LEARNING ABOUT POLICE BIAS:

How Monitoring the Police Changes Prosecutors' Beliefs and Decisions*

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Abstract

In many high-stakes settings, decision-makers rely on earlier actors for information that may be distorted by bias. What happens to these decision-makers' beliefs and choices when they can monitor earlier actors? We study this question using the rollout of police body-worn cameras (BWCs). We find that monitoring from BWCs reduces Black incarceration rates by 10.5%, explaining about a quarter of a decade-long decline in Black incarceration. Most of this decline reflects changes in downstream decisions rather than changes in arrests. To study the role of beliefs, we fielded an original survey of 203 North Carolina prosecutors and linked it to their half-million cases. About one quarter of the monitoring effect reflects learning about information from police: prosecutors with greater BWC exposure view police as more biased and unreliable.

Keywords: monitoring, biased beliefs, bodyworn cameras, prosecutorial discretion, systemic discrimination, racial disparities, criminal justice system

JEL classification: J15, K14, K42, D82, D83

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In many high-stakes settings, individuals learn about the world from intermediaries. Hiring managers learn about applicants from interviewers, editors learn about manuscripts from referees, and prosecutors learn about defendants from police. If intermediaries provide biased information, downstream decision-makers face a difficult inference problem: separating the truth from shading or bias. Rather than wrestling with this uncertainty, decision-makers may take a cognitive shortcut and act as if "what they see is all there is" (Kahneman, 2011; Enke, 2020). And over time, decision-makers who are repeatedly told that women are less qualified or Black people are more criminal may come to internalize these reports in their beliefs. What happens when downstream decision-makers can directly monitor earlier actors? Do they come to recognize earlier bias and revise their beliefs about the underlying reality?

We study these questions in the U.S. criminal justice system, where concerns about racial disparities raise the stakes of this inference problem. Each year, police arrest seven million Americans (FBI, 2019), and courts sentence nearly half a million people to prison (Carson, 2019). Prosecutors have considerable discretion over which arrests lead to prison sentences.[1] In most cases, prosecutors rely on the police officer's report of the arrest, which may reflect both the truth and shading or bias. Over the last couple of decades, over half of U.S. police departments have adopted body-worn cameras (BWCs), which record officers' arrests, calls for service, and other official actions (BJS, 2016, 2020). Although the footage is rarely shared with the public, it is routinely shared with prosecutors (and defense attorneys). Using the staggered rollout of BWCs, we test how increased monitoring of police shifts prosecutors' beliefs and choices. To measure beliefs, we field an original survey of 203 North Carolina prosecutors, which we link to their half a million cases.

Our qualitative interviews with prosecutors indicate that BWCs help them learn what happened during arrests. Despite sometimes being blurry or incomplete, prosecutors said that the footage can change their view of a *specific* arrest relative to what the police report suggested. For instance, the footage may reveal that it was unclear whether the

[1] For legal scholarship on prosecutorial power, see, e.g., Stith (2008) and Pfaff (2017). For empirical work on prosecutors' central role, see (e.g., Rehavi and Starr, 2014; Tuttle, 2019; Sloan, 2019; Agan et al., 2023).

defendant had committed a crime at all. As one prosecutor put it, BWCs "help us all get to what actually happened... to the truth of the matter." In addition, repeatedly observing discrepancies in police reports sometimes reshapes prosecutors' beliefs about police in *all* their cases, even in those where the footage itself offers no smoking gun evidence of misrepresentation. Several prosecutors reported growing skepticism of vague justifications for stops and searches, including "furtive movements," "weaving in the lane," and "having trouble finding ID."

To estimate BWCs' effects on criminal-justice outcomes, we use a stacked difference-in-differences design (Cengiz et al., 2019) and show robustness to alternative estimators (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Gardner, 2021; Sun and Abraham, 2021). Hand-collected city council minutes and local news articles suggest that funding constraints, rather than local politics or policing priorities, drove adoption timing. Typically, the question was not *whether* to adopt BWCs but *how* to finance them, with funding logistics often generating multi-year delays. Consistent with this institutional context, before BWCs were actually rolled out, criminal-justice contacts were similar — in both levels and trends — in places that adopted BWCs earlier and those that adopted later. In addition, our results are robust to limiting to adoption events less likely to be confounded by contemporaneous policy shifts (e.g., electing a new sheriff).

We find that BWCs reduce incarceration rates for Black people by 10.5% (0.029 pp), while leading to an insignificant increase for white people of 2.0% (0.001 pp, p-value of the difference = 0.0062). We find similar effects on conviction and jail time. The rollout of BWCs may help explain recent secular declines in incarceration rates for Black Americans. Incarceration rates of Black North Carolinians fell by 23% over the 2010s, mirroring national trends (Carson, 2019). Our estimates indicate that the introduction of BWCs can explain more than a quarter of the aggregate decline in North Carolina.

Using a Oaxaca–Blinder decomposition, we find that the decline in incarceration disparities is driven primarily by changes in how arrests are handled by the courts, rather than by changes in arrests themselves. Although arrest disparities do fall, the decline is concentrated in low-level arrests that almost never result in incarceration. Holding fixed the mapping from specific arrest charges to incarceration rates, only about one-sixth of the downstream effect is attributable to changes in the number or composition of arrests. Instead, the bulk of the decline in incarceration reflects changes in charging, dismissal, and sentencing decisions after arrest.

To better understand the downstream effects of BWCs, we fielded an original survey of 203 North Carolina prosecutors, which we linked to the half million cases they handled over the previous twenty-five years. Our survey elicits prosecutors' beliefs about police bias by asking them how much racial disparities in the criminal justice system are driven by Black people's crimes being perceived as more severe than the same crimes committed by white people. We elicit prosecutors' tendency to view police reports as "all there is" by asking them how often they feel certain about what the defendant did after reading the police report. These elicitations allow us to test whether prosecutors update their beliefs after watching BWC footage.

We estimate the BWCs' effects on beliefs using a difference-in-differences design that exploits variation in prosecutors' exposure to BWCs. Since exposure is jointly determined by both the prosecutor's hire dates and the county's BWC adoption timing, we can control for both experience and county effects. BWC adoption does not appear to shift the composition of prosecutors: turnover rates are similar before and after BWC adoption, as is the share of Black and liberal prosecutors.

We find that prosecutors with more exposure to BWCs believe that police reports are less reliable and that racial disparities in justice involvement are driven more by bias and less by true differences in conduct. The shift in prosecutors' beliefs appears to generate real-world effects: prosecutors with more prior BWC exposure tend to have lower incarceration disparities relative to others who handle the same rotation of cases — and so have the same current exposure to BWCs.[2] A back-of-the-envelope calculation suggests that approximately a quarter of BWCs' aggregate impact on incarceration disparities re-

[2]Three-fourths of prosecutors report that their units assign cases using a rotation. We validate this by showing balance and by using a mover design as in Chetty et al. (2014); Finkelstein et al. (2016).

flects prosecutors learning about the data-generating process in their cases. This learning mechanism is consistent with the escalating effects of BWCs: in our five-year post-period, incarceration rates of Black people gradually decline over time. Taken together, these results suggest that large and persistent racial disparities in the criminal justice system arise, in part, from informational frictions — which could be mitigated by monitoring.

We also use our survey to understand the implications of prosecutors' tendency to treat police reports "as all there is." Do prosecutors who take police reports at face value pass through more disparities in arrests before BWCs? And do these prosecutors come to internalize the reality presented in police reports, leading them to increase disparities over the course of their careers?

We find that prior to BWC adoption, prosecutors who more frequently take police reports at face value increase incarceration disparities relative to others who handle the same rotation of cases. This suggests that simply placing more weight on police reports increases disparities. After BWCs are adopted, disparities decline for *all* prosecutors and converge to the level previously realized by only the most skeptical prosecutors before BWCs. One interpretation is that BWC footage allows prosecutors to cross-check police reports, making their baseline faith in police less consequential. Another interpretation is that BWCs incentivize police to write more accurate reports, and so trust in police matters less since the police themselves have become more trustworthy. Either way, monitoring police with BWCs appears to disrupt the pass through of disparities introduced by police.

Before BWCs, we further find that incarceration disparities increase sharply with experience among prosecutors who take police reports at face value. By contrast, among more skeptical prosecutors, the relationship between experience and incarceration disparities is relatively flat. These patterns suggest that prosecutors who treat police reports as the ground truth gradually come to believe that racial differences in crime are larger than they initially thought. While research increasingly shows that biased beliefs drive disparities (Bordalo et al., 2016; Agan and Starr, 2018; Arnold et al., 2018; Chan, 2022), their origins are less clear. Our results provide evidence that biased beliefs may stem from earlier bias within the system itself.

Although our paper focuses on prosecutors and police, similar mechanisms likely operate in other high-stakes contexts. In hiring, publishing, admissions, lending, and child welfare, decision-makers must rely on information from earlier actors that is subjective but often appears objective. Prosecutors are highly-educated professionals with extensive experience interpreting cases and so may be especially well-equipped to detect and correct for bias. Yet even they struggle to adjust for distortions in upstream information.

We show that monitoring earlier actors can substantially reduce systemic disparities. The paper's core contribution is to unpack why. We provide a collage of evidence — from administrative data, original surveys, and qualitative interviews — that some prosecutors treated bias embedded in police reports as the truth, and that BWC monitoring allowed them to update their beliefs toward a different reality.

This work contributes to the literature on monitoring by investigating its systemic effects rather than focusing solely on the direct effects on those *being monitored*. For instance, prior work investigates how monitoring workers impacts their productivity and morale (e.g., Nagin et al., 2002; Gosnell et al., 2020; Adhvaryu et al., 2024); how monitoring politicians impacts their tendency to engage in corruption (e.g., Olken, 2007; Bobonis et al., 2016; Banerjee et al., 2020); how monitoring factories impacts their environmental violations (e.g., Duflo et al., 2013; Shimshack, 2014); and how monitoring police impacts use of force, arrests, and civilian complaints (e.g., Rivera and Ba, 2026; Barbosa et al., 2021; Braga et al., 2022; Kim, 2024). Our paper highlights a distinct role for monitoring in complex systems. By reducing information frictions, it can change beliefs and choices throughout the organization, ultimately attenuating disparities that originate upstream.

Our paper contributes to the literature on systemic discrimination by modeling and measuring how decision-makers process potentially biased information. A long literature recognizes that empirically identifying discrimination is challenging because relevant signals — such as work history — may themselves reflect past discrimination (e.g., Cain, 1986; Neal and Johnson, 1996; Blank, 2005). Recent work develops econometric methods to decompose observed disparities into contributions from different stages of a system (Baron et al., 2024; Bohren et al., 2025). We study a distinct mechanism: how later actors' interpretation of upstream information can amplify or attenuate disparities.

Finally, we contribute to the literature on BWCs by tracing their effects across multiple stages of the criminal justice system. Most existing work focuses on arrests, use of force, civilian complaints, and other outcomes at the policing stage.[3] Early evaluations of BWCs yielded small and inconclusive effects on arrests and police misconduct (Lum et al., 2020; Williams Jr et al., 2021). Yet recent work finds, consistent with our results, that BWCs reduce low-level arrests (Bollman, 2021; Barbosa et al., 2021).[4] We show that BWCs have downstream effects, explaining more than a quarter of the decline in Black incarceration during the 2010s. These findings differ from Bollman (2021), who studies the impact of BWCs on *incarceration conditional on arrest* rather than overall incarceration. Like us, Bollman finds that BWCs reduce low-level arrests. But by examining incarceration conditional on arrest, her downstream analysis excludes the low-level arrests that BWCs eliminate. Conditioning on the remaining, more serious arrests may therefore obscure reductions in overall incarceration. We instead use an Oaxaca-Blinder decomposition to isolate the contribution of downstream decision-making.

Section I of our paper provides institutional background from interviews of prosecutors about BWCs. Section II presents our model. Section III describes our data. Section IV analyzes the rollout of BWCs, and Section V, our survey of prosecutors.

I Prosecutors' Use of Body-Worn Cameras

We interviewed fourteen prosecutors and three judges to learn how they use BWC footage. These conversations motivate the mechanisms that we formalize in the model. The prosecutors and judges we interviewed explained that BWC footage almost always exists in cases involving police arrests. "[It] is super rare that there's no footage when I expect it,"

[3]Recent work finds that BWCs substantially reduce police use of deadly force (Barbosa et al., 2021; Miller and Chillar, 2022; Kim, 2024), civilian complaints against police (Braga et al., 2022; Ferrazares, 2024), and racial disparities in how complaints are handled (Çubukçu et al., 2021).

[4]By contrast, Kang (2023) finds that stops increase once New Orleans adopted BWCs.

one prosecutor told us. “There’s never a good reason for it.” In the rare instances where footage is missing, this prosecutor said he views it as a red flag and often drops the case.[5] Consistent with this, Jordan et al. (2025) finds that 93% of arrests in Chicago have BWC footage from at least one arresting officer.

The footage does not merely exist. It is watched. “I would say [prosecutors and defense attorneys] watch it in every case where it exists,” one judge told us. “Sometimes an intern will watch it first, but someone is watching it. Time and time again, I’m asked to continue trials because someone needs to look at the video.” A prosecutor explained that he sets up two monitors: “the [police] report on one screen and body-worn on the other.”

Prosecutors told us they watch BWC footage because it can make or break their case. The defense attorney receives it during discovery, so prosecutors need to know what’s in it going into plea negotiations. As one prosecutor put it, “Often, it’s the best evidence in my case.” Another echoed this view, explaining that the footage either helps to “nail down the person’s guilt” or is “disastrous to my case.” “Either way, I want to see it.” In short, BWCs can help both sides learn what actually happened in the individual case.

The footage can reveal information about the defendant. It may expedite a plea by showing a confession or a DWI defendant “falling into the side of the road.” But it can also show that the defendant’s conduct is “not as bad as what your mind is thinking" from the written account. The video can also provide *context* about the crime: one prosecutor said, “If you see a hungry guy [go] into a house to steal a sandwich, it could be the same charge [as if he stole] jewelry or a gun, but the punishment should be different.”

The video may also reveal information about the police officer — that he mischaracterized what happened or that what appeared objective in his report was, in fact, “a judgment call.” One prosecutor told us that an officer claimed a man was “making furtive movements” to justify the stop, but in the footage “the guy was just sitting there.” Another judge recalled a case where, “The cop said there was weaving, but then you see the video

[5]Specifically, this prosecutor told us in one rare instance, where an officer had obtained a search warrant on a house, the officer had his camera on “for the protective sweep but not the search itself.” This prosecutor said he was “not happy with that decision” and ultimately decided not to proceed on the case.

and think, 'Where's the weave?' The person looks like he's just driving around a turn in the road, and you think, 'I did that on the way to work this morning."' In cases where the footage reveals that the officer never had probable cause for the stop, one prosecutor said that he "fills out a dismissal and is done with it."

In addition to helping prosecutors and judges learn about individual cases, it may also help them learn over time about defendant conduct and police reliability *in all their cases*, even those where the footage does not directly change their perception of that specific case. For instance, prosecutors reported growing skepticism of certain justifications for stops and searches, including "furtive movements," "weaving in the lane," "seeming nervous," "having trouble finding ID," or "not responding well to questions."

BWCs can also reveal an officer's use — or misuse — of force. One prosecutor told us that he gives defendants "a better deal" if he sees that "law enforcement beat [the person's] ass." Over time, he became attuned to officers' language that sanitizes their use of force:

> If you've done this long enough, you know which language to be skeptical of. It's typically passive language like 'the defendant was assisted to the ground and placed in handcuffs... anodyne stuff... language that is functionally state violence. It makes me think, 'Ok, let's look at the body-worn.' And then we see that means three officers brought him to the ground.

He added that such footage can be "very disorienting," especially for new prosecutors. "The officer will draw it out in reports," but the video shows "just how quickly cops get people on the ground." All the prosecutor sees is the "butt of a gun" filling the screen.

Finally, BWCs may shift officers' own actions since they know they can be scrutinized more directly. As one judge put it, "if [the officer] knows the whole thing is being recorded, he's going to elevate his game." Especially for low-level offenses, officers may decide not to pursue the arrest at all. Officers may also change how they characterize what happened. One prosecutor said he counsels police to "put everything in [the report]: the good, the bad, and the ugly. It'll all come out eventually."

II Model

We model how prosecutors (mis)process potentially racially biased signals from police. The model extends the canonical statistical-discrimination model in Phelps (1972) to incorporate two behavioral mechanisms. First, in the spirit of Kahneman (2011), prosecutors may, at times, treat police reports as ground truth rather than as noisy, potentially biased signals. Second, prosecutors may have inaccurate beliefs about the source of disparities in police signals — specifically, how much these disparities reflect police bias versus true differences in crime. We microfound these inaccurate beliefs as partly reflecting prosecutors updating toward the signals they receive from police. The model then introduces monitoring, which provides prosecutors with an additional unbiased signal about the arrest. Finally, we map the model to our empirical analyses.

Timing & Information Structure. Individual i engages in conduct that may be criminal. The conduct's latent severity, θ_i, is drawn from a type distribution that may differ by race, $R \in \{B, W\}$: $\theta_i \sim N(\mu_{R(i)}, 1/\tau_\theta)$ where $\kappa = \mu_B - \mu_W$. The police officer directly observes the conduct and the individual's race and then writes a report describing the incident.[6] This report is a noisy, potentially biased signal of i's type, $s_i = \theta_i + \gamma_{R(i)} + \epsilon_i$ where $\epsilon_i \sim N(0, 1/\tau_s)$. $\gamma_{R(i)}$ reflects the officer's bias against a given racial group and b_i, the Black-white gap in his bias, which could reflect taste-based discrimination or inaccurate beliefs.[7] The prosecutor receives this signal, s_i, and then chooses how much to press for an incarceration sentence in negotiations with the defense attorney.

The Prosecutor's Payoffs. The benefits of incarceration are increasing in crime severity, θ_i, and the prosecutor's taste-based bias, $\zeta_{R(i)}$. Prosecutors may care about crime severity because of retribution, deterrence, or incapacitation — motives that may reflect the prosecutor's own values or their beliefs about what voters or their elected superiors want. The

[6]We assume officers always send a report to prosecutors. In practice, officers may decline to arrest in low-severity cases, but we abstract from this since such cases play a limited role in incarceration outcomes.

[7]These bias terms could also reflect strategic signaling by police, who adjust reports based on how they expect prosecutors to respond to them. This mechanism magnifies b_i if prosecutors are less biased than police against Black versus white people and reduces b_i if prosecutors are relatively more biased against Black people than police. This follows from applying Kartik et al. (2007)'s model of strategic communication.

costs of securing a higher probability of incarceration increase convexly since the prosecutor needs to make the case increasingly airtight. Together, this implies that her preferred incarceration probability increases proportionally with i's type and her taste-based bias, up to a first order. The challenge for the prosecutor is that she cannot directly observe type and instead must form beliefs about it based on her priors and the signal from police.

The Prosecutor's Beliefs. The prosecutor knows that the observed police signals for a given race, $\bar{s}_R \equiv \mathbb{E}[s_i \mid R]$, reflect a combination of true type (μ_R) and police bias (γ_R), but she doesn't know the contribution of each of these factors. Instead, she forms subjective beliefs about each group's average type ($\mu_{p,R} \equiv \mathbb{E}_p[\mu_R]$) and police bias for each group ($\gamma_{p,R} \equiv \mathbb{E}_p[\gamma_R]$). These subjective beliefs are disciplined only by the fact that the two must sum to the group's average observed signal: $\bar{s}_R = \mu_{p,R} + \gamma_{p,R}$.

A rational prosecutor should correct each signal using her belief about police bias for that individual's racial group, $s_i - \gamma_{p,R}$. Yet she may not always incur the cognitive cost required to process the police signal and perform this correction. Instead, she may take a shortcut and treat the police signal as the true type ($s_i = \theta_i$). Indeed, our qualitative interviews with prosecutors indicate that they sometimes simply accept police justifications for stopping people — such as "furtive movements," "weaving in the lane," or "seeming nervous." Prosecutors may vary in the share of their cases in which they take police signals at face value, $\alpha_p \in [0, 1]$. This behavioral tendency could reflect a variety of microfoundations, including cognitive load, time pressure, or intrinsic trust in police.

When the prosecutor treats the police signal as the truth, she also uses it to update her prior about the Black-white gap in types ($\kappa_p[t] \equiv \mathbb{E}_{p,t}[\mu_B - \mu_W]$). As the prosecutor sees more signals, her belief about this gap converges to the reality presented by the police.

We study the introduction of monitoring using the staggered rollout of BWCs. Their effect on incarceration disparities reveals whether *either* behavioral mechanism is operative — but do not, on their own, isolate which one. For example, disparities may decline because prosecutors had been overestimating the extent to which arrest disparities reflect differences in crime ($\kappa_p[t] > \kappa$), or because they had been taking biased police signals at

Mechanism	Potential Empirical Result	Implied Model Parameters
Any behavioral bias	The introduction of BWCs...	
Test: Section IV; Fig 3	Changes disparities	$\kappa_p[t] \neq \kappa \vee (\alpha_p \neq 0 \wedge b \neq \kappa_p[t] - \kappa)$
	Has no impact on disparities	$\kappa_p[t] = \kappa \wedge (\alpha_p = 0 \vee b = \kappa_p[t] - \kappa)$
Inaccurate beliefs	More BWC exposure leads prosecutors to think race gaps in crime are...	
Test: Section V.B; Fig 4		
	Smaller	$\kappa_p[t] > \kappa$
	The same	$\kappa_p[t] = \kappa$
	Larger	$\kappa_p[t] < \kappa$
Taking police at face value	Before BWCs, prosecutors who more often take police at face value...	
Test: Section V.C; Fig 5	Increase disparities	$b > \bar{\kappa}_p - \kappa$
	Have no effect on disparities	$b = \kappa_p[t] - \kappa$
	Decrease disparities	$b < \kappa_p[t] - \kappa$
Mistaken learning from police signals	Before BWCs, prosecutors who more often take police at face value...	
Test: Section V.D; Fig 6	Increase disparities with tenure	$b > \kappa_p[0] - \kappa$
	Have no change with tenure	$b = \kappa_p[0] - \kappa$
	Decrease with tenure	$b < \kappa_p[0] - \kappa$

$\kappa \equiv$ True racial differences in conduct $\kappa_p[t] \equiv$ Prosecutor prior after time t pre-BWC
$b \equiv$ Bias in police signal $\alpha_p \equiv$ Share of police signals treated as the truth

face value ($\alpha_p \neq 0 \vee b > \kappa_p[t] - \kappa$).

We use our survey to distinguish between mechanisms. First, we test whether greater BWC exposure shifts prosecutors' beliefs about the sources of racial disparities in arrests. If prosecutors with more exposure attribute arrest disparities less to differences in crime and more to police bias, this implies that they had previously misjudged both, overstating differences in crime and understating bias ($\kappa_p[t] > \kappa$ and $b_p[t] < b$).

