				
Black Def.*White Prosecutor	0.0768*** (0.0252)	0.0670*** (0.0254)	0.0109 (0.0232)	0.0104 (0.0219)
Observations	14222	15717	15593	14098
Outcome Mean	0.404	0.391	0.807	0.864
Panel C: All Crimes				
<i>Outcome: Adjournment in Contemplation of Dismissal</i>				
Black Def.*White Prosecutor	-0.0110 (0.0137)	-0.00770 (0.0131)	0.00822 (0.0155)	-0.000464 (0.0120)
Observations	37859	41589	37807	34077
Outcome Mean	0.312	0.324	0.0801	0.0395
Panel D: Property Crimes				
<i>Outcome: Adjournment in Contemplation of Dismissal</i>				
Black Def.*White Prosecutor	-0.0868*** (0.0317)	-0.0735*** (0.0280)	0.00620 (0.0186)	-0.00777 (0.0141)
Observations	14222	15717	15593	14098
Outcome Mean	0.419	0.424	0.0734	0.0313
Prosecutor & Def. Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	Y	Y	Y	Y
Prosecutor FE	Y	Y	Y	Y
Interactions	Y	Y	Y	Y

Standard errors in parentheses

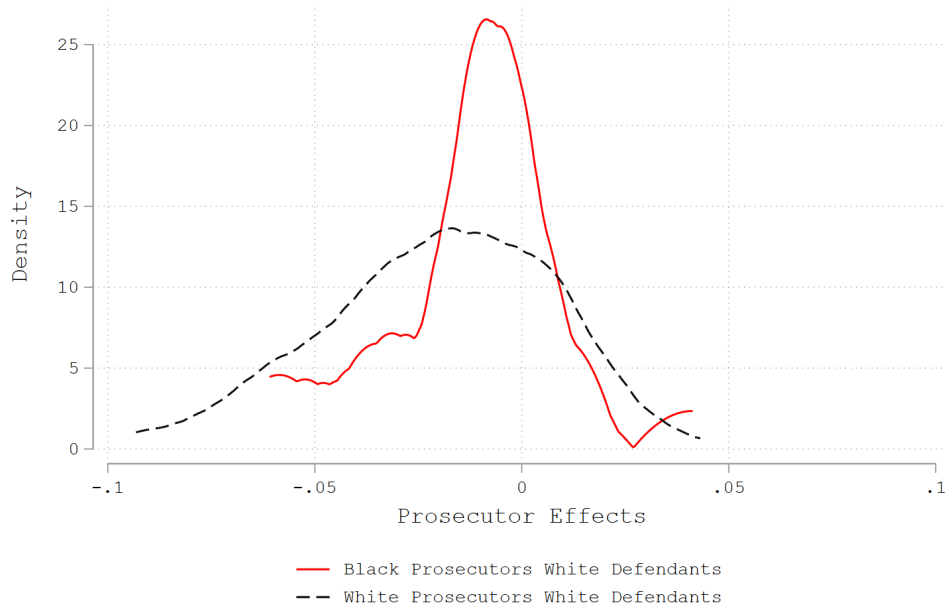
* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports interaction coefficient (β_3) between defendant race (Black) and prosecutor race (White) from the regression: $Outcome_c = \beta_0 + \beta_1 I(Black\ Defendant)_c + \beta_2 I(White\ Prosecutor)_c + \beta_3 I(Black\ Defendant \times White\ Prosecutor)_c + \epsilon_c$. All models include screening date fixed effects, case controls, prosecutor fixed effects and interactions. An Adjournment in Contemplation of Dismissal (ACD) is a resolution in which the case is suspended and dismissed after 6–12 months if the defendant avoids new arrests; it is not a conviction or admission of guilt. Standard errors are clustered at the prosecutor level.