Second, we examine prosecutors' tendency to treat police reports "as all there is." Prosecutors who more frequently take police reports at face value place more weight on police signals and less on their own priors. If these prosecutors increase incarceration disparities relative to their peers, this indicates that racial differences in police signals exceeded prosecutors' priors about racial differences in crime ($b > \bar{\kappa}_p - \kappa$).

Finally, we study how these mechanisms interact. We test whether prosecutors who take police reports at face value learn from these potentially biased signals by estimating whether their impacts on disparities systematically change as they gain experience. Before BWCs, if disparities increase with tenure among prosecutors who take police signals at face value, this suggests that prosecutors start out believing that race gaps in types are smaller than what police reports would suggest ($\kappa_p[0] < b + \kappa$). In this case, prosecutors' biased beliefs at time t partially reflect them internalizing past police signals.

III Data

Our paper uses three primary datasets. First, we use administrative data on state criminal cases from North Carolina, which covers nearly four million cases between 1995 and 2019. Second, we compile data on the timing of BWC adoption in North Carolina. Finally, we link in surveys of 203 North Carolina prosecutors, whom we recruited in partnership with sixteen offices. Surveyed prosecutors collectively handled about half a million cases.

III.A North Carolina Court Records

We use court records from the North Carolina Administrative Office of the Courts (AOC). Our AOC data include all state felony cases from 1995 to 2019, as well as all misdemeanor cases since 2010. As a result, we have all criminal cases for four years prior to the year the first place adopted BWCs (in 2014). Since police officers are the charging agency in North Carolina, the AOC data contain the near universe of arrests, and each record details the arrest charges chosen by the police.[8]

The court records allow us to reconstruct much of the information that the prosecutor sees when first opening a defendant's case file, including demographics (race, age, and gender) and the officer's chosen arrest charge(s). The records also detail case outcomes, including conviction charges, jail time, and incarceration sentences.[9] As is typical in U.S.

[8]Rose and Shem-Tov (2021) validate this in Charlotte: 93.3% of bookings appear in the AOC data.

[9]We define incarceration sentences as incarceration terms of at least six months since these sentences are served in state prison (rather than county jail) and are primarily post-conviction (rather than pretrial).

courts, almost all cases (>99%) resolve via a negotiated plea rather than a trial. We use fuzzy string matching to create consistent prosecutor identifiers, which allow us to link in our survey of prosecutors (see Appendix B). Finally, the court records detail the county handling the case but not the law enforcement agency. As a result, we merge in BWC adoption timing at the county level rather than at the agency level.

III.B Body-Worn Cameras

Adoption Timing. Different parts of North Carolina adopted BWCs at different times (Figure 1(a)). To determine adoption timing, we began with data from the Bureau of Justice Statistics Law Enforcement Management and Administrative Statistics (LEMAS), which covers a sample of law enforcement agencies (BJS, 2016, 2020). Since the LEMAS only offers snapshots of BWC usage in a subset of agencies, we supplemented it by searching local news articles and calling agencies.[10] We aggregate BWC treatment information to the county level and classify a county as treated when at least half of officers work in agencies with BWCs. These data efforts allow us to identify the adoption year for 48 counties, covering 77% of cases. The long time-span of the court records allows us to estimate BWCs' impacts for five years post-adoption. As a result, we can assess BWCs' impacts in a larger jurisdiction and over a longer time horizon than previous studies.[11]

Arrest & Sentencing Outcomes. BWCs could affect each stage of the criminal justice system — including arrest, charging, and sentencing. As a result, we focus on changes in per capita outcomes rather than on the likelihood of each outcome conditional on reaching that stage. For example, we study changes in per capita incarceration rates, not incarceration conditional on arrest. Such conditioning would introduce selection bias if BWCs alter *which* cases reach a given stage (Knox et al., 2020; Goncalves et al., 2025). Section IV.B uses a Oaxaca-Blinder decomposition to analyze the contribution of each stage.

[10] This data collection identified adoption years for 103 additional agencies: 45 not sampled by LEMAS and 58 that did not report a specific adoption year. It also revealed that the adoption year in LEMAS often reflected a pilot program rather than broad adoption across patrol officers.

[11] Kim (2024) evaluates BWCs' impacts nationally but narrows the analysis to within one year around BWC adoption and primarily focuses on police use of force as the outcome.

We measure per capita arrest rates for each county c, year t, and racial group r:

$$\text{Per capita arrest rate}_{ctr} = 100 \cdot \frac{\#\ \text{Arrests}_{ctr}}{\text{Total Population}_{ctr}}. \quad (1)$$

We construct analogous measures for per capita conviction and incarceration rates. To construct the numerators, we aggregate case-level data from administrative court records. For incarceration, this measures the flow of new incarceration sentences rather than the stock of people in prison. To construct the denominators, we use population counts by race, year, and county (U.S. Census Bureau, a).

Summary Statistics. Table 1 presents summary statistics from the North Carolina court records, both overall and by BWC adoption year. Black people in North Carolina are more likely to come into contact with the criminal justice system than white people. Indeed, they are about three times as likely to be arrested, with 6.7% of Black people arrested each year compared to 2.3% of white people (see Column 1, Rows 2−3). This initial disparity persists to sentencing: Black people are about three times as likely to be convicted as white people (Rows 5-6) and are about five times as likely to be incarcerated (Rows 8−9).

Across BWC adoption groups, disparities in per capita arrest, conviction, and incarceration rates are nearly identical (Rows 1−9 in Table 1).[12] Compared to later adopters, earlier adopters have a higher share of Black residents and are more urban and politically liberal (Rows 10-12). These demographic differences are especially pronounced for never-adopting counties. Reassuringly, we find that these county-level characteristics do not systematically change around BWC adoption (see Table A.2 and Section IV.A).

III.C Prosecutor Survey

We fielded a survey of North Carolina prosecutors with support from the North Carolina Conference of District Attorneys and participating elected District Attorneys (DAs). Sixteen of the state's forty-three DAs participated, including those in the state's four largest cities — Charlotte, Raleigh, Greensboro, and Durham (see the map in Figure 1(b)). We

[12]This also holds in the years before any North Carolina county had adopted BWCs (see Table A.1).

fielded the survey between May and November 2020, following an in-person pilot in two additional offices in November 2019.

The survey elicited prosecutors' beliefs about police reports and the sources of racial disparities in the criminal justice system (described in Section V), in addition to their demographics and professional backgrounds. Prosecutors completed the survey online, and in most offices, the elected DA directly emailed the survey link to prosecutors, yielding a 39% participation rate. We surveyed 203 prosecutors, 90 of whom chose to debrief with us over Zoom. We linked 175 respondents to the court records (an 86% match rate), of whom 163 answered the question about police reports. On average, surveyed prosecutors handled 69 felonies and 332 misdemeanors per year, collectively handling over half a million cases between 1995 and 2019. Surveyed prosecutors' experience, demographics, and politics are broadly representative of line prosecutors in North Carolina (see Table A.3 and Appendix C for details on the North Carolina Voter Records data).

IV Body-Worn Cameras

IV.A Empirical Design

Our empirical design leverages the staggered rollout of BWCs across North Carolina from 2014 to 2019 (shown in Figure 1(a)). We use the variation in adoption timing to estimate how BWCs affect per capita outcomes, Y_{rct}, for racial group r in county c and year t. Our preferred approach uses a stacked difference-in-differences (DiD) framework (Cengiz et al., 2019; Deb et al., 2024), but we show robustness to alternative estimators. For each BWC adoption event, we construct a separate sub-experiment dataset, s, with a control group of untreated counties that have not yet adopted BWCs (or never will).[13] We then concatenate these datasets into a single "stacked" dataset, where we index observations by s since some appear multiple times as controls for different sub-experiments. We

[13]Each dataset contains the observations for the treatment group — i.e., the sub-experiment's adopting county — and those of the comparison group — i.e., all counties that are untreated in a given year. Limiting the control group prevents the DiD estimator from using already-treated units as controls, which would yield misleading comparisons in the presence of dynamic treatment effects (Goodman-Bacon, 2021).

allow the fixed effects for county (c) and year (t) to flexibly vary by sub-experiment (s) in:

$$\begin{aligned} Y_{rcts} &= \beta^{B} \text{ Adopted BWC}_{cts} + \mu^{B}_{cs} + \mu^{B}_{ts} + \epsilon_{rcts} && \text{if } r \text{ is Black} \\ Y_{rcts} &= \beta^{W} \text{ Adopted BWC}_{cts} + \mu^{W}_{cs} + \mu^{W}_{ts} + u_{rcts} && \text{if } r \text{ is White.} \end{aligned} \tag{2}$$

We also analyze racial disparities directly:

$$Y_{rcts} = \beta^{B-W} \text{ Adopted BWC}_{cts} \times \text{Black}_{r} + \beta^{W} \text{Adopted BWC}_{cts} + \mu_{rcs} + \mu_{rts} + \epsilon_{rcts}. \tag{3}$$

We weight these regressions by the population of the relevant racial group, county, and year (U.S. Census Bureau, a). We limit to the five years around adoption and cluster standard errors at the county level.

Our coefficients of interest — β^{B-W}, β^{B}, and β^{W} — reflect changes in adopting counties relative to contemporaneous changes in control counties. These estimates will capture BWCs' causal effects if criminal-justice outcomes in treatment and control counties would have followed parallel trends in the absence of different BWC timing. The DiD design helps to rule out other potential confounding factors. First, county fixed effects net out time-invariant differences across counties. For example, if more urban counties are more likely to adopt BWCs and also tend to have higher (or lower) incarceration rates at baseline, this would not bias our results. Second, time fixed effects net out trends that are common across counties, such as from state-wide policy changes.

One may worry that different BWC adoption groups would have had different trends even without BWCs. To probe threats to identification, we investigate the institutional context of BWC adoption and do a series of empirical checks on the design.

To understand the institutional context, we hand-checked all city-council minutes that mention BWCs. BWCs were almost always a free-standing agenda item, with debates centering on timing and funding rather than adoption itself. On average, more than two years elapsed between initial discussions and implementation. The upfront cost of cameras and ongoing expense of data storage slowed the pace of adoption. Most jurisdictions also trialed BWCs in small pilots before rolling them out to all beat officers, further contributing to delays. We show robustness to limiting our analysis to places that eventually adopted BWCs but did so at different times, as well as to places where adoption timing was more plausibly exogenous: those without a recent new sheriff, those without concurrent policing reforms, and those where BWCs were federally, rather than locally, funded.

In balance tests in Table A.2, we find that BWC adoption is unrelated to changes in a variety of county-level characteristics, including racial composition, politics, and economic activity. In addition, trends in police killings were similar in adopting and control counties before BWC adoption (Figure A.8), suggesting that BWCs were not adopted in response to local instances of police violence that may have prompted other policy changes.

Finally, we check for pre-trends. Visually, we do not see any evidence of pre-trends using a dynamic version of Equation 2. Defining e as the event time relative to BWC adoption in the focal county of sub-experiment s, we estimate:

$$Y_{rcts} = \sum_{e=-5}^{5} \beta_e \mathbb{1}[c = s] \cdot \mathbb{1}[t - e = t_s] + \mu_{cs} + \mu_{ts} + u_{rcts} \quad \text{for r is Black and r is white} \tag{4}$$

We estimate dynamics for our primary stacked estimator as well as for the alternative estimators proposed in De Chaisemartin and d'Haultfoeuille (2020); Callaway and Sant'Anna (2021); Gardner (2021); Sun and Abraham (2021). We also follow the method proposed in Roth (2022) and contrast our results with the worst-case-scenario trends that we would have power to detect in the pre-period. Finally, we test the robustness of our results to controlling for differential linear trends across BWC adoption groups.

IV.B Results

Arrests. BWCs reduce arrest rates, especially for Black people. Figure 2 illustrates BWCs' effects on arrests. Prior to BWC adoption, arrest rates for both Black and white people evolved similarly in counties about to adopt BWCs and those not about to adopt. However, once BWCs are adopted, arrests of Black people fall by 0.51 pp or 7.2% (Column 2 of Table 2(a), p-value = 0.063). By contrast, arrests of white people insignificantly fall by

0.07 pp (Column 3). As a result, disparities in arrest rates decline (Column 4). We see similar declines in disparities in traffic stops (Table A.4(a)).

BWCs' impacts on arrests are heavily concentrated in low-level offenses, as shown in Figure 2(c). The largest declines in arrests are for public-order offenses like loitering and drug possession, which almost never lead to incarceration. Indeed, 96% of the aggregate decline in arrest rates for Black people comes from charges with less than a 1% chance of resulting in incarceration (Figure A.2). We see little change for more serious offenses such as homicide and sex crimes, where victims and witnesses — rather than police — often determine whether an arrest occurs.

While these impacts on arrest rates could not, on their own, substantially reduce incarceration, monitoring from BWCs could still impact incarceration by changing downstream decision-makers' information sets and resulting beliefs and choices.

Downstream Outcomes. BWCs reduce incarceration rates for Black people, but have no significant effect for white people, as shown in Figure 3. For both races, incarceration rates follow similar trends in treated and control counties prior to BWC adoption. Once BWCs are adopted, rates for Black people begin to decline, a pattern we see across alternative DiD estimators (Figure 3(c)). Our pooled estimate indicates that BWCs reduce Black people's incarceration rate by 10.5% (0.029 pp, Column 2 of Table 2(c)), while white people's incarceration rate insignificantly increases (Column 3). As a result, 14% of the baseline disparity in incarceration rates is eliminated by BWCs (p-value = 0.0062, Column 4). We see similar patterns for conviction (Table 2(b)) and jail time (Table A.4(b)).

In Figure 3, the decline in incarceration for Black people grows over time. This escalating pattern could reflect prosecutors gradually learning about police reliability and defendant conduct as they gain exposure to BWC footage. It could also reflect improvements in BWC technology or changes in activation policies and their enforcement. Consistent with this, Jordan et al. (2025) find that activation rates rose over time in Chicago.

Decomposing the Incarceration Effect. We decompose the total change in incarceration into the relative contributions of the arrest and post-arrest stages (Oaxaca, 1973; Blinder, 1973). We estimate the contribution coming from (1) changes in arrest rates for specific charges, holding fixed the relationship between arrest charges and incarceration, and (2) the residual change in incarceration due to shifts downstream of arrest.[14] Holding constant the relationship between arrest charges and incarceration, BWCs' arrest effects would have decreased incarceration rates for Black North Carolinians by 0.0048 pp, which is 16.3% of the total incarceration effect (of 0.029 pp). Thus, neither the change in the rate nor the composition of arrests can explain the bulk of the incarceration effect. Instead, about 84% can be explained by a change in post-arrest decision-making.

Robustness. The estimated effects of BWCs are robust to various checks on our design. In Table 2, Column 5 excludes never-adopters from our controls, and Column 6 excludes neighboring counties to handle potential spatial spillovers. In Column 7, we allow for adoption-group specific pre-trends, which leads to slightly attenuated but qualitatively similar results. The results hold when excluding counties that elected a new sheriff just before BWC adoption (Figure A.4), dropping counties with concurrent policing reforms (Figure A.3), and restricting to the nine federally-funded adoption events (Figure A.5). Finally, we find broadly similar effects across the twenty-six adoption events (Figure A.7).

We find no evidence of differences in pre-trends for arrest disparities (p-value $= 0.85$) or incarceration disparities (p-value $= 0.35$). These nulls are not due to a lack of statistical power. Figure A.6 uses Roth (2022)'s method to compare our event-study results with the worst-case scenario trends we have the power to detect. Most of the event-study confidence intervals do not overlap with the worst-case trend we have 50% power to detect, and many do not overlap with the worst-case trend we have 90% power to detect.

The absence of pre-trends is especially informative given the average delay of 2.1 years between initial discussions to adopt BWCs and their ultimate rollout to officers, which

[14] To assess the contribution of arrests, we predict incarceration at arrest using charge-specific incarceration rates from counties that had not yet adopted BWCs and then apply Equation 3 to these predictions, yielding β^{Arrest}. The post-arrest contribution is then $\beta^{\text{Post-Arrest}} = \beta^{\text{Total}} - \beta^{\text{Arrest}}$.

would likely lead any confounding factors that coincided with the initial adoption decision to show up prior to the actual adoption year.

V Survey

We use our survey of prosecutors to unpack BWCs' downstream effects on incarceration outcomes. Section V.A describes our two primary survey questions. Section V.B examines prosecutors' beliefs about the sources of disparities in justice involvement. Sections V.C-V.D explore prosecutors' tendency to take police reports at face value.

V.A Elicited Beliefs

Our survey elicited prosecutors' beliefs about how much racial disparities in the criminal justice system reflect racial differences in crime versus racial bias. We followed the approach of the General Social Survey, which first presents facts about racial disparities and then asks respondents about potential drivers.[15] The survey gave prosecutors facts about existing disparities in criminal-justice outcomes and then asked them to rate (on a scale from 0 to 100) the importance of five potential drivers of these disparities (Figure 4(a)).

On average, prosecutors attributed 18% of disparities to biased perceptions of conduct — i.e., "the current conduct of a black defendant is perceived to be more serious than the same conduct committed by a white defendant" (in green in Figure 4(a)). They attributed 21% of disparities to Black defendants having more severe current or past conduct (in orange and red). Finally, prosecutors believed disparities reflected Black defendants having more criminal-history prior points and lower quality legal representation (accounting for 24% and 14% of disparities, respectively).

Our survey also elicited prosecutors' tendency to take police reports at face value: we asked about the share of cases they felt uncertain about the defendant's true conduct (a) after first reading the police report and (b) at the case's final disposition (Figure 4(b)). Prosecutors almost never felt uncertain at the final disposition. But they were uncertain

[15] Our pilot suggested this approach reduced prosecutors' reluctance to answer questions about race. Yet a quarter still did not respond. Non-response is not correlated with BWC exposure: one year of exposure is associated with an insignificant 2.2 pp lower non-response rate (p-value = 0.44).

42% of the time after reading the police report, implying that they trusted the police's account in the majority of cases.

These elicitations predict the incarceration disparities in prosecutors' past cases, even after controlling for prosecutor race and politics (Figure A.9; the design is discussed in Section V.C). Prosecutors who attribute disparities more to biased perceptions of conduct significantly reduce disparities relative to others in the same case rotation, while the opposite appears to be true of prosecutors who attribute disparities more to differences in conduct.[16] Prosecutors' trust in the police also matters: those who more frequently trust police reports increase disparities relative to less certain prosecutors.

V.B Learning from BWCs

This section explores whether monitoring from BWCs shifts prosecutors' beliefs and how much this learning contributes to BWCs' impacts on incarceration disparities.

Exposure Design

Our design leverages the fact that prosecutors differ in the number of years they have been exposed to BWCs based on (i) when they started working in the county and (ii) when their county adopted BWCs. We use this variation in a difference-in-differences design. We first compare prosecutors working in the same county who start in different years — and so differ in their BWC exposure as well as their years of experience. We then compare this first difference to an analogous difference in counties that adopted BWCs later (or not at all), where prosecutors who started earlier versus later differ only in years of experience — and not their exposure to BWCs.

To illustrate, consider Greensboro, which adopted BWCs in 2014, and Raleigh, which adopted them in 2018. Our exposure design first compares prosecutors who started in different years in Greensboro, say in 2014 versus 2016. Since Greensboro adopted in 2014, 2014-hires have longer BWC exposure than 2016-hires. This within-county comparison nets out county-level differences. We then contrast this difference with the same start-

[16] Prosecutors who attribute disparities more to racial gaps in legal representation have significantly lower disparities, while those who attribute disparities more to prior points have insignificantly higher disparities.

date comparison in Raleigh. Since Raleigh adopted in 2018, 2014-hires and 2016-hires do not differ in their BWC exposure, only in their years of experience. This second difference nets out average differences across prosecutors with different tenure.

Letting p index prosecutors, c the county, and τ prosecutor tenure, we estimate:

$$\text{Belief}_p = \phi \text{Years of BWC}_p + \mu_{c(p)} + \mu_{\tau(p)} + \epsilon_p. \tag{5}$$

County fixed effects, $\mu_{c(p)}$, absorb any county-wide differences in beliefs, which could reflect, for instance, differences in crime rates, police practices, or the racial or political composition of prosecutors. Tenure fixed effects, $\mu_{\tau(p)}$, absorb any systematic differences in beliefs by experience, which could reflect, for instance, a hardening effect of working in the system or the cumulative effect of working with police.

To interpret ϕ as the causal effect of BWC exposure on beliefs, it must be that the relationship between prosecutors' beliefs and tenure would have been similar in counties that adopted earlier versus later in the absence of BWCs. We probe this assumption by focusing on a set of prosecutors who differ in their tenure but *not* in their exposure to BWCs. To make this concrete, consider all prosecutors hired before 2014, the first year BWCs were adopted in North Carolina. For all of these prosecutors, more years of experience do not lead to more years of BWC exposure within any county. Under our identifying assumption, the county-level relationship between these prosecutors' beliefs and tenure should be similar, regardless of the county's BWC adoption year. Figure A.15 shows the results of this placebo check: when limiting to prosecutors hired before BWC adoption, the relationship between tenure and beliefs is similar for all adopting cohorts.

We estimate the analogue of Equation 5 in the court records to evaluate how exposure to BWCs predicts a prosecutor's effects on incarceration disparities:

$$\begin{aligned} \text{Incarceration}_i =& \gamma \text{ Years of BWC}_{p(i)} \times \text{Black}_i + \theta \text{ Years of BWC}_{p(i)} \\ &+ \mu_{\text{Black}_i, c(i), t(i)} + \mu_{\text{Black}_i, \tau_{p(i),t(i)}} + \mu_{\text{unit}(i)} + \zeta_i. \end{aligned} \tag{6}$$

where unit fixed effects ($\mu_{\text{unit}(i)}$) allow us to compare prosecutors handling the same rotation of cases (discussed and validated in Section V.C). The coefficient of interest, γ, aims to test whether prosecutors who have seen more BWC footage over their careers make different decisions, potentially because they have gradually learned from all the footage they have seen. We want to separate this long-run learning mechanism from the immediate effect of BWCs on individual cases, namely that the footage can change a prosecutor's view of a specific case. To do this, we control for current BWC adoption and its potentially racially-disparate impact by including race-county-year fixed effects ($\mu_{\text{Black}_i,c(i),t(i)}$).

This design relies on an analogous identifying assumption as above, which we probe with a similar placebo check: before any county adopted BWCs — when no prosecutor had any exposure to BWCs — the relationship between a prosecutor's tenure and their disparate impacts should be similar, regardless of their county's ultimate adoption year. Reassuringly, we find that the tenure-disparities relationship is similar in early and later adopting counties in the years before any county adopted BWCs (Figure A.16).

Results

Beliefs about Sources of Disparities. Prosecutors with more BWC exposure believe that racial disparities in justice involvement are driven more by racial bias and less by true differences in crime. After controlling for county and tenure (Equation 5), each additional year of BWC exposure is associated with a 4.4 pp increase in the share of disparities that prosecutors attribute to bias — i.e., to the same conduct being *perceived* differently when the defendant is Black rather than white (the fourth coefficient in Figure 4(b), p-value = 0.044). Consistent with this, prosecutors come to believe that disparities are less warranted by true differences in either past or current crime (the first and third points in Figure 4(b)). These shifts in beliefs are robust to controlling for prosecutors' race and politics. Taken together, these findings suggest that BWC footage shows prosecutors a different reality from the one previously presented by police: one where disparities in arrests are driven more by racial bias and less by Black people having worse conduct.