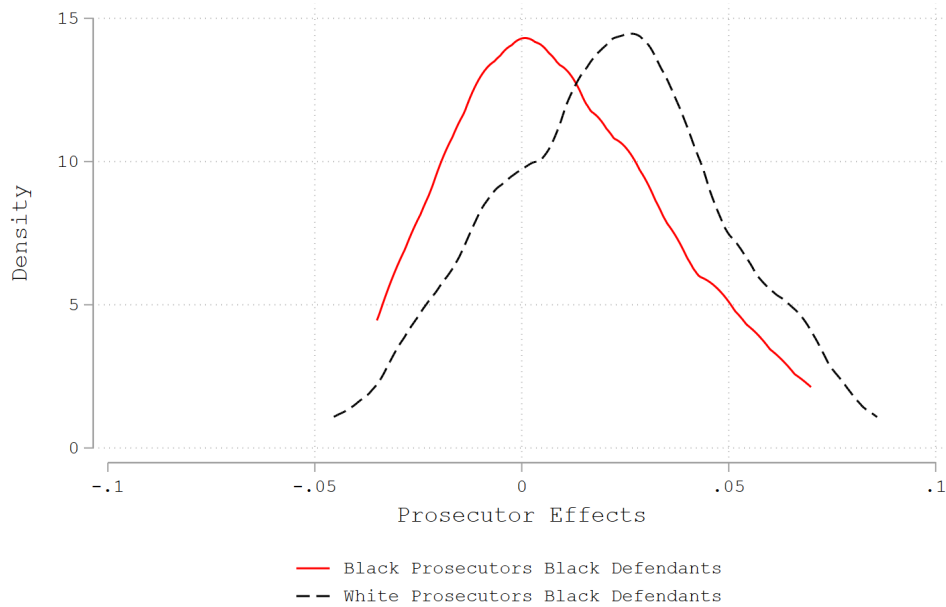
A Online Appendix

A.1 Tables and Figures

Figure A.1: Random Effects by Prosecutor



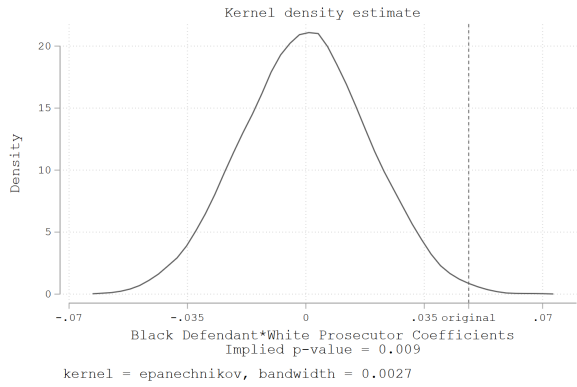
(a) White Defendants



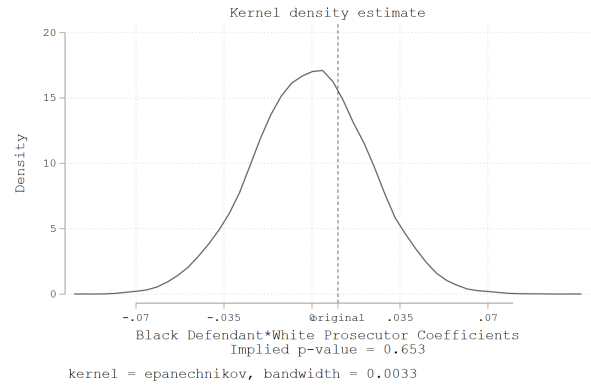
(b) Black Defendants

Notes: These figures display the distribution of prosecutor-specific effects on conviction rates, stratified by prosecutor and defendant race. The analysis is restricted to property crime cases, and prosecutors with 25 or fewer cases are excluded. Prosecutor effects are estimated using a two-stage procedure. First, conviction outcomes are residualized by removing screening date fixed effects. Second, the residuals are used in a linear mixed-effects model estimated via the `mixed` command in Stata, with random intercepts at the prosecutor–defendant race group level and controls for defendant characteristics and arrest zip code. Empirical Bayes shrinkage is applied to generate the estimated random effects. The first panel shows estimated effects for White defendants, comparing Black and White prosecutors. The second panel shows effects for Black defendants. The combined Kolmogorov–Smirnov p -value is 0.065 for White defendants and 0.093 for Black defendants.

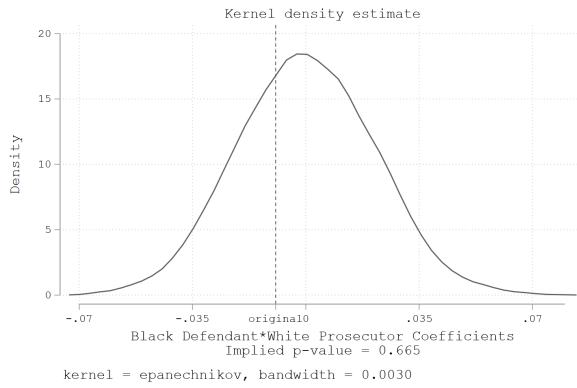
Figure A.2: Permutation Results for Cross-Race Effects by Crime Type



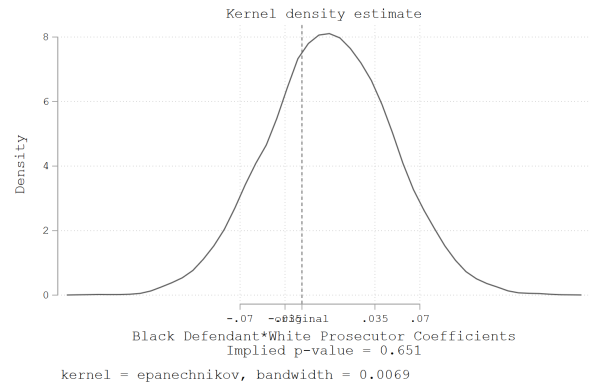
(a) Property Crimes



(b) Drug Crimes



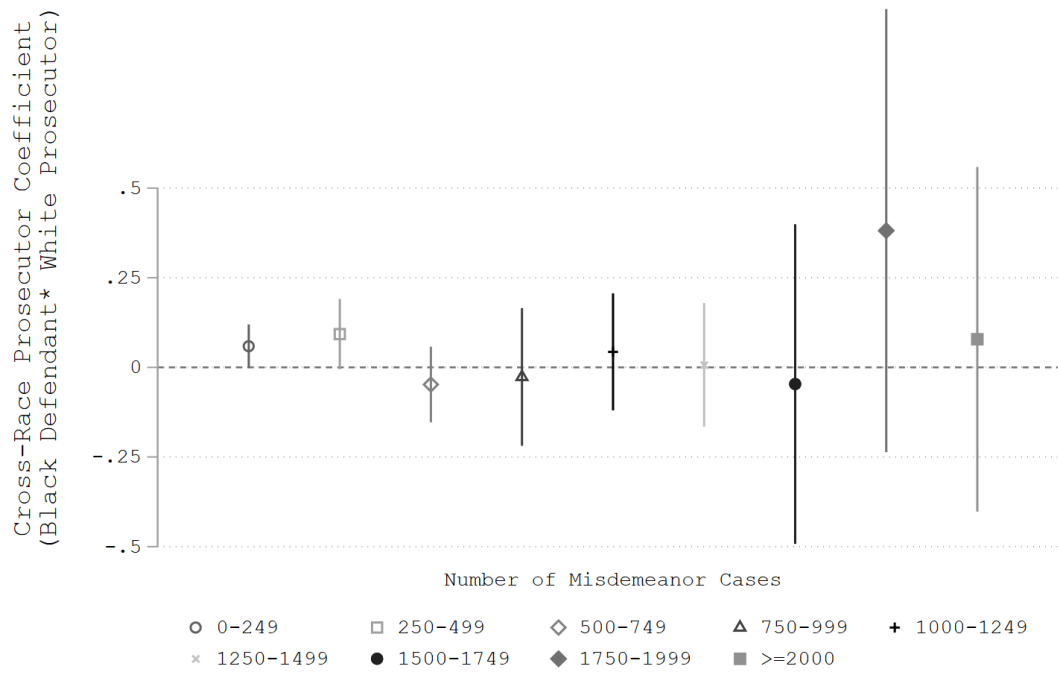
(c) Other Crimes



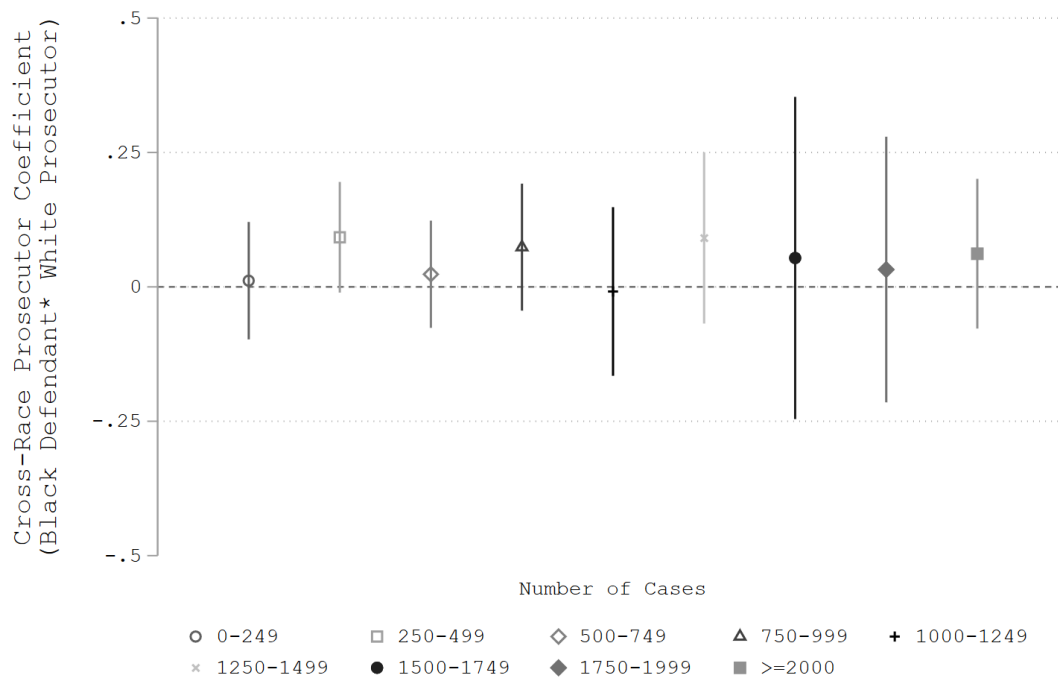
(d) Person Crimes

Notes: This figure presents the results from a permutation exercise where I randomly reassigned defendant race and estimated the effect of an cross-race prosecutor (β_3 from Equation (1)) for 10,000 replications. The distribution of β_3 coefficients for each crime type are presented. The dashed line denotes the original estimate from column 1 of Table A.3.

Figure A.3: Cross-Race Effects by Number of Cases (Property Crimes)



(a) Effects by Number of Misdemeanor Cases Assigned



(b) Effects by Number of Cases Assigned

Notes: These figures show estimates of cross-race effects for property crimes by the number of cases previously handled by prosecutors. Each coefficient is from a separate regression. Standard errors are clustered at the prosecutor level.