Reassuringly, we find that BWC exposure is unrelated to prosecutors' beliefs about drivers of disparities that one would *not* expect to be revealed by BWC footage. Beliefs about racial differences in the quality of defense counsel and criminal record length ("prior points"), both shown in gray in Figure 4(b), do not shift with BWC exposure.

Beliefs about Police. Prosecutors with greater BWC exposure are less likely to take police reports at face value. Each additional year of BWC exposure is associated with a 4.5 pp decline in the share of cases where prosecutors feel certain about the defendant's conduct after reading the police report (p-value = 0.09).[17] In our interviews (Section I), prosecutors told us that BWC footage can shift their view of a case: sometimes the footage directly contradicts the police report; other times it reveals that the officer's characterization was a judgment call; and still other times, it provides context that was missing from the report. Cumulatively, these experiences could lead prosecutors to view police reports as less definitive, even if they do not come to believe that officers are lying to them more than they initially thought. Monitoring from BWCs could also push in the other direction by incentivizing police to write more accurate reports. All else equal, this would lead prosecutors to trust officers' accounts more than they did before. Our results suggest that prosecutor updating is dominated by their increased awareness that the police report is not the whole story, rather than by any increase in police reliability.

Alternative Explanations. The shift in prosecutors' beliefs is unlikely to reflect changes in the selection of prosecutors. Prosecutor turnover is similar before and after a county adopts BWCs (rows 9−10, Table A.2), as is the share of Black and liberal prosecutors (rows 11−12). The shift in beliefs is similar after controlling for prosecutor race and politics (Figure 4b and Table A.9). Thus, our results likely do not simply reflect a more general shift toward progressivism that happens to coincide with the timing of BWC adoption.

Implications for Disparities. To understand the consequences of the shift in prosecutors' beliefs, we apply our exposure design to the realized disparities in their cases. Prosecutors with more past exposure to BWCs reduce incarceration disparities relative to others in their office who have the same current access to BWC footage. After accounting for

[17]Prosecutors' degree of certainty at the final resolution of the case does not shift with BWC exposure. This is unsurprising given prosecutors' high baseline degree of certainty at this stage (see Figure A.10).

county and tenure effects, each additional past year of BWC exposure is associated with a 0.30 pp reduction in incarceration disparities (p-value = 0.037), which represents a 5% reduction in Black defendants' incarceration rate (the right-most point in Figure 4(b)) and a 10% reduction of the raw racial disparity in incarceration rates. Therefore, the cumulative effect of BWC exposure appears to reduce incarceration disparities, even holding constant the direct effect of watching the footage on the *individual case*.

We can also approximate the real-world implications of prosecutor learning using the estimated effects of beliefs on disparities. If we make the strong (or, perhaps, heroic) assumption that these relationships are causal, we can estimate how the shift in beliefs affects disparities by rescaling each change in belief (from Figure 4(b)) by the estimated relationship between that belief and incarceration disparities (from Figure A.9(b)). This exercise suggests that prosecutor learning reduces incarceration disparities by $0.1-0.2$ pp per year of BWC exposure, similar to the results of the analysis above.[18]

Using these estimates, we decompose BWCs' total effect on incarceration disparities into the component driven by learning. Our back-of-the-envelope calculation indicates that learning accounts for 24% of the aggregate effect, split roughly evenly between changes in beliefs about police reliability and whether bias or conduct drives disparities in justice involvement (see Figure 4). Together, these results suggest that BWCs reduce disparities in part by shifting prosecutors' beliefs about the data-generating process in their cases.

V.C Certainty in Police Reports

When prosecutors watch more BWC footage, their beliefs about their cases shift, suggesting that their original beliefs had been inaccurate. This section considers the source of these biased beliefs by investigating prosecutors' tendency to interpret police reports as "all there is." First, we investigate whether prosecutors who more frequently view police reports as the whole story passed through more of the potential bias in these reports. Then, we test whether these prosecutors updated toward the reality presented by police,

[18] We sum the seven rescaled beliefs to get the predicted contribution of prosecutor learning to the change in incarceration disparities. This exercise yields an estimate of 0.1 if we estimate the relationship between beliefs and disparities jointly, and 0.2 if we estimate each relationship in seven separate regressions.

leading the disparities in their cases to increase over the course of their careers.

Empirical Design. We estimate racial disparities in incarceration outcomes across prosecutors who handle similar cases but have different levels of certainty in police reports. We first focus on the period before BWCs are introduced:

$$\text{Incarceration}_i = \gamma \text{Black}_i \times \text{Certainty}_{p(i)} + \delta \text{Black}_i + \mu_{p(i)} + X_i' \Omega + v_i \quad \text{before BWCs}, \tag{7}$$

where $\text{Certainty}_{p(i)}$ is the share of cases the prosecutor recalls feeling certain about the defendant's conduct after reading the police report (on a scale from 0 to 1) and $\mu_{p(i)}$ are prosecutor fixed effects. X_i is a vector of case characteristics used to determine case assignment, specifically the "unit" of the case defined by the office, year, and crime type.

We would like to interpret γ as the causal effect of prosecutors' certainty in police reports on the racial disparities in their cases. There are several threats to this interpretation.

First, prosecutors who differ in their trust in the police may handle different kinds of cases. Yet 77% of surveyed prosecutors reported that cases were assigned based on a rotation system in their unit (e.g., Charlotte's drug unit). Furthermore, the composition of a prosecutor's caseload is not related to her estimated impact on incarceration disparities (Figure A.12). Finally, when a prosecutor moves to a different office, her estimated disparate impact moves with her (Figure A.13). This stability suggests that our findings are not driven by non-random case assignment or by match effects with local judges or defense attorneys, both of which would vary across offices.[19]

Second, taking police reports at face value could be correlated with prosecutors' other tendencies and attitudes, which could independently influence the disparities in their cases. Reassuringly, trust in police reports is not significantly related to prosecutors' race, politics, or beliefs about the sources of disparities in justice involvement (Figure A.10(b)). Prosecutors' trust in police reports is also not simply a function of where they work: their

[19]For the mover design, we regress prosecutor p's incarceration disparity in office k on the forecast of this disparity from office j. An estimate of $\hat{\beta} = 1$ indicates an unbiased forecast, and we find $\hat{\beta} = 0.9$. We limit to moves that span judicial districts, so local matches with judges and defense attorneys are disrupted.

office explains only 8% of the variation in beliefs (Figure A.11). The question about certainty in police reports may have been less polarizing because of its neutral phrasing and its placement in the survey *before* any questions about race or bias.

Third, the timing of the elicitations raises concerns about reverse causality. The survey was conducted after many prosecutors had access to BWCs and after all cases in our sample had been resolved. The idiosyncrasies in prosecutors' past cases may, therefore, have influenced their beliefs. Consider a prosecutor who happened to see more BWC footage contradicting officers' accounts of arrests of Black people, and, as a result, dismissed more cases with Black arrestees. These experiences may have caused the prosecutor to express less certainty in police reports on our survey. In this case, the prosecutor's belief was an output of her past experiences rather than an input into her decisions.

We conduct a placebo test to probe whether prosecutors' beliefs — rather than alternative explanations — drive incarceration disparities. We estimate Equation 7 during the period after BWCs are adopted. If prosecutors' beliefs themselves matter, those beliefs should become less predictive of disparities once BWC footage is available. With BWC footage, prosecutors can directly assess the reliability of police reports, allowing them to disregard some reports they otherwise would have trusted. In addition, trusting police reports may matter less if BWC monitoring incentivizes police to write more reliable reports in the first place. By contrast, if the results were driven by non-random case assignment, correlated tastes, or reverse causality, we would expect beliefs to continue to predict disparities.[20] Instead, we find that prosecutors' beliefs about police reliability only predict their disparate impacts before BWCs — and cease to be predictive once there is BWC footage.

Results. Figure 5 shows the relationship between prosecutors' certainty in police reports and the incarceration disparities in their cases. Before BWCs are introduced, prosecutors who are more certain about what happened after reading police reports increase disparities relative to prosecutors in the same unit who are more skeptical of police reports. Our estimate indicates that a prosecutor who is always certain of police reports increases in-

[20] Reverse causality likely makes elicited beliefs *more*, not less, predictive of disparities after BWC adoption — if, for example, idiosyncratic exposure to BWC footage drove our results.

carceratoin disparities by 1.52 pp compared to a prosecutor who always questions them (p-value = 0.048, Column 4, Table 3), relative to an average disparity of 3.1 pp. These results are robust to controlling for the defendant's specific arrest charge, criminal record, and demographics (Table 3), as well as prosecutor race and politics (Table A.6).

Prosecutors who more frequently treat the police report as the ground truth — who presumably put less weight on their prior beliefs — tend to increase disparities in outcomes. This result, therefore, suggests that police reports painted a more racially disparate picture than prosecutors initially believed.

After BWCs are introduced, there is no systematic relationship between prosecutor certainty and their disparate impacts.[21] Visually, we see that all prosecutors in counties with BWCs reduce disparities to the same level as the most skeptical prosecutors in counties without BWCs. Once prosecutors can directly cross-check police reports with video footage — and police can anticipate this — it appears that prosecutors' baseline trust in police becomes less consequential. This pattern suggests that monitoring police acts as a substitute for prosecutors' skepticism of them.

We also find that prosecutor certainty in police reports is more predictive of disparities in cases where police are typically the sole source of information — specifically, in public order, drug, and weapon possession cases. In these cases, prosecutors' trust in police reports strongly predicts disparities (in blue in Figure 5(b)). By contrast, for violent and property cases — where victims and witnesses typically provide independent information about the arrest — prosecutors' trust in police does not predict disparities (in gray). Taken together, prosecutors' beliefs about police predict disparities exactly when police have the most control over the narrative — i.e., when they are not being monitored by BWCs and when they are the primary source of information about the arrest.

[21] The relationship between prosecutor certainty and disparities is significantly different in counties with versus without BWCs (p-value = 0.050, Column 1 of Table A.5).

V.D Mistaken Learning

Over time, a prosecutor who takes police reports at face value may internalize the racial disparities in those reports into her beliefs about the world. Repeatedly reading that Black people's crimes are more serious than white people's may convince her that real differences in crime are larger than she initially thought. This dynamic should be more pronounced before BWCs, when prosecutors cannot monitor the reliability of police reports. In this section, we examine how racial disparities in prosecutors' incarceration decisions evolve as they gain experience, and whether this evolution depends on their certainty in police reports. We focus on the period before BWC adoption and estimate:

$$\begin{aligned}\text{Incarceration}_i =& \omega \text{Black}_i \times \text{Certainty}_{p(i)} \times \text{Tenure}_{p(i),t(i)} + \delta \text{Black}_i \times \text{Tenure}_{p(i),t(i)} \\ &+ \delta \text{Certainty}_{p(i)} \times \text{Tenure}_{p(i),t(i)} + \tau \text{Tenure}_{p(i),t(i)} + \mu_{p(i),\text{Black}_i} + X_i'\gamma + v_i.\end{aligned} \tag{8}$$

The coefficient of interest, ω, captures whether the disparities of more certain prosecutors increase more (or less) with tenure than those of less certain ones. The fixed effects $\mu_{p(i),\text{Black}_i}$ net out stable differences in disparities across prosecutors. X_i is a vector of case characteristics, including office-by-unit fixed effects. Our preferred specification also includes flexible time controls — year-by-race-by-county fixed effects — to ensure the tenure coefficients capture experience rather than time effects.

Figure 6 shows how incarceration disparities vary with tenure for prosecutors with different levels of certainty in police reports. Among more skeptical prosecutors (in the left and middle panels), disparities remain stable with experience. By contrast, among those in the top third of certainty — who take police reports at face value 74-100% of the time — disparities rise sharply with tenure. An additional four years of experience (about one standard deviation) increases disparities by 1.1 pp, which is more than a third of the raw race disparity in incarceration. Table A.7 tests this relationship using a continuous interaction between certainty and tenure (Equation 8). Effect sizes are similar across a variety of specifications, with disparities always increasing significantly more with tenure for more certain prosecutors. This pattern is also robust to controlling for prosecutor race and politics (Table A.8).

This pattern is consistent with prosecutors who take police reports at face value coming to view true differences in crime as larger than they thought. Suggestively, their beliefs became inflated relative to the truth, since prosecutors revise their beliefs downward once BWCs offered a more objective view of arrests (Section V.B). Together, these results suggest that biased beliefs may have originated within the criminal justice system itself.

VI Conclusion

We investigate why decision-makers often fail to account for other people's biases. We model two behavioral mechanisms: decision-makers may take upstream information at face value, and they may hold biased beliefs, which may partly reflect exposure to earlier bias in the system itself. Our test case is prosecutors' response to information from police.

We use the introduction of monitoring from BWCs to show that prosecutors had previously misprocessed information from police. We then unpack mechanisms using an original survey of prosecutors, which we link to their administrative court records. With more exposure to BWC footage, prosecutors revised their beliefs about racial differences in crime downward, suggesting that they had initially overestimated these differences. Nonetheless, we find that prosecutors who took police reports at face value increased disparities relative to those who likely relied more on their own inflated beliefs. Together, these results suggest that officers wrote racially biased police reports before being monitored by BWCs.

Although we focus on prosecutors and police, our findings have implications for other settings where decision-makers rely on intermediaries for information. Similar dynamics may arise at other stages of the criminal justice system, as well as in hiring, credit assessments, and college admissions. In these contexts, monitoring earlier actors — or simply making later decision-makers more aware of the potential for earlier bias — may be an effective way to curb systemic discrimination.

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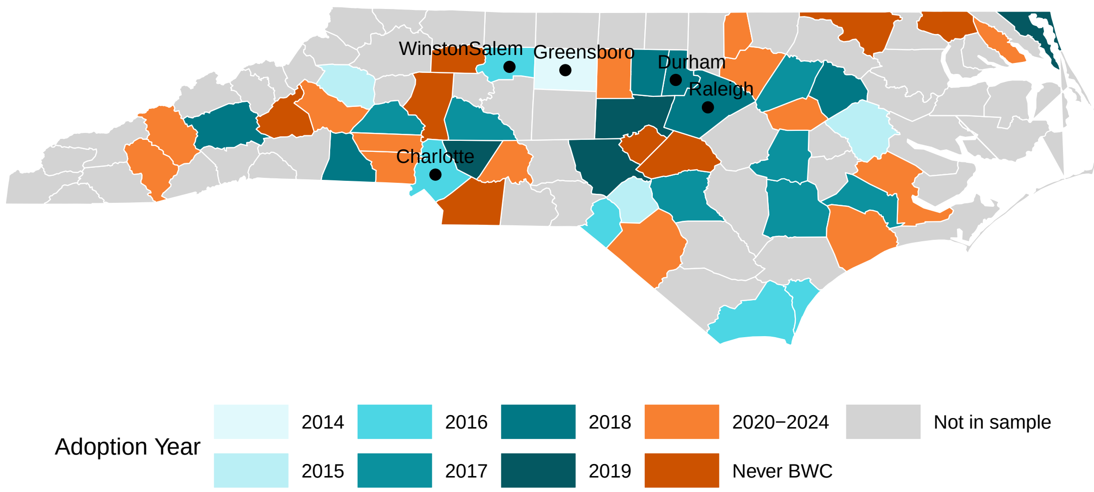
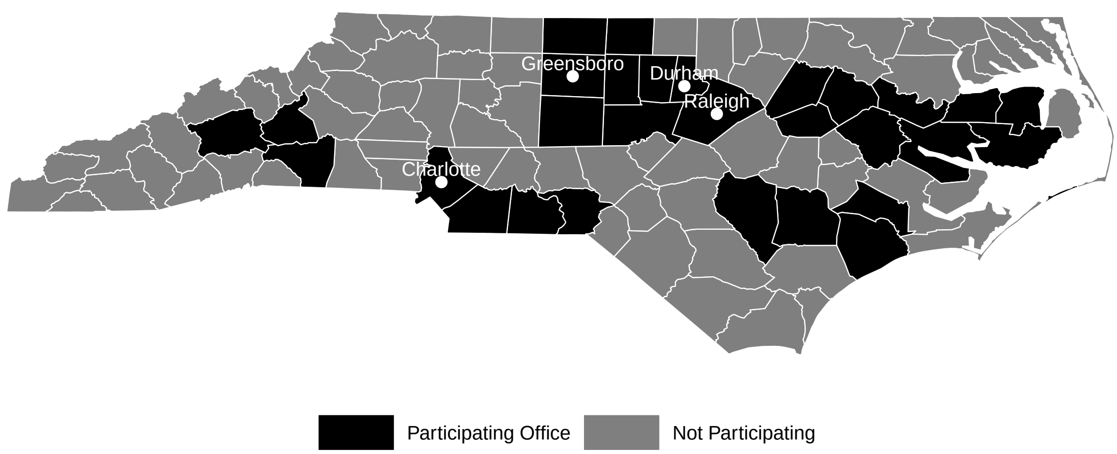
Figure 1: Geographic Coverage of Analyses

(a) Body-Worn Camera Adoption Across North Carolina

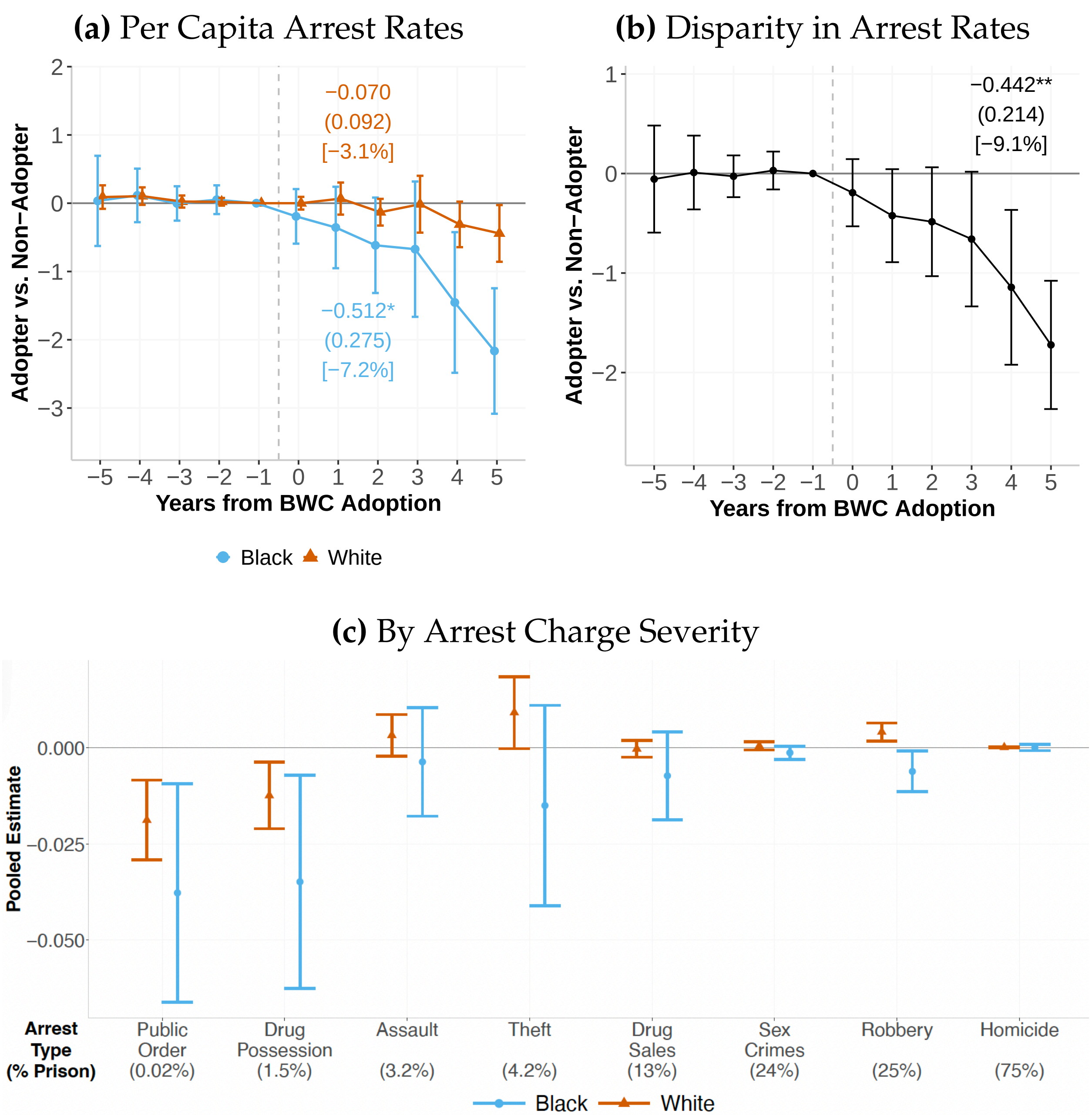


(b) Map of Offices Participating in our Survey



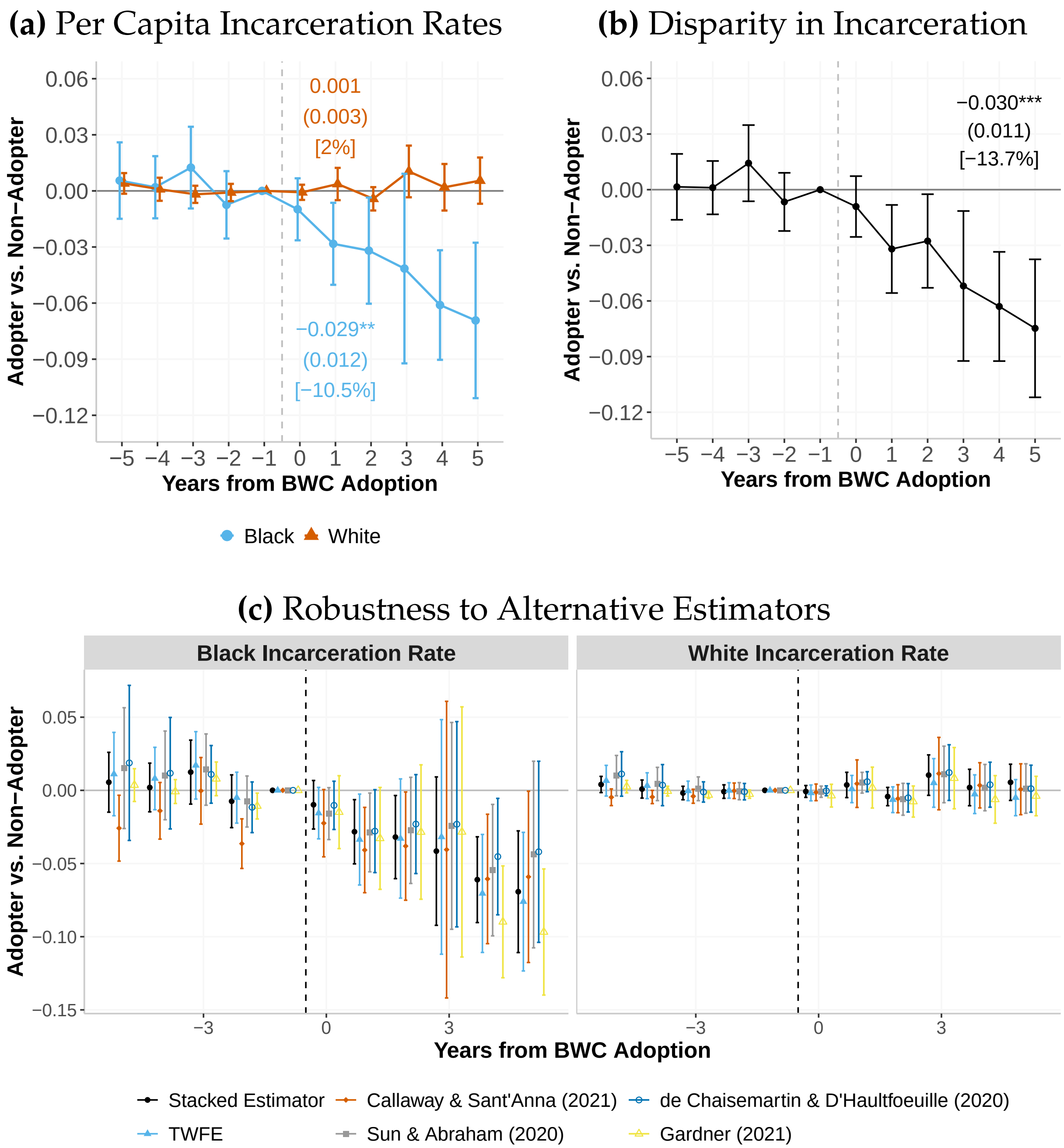
Notes: This figure illustrates the geographic coverage of North Carolina for our two analyses. Map (a) illustrates the staggered timing of body-worn cameras (BWCs) adoption across North Carolina counties. Section III.B describes how we determined the year of BWC adoption, by supplementing existing surveys (BJS, 2016, 2020). The counties shown in blue adopted BWCs between 2014 and 2019, during the time period covered by the state court records data; the counties in orange adopted BWCs, but after the period covered by the court records (2020-2024). For the counties in grey, we were unable to determine whether and when the adopted BWCs. BWC adoption timing is determined for 48 counties and 78% of cases in the state court records. Map (b) illustrates the North Carolina prosecutor offices that participated in our survey. Each color represents a different prosecutor office, some of which span multiple counties. The four largest cities — Charlotte, Raleigh, Greensboro, and Durham — are highlighted.