Table A.1: Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Black Defendants	White Defendants	Drug Crimes	Property Crimes	Person Crimes	Other Crimes
Panel A: Outcomes							
Decline to Prosecute	0.00333	0.00374	0.00209	0	0.00845	0	0
Charges Increased	0.0523	0.0547	0.0450	0.0220	0.0738	0.0885	0.0397
Pretrial Detention	0.0935	0.108	0.0505	0.0892	0.0894	0.218	0.0734
Case Dismissed	0.201	0.203	0.195	0.180	0.138	0.587	0.213
ACD	0.196	0.162	0.299	0.260	0.238	0.0962	0.112
Conviction	0.598	0.630	0.502	0.561	0.615	0.313	0.673
Panel B: Case Characteristics							
Black Defendant	0.754	1	0	0.753	0.773	0.728	0.734
Defendant Age	34.57 (12.90)	34.77 (13.06)	33.97 (12.35)	35.24 (12.78)	33.47 (13.37)	33.38 (12.40)	35.78 (12.31)
Defendant Male	0.813	0.817	0.801	0.864	0.750	0.775	0.864
Any Prior Arrests	0.500	0.581	0.250	0.594	0.523	0.420	0.412
Any Prior Convictions	0.450	0.528	0.214	0.530	0.473	0.354	0.379
Prior Arrests	4.218 (9.732)	5.046 (10.54)	1.685 (5.994)	5.543 (11.17)	4.658 (10.15)	2.201 (5.408)	3.045 (8.417)
Prior Convictions	4.452 (10.35)	5.334 (11.22)	1.752 (6.324)	5.869 (11.88)	5.085 (10.71)	1.858 (5.228)	3.082 (9.087)
Screen EAP Online	0.175	0.190	0.132	0.127	0.318	0.0130	0.0639
Screen Dat	0.294	0.261	0.396	0.347	0.300	0.185	0.270
Screen In Person	0.0304	0.0309	0.0288	0.0185	0.0149	0.120	0.0396
Screen Telephone	0.500	0.518	0.443	0.508	0.367	0.682	0.627
Black Prosecutor	0.135	0.136	0.133	0.139	0.134	0.132	0.133
White Prosecutor	0.865	0.864	0.867	0.861	0.866	0.868	0.867
Female Prosecutor	0.592	0.592	0.593	0.604	0.591	0.600	0.584
Observations	75666	57028	18638	18061	29815	5231	22559
mean coefficients; sd in parentheses							

Table A.2: Estimates of Cross-Race Effects for Defendant Conviction - Full Sample

	(1)	(2)	(3)	(4)
Outcome: Conviction				
Black Defendant*White Prosecutor	0.0213* (0.0121)	0.0148 (0.00980)	0.0148 (0.00993)	0.0193* (0.0115)
Observations	75666	75666	75666	75666
Outcome Mean	0.598	0.598	0.598	0.598
Prosecutor and Defendant Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	-	Y	Y	Y
Prosecutor FE	-	-	Y	Y
Interactions	-	-	-	Y

Standard errors in parentheses

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

Notes: This table reports interaction coefficients (β_3) between defendant race (Black) and prosecutor race (White) from the regression: $Conviction_c = \beta_0 + \beta_1 I(Black\ Defendant)_c + \beta_2 I(White\ Prosecutor)_c + \beta_3 I(Black\ Defendant \times White\ Prosecutor)_c + \epsilon_c$. All models include screening date fixed effects. Specifications vary by included controls: baseline (race only); case controls (crime type—drug, person, property—plus defendant demographics, criminal history, arrest characteristics, arrest ZIP code, and prosecutor gender); prosecutor fixed effects; and full interactions between prosecutor race and controls. Standard errors are clustered at the prosecutor level. This table also matches Figure 1.