Figure 2: The Impact of Body-Worn Cameras on Arrest Rates



Notes: This figure illustrates the changes in arrests rates in North Carolina counties that adopt BWCs relative to control counties that have not yet adopted BWCs (or never do). Panel (a) plots per capita arrest rates by race, and panel (b), disparities in these rates. Both show estimates from Equation 4, with the year before BWC adoption as the reference year. All specifications are weighted by population. The annotated coefficients are pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years before; the bracketed numbers show the pooled estimates as percentages of the dependent mean. Figure A.1 shows these event studies with alternative DiD estimators. Panel (c) illustrates BWCs' effects by the severity of the arrest offense. On the x-axis are types of arrest charges are ordered based on the percent chance that the arrest leads to incarceration. The y-axis shows the pooled DiD estimate. All error bars show 95% confidence intervals with standard errors clustered by county. All specifications weight by population. *p<0.1; **p<0.05; ***p<0.01.

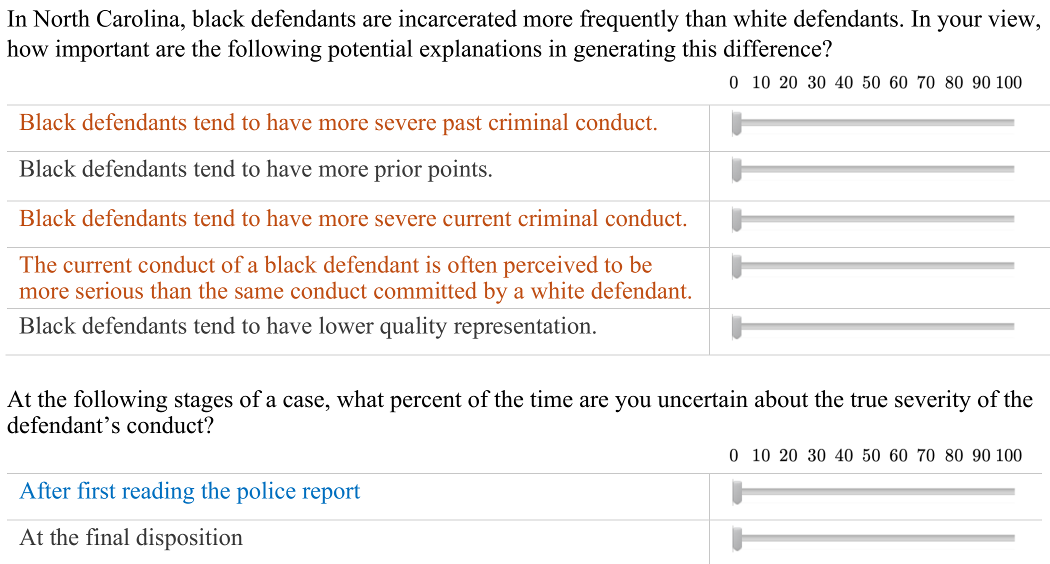
Figure 3: Body-Worn Camera Effects on Incarceration Rates



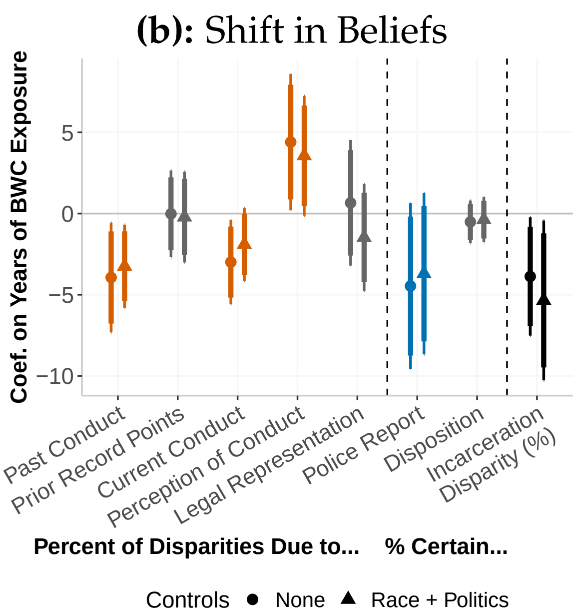
Notes: This figure illustrates the changes in incarceration rates in North Carolina counties that adopt BWCs relative to control counties that have not yet adopted BWCs (or never do). Panel (a) plots per capita incarceration rates by race. Panel (b) shows disparities in incarceration rates. Both show estimates from Equation 4, with the year before BWC adoption as the reference year. The annotated coefficients are pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years before; the bracketed numbers show the pooled estimates as percentages of the dependent mean. Panel (c) shows robustness to using alternative DiD estimators: our preferred stacked approach (also shown in Panel (a)), a two way fixed effects (TWFE) estimator, and the estimators proposed by Callaway and Sant'Anna (2021), Sun and Abraham (2021), De Chaisemartin and d'Haultfoeuille (2020), and Gardner (2021). All error bars show 95% confidence intervals with standard errors clustered by county, and all specifications weight by population. To calculate per capita incarceration rates, we divide the total number of people of each race sentenced to prison by Census population counts, as in Equation 1. *p<0.1; **p<0.05; ***p<0.01.

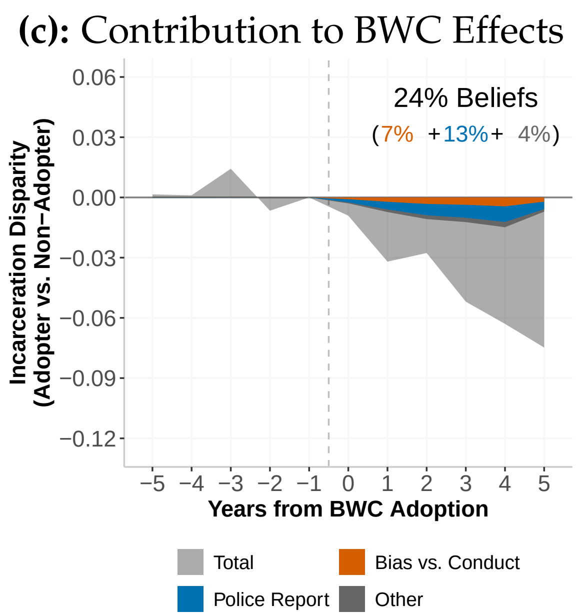
Figure 4: The Relationship Between BWC Exposure & Prosecutors' Beliefs

(a): Question Interfaces



Colors added for clarity

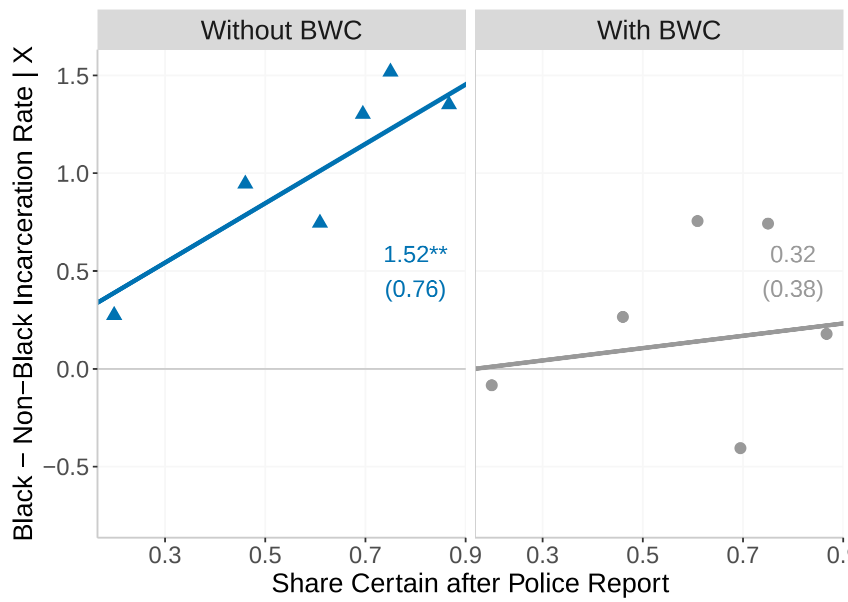




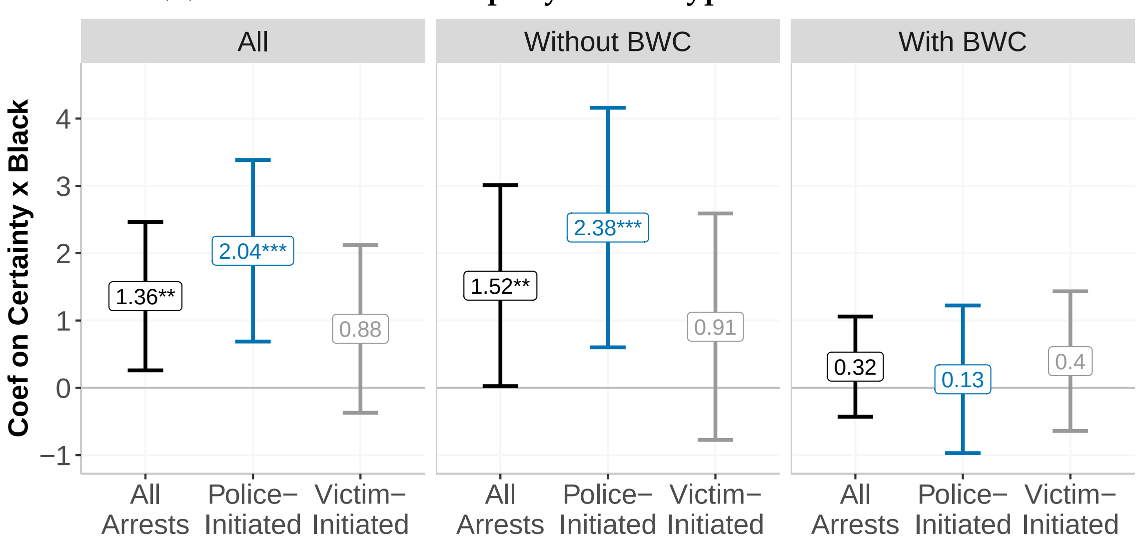
Note: This figure illustrates the relationship between prosecutors' years of exposure to body-worn cameras (BWCs) and their beliefs about their cases as well as their impacts on incarceration disparities. Panel (a) shows the interfaces for our two survey questions, highlighted to match the colors in Panel (b). The first seven points of Panel (b) show the relationship between prosecutors' years of BWC exposure and their elicited beliefs from Panel (a), using Equation 5. The last point reflects the percent change in prosecutors' incarceration disparities with years of exposure to BWCs, using Equation 6, rescaled by the average incarceration rate. Thick (thin) error bars represent 90% (95%) confidence intervals, with standard errors clustered by prosecutor. Panel (c) shows a back-of-the-envelope calculation of the contribution of these belief shifts to BWCs' effect on incarceration disparities assuming that the correlation between prosecutors' beliefs and their incarceration disparities (in Figure A.9) is causal.

Figure 5: Prosecutors' Certainty After Reading Police Reports and Their Disparate Impacts

(a): Disaggregated Relationship

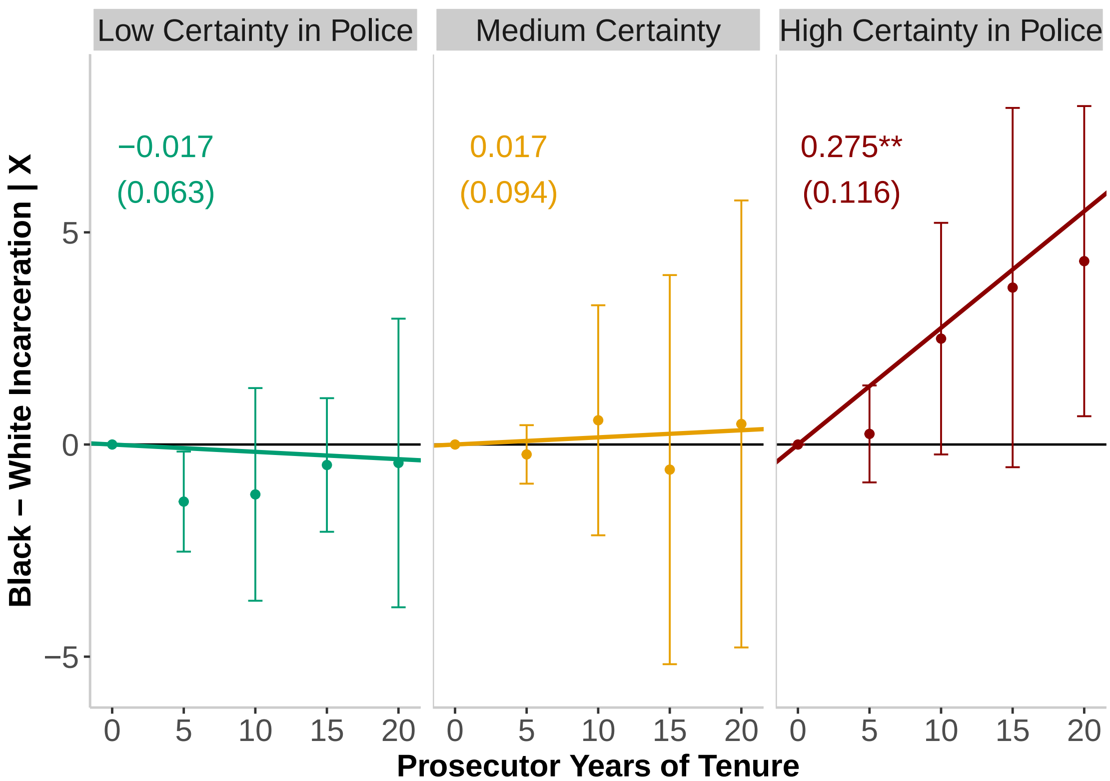


(b): Linear Relationship: by Arrest Type and BWC Status



Notes: This figure shows the relationship between prosecutors' certainty in police reports and the racial disparities in incarceration rates in their cases, conditional on case controls X_i. Specifically, prosecutors' degree of certainty in police reports comes from our survey and reflects the share of cases the prosecutor recalls being certain of the defendant's conduct after reading the police report on a scale from 0 to 1 (see Figure A.10(a) for the question interface). Panel (a) compares prosecutors with and without access to BWCs. Each point reflects the percentage point disparity in incarceration rates for prosecutors in a different quintile of the distribution of certainty in police reports. Panel (b) shows the aggregate linear relationship between beliefs and disparate impacts for all arrests (from Equation **??**), police-initiated arrests (i.e., public order, drug possession and weapon possession arrests), and victim-initiated arrests (i.e., violent and property arrests). The left panel includes the full sample, and the middle and right panels compare prosecutors who work in counties without BWCs to those in counties with BWCs (from Equation **??**). All lines and annotated coefficients reflect linear fits estimated at the case-level. Error bars represent 95% CIs. Standard errors are clustered by prosecutor. Incarceration outcomes come from North Carolina court records from 1995 to 2019. *p<0.1; **p<0.05; ***p<0.01.

Figure 6: How Incarceration Disparities Evolve with Tenure for Prosecutors with Different Levels of Certainty in Police Reports Prior to BWCs



Notes: This figure illustrates how incarceration disparities evolve with tenure across prosecutors with different certainty in police reports, where we limit the sample to years before BWCs were adopted in the given county. Each panel represents a different third of the distribution of certainty: low certainty prosecutors take the police report at face value in 0-49% of their cases; medium certainty prosecutors, in 50-73% of their cases; and high certainty prosecutors, in 74-100% of their cases. Each point represents the incarceration disparities within a different prosecutor tenure group, discretized into 5-year bins. The fit lines and annotated coefficients represent the linear relationship between tenure and incarceration disparities within each tercile of certainty. These results come from a discretized version of Equation 8, where certainty is in thirds rather than continuous. Each specification includes race-specific prosecutor fixed effects, so that the evolution of disparities holds prosecutor selection constant. Each specification also includes office crime-unit fixed effects and race-by-county-by-year fixed effects to allow for place specific shifts in the treatment of race over time (included in the preferred specification in Column 3 of Table A.7). Error bars represent 95% confidence intervals. Standard errors are clustered by prosecutor. Prosecutors' certainty in police reports come from our survey (see Figure A.10(a) for the question interface), and incarceration outcomes come from North Carolina administrative records. *p<0.1; **p<0.05; ***p<0.01.

Table 1: Summary Statistics of the North Carolina Court Records: By BWC Adoption Cohort

	Full Sample (1)	BWC Sample (2)	BWC Adoption Year: Early (2014-16) (3)	BWC Adoption Year: Middle (2017-19) (4)	BWC Adoption Year: Late (2020-24) (5)	BWC Adoption Year: Never (—) (6)
Per Capita Outcomes						
% Arrested	3.19	3.19	3.41	2.94	3.42	2.68
Black	6.67	6.60	6.60	6.59	6.78	6.20
White	2.27	2.18	2.14	1.94	2.71	2.07
% Convicted	1.21	1.19	1.14	1.20	1.32	1.04
Black	2.73	2.65	2.41	2.85	2.73	2.64
White	0.83	0.78	0.66	0.76	1.02	0.78
% Incarcerated	0.11	0.11	0.11	0.11	0.11	0.10
Black	0.31	0.30	0.28	0.32	0.29	0.31
White	0.06	0.06	0.05	0.06	0.08	0.06
County Characteristics						
% Black	23.1	24.3	28.5	24.2	19.8	16.2
% Urban (2010 Census)	57.7	65.5	78.6	68.2	48.8	32.7
% County Voters Reg. Democrat	37.1	38.1	40.3	39.1	35.4	28.8
Prosecutor Demographics						
% Black Prosecutor	9.0	9.1	9.8	9.5	8.4	6.1
% Female Prosecutor	35.4	34.9	30.3	36.8	36.8	41.9
% Registered Democrat	42.0	45.1	41.6	49.9	46.1	37.9
Prosecutor Age (in Years)	39.5	39.0	38.3	38.6	40.7	38.1
Prosecutor Tenure (in Years)	4.8	4.7	4.7	4.5	5.0	4.7
# Cases	3,810,926	2,949,514	1,055,078	1,093,464	593,515	207,457
# Prosecutors	2,408	2,087	726	907	568	330
# Offices	39	33	8	14	12	7
# Counties	100	48	9	17	14	8

Notes: This table presents summary statistics from the North Carolina Administrative Office of the Courts from 1995 to 2019. Column 1 includes all court cases and column 2, all cases in our BWC sample, where we can identify whether and when the county handling the case adopted BWCs. The rest of the table shows summary statistics separately for counties that adopted BWCs in different years —2014–2016 (in Column 3), 2017–2019 (Column 4), 2020-2024 (Column 5), and never adopters (Column 6). Prosecutor demographics and political affiliation come from the state voter records. Per capita incarceration and arrest rates are constructed from the court records and Census population counts by race (U.S. Census Bureau, a).

Table 2: The Effects of Body-Worn Cameras

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(a) Per Capita Arrests						
	Overall	Black	White	Disparity			
Black x BWC				-0.442^{**}	-0.388^{*}	-0.440^{**}	-0.362
				(0.214)	(0.213)	(0.216)	(0.228)
BWC	-0.285^{**}	-0.512^{*}	-0.070	-0.070	0.043	-0.063	-0.059
	(0.129)	(0.275)	(0.092)	(0.092)	(0.089)	(0.094)	(0.104)
Baseline Dependent Mean	3.51	7.14	2.27	3.51	3.51	3.51	3.51
Baseline Disparity				4.86	4.86	4.86	4.86
Percentage Change	-8.10%	-7.18%	-3.10%	-9.08%	-7.97%	-9.04%	-7.44%
	(b) Per Capita Convictions						
	Overall	Black	White	Disparity			
Black x BWC				-0.260^{***}	-0.229^{***}	-0.256^{***}	-0.194^{***}
				(0.071)	(0.070)	(0.074)	(0.074)
BWC	-0.133^{***}	-0.271^{***}	-0.011	-0.011	0.050^{*}	-0.004	-0.029
	(0.040)	(0.086)	(0.031)	(0.031)	(0.028)	(0.032)	(0.037)
Baseline Dependent Mean	1.27	2.67	0.783	1.27	1.27	1.27	1.27
Baseline Disparity				1.89	1.89	1.89	1.89
Percentage Change	-10.5%	-10.1%	-1.35%	-13.8%	-12.2%	-13.6%	-10.3%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(c) Per Capita Incarceration Sentences						
	Overall	Black	White	Disparity			
Black x BWC				-0.030^{***}	-0.030^{***}	-0.031^{***}	-0.022^{**}
				(0.011)	(0.011)	(0.011)	(0.011)
BWC	-0.013^{***}	-0.029^{**}	0.001	0.001	0.005^{*}	0.002	-0.003
	(0.005)	(0.012)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
Baseline Dependent Mean	0.11	0.28	0.056	0.11	0.11	0.11	0.11
Baseline Disparity				0.22	0.22	0.22	0.22
Percentage Change	-11.8%	-10.5%	1.96%	-13.7%	-13.4%	-14.0%	-10.1%
Exclude Never Adopting Counties					✓		
Exclude Neighboring Counties						✓	
Adoption Group x Race x Year Trends							✓
# Treated Counties	26	26	26	26	26	26	26
# Control Counties	48	48	48	48	40	48	48
# Years	11	11	11	11	11	11	11
# Observations	8,034	8,034	8,034	16,068	12,740	15,008	16,068

Notes: This table analyzes the change in per capita arrests, conviction, and incarceration in newly-adopting BWC counties compared to contemporaneous changes for not-yet or never adopting counties in North Carolina. Columns 1−3 estimate Equation 2, where each observation is a county in a given year (and sub-experiment).Columns 4−7 estimate Equation 3, which includes observations for Black and white people. Column 5 excludes never adopting counties, Column 6 excludes neighboring counties, and Column 7 allows for adoption group specific pre-existing trends in arrests. The baseline mean is the average in treated counties before they adopt BWCs. The percentage change in Columns 1−3 divides the coefficient on BWC by this baseline mean. The baseline disparity in Columns 4−7 is the percentage-point race gap in outcomes, and the percentage change divides the coefficient on Black x BWC by this gap. Standard errors are clustered by county. *p<0.1; **p<0.05; ***p<0.01.

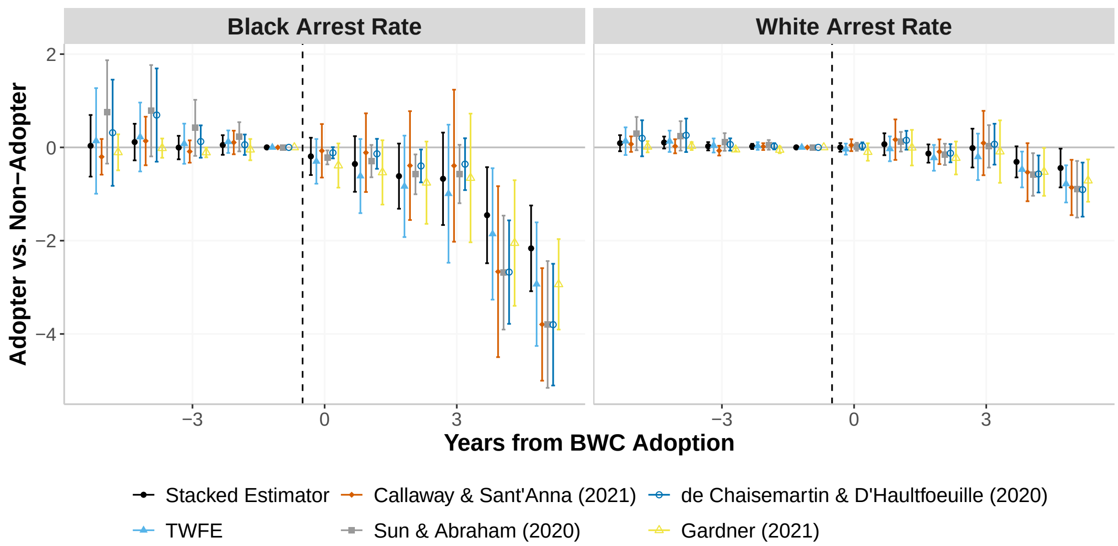
Table 3: The Relationship Between Prosecutors' Certainty After Reading Police Reports and the Incarceration Disparities in Their Cases

	% Incarcerated >6mo								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black x Share Certain After Reading Police Report	1.36**	0.84**	0.77**						
	(0.56)	(0.39)	(0.39)						
No BWC x Black x Share Certain				1.52**	1.11**	1.14*			
				(0.76)	(0.54)	(0.59)			
BWC x Black x Share Certain				0.32	−0.20	−0.38			
				(0.38)	(0.41)	(0.37)			
Police-Init. x No BWC x Black x Certain							2.38***	1.91***	2.25***
							(0.91)	(0.55)	(0.80)
Police-Init. x BWC x Black x Certain							0.13	−0.27	−0.55
							(0.56)	(0.42)	(0.36)
Other Arrest x No BWC x Black x Certain							0.91	0.61	0.47
							(0.86)	(0.63)	(0.62)
Other Arrest x BWC x Black x Certain							0.40	−0.19	−0.30
							(0.53)	(0.59)	(0.56)
Office Crime-Unit FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Prosecutor FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sentencing Guidelines		✓	✓		✓	✓		✓	✓
Charge FE		✓	✓		✓	✓		✓	✓
Demographics		✓	✓		✓	✓		✓	✓
Office Crime-Unit x Black FE			✓			✓			✓
Dependent Mean	6.01	6.01	6.01	6.03	6.03	6.03	6.03	6.03	6.03
Mean Share Certain	0.58	0.58	0.58	0.58	0.58	0.58	0.58	0.58	0.58
Std. Dev. in Share Certain	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.24	0.24
# Cases	505,787	505,787	505,787	479,602	479,602	479,602	479,602	479,602	479,602
# Prosecutors	163	163	163	162	162	162	162	162	162
R^2	0.32	0.43	0.45	0.32	0.43	0.45	0.32	0.43	0.45
Adjusted R^2	0.31	0.40	0.42	0.31	0.41	0.42	0.31	0.41	0.42

Notes: This table analyzes the relationship between prosecutors' certainty in police reports and the racial disparities in incarceration rates in their cases, also shown in Figure 5. Column 1−3 estimate Equation **??** for the full sample of cases handled by prosecutors who participated in the survey; column 4−6 estimate Equation **??** for cases handled by surveyed prosecutors in counties where we can determine BWC adoption timing; and column 7−9 estimate Equation **??** fully interacted with the arrest type, where we also limit to counties with known BWC adoption timing. The sentencing guidelines controls are fixed effects for the defendant's "offense class" at arrest, which is determined by the defendant's most severe arrest charge, interacted with the defendant's criminal record "prior points," which are a weighted sum of prior convictions, and the year of the case. The demographic controls include an indicator for defendant gender and a quartic in defendant age. Police-initiated arrests include drug, public order, and weapon possession offenses. Table A.6 shows robustness to controlling for prosecutors' race and political identities. The survey question interface for prosecutors' certainty in police reports is shown in Figure A.10(a). There are 369,474 cases in counties without BWCs and 110,128 in counties with BWCs. There are 172,795 police-initiated cases (with 133,594 in counties without BWCs and 39,201 in counties with BWCs); and there are 306,807 non-police initiated cases (with 235,880 in counties without BWCs and 70,927 in counties with BWCs). Standard errors are clustered by prosecutor. *p<0.1; **p<0.05; ***p<0.01.

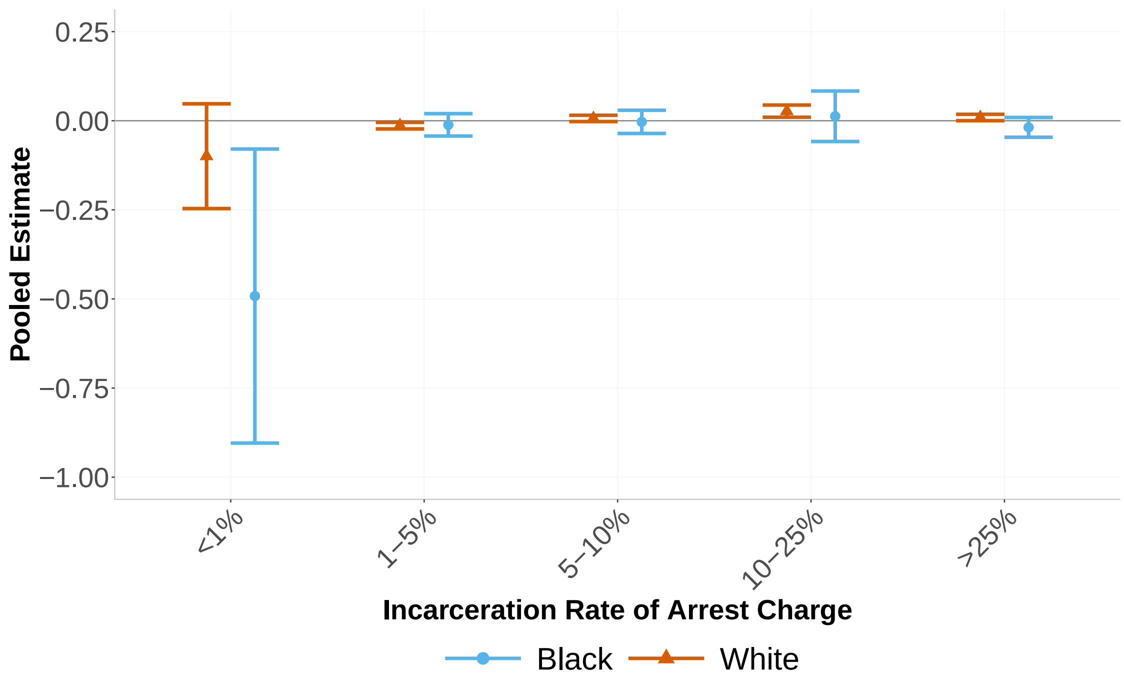
A. Online Appendix

Figure A.1: The Impact of Body-Worn Cameras on Arrest Rates: Robustness to Alternative Estimators



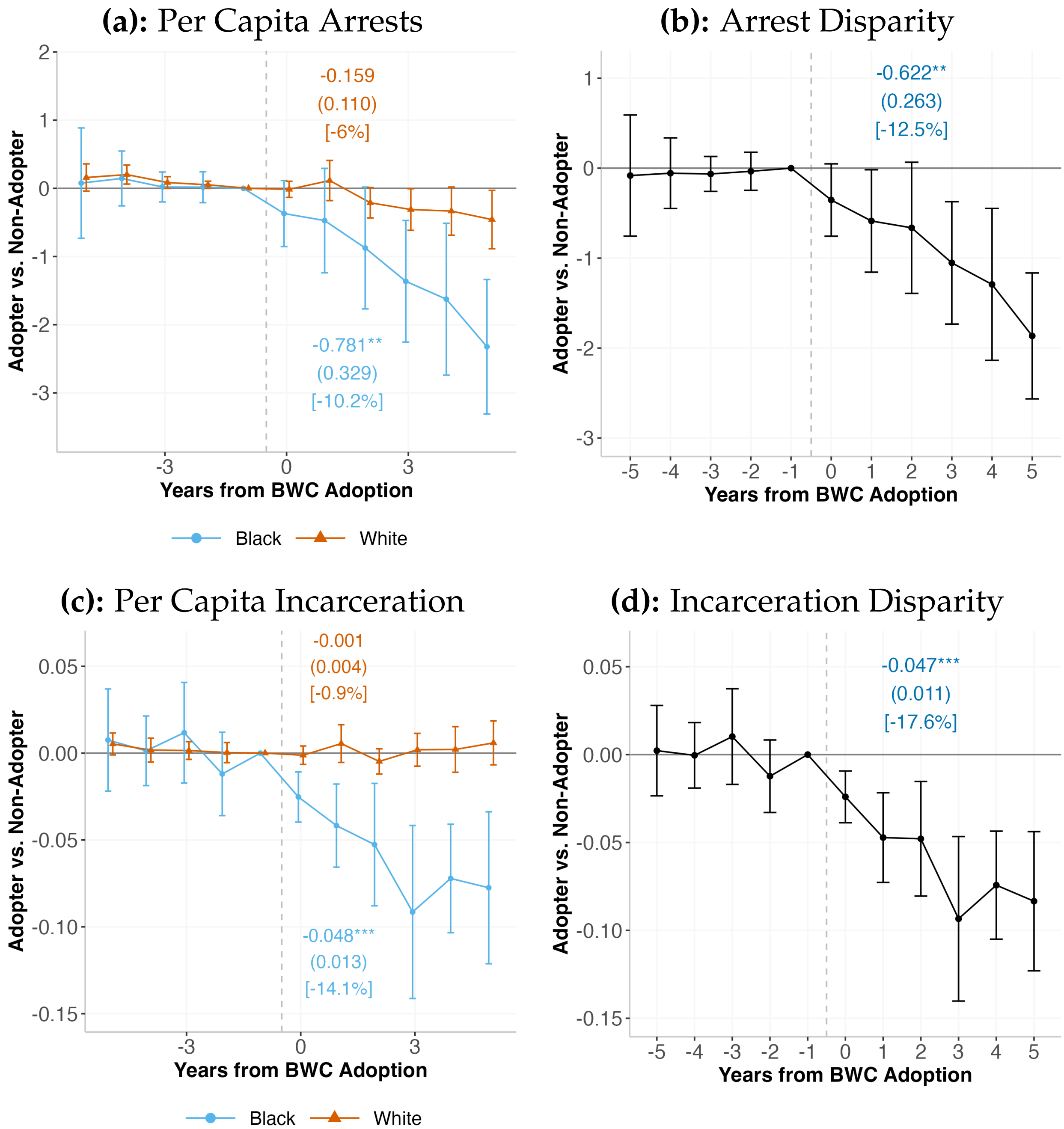
Notes: This figure illustrates the estimated effects of BWCs on arrests rates using alternative DiD estimators: our preferred stacked approach (also shown in Figure 2(a)), a two way fixed effects (TWFE) estimator, and estimators proposed by Callaway and Sant'Anna (2021), Sun and Abraham (2021), De Chaisemartin and d'Haultfoeuille (2020), and Gardner (2021). All error bars show 95% confidence intervals with standard errors clustered by county. *p<0.1; **p<0.05; ***p<0.01.

Figure A.2: Body-Worn Cameras' Effects by Severity of the Arrest Charge



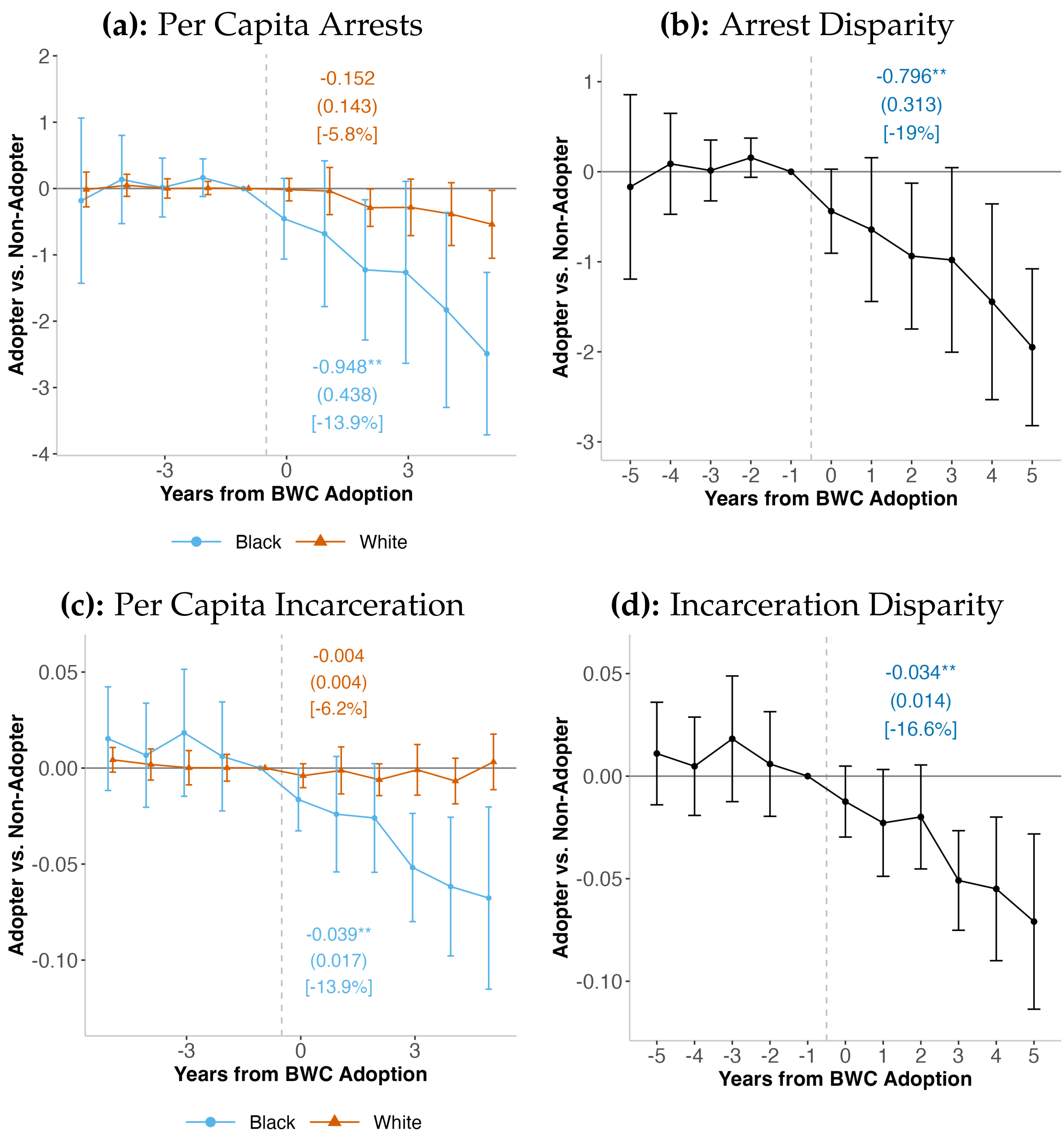
Notes: This figure illustrates the impact of body-worn cameras (BWCs) on per capita arrests rates, separately for arrest charges that have different percent chances of leading to incarceration. All points reflect pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years prior to BWC adoption. All error bars represent 95% confidence intervals with standard errors clustered by county, and all specifications weight by population. *p<0.1; **p<0.05; ***p<0.01.

Figure A.3: Robustness to Excluding Counties with New Elected Sheriffs



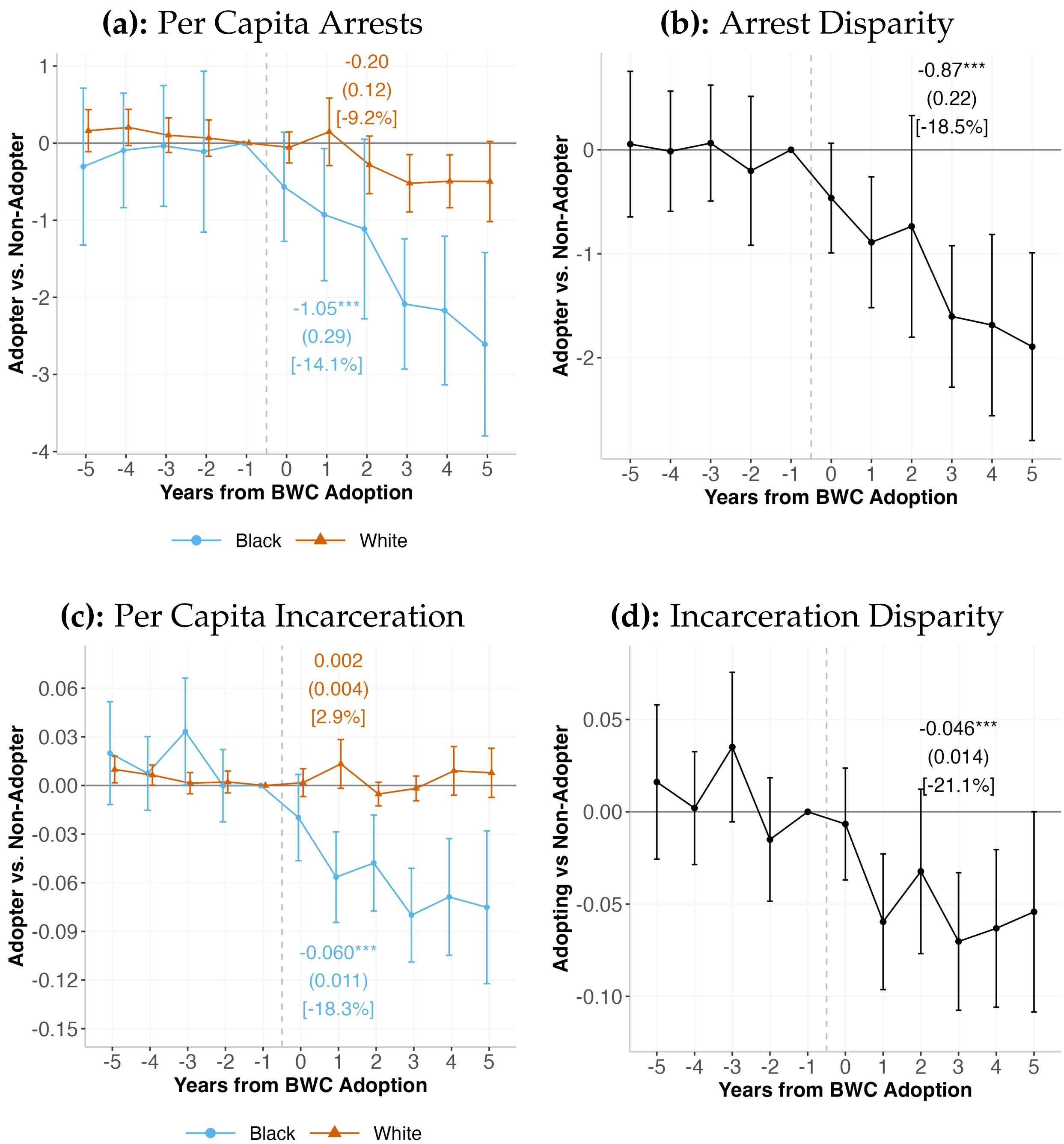
Notes: This figure illustrates the changes in arrests and incarceration rates in NC counties that adopt body-worn cameras (BWCs) relative to counties that have not yet adopted BWCs (or never do) — but restricts to counties whose sheriffs do not change in the election preceding the adoption of BWCs. For instance, Chatham County is omitted since Michael Roberson won the election for sheriff in 2018, the incumbent sheriff was Richard Webster, and Chatham adopted BWCs the following year (in 2019). Since elections occur every four years, counties who elect new sheriffs within three years of BWC adoption are omitted. This restriction means that 7 (of the 26) treated counties are omitted. Sheriff election data from the NC Election Board (NC State Board of Elections). Panel (a) plots per capita arrest rates by race, and panel (b), disparities in these rates. Panel (c) plots per capita incarceration rates by race, and panel (d), disparities. All plots show estimates from Equation 4, with the year before BWC adoption as the reference. All specifications are weighted by population. The annotated coefficients are pooled estimates from Equation 2, comparing the five years post-BWC to the five years before; bracketed numbers show the pooled estimates as percentages of the dependent mean. Error bars show 95% confidence intervals with standard errors clustered by county. *p<0.1; **p<0.05; ***p<0.01.