Table A.3: Estimates of Cross-Race Effects in Defendant Conviction by Crime Type

	(1)	(2)	(3)	(4)
Panel A: Drug Crimes				
<i>Outcome: Guilty</i>				
Black Defendant*White Prosecutor	0.0103 (0.0249)	0.0124 (0.0208)	0.0137 (0.0226)	0.0190 (0.0266)
Observations	18061	18061	18061	18061
Outcome Mean	0.561	0.561	0.561	0.561
FDR q-value	0.747	0.635	0.854	0.809
Panel B: Person Crimes				
<i>Outcome: Guilty</i>				
Black Defendant*White Prosecutor	-0.0218 (0.0564)	-0.0349 (0.0539)	0.0102 (0.0555)	0.0260 (0.0567)
Observations	5231	5231	5231	5231
Outcome Mean	0.313	0.313	0.313	0.313
FDR q-value	0.747	0.635	0.854	0.809
Panel C: Other Crimes				
<i>Outcome: Guilty</i>				
Black Defendant*White Prosecutor	-0.00932 (0.0196)	-0.0181 (0.0183)	-0.0192 (0.0189)	-0.0124 (0.0226)
Observations	22559	22559	22559	22559
Outcome Mean	0.673	0.673	0.673	0.673
FDR q-value	0.747	0.635	0.781	0.809
Panel D: Drug & Person & Other Crimes				
<i>Outcome: Guilty</i>				
Black Defendant*White Prosecutor	0.00447 (0.0138)	-0.00568 (0.0120)	-0.00444 (0.0120)	-0.000492 (0.0141)
Observations	45851	45851	45851	45851
Outcome Mean	0.588	0.588	0.588	0.588
FDR q-value	0.747	0.635	0.854	0.973
Panel E: Property Crimes				
<i>Outcome: Guilty</i>				
Black Defendant*White Prosecutor	0.0481*** (0.0159)	0.0469*** (0.0142)	0.0481*** (0.0143)	0.0535*** (0.0143)
Observations	29815	29815	29815	29815
Outcome Mean	0.615	0.615	0.615	0.615
FDR q-value	0.013	0.006	0.005	0.002
Prosecutor and Defendant Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	-	Y	Y	Y
Prosecutor FE	-	-	Y	Y
Interactions	-	-	-	Y

Standard errors in parentheses

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

This table reports interaction coefficients (β_3) between defendant race (Black) and prosecutor race (White) from the regression: $Conviction_c = \beta_0 + \beta_1 I(Black\ Defendant)_c + \beta_2 I(White\ Prosecutor)_c + \beta_3 I(Black\ Defendant \times White\ Prosecutor)_c + \epsilon_c$. All models include screening date fixed effects. Specifications vary by included controls: baseline (race only); case controls (see Table 2); prosecutor fixed effects; and full interactions between prosecutor race and controls. Standard errors are clustered at the prosecutor level. FDR q -values adjust for testing across five crime types using Anderson (2008) and are interpreted as two-sided p -values. This table also matches Figure 1.

Table A.4: Cross-Race Conviction Effects by Crime Type (Minority Defendants & Prosecutors)

	(1)	(2)	(3)	(4)
Panel A: Drug Crimes				
<i>Outcome: Convicted</i>				
Minority Defendant*White Prosecutor	-0.0117 (0.0219)	0.00115 (0.0177)	-0.00220 (0.0184)	-0.00704 (0.0198)
Observations	33493	33493	33493	33493
Outcome Mean	0.528	0.528	0.528	0.528
Panel B: Person Crimes				
<i>Outcome: Convicted</i>				
Minority Defendant*White Prosecutor	-0.0232 (0.0317)	-0.0198 (0.0318)	-0.00792 (0.0326)	0.00653 (0.0322)
Observations	10378	10378	10378	10378
Outcome Mean	0.294	0.294	0.294	0.294
Panel C: Other Crimes				
<i>Outcome: Convicted</i>				
Minority Defendant*White Prosecutor	-0.00564 (0.0143)	-0.00856 (0.0138)	-0.0102 (0.0139)	-0.0104 (0.0151)
Observations	42663	42663	42663	42663
Outcome Mean	0.656	0.656	0.656	0.656
Panel D: Drug & Person & Other Crimes				
<i>Outcome: Convicted</i>				
Minority Defendant*White Prosecutor	-0.00322 (0.0123)	-0.00759 (0.00986)	-0.0107 (0.00996)	-0.0127 (0.0111)
Observations	86534	86534	86534	86534
Outcome Mean	0.563	0.563	0.563	0.563
Panel E: Property Crimes				
<i>Outcome: Convicted</i>				
Minority Defendant*White Prosecutor	0.0413*** (0.0121)	0.0300** (0.0118)	0.0293** (0.0118)	0.0295** (0.0116)
Observations	54163	54163	54163	54163
Outcome Mean	0.584	0.584	0.584	0.584
Prosecutor and Defendant Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	-	Y	Y	Y
Prosecutor FE	-	-	Y	Y
Interactions	-	-	-	Y