Figure A.4: Robustness to Excluding Counties with Coincident Policing Reforms



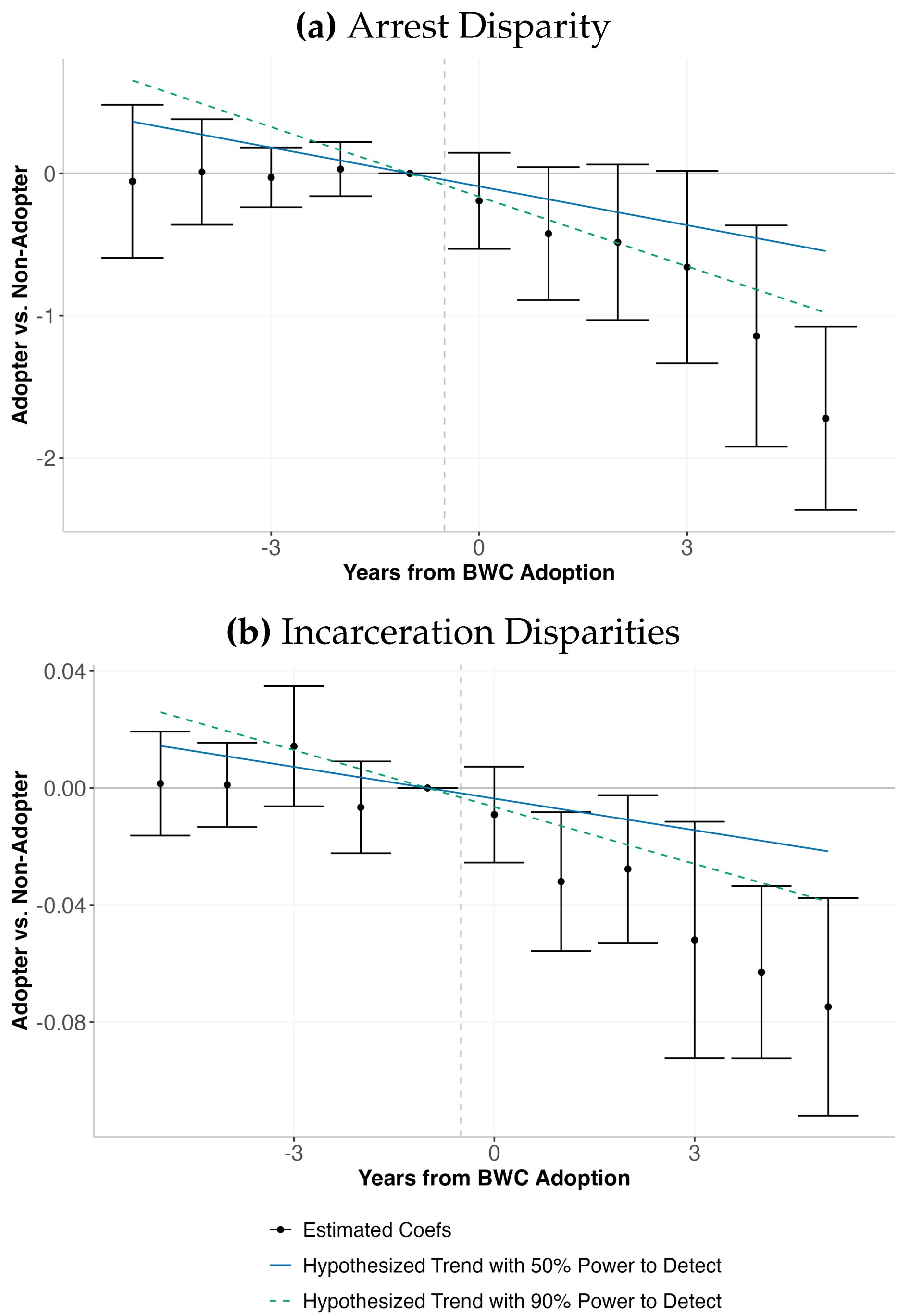
Notes: This figure illustrates the changes in arrests and incarceration rates in NC counties that adopt body-worn cameras (BWCs) relative to counties that have not yet adopted BWCs (or never do) — but omits counties that adopted policing reforms in the year they adopted BWCs. This restriction means that 2 (of the 26) treated counties are omitted. To obtain data on policing reforms, we hand-collected information online about law enforcement agencies reforms and implementation timing. The two leading types of reforms were mandatory deescalation and debiasing trainings for officers. Panel (a) plots per capita arrest rates by race, and panel (b), disparities in these rates. Panel (c) plots per capita incarceration rates by race, and panel (d), disparities. All plots show estimates from Equation 4, with the year before BWC adoption as the reference. All specifications are weighted by population. The annotated coefficients are pooled estimates from Equation 2, comparing the five years post-BWC to the five years before; bracketed numbers show the pooled estimates as percentages of the dependent mean. Error bars show 95% confidence intervals with standard errors clustered by county. *p<0.1; **p<0.05; ***p<0.01.

Figure A.5: Robustness to Limiting to Federally Funded BWCs



Notes: This figure illustrates the changes in arrests and incarceration rates in North Carolina counties that adopt BWCs using federal grants relative to control counties that have not yet adopted BWCs (or never do). Nine of the twenty-six BWC adoption events were federally funded. Panel (a) plots per capita arrest rates by race, and panel (b), disparities in these rates. Panel (c) plots per capita incarceration rates by race, and panel (d), disparities in these rates. All plots show estimates from Equation 4, with the year before BWC adoption as the reference year. All specifications are weighted by population. The annotated coefficients are pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years before; the bracketed numbers show the pooled estimates as percentages of the dependent mean. All error bars show 95% confidence intervals with standard errors clustered by county. To calculate per capita rates, we divide the total number of observed arrests and incarceration sentences for each race by Census population counts, as in Equation 1. *p<0.1; **p<0.05; ***p<0.01.

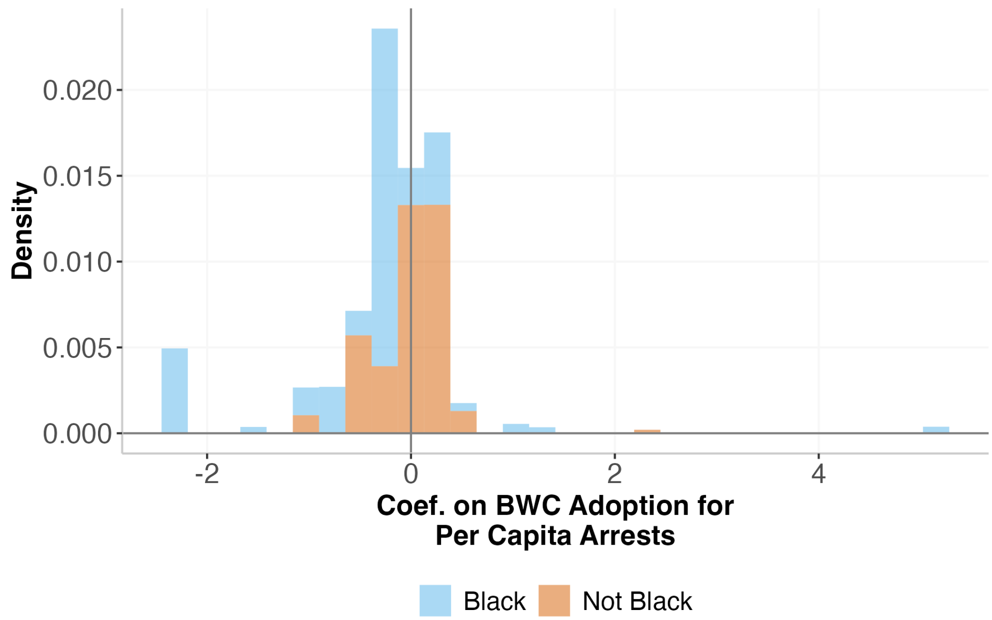
Figure A.6: Roth (2022) Robustness to Differential Trends



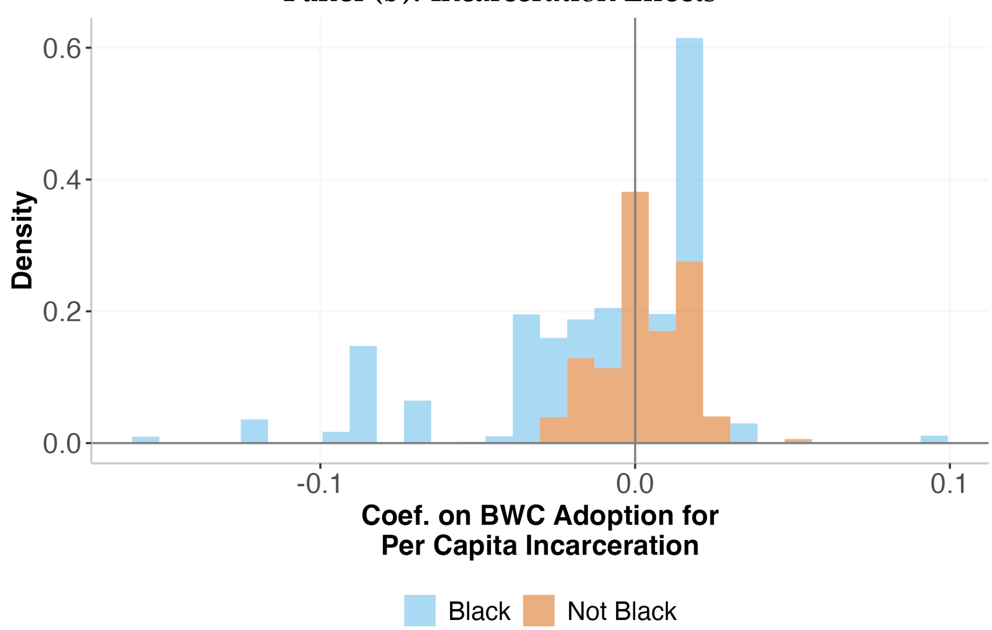
Notes: This figure investigates the robustness of the results to plausible pre-existing trends in adopting versus non-adopting counties. The tests proposed by Roth (2022) juxtaposes the estimated event study coefficients (in black) with worst-case pre-existing trends that we would have 50% power to detect (in blue). For reference, we also plot the worst-case hypothesized trend that we would have 90% power to detect in green dashed lines. Panel (a) focuses on arrest disparities and (b) on incarceration disparities.

Figure A.7: Distribution of Changes in Per Capita Arrest Incarceration Rates around Body-Worn Camera Adoption

Panel (a): Arrest Effects

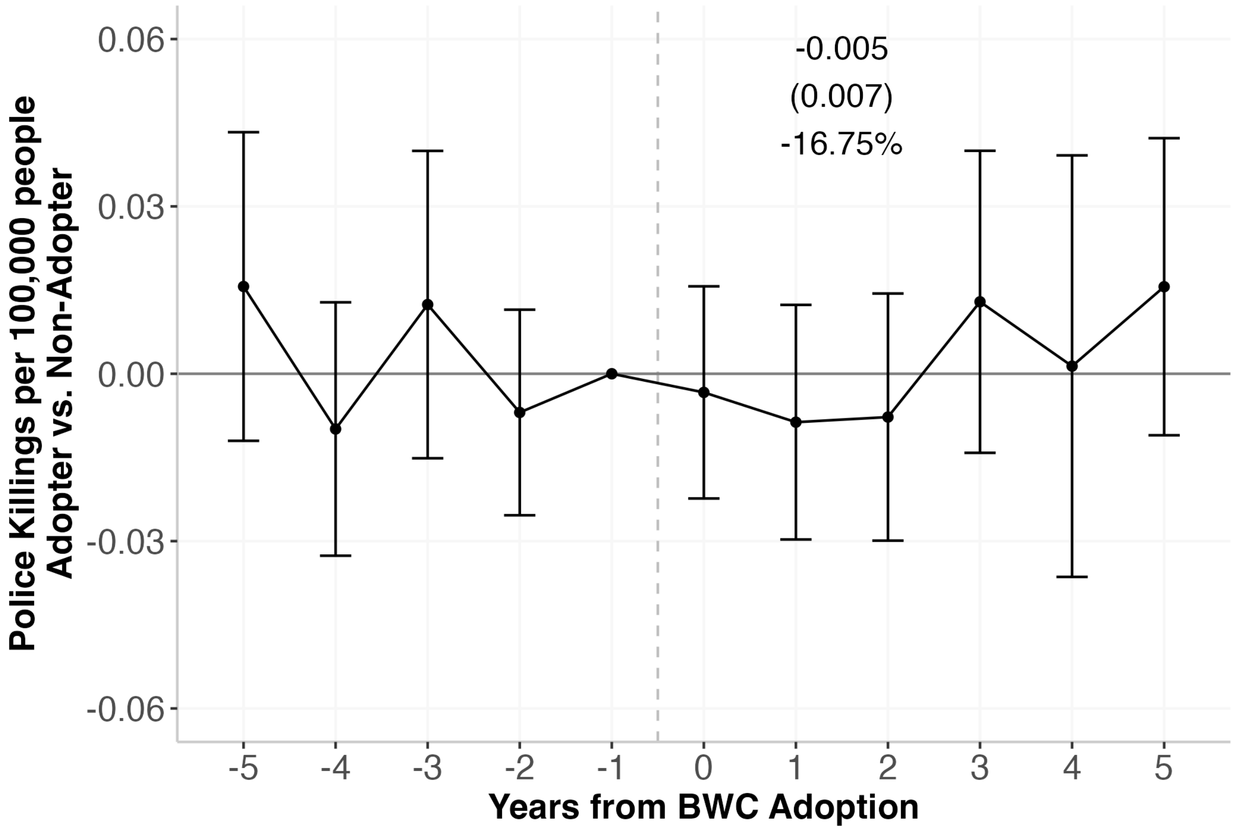


Panel (b): Incarceration Effects



Notes: This figure shows the distribution of estimated effects of BWC adoption on per capita arrest and incarceration rates for Black and non-Black residents. Each estimate comes from Equation 2 and compares one of the treated, adopting counties to all the not-yet adopters.

Figure A.8: Changes in Police Killings Around BWC Adoption in North Carolina



Notes: This figure illustrates the impact of body-worn cameras (BWCs) on police killings of civilians in North Carolinia. The error bars represent 95% confidence intervals with standard errors clustered by county. The annotated coefficients reflect pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years prior to BWC adoption. The bracketed numbers show the coefficients as percentages of the dependent mean.

Figure A.9: The Relationship Between Prosecutors' Beliefs & Incarceration Disparities in Past Cases

(a): Question Interfaces

In North Carolina, black defendants are incarcerated more frequently than white defendants. In your view, how important are the following potential explanations in generating this difference?

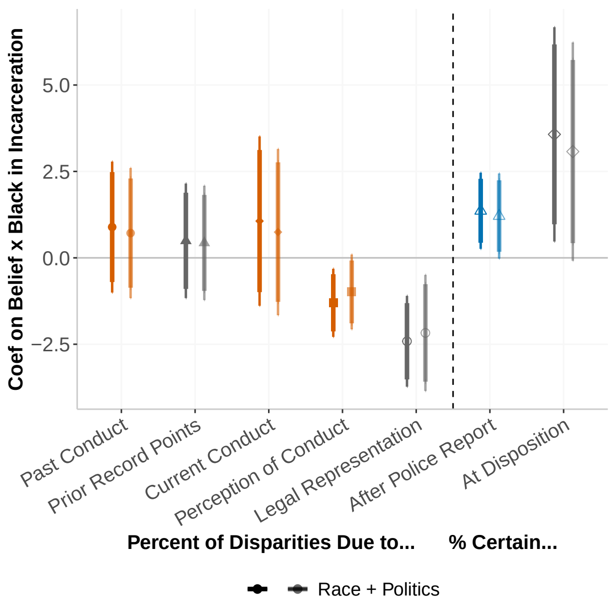
	0 10 20 30 40 50 60 70 80 90 100
Black defendants tend to have more severe past criminal conduct.	
Black defendants tend to have more prior points.	
Black defendants tend to have more severe current criminal conduct.	
The current conduct of a black defendant is often perceived to be more serious than the same conduct committed by a white defendant.	
Black defendants tend to have lower quality representation.	

At the following stages of a case, what percent of the time are you uncertain about the true severity of the defendant's conduct?

	0 10 20 30 40 50 60 70 80 90 100
After first reading the police report	
At the final disposition	

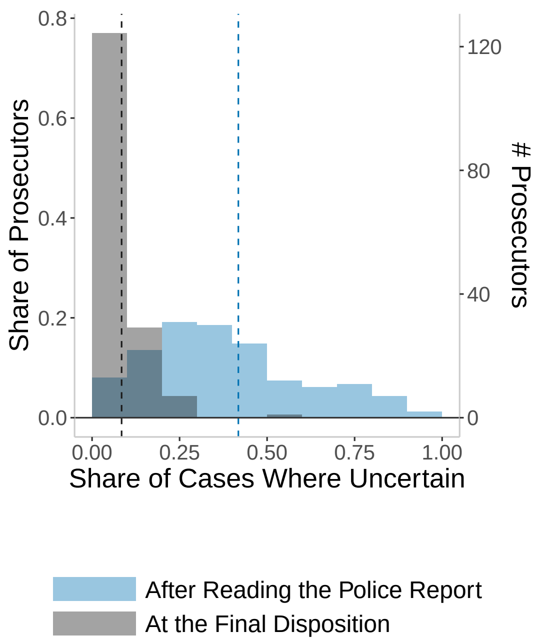
Colors added for clarity

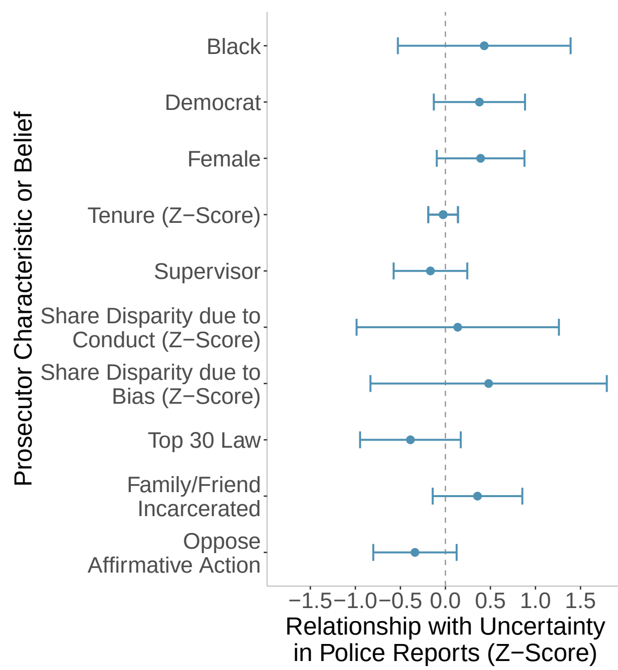
(b): Relationship with Disparities



Note: This figure illustrates the relationship between prosecutors' elicited beliefs in our 2020 survey and their estimated impacts on incarceration disparities in their cases from 1995 to 2019. Panel (a) shows the interface for two survey questions: one about the sources of racial disparities in criminal-justice outcomes and the other about the prosecutors' certainty about what happened at different points in the case. We have highlighted each of the sliders in colors to match those in Panel (b). In Panel (b), each point represents the relationship between the elicited belief (on a scale from 0 to 1) and the prosecutor's incarceration rates for Black versus white defendants relative to others' in their crime unit, using a version of Equation **??**. Wide error bars represent 90% confidence intervals, and thin error bars represent 95% confidence intervals. Standard errors are clustered by prosecutor.

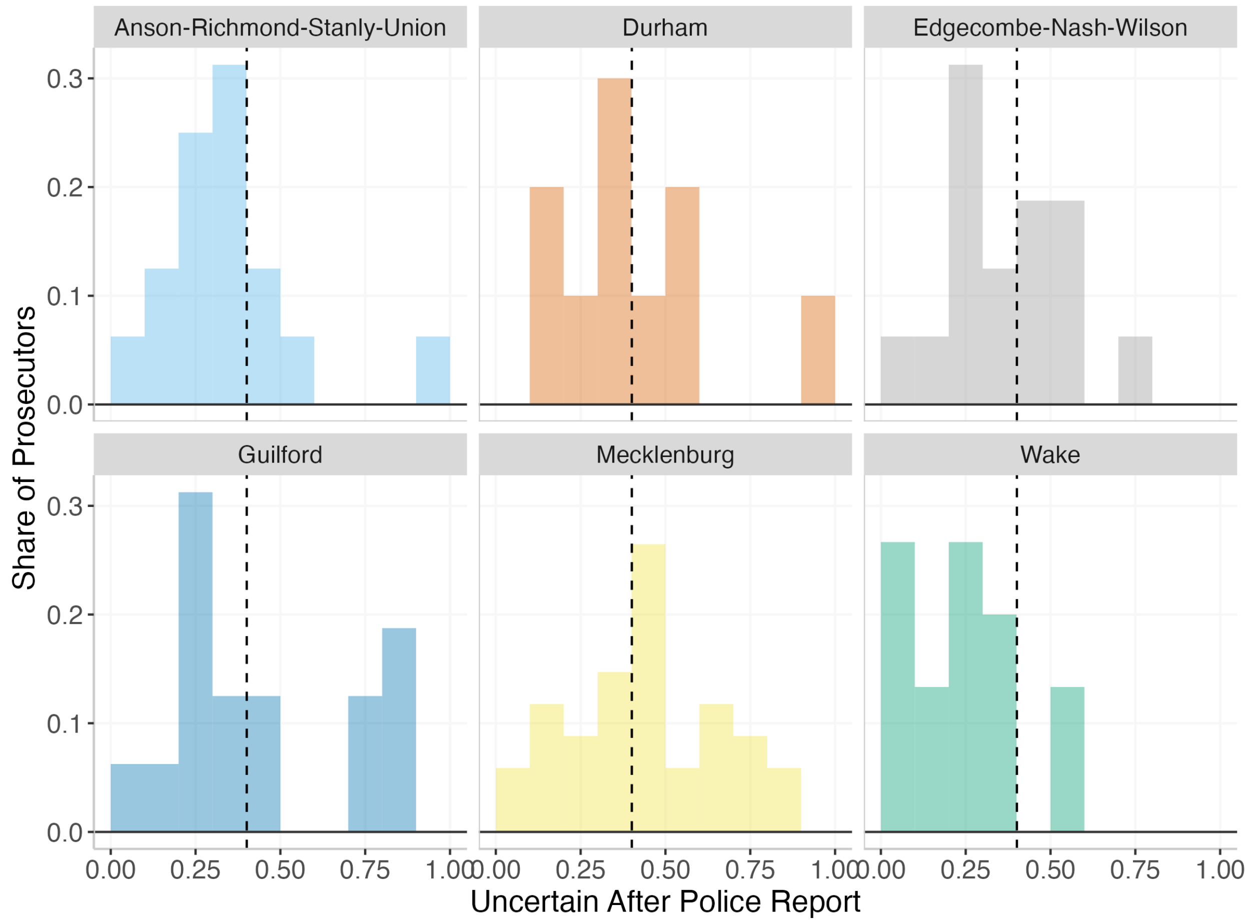
Figure A.10: Prosecutors' Certainty in Police Reports





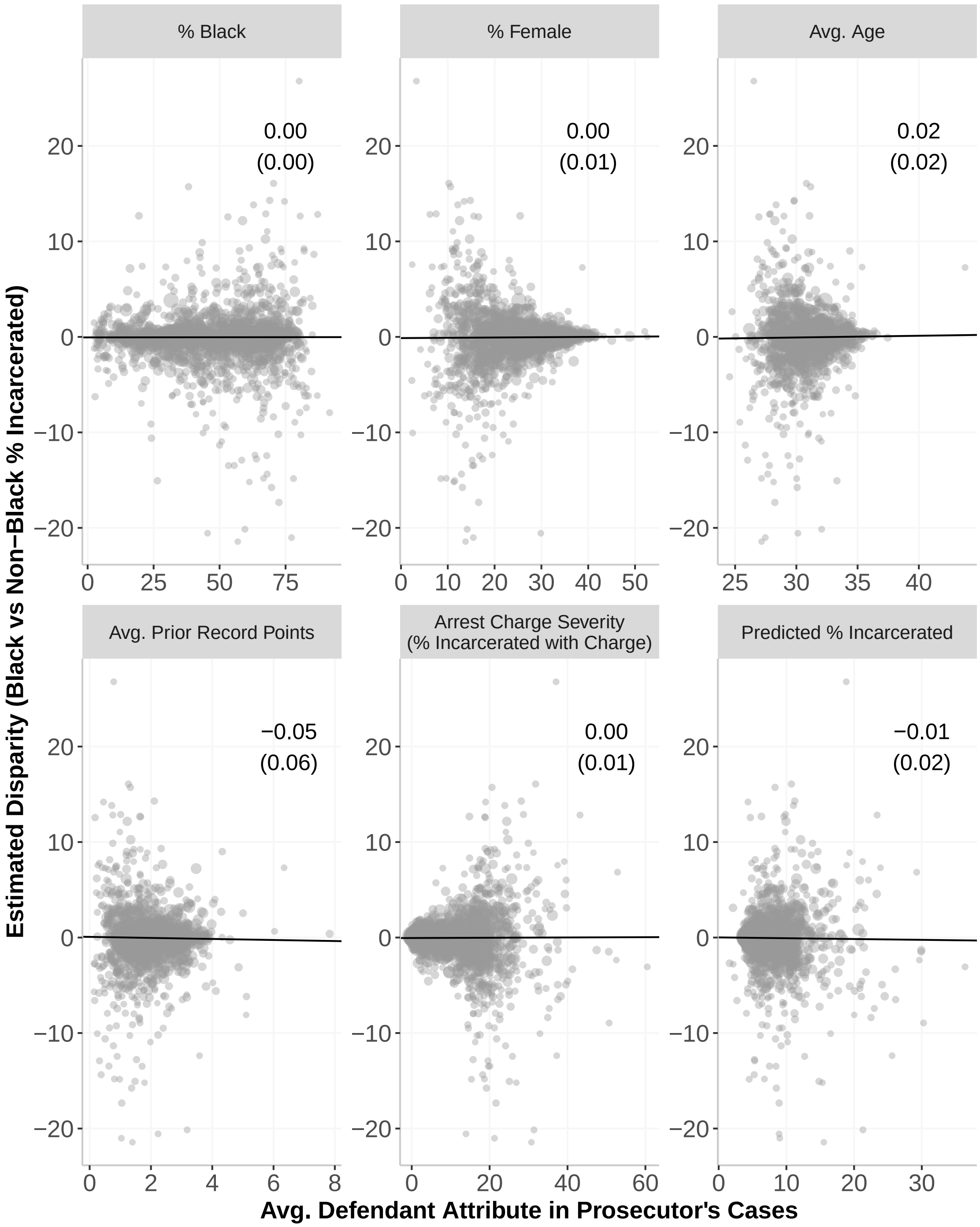
Notes: This figure summarizes prosecutors' degree of certainty about the defendant's conduct after reading the police report. Panel (a) shows the distribution of responses among the 163 prosecutors who answered the question. Panel (b) shows the coefficients from separate regressions that regress the z-score of prosecutors' uncertainty after reading police reports on the prosecutor characteristic or belief. Data on prosecutors' race and political affiliation come from the state voter records. All other characteristics come from our survey of prosecutors. The sixth and seventh points show prosecutors' beliefs about the sources of racial disparities in the criminal justice system — specifically, how much disparities reflect racial differences in conduct (the sixth point) and how much disparities reflect bias in how Black people's conduct is perceived (the seventh point). Figure A.14 shows the relationship between prosecutors' certainty in police reports and prosecutors' beliefs about the sources of disparities for each of the five sub-questions in the survey question about the sources of disparities (see panel (a) of Figure 4 for the question interface). Error bars represent 95 percent confidence intervals.

Figure A.11: Uncertainty Varies Widely Within Office



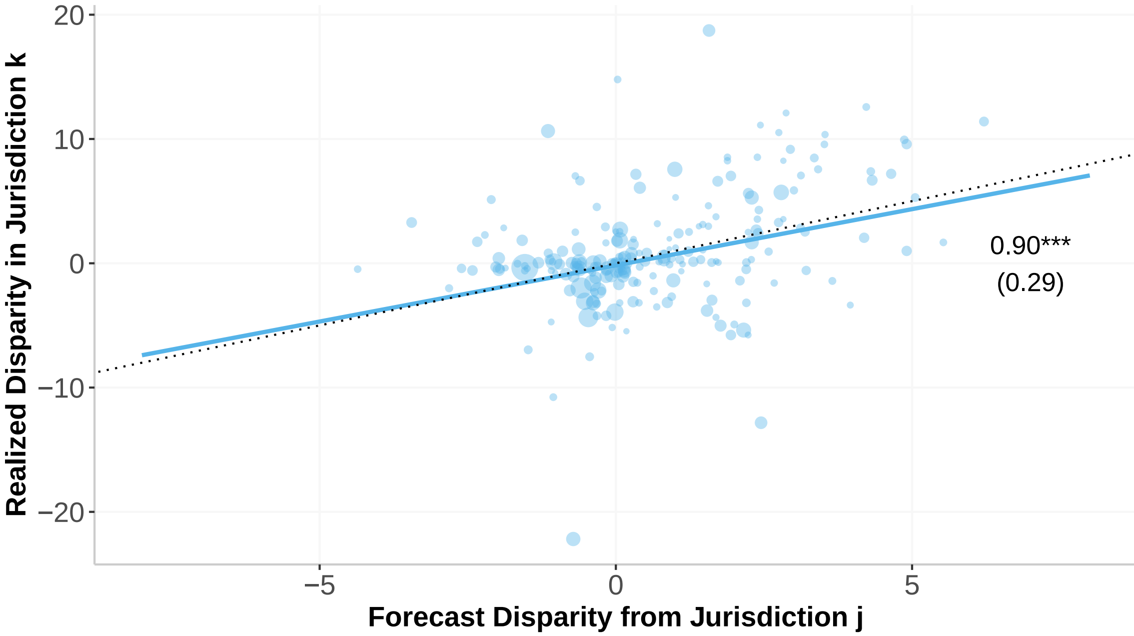
Notes: This figure shows the empirical distribution of prosecutors' uncertainty about the accuracy of police reports across the six largest participating offices in the survey. The adjusted R^2 indicates office explains 8% of variation in uncertainty.

Figure A.12: Balance in Case Characteristics Across Prosecutors with Different Disparate Impacts



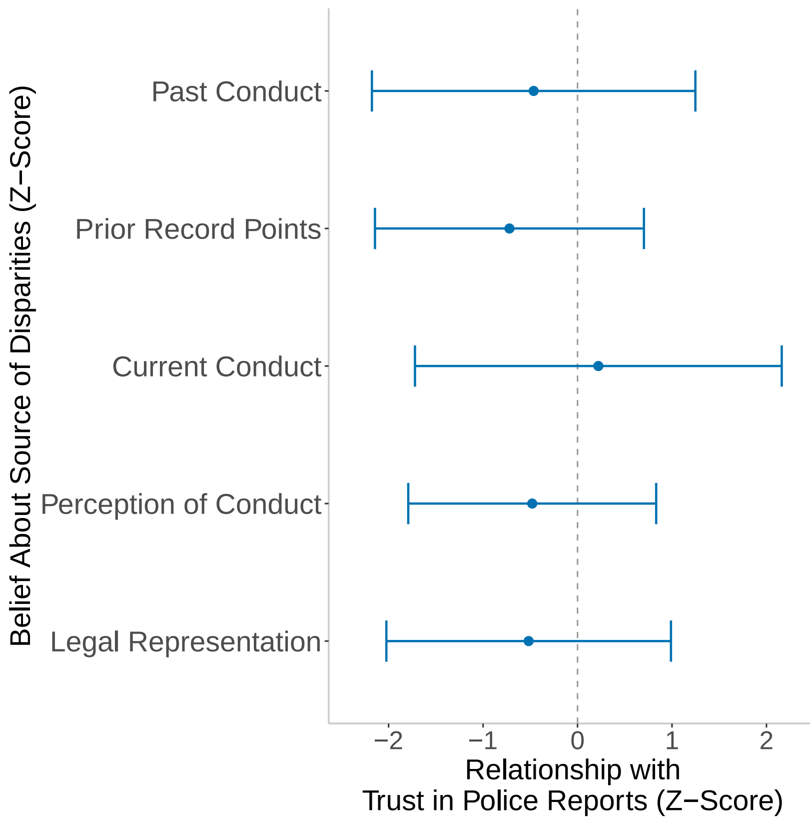
Notes: This figure illustrates the balance in case characteristics across prosecutors with different estimated impacts on racial disparities in incarceration rates. The x-axis plots an attribute of a prosecutor's cases, such as the percent of her cases with a Black defendant or a female defendant. The y-axis plot the prosecutor's estimated impact on racial disparities, residualized by unit and sentencing guidelines recommendations. Each point reflects a different prosecutor, limited to the 1,695 prosecutors who handled at least twenty cases of Black defendants and twenty cases of non-Black defendants. The annotated coefficient reflects the estimated linear relationship, weighted by the prosecutor's caseload. Standard errors are clustered by prosecutor. *p<0.1; **p<0.05; ***p<0.01.

Figure A.13: Mover Validation of Prosecutors' Disparate Impacts



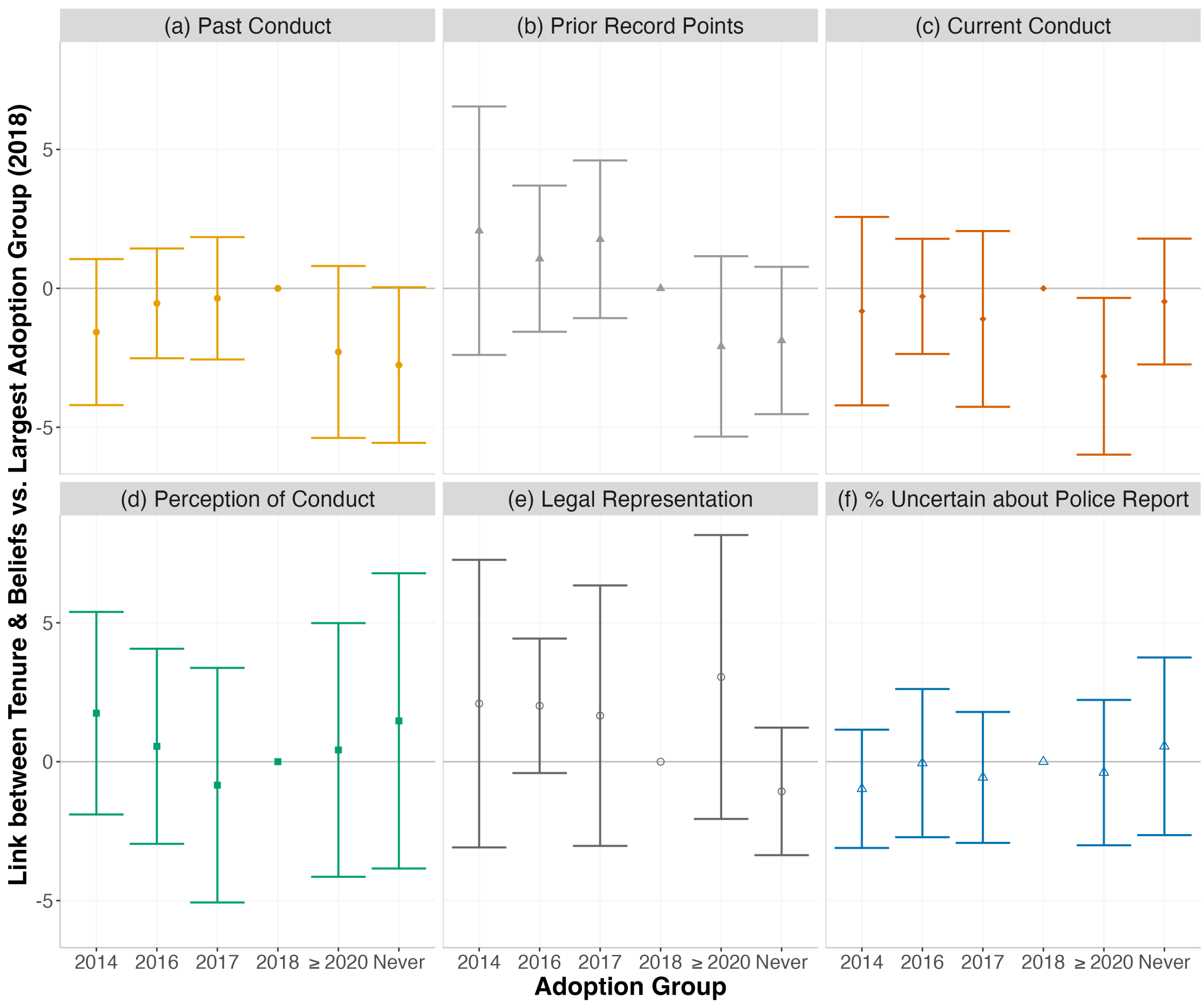
Notes: This figure illustrates the mover design to validate prosecutors' estimated effects on racial disparities in incarceration rates, after residualizing by unit and sentencing guidelines recommendations. The figure tests the validity of the design, using the 87 prosecutors who moved across judicial districts. The x-axis plots the forecast of prosecutor p's impact on racial disparities based on her outcomes in office j and shrinking to account for sampling variation. The y-axis plots her realized, residual incarceration disparity in office k. The dotted black line reflects the 45° line, which would be the fit line if forecasts were unbiased predictions. The light blue line plots the fit line, and the annotated coefficient shows the slope of the fit line. Standard errors are clustered by prosecutor. Each point reflects a bilateral move for a prosecutor who handled at least twenty cases of Black defendants and twenty cases of non-Black defendants in both office j and k. *p<0.1; **p<0.05; ***p<0.01.

Figure A.14: The Relationship Between Prosecutors' Certainty in Police Reports and Their Beliefs About the Sources of Disparities



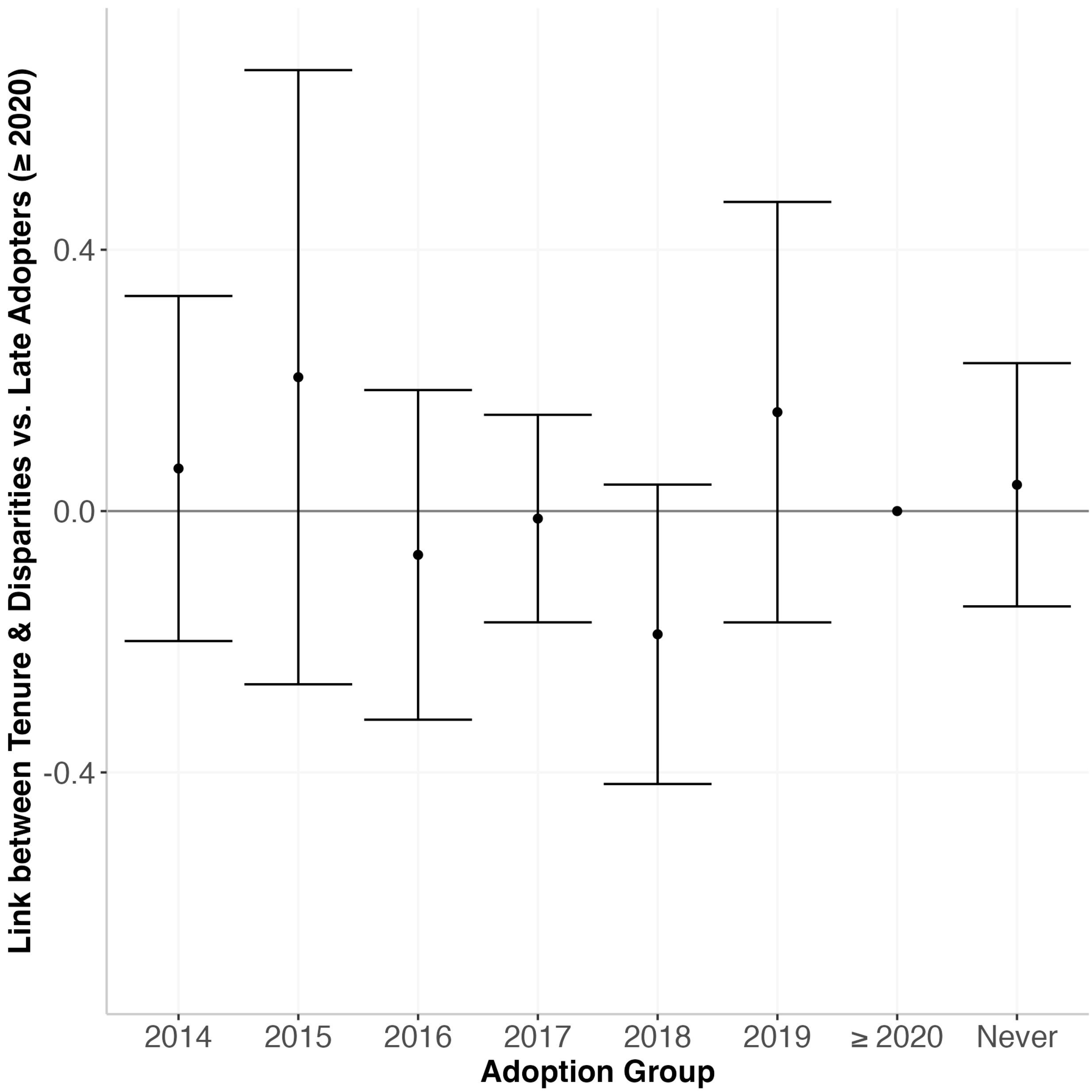
Notes: This figure illustrates the relationship between prosecutors' certainty in police reports and their beliefs about the sources of racial disparities. Both questions come from our survey of prosecutors. The interface for the question about certainty in police reports is shown in Figure A.10(a) and for the question about the sources of disparities in Figure 4(a). Each point reflects the coefficients from a separate regression that regresses prosecutors' certainty in police reports on one of the five beliefs about the source of disparities. Error bars represent 95 percent confidence intervals. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure A.15: Placebo Check: Relationship Between Tenure & Beliefs Among Prosecutors Hired Before BWC Adoption



Notes: This figure evaluates whether there is a differential relationship between tenure and beliefs across counties with different BWC adoption timing for prosecutors who were all hired before their county adopted BWC and so do not differ in their BWC exposure. The x-axis plots the BWC adoption year of the focal group; the y-axis plots the differential relationship between tenure and beliefs in the adoption group relative to the largest adoption group (prosecutors in counties that adopted BWCs in 2018). The adoption groups for 2015 (2019) is omitted because it only has one (two) prosecutor(s). The error bars represent 95% confidence intervals with standard errors clustered by prosecutor. In Panels (a)-(e), the dependent mean is prosecutor's beliefs about the percent of racial disparities in criminal justice outcomes that can be attributed to a given factor (see Figure 4(a) for the question interface). In Panel (f), the dependent variable is the percent of cases that the prosecutor recalls feeling uncertain about the defendant's true conduct after reading the police report (see Figure A.10(a) for the question interface).

Figure A.16: Placebo Check: The Relationship Between Tenure and Incarceration Disparities Before Any County Adopted BWCs



Notes: This figure evaluates whether there is a differential relationship between tenure and incarceration disparities across counties with different BWC adoption timing in the years before any North Carolina county adopted BWCs (from 1995 to 2014). The x-axis plots the BWC adoption year of the focal group; the y-axis plots the differential relationship between tenure and incarceration disparities in the adoption group relative to counties that adopted BWCs between 2020 and 2024, always restricting to the years before any county adopted BWCs. The error bars represent 95% confidence intervals with standard errors clustered by prosecutor.

Table A.1: Summary Statistics in the Years Before Any BWCs (2010-2013)

			BWC Adoption Year			
	Full Sample	**BWC Sample**	**Early** (2014-16)	**Middle** (2017-19)	**Late** (2020-24)	**Never** (—)
	(1)	(2)	(3)	(4)	(5)	(6)
Per Capita Outcomes						
% Arrested	4.23	4.23	4.58	3.85	4.52	3.69
Black	8.71	8.68	8.76	8.60	8.83	8.20
White	2.99	2.88	2.89	2.53	3.59	2.86
% Convicted	1.58	1.56	1.54	1.55	1.71	1.31
Black	3.54	3.49	3.27	3.74	3.52	3.21
White	1.06	0.99	0.86	0.97	1.31	0.98
% Incarcerated	0.12	0.12	0.12	0.12	0.12	0.11
Black	0.33	0.33	0.31	0.35	0.32	0.34
White	0.07	0.06	0.05	0.06	0.08	0.07
County Characteristics						
% Black	23.3	24.4	28.5	24.2	19.2	16.8
% Urban (2010 Census)	59.1	66.9	79.3	68.9	49.5	31.6
% County Voters Reg. Democrat	37.4	38.4	40.3	39.3	35.2	29.6
Prosecutor Demographics						
% Black Prosecutor	9.1	8.9	9.7	9.5	8.3	2.2
% Female Prosecutor	39.4	38.3	34.4	37.1	42.2	54.1
% Registered Democrat	41.7	45.9	42.4	50.5	51.1	29.0
Prosecutor Age (in Years)	41.0	40.3	40.0	39.3	42.1	40.0
Prosecutor Tenure (in Years)	3.6	3.6	3.7	3.4	3.6	3.4
# Cases	1,465,912	1,148,992	425,644	440,612	213,612	69,124
# Prosecutors	1,721	1,462	511	617	362	194
# Offices	39	33	8	14	12	7
# Counties	100	48	9	17	14	8

Notes: This table reproduces the summary statistics from Table 1 but restricts to the years before any county adopted BWCs, 2010 through 2013.