Standard errors in parentheses

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

Notes: This table reports interaction coefficients (β_3) between defendant race (Minority) and prosecutor race (White) from the regression: $Convicted_c = \beta_0 + \delta \cdot DefendantRace_c + \gamma \cdot ProsecutorRace_c + \beta_3 I(Minority Defendant_c \times White Prosecutor_c) + \epsilon_c$ where $DefendantRace_c$ includes indicators for Black, Hispanic, Asian, and Other defendants, and $ProsecutorRace_c$ includes indicators for Black, Hispanic, and Asian prosecutors (in some specifications & samples there is not sufficient sample for Hispanic and Asian indicators). Minority defendants include all nonwhite individuals: 55% are Black, 40% Hispanic, and 5% Asian. Minority prosecutors are 49% Black, 26% Hispanic, and 25% Asian. All models include screening date fixed effects. Specifications vary by included controls: baseline (race only); case controls (see Table 2); prosecutor fixed effects; and full interaction between prosecutor race and controls. Standard errors are clustered at the prosecutor level.

Table A.5: Cross-Race Conviction Effects by Crime Type (Hispanic Defendants & Prosecutors)

	(1)	(2)	(3)	(4)
Panel A: Drug Crimes				
<i>Outcome: Convicted</i>				
Hispanic Defendant*White Prosecutor	-0.0425 (0.0480)	-0.00538 (0.0386)	-0.0134 (0.0411)	-0.0226 (0.0452)
Observations	14243	14243	14243	14243
Outcome Mean	0.431	0.431	0.431	0.431
Panel B: Person Crimes				
<i>Outcome: Convicted</i>				
Hispanic Defendant*White Prosecutor	-0.0590 (0.0470)	-0.0720 (0.0442)	-0.0392 (0.0471)	0.00595 (0.0528)
Observations	4539	4539	4539	4539
Outcome Mean	0.293	0.293	0.293	0.293
Panel C: Other Crimes				
<i>Outcome: Convicted</i>				
Hispanic Defendant*White Prosecutor	-0.0186 (0.0272)	-0.0121 (0.0254)	-0.0117 (0.0254)	-0.0185 (0.0334)
Observations	17285	17285	17285	17285
Outcome Mean	0.656	0.656	0.656	0.656
Panel D: Drug & Person & Other Crimes				
<i>Outcome: Convicted</i>				
Hispanic Defendant*White Prosecutor	-0.0270 (0.0268)	-0.0180 (0.0193)	-0.0213 (0.0197)	-0.0365* (0.0219)
Observations	36067	36067	36067	36067
Outcome Mean	0.522	0.522	0.522	0.522
Panel E: Property Crimes				
<i>Outcome: Convicted</i>				
Hispanic Defendant*White Prosecutor	0.0107 (0.0332)	0.0173 (0.0288)	0.0180 (0.0286)	0.0101 (0.0235)
Observations	20032	20032	20032	20032
Outcome Mean	0.529	0.529	0.529	0.529
Prosecutor and Defendant Race Indicators	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	-	Y	Y	Y
Prosecutor FE	-	-	Y	Y
Interactions	-	-	-	Y

Standard errors in parentheses

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

Notes: This table reports interaction coefficients (β_3) between defendant ethnicity (Hispanic) and prosecutor race (White) from the regression: $Convicted_c = \beta_0 + \delta \cdot I(Hispanic Defendant_c) + \gamma \cdot I(Hispanic Prosecutor_c) + \beta_3 I(Hispanic Defendant_c \times White Prosecutor_c) + \epsilon_c$. The sample is restricted to Hispanic and white prosecutors and defendants. All models include screening date fixed effects. Specifications vary by included controls: baseline (race only); case controls (see Table 2); prosecutor fixed effects; and full interactions between prosecutor race and controls. Standard errors are clustered at the prosecutor level.

Table A.6: Disparities in Conviction Rates by Defendant and Prosecutor Race

	(1)	(2)	(3)	(4)
Outcome: Conviction				
Black Defendant	0.114*** (0.00519)	0.0706*** (0.00469)	0.0710*** (0.00477)	0.0710*** (0.00477)
White Prosecutor	0.0175 (0.0120)	0.0153 (0.0110)		
Observations	75666	75666	75666	75666
Outcome Mean	0.598	0.598	0.598	0.598
Screening Date FE	Y	Y	Y	Y
Case-Level Controls	-	Y	Y	Y
Prosecutor FE	-	-	Y	Y
Interactions	-	-	-	Y

Standard errors in parentheses

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

Notes: This table reports the coefficient on *Black Defendant* and *White Prosecutor* from the regression: $Conviction_c = \beta_0 + \beta_1 I(Black\ Defendant)_c + \beta_2 I(White\ Prosecutor)_c + \epsilon_c$. All models include screening date fixed effects. Specifications vary by included controls: baseline (race only); case controls (crime type—drug, person, property—plus defendant demographics, criminal history, arrest characteristics, arrest ZIP code, and prosecutor gender); prosecutor fixed effects; and full interactions between prosecutor race and controls. Standard errors are clustered at the prosecutor level.

A.2 Missing Values

One limitation of the data is that a small share of cases are missing key covariates: defendant race (1.6%), and prosecutor race (4.9%). This section demonstrates that the main results are robust to these missing values.

Column 1 of Table A.7 replicates the baseline estimate for property crimes. Column 2 includes dummies for missing values in age, gender, and ZIP code—interacted with prosecutor race—and assigns zeros to missing entries. The coefficient on *Black Defendant* * *White Prosecutor* remains virtually unchanged and statistically significant at the 1% level.

For prosecutor race, 99% of missing values stem from two prosecutors. Columns 3 and 4 impute their race as white or Black, respectively. Results remain stable and significant. For defendant race, columns 5–8 test a range of imputations: assuming all missing values are Black (col. 5), all white (col. 6), or a weighted mix (50% Black in col. 7, 79% Black in col. 8). Across all assumptions, the estimated cross-race effect persists and is statistically significant. These checks confirm that missing data do not account for the main finding.

Table A.7: Missing Values for Property Crimes

	Original Estimate	Missing Controls	Missing Prosecutor Race		(5)	(6)	Missing Defendant Race	(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Outcome: Guilty</i>								
Black Def* White Pros.	0.0469*** (0.0138)	0.0469*** (0.0138)	0.0472*** (0.0137)	0.0441*** (0.0134)	0.0486*** (0.0137)	0.0422*** (0.0144)	0.0462*** (0.0142)	0.0594*** (0.0175)
Observations	29815	29851	30132	30132	30248	30248	30248	29815
Outcome Mean	0.615	0.614	0.615	0.615	0.613	0.613	0.613	0.615
Pros. & Def. Race Indicators	Y	Y	Y	Y	Y	Y	Y	Y
Screening Date FE	Y	Y	Y	Y	Y	Y	Y	Y
Case-Level Controls	Y	Y	Y	Y	Y	Y	Y	Y
Prosecutor FE	Y	Y	Y	Y	Y	Y	Y	Y
Interactions	Y	Y	Y	Y	Y	Y	Y	Y
Missing Control Indicators	-	Y	-	-	-	-	-	-
Missing Defendant Race	-	-	-	-	Black	White	.5 Black	.79 Black
Missing Prosecutor Race	-	-	White	Black	-	-	-	-

Standard errors in parentheses

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

Notes: This table reports the coefficient on the interaction of Black Defendant and White Prosecutor from regressions of *Guilty* on indicators for prosecutor race, defendant race, and their interaction. All specifications include screening date fixed effects, prosecutor fixed effects, case-level controls, and interaction terms, consistent with column 4 of Table A.3. Column 1 repeats the original estimate. Column 2 adds indicators for missing case-level controls. Columns 3 and 4 impute missing prosecutor race as White or Black, respectively. Columns 5 through 8 replace missing defendant race as Black, White, 0.5 Black, or 0.79 Black (the sample mean). Standard errors are clustered at the prosecutor level.