Table A.2: Balance in County & Prosecutor Traits Around BWC Adoption

	Δ_{DiD}	**Baseline Mean**	**Percent of Baseline Mean**
County Demographics			
Log Population	0.01 (0.01)	12.75	0.07%
% Black	0.14 (0.17)	25.43	0.57%
County Economics			
% Unemployed	0.03 (0.11)	6.90	0.36%
% Poverty	0.10 (0.27)	15.98	0.63%
Log Yearly Earnings ($)	0.00 (0.01)	8.20	0.02%
Black Log Yearly Earnings ($)	-0.01 (0.01)	7.73	-0.09%
White Log Yearly Earnings ($)	0.01 (0.01)	8.30	0.08%
County Politics			
% Democratic Votes	-1.90* (1.15)	54.64	-3.47%
Prosecutor Characteristics			
% First-Year Hires	-0.59 (0.93)	11.08	-5.35%
% Departures	0.00 (0.51)	1.70	0.06%
% Democrat	0.49 (0.72)	13.34	3.68%
% Black	-0.03 (0.36)	4.18	-0.63%
# Treated Counties	27		
# Control Counties	22		
# Years	11		
# Observations	8,501		

Notes: This table shows changes in county and prosector characteristics in newly-adopting BWC counties relive to contemporaneous changes for not-yet or never adopting counties in NC. Column 1 estimates Equation 2, where each observation is a county in a given year (and sub-experiment), and the dependent variable is the characteristic in the given row, aggregated to the county by year level. Column 2 is the mean of each characteristic among treated counties before adopting BWCs, and Column 3 shows the change in each characteristic as a percent of the pre-period mean. County demographics use population counts by race (U.S. Census Bureau, a). County unemployment rates come from the NC Department of Commerce (NC Department of Commerce); poverty rates, from the Census's Small Area Income and Poverty Estimates (U.S. Census Bureau, b); and average earnings, from the Census's Quarterly Workforce Indicators (U.S. Census Bureau, c). The county democratic vote share is the share of votes cast for democratic candidates in congressional and presidential elections (in 2010, 2012, 2014, 2016, and 2018) (NC State Board of Elections). Prosecutor hires and departures are calculated based on the first and last year a prosecutor appears in the NC court records. Prosecutors' politics and race come from the NC voter records. Standard errors are clustered by county. *p<0.1; **p<0.05; ***p<0.01.

Table A.3: Summary Statistics: Survey Representativeness

	All Years (1995-2019)			Most Recent Year (2019)		
	(1)	(2)	(3)	(4)	(5)	(6)
Prosecutor Demographics						
% Black Prosecutor	9.1	10.2	8.8	9.8	11.1	10.4
% Female Prosecutor	37.0	36.0	38.9	46.0	46.6	46.4
% Registered Democrat	42.0	46.5	46.2	43.0	48.9	51.8
Prosecutor Age (in Years)	38.4	37.2	36.2	39.4	38.3	37.8
Prosecutor Tenure (in Years)	4.9	5.0	6.7	6.4	6.2	7.9
Jurisdiction Characteristics						
% County Voters Reg. Democrat	37.1	40.6	41.7	36.5	39.9	41.1
% Urban (2010 Census)	57.7	66.1	67.7	56.5	64.5	63.3
Sentencing Outcomes						
% Incarcerated (> 6mo)	6.4	6.5	6.0	3.0	2.8	4.4
% Conviction	44.8	43.1	41.4	31.8	28.8	33.6
% Trial	0.7	0.7	0.6	0.3	0.3	0.5
Defendant Demographics						
% Black Defendant	45.3	53.2	55.7	43.5	51.4	52.8
% Female Defendant	27.5	26.5	26.5	30.0	28.9	27.4
Defendant Age (in Years)	31.3	31.1	30.9	32.7	32.3	32.2
# Cases	3,810,926	1,993,034	505,787	358,999	184,932	57,714
# Prosecutors	1,925	1,107	163	926	520	163
# Counties	100	39	52	100	40	40
# Counties with BWCs	26	13	13	26	13	13
# Offices	39	16	20	39	16	20
Participating Offices		✓			✓	
Participating Prosecutors			✓			✓

Notes: This table describes our sample of cases. Column 1 includes all cases in North Carolina between 1995 and 2019. Column 2 includes cases handled by one of the sixteen offices that participated in our 2020 survey of North Carolina prosecutors. Column 3 includes the cases handled by surveyed prosecutors, who worked in a participating office in 2020 but may have previously worked in a different office. Columns 4−6 repeat these statistics limiting to cases in 2019.

Table A.4: The Effect of Body-Worn Cameras on Traffic Stops and Jail Time

(a) Per Capita Traffic Stops

	Overall	Black	White	Disparity			
Black x BWC				-1.313^{*}	-1.223^{*}	-1.045	-2.062^{***}
				(0.704)	(0.713)	(0.641)	(0.666)
BWC	-1.200^{**}	-2.165^{**}	-0.853^{**}	-0.853^{**}	-1.051^{***}	-0.763^{**}	-1.030^{***}
	(0.566)	(1.034)	(0.394)	(0.394)	(0.380)	(0.348)	(0.348)
Baseline Dependent Mean	8.91	17.08	6.10	8.91	8.91	8.91	8.91
Baseline Disparity				10.97	10.97	10.97	10.97
Percentage Change	-13.5%	-12.7%	-14.0%	-12.0%	-11.1%	-9.52%	-18.8%

(b) Per Capita Jail Time

	Overall	Black	White	Disparity			
Black x BWC				-0.169^{***}	-0.152^{***}	-0.165^{***}	-0.097^{**}
				(0.057)	(0.056)	(0.058)	(0.039)
BWC	-0.092^{***}	-0.182^{***}	-0.013	-0.013	0.017	-0.008	0.012
	(0.030)	(0.067)	(0.020)	(0.020)	(0.019)	(0.019)	(0.020)
Baseline Dependent Mean	0.77	1.67	0.462	0.77	0.77	0.77	0.77
Baseline Disparity				1.20	1.20	1.20	1.20
Percentage Change	-11.9%	-10.9%	-2.88%	-14.0%	-12.6%	-13.7%	-8.1%
Exclude Never Adopting Counties					✓		
Exclude Neighboring Counties						✓	
Adoption Group x Race x Year Trends							✓
# Treated Counties	26	26	26	26	26	26	26
# Control Counties	48	48	48	48	40	48	48
# Years	11	11	11	11	11	11	11
# Observations	8,034	8,034	8,034	16,068	12,740	15,008	16,068

Notes: This table analyzes the changes in rates of per capita traffic stops and jail time in newly-adopting BWC counties compared to contemporaneous changes for not-yet or never adopting counties in North Carolina. Annual data on traffic stops by race and law enforcement agency (which we aggregate to the county level) come from (NCSBI, 2024; Baumgartner, 2024). Data on jail sentences from the North Carolina court records. We define jail as a carceral sentence of any length (including pretrial conviction). Columns 1−3 estimate Equation 2, where each observation is a county in a given year (and sub-experiment), and the dependent variable is the per-capita rate for all people in Column 1, Black people in 2, and white people in 3. Columns 4−7 estimate Equation 3, which includes observations for both Black and white people. Column 5 excludes never adopting counties, Column 6 excludes neighboring counties, and Column 7 allows for adoption group specific pre-existing trends. The baseline mean is the average in treated counties before they adopt BWCs. In Columns 1−3, the percentage change divides the coefficient on BWC by this baseline mean. In Columns 4−7, the baseline disparity is the percentage-point race gap in outcomes, and the percentage change divides the coefficient on Black x BWC by this gap. Standard errors are clustered by county. *p<0.1; **p<0.05; ***p<0.01.

Table A.5: Heterogeneity in the Relationship Between Prosecutors' Uncertainty About Police Reports and Their Incarceration Disparities

	% Incarcerated >6mo		
	By BWC		By Arrest Type
	(1)	(2)	(3)
Police-Initiated x No BWC x Black x Share Trust		2.17***	1.23*
		(0.74)	(0.63)
Police-Initiated x BWC x Black x Share Trust			−0.09
			(0.71)
Police-Initiated x Black x Share Trust		−0.37	
		(0.47)	
No BWC x Black x Share Trust	1.36*		0.56
	(0.69)		(0.67)
BWC x Black x Share Trust			−0.28
			(0.53)
Black x Share Trust	−0.29		
	(0.38)		
Other Arrest x No BWC x Black x Share Trust		0.84	
		(0.84)	
Other Arrest x Black x Share Trust		−0.28	
		(0.53)	
Office Crime-Unit FE	✓	✓	✓
Prosecutor FE	✓	✓	✓
Sentencing Guidelines	✓	✓	✓
Dependent Mean	6.03	6.03	6.03
# Cases	479,602	479,602	479,602
# Prosecutors	162	162	162
R^2	0.42	0.42	0.42
Adjusted R^2	0.40	0.40	0.40

Notes: This table analyzes heterogeneity in the relationship between prosecutors' certainty in police reports and the incarceration disparities in their cases. Column 1 tests how this relationship varies by BWC-adoption status overall. Column 2 tests how this relationship varies by BWC-adoption status separately for police-initiated arrests (i.e., drug, public order, and weapon possession offenses) and other arrests. Column 3 tests heterogeneity for police-initiated arrests versus other arrests separately for counties with BWCs and without BWCs. Each column include our preferred set of controls, as in Equation **??**. Standard errors are clustered by prosecutor. *p<0.1; **p<0.05; ***p<0.01.

Table A.6: Robustness of the Relationship Between Prosecutor Certainty & Disparities: Controlling for Prosecutor Race and Politics

	% Incarcerated >6mo					
	(1)	(2)	(3)	(4)	(5)	(6)
Black x Prosecutor Trust	0.79^{**}	0.65^{*}				
	(0.37)	(0.35)				
No BWC x Black x Trust			1.24^{**}	1.14^{**}		
			(0.60)	(0.57)		
BWC x Black x Trust			-0.57	-0.69^{*}		
			(0.36)	(0.38)		
Police-Init. x No BWC x Black x Trust					2.30^{***}	2.15^{***}
					(0.82)	(0.72)
Police-Init. x BWC x Black x Trust					-0.71^{*}	-1.13^{**}
					(0.40)	(0.48)
Other Arrest x No BWC x Black x Trust					0.60	0.54
					(0.62)	(0.62)
Other Arrest x BWC x Black x Trust					-0.50	-0.46
					(0.51)	(0.51)
Office Crime-Unit x Black FE	✓	✓	✓	✓	✓	✓
Prosecutor FE	✓	✓	✓	✓	✓	✓
Sentencing Guidelines	✓	✓	✓	✓	✓	✓
Charge FE	✓	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓	✓
Prosecutor Black x Black FE		✓				
Prosecutor Democratic x Black FE		✓				
Pros. Black x BWC x Black FE				✓		
Pros. Democratic x BWC x Black FE				✓		
Pros. Black x BWC x Police-Init. x Black FE						✓
Pros. Democratic x BWC x Police-Init. x Black FE						✓
Dependent Mean	6.01	6.15	6.03	6.15	6.03	6.15
Mean Share Trust	0.58	0.58	0.58	0.58	0.58	0.58
Std. Dev. in Share Trust	0.24	0.24	0.24	0.24	0.24	0.24
Mean Prosecutor Black	0.09	0.09	0.09	0.09	0.09	0.09
Mean Prosecutor Democrat	0.51	0.51	0.52	0.52	0.52	0.52
# Cases	505,787	492,467	479,602	468,439	479,602	468,439
# Prosecutors	163	160	162	159	162	159
R^2	0.45	0.45	0.46	0.46	0.46	0.46
Adjusted R^2	0.42	0.42	0.43	0.43	0.43	0.43

Notes: This table tests the robustness of the relationship between prosecutors' certainty in police reports and their incarceration disparities to the addition of controls for prosecutors' race and political affiliation. For reference, Columns 1, 3, and 5 replicate Columns 3, 6, and 9 of Table 3, and Columns 2, 4, and 6 add controls for a prosecutor's race and whether they are a Registered Democrat. Data on prosecutors' race and political affiliation come from the state voter records. Standard errors are clustered by prosecutor. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.7: How Incarceration Disparities Evolve with Tenure For Prosecutor with Different Certainty After Reading Police Reports (before BWCs are adopted)

	% Incarcerated >6mo						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Prosecutor Tenure x Share Certain x Black	0.66***	0.72***	0.64***	0.52***	0.50***	0.51***	0.56***
	(0.10)	(0.09)	(0.14)	(0.17)	(0.16)	(0.08)	(0.10)
Tenure x Black	−0.25***	−0.30***	−0.27***	−0.30***	−0.28***	−0.22***	−0.25***
	(0.05)	(0.05)	(0.07)	(0.09)	(0.09)	(0.07)	(0.08)
Share Certain x Tenure	0.25	0.25	0.07	0.14	0.13	0.13	0.15
	(0.26)	(0.25)	(0.12)	(0.13)	(0.12)	(0.11)	(0.11)
Tenure	−0.11	−0.11	−0.06	−0.05	−0.04	−0.09	−0.03
	(0.17)	(0.16)	(0.11)	(0.11)	(0.10)	(0.10)	(0.14)
Office Crime-Unit FE	✓	✓	✓	✓	✓	✓	✓
Black x Year x County FE		✓	✓	✓	✓	✓	✓
Prosecutor x Race FE			✓	✓	✓	✓	✓
Sentencing Guidelines				✓	✓	✓	✓
Charge FE					✓	✓	✓
Demographics					✓	✓	✓
Office Crime-Unit x Black FE						✓	✓
Prosecutor Certainty x Year x Black							✓
Dependent Mean	6.72	6.72	6.72	6.72	6.72	6.72	6.72
Mean Share Certain	0.60	0.60	0.60	0.60	0.60	0.60	0.60
Std. Dev. in Share Certain	0.23	0.23	0.23	0.23	0.23	0.23	0.23
Mean Tenure	5.18	5.18	5.18	5.18	5.18	5.18	5.18
Std. Dev. in Tenure	4.37	4.37	4.37	4.37	4.37	4.37	4.37
# Cases	332,963	332,963	332,963	332,963	332,963	332,963	332,963
# Prosecutors	141	141	141	141	141	141	141
R^2	0.32	0.33	0.34	0.43	0.45	0.47	0.47
Adjusted R^2	0.31	0.31	0.32	0.41	0.42	0.43	0.44

Notes: This table tests how incarceration disparities evolve with tenure across prosecutors with different levels of certainty in police reports, where all specifications limit to the years before BWCs were adopted in the given county. Column 3 presents the estimates from our preferred specification in Equation 8. Columns 1-2 uses more parsimonious controls, and Columns 4-7 add more extensive controls. Standard errors are clustered by prosecutor. Table A.8 shows robustness to controlling for prosecutors' race and political affiliations. The survey question interface for prosecutors' certainty in police reports is shown in Figure A.10(a).*p<0.1; **p<0.05; ***p<0.01.

Table A.8: Robustness of the Relationship Between Disparities and Tenure by Certainty in Police Reports: Controlling for Prosecutor Race and Politics

	% Incarcerated >6mo	
	(1)	(2)
Prosecutor Trust x Tenure x Black	0.56***	0.41***
	(0.10)	(0.15)
Prosecutor Black x Tenure x Black		−0.11
		(0.16)
Prosecutor Democratic x Tenure x Black		−0.17*
		(0.10)
Tenure x Black	−0.25***	−0.06
	(0.08)	(0.15)
Trust x Tenure	0.15	0.25*
	(0.11)	(0.15)
Prosecutor Black x Tenure		−0.32
		(0.23)
Prosecutor Democratic x Tenure		0.07
		(0.10)
Tenure	−0.03	−0.14
	(0.14)	(0.14)
Office Crime-Unit x Black FE	✓	✓
Black x Year x County FE	✓	✓
Prosecutor x Race FE	✓	✓
Sentencing Guidelines + Charge FE	✓	✓
Demographics	✓	✓
Prosecutor Trust x Year x Black	✓	✓
Dependent Mean	6.72	6.85
Mean Share Trust	0.60	0.60
Std. Dev. in Share Trust	0.23	0.23
Mean Tenure	5.18	5.18
Std. Dev. in Tenure	4.37	4.37
Mean Prosecutor Black	0.08	0.08
Mean Prosecutor Democratic	0.53	0.53
# Cases	332,963	325,695
# Prosecutors	141	138
Adjusted R^2	0.44	0.43

Notes: This table analyzes how incarceration disparities evolve with tenure for prosecutors with different levels of certainty in police reports, as in Table A.7. For reference, Column 1 replicates the final column of Table A.7 and Column 2 adds controls for prosecutor race and political affiliation. Standard errors are clustered by prosecutor. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.9: Robustness of the Relationship between Exposure to BWCs and Prosecutors' Beliefs

	(a) % Disparites Due to Past Conduct			**(b)** % Due to Prior Points		
Years of BWC Exposure	−3.94**	−3.08**	−2.98**	−0.02	0.11	−0.38
	(1.73)	(1.34)	(1.43)	(1.36)	(1.44)	(1.47)
Prosecutor Black		−14.75**	−15.12**		−2.27	−0.46
		(6.02)	(6.38)		(7.03)	(6.42)
Prosecutor Democrat			2.10			−10.15
			(4.79)			(8.32)
Dependent Mean	13.71	13.71	13.71	23.73	23.73	23.73
Observations	119	119	119	119	119	119
	(c) % Due to Current Conduct			**(d)** % Due to Perception of Conduct		
Years of BWC Exposure	−2.99**	−2.09*	−1.89*	4.40**	3.35*	3.17*
	(1.33)	(1.09)	(1.13)	(2.15)	(1.89)	(1.89)
Prosecutor Black		−15.53***	−16.25***		18.06**	18.70**
		(4.74)	(4.89)		(7.66)	(7.73)
Prosecutor Democrat			4.06			−3.62
			(4.66)			(7.25)
Dependent Mean	7.35	7.35	7.35	18.27	18.27	18.27
Observations	119	119	119	119	119	119
	(e) % Due to Legal Rep.			**(f)** % Uncertain after Police Report		
Years of BWC Exposure	0.66	−1.05	−0.42	4.47*	3.95	4.27
	(1.97)	(1.65)	(1.65)	(2.60)	(2.53)	(2.58)
Prosecutor Black		29.38***	27.06***		8.74	7.13
		(9.34)	(9.22)		(6.38)	(6.81)
Prosecutor Democrat			13.02**			5.84
			(5.68)			(6.53)
Dependent Mean	13.54	13.54	13.54	42.54	42.54	42.54
Observations	119	119	119	138	138	138

Notes: This table analyzes the relationship between prosecutors' past exposure to BWC and their elicited beliefs on the survey. Each specification estimates Equation 5, with fixed effects for prosecutor office and tenure. In Panels (a)-(e), the dependent mean is prosecutor's beliefs about the percent of disparities due to different factors. The interface for this question is in Figure 4(a). In Panel (f), the dependent variable is the percent of cases that the prosecutor recalls feeling uncertain about the defendant's true conduct after reading the police report. The interface for this question is in Figure A.10(a). About a quarter of prosecutors did not respond to the question about the sources of racial disparities reducing the sample size. Non-response is uncorrelated with BWC exposure: an additional year of BWC exposure is associated with an insignificant 2.2 pp reduction in non-response (p-value = 0.44). Standard errors are clustered by prosecutor. *p<0.1; **p<0.05; ***p<0.01.

A. Model Extension

Our baseline model assumes that prosecutors no longer *ever* takes the police report at face value after gaining access to the unbiased signal of type. Here, we relax that assumption and simply assume that prosecutors become less likely to take the police report at face value, $\alpha_{p,f} < \alpha_{p,0}$.

After the shock, the prosecutor's incarceration disparity would then be:

$$\underbrace{\kappa}_{\substack{\text{Race Gap}\\\text{in Crime}}} + \underbrace{(\zeta_B - \zeta_W)}_{\substack{\text{Prosecutor}\\\text{Taste-Based Bias}}} + \underbrace{\alpha_{p,f} b_f}_{\substack{\text{Taking Police}\\\text{At Face Value}}} + \underbrace{(1-\alpha_{p,f})\frac{\tau_\theta+\tau_s}{\tau_\theta+\tau_s+\tau_f}(\hat{\kappa}_{p,f}-\kappa)}_{\substack{\text{Prosecutor Biased}\\\text{Beliefs about Type}}}$$

The change in disparities after the shock is then proportional to:

$$(1-\alpha_{p,f})\Big[\underbrace{-\frac{\tau_f}{\tau_\theta+\tau_s+\tau_f}(\hat{\kappa}_{p,f}-\kappa)}_{\substack{\text{Updating about the}\\\text{Specific Case}}} + \underbrace{(\hat{\kappa}_{p,f}-\hat{\kappa}_p)}_{\substack{\text{Learning}\\\text{about Types}}}\Big] - \underbrace{(\alpha_p-\alpha_{p,f})(b-(\hat{\kappa}_p-\kappa))}_{\substack{\text{Learning to Take Police}\\\text{Less at Face Value}}} - \underbrace{\alpha_{p,f}(b-b_f)}_{\substack{\text{Police Becoming}\\\text{More Trustworthy}}}.$$

The first term reflects the prosecutor using the new unbiased signal to update about individual cases and thus relying less on her potentially biased prior beliefs. The second and third terms reflect the prosecutor updating across all of her cases: over time, she forms different beliefs about racial differences in type and learns to rely less on police signals. The fourth term reflects the fact that the prosecutor's tendency to take the police report at face value matters less since the police have become more trustworthy.

B. Constructing Prosecutor and Case Identifiers

In the North Carolina court records, there are two features of the raw data that might confound analyses of prosecutorial discretion. First, the data typically records the prosecutor assigned to the case but lacks a consistent identifier for each prosecutor. Second, the unit of observation — the "docket" — does not always reflect the unit at which decisions are made because multiple dockets are often handled together in a single "case".[22] Identifying

[22]For some defendants, multiple charges are brought at the same time but filed under different docket numbers. For other defendants, multiple charges enter the court system separately but are resolved together

the "case" helps us better identify the sequence of discretionary choices, as we can track the most serious charge on the case at each juncture of the process and how this charging choice interacts with the mandates of North Carolina's sentencing guidelines.

B.1. Constructing Prosecutor Identifiers

The court records start with a set of strings that inconsistently identify prosecutors. For example, Emma K. Harrington might also be recorded as Attorney E Harrington or EKH. We first strip off generic designators (like attorney) and parsed names into first, middle, and last names or initials based on the punctuation of the name. Second, within jurisdictions, we create all possible pairs of names and use string distance algorithms to link together distinct names that we believe reflect the same prosecutors. This generates a refined set of prosecutor names.[23] Third, we link prosecutor identifiers that only have initials to the refined prosecutor names. Fourth, we link prosecutors with similar names across jurisdictions by hand, looking up prosecutors with common matching names or similar but not identical names in different offices to see if they likely were the same individual. We also looked up by hand all women with the same first name to see if there was evidence of a marriage that resulted in a name change.

B.2. Constructing Case Identifiers

We use two rules to determine whether dockets are consolidated into cases: (1) we combine dockets that are flagged in the court records as "consolidated for judgment" for sentencing and (2) we combine dockets when the timing of the dockets are proximate or overlapping. Specifically, we consolidate dockets when the charging or disposition dates occur in the same week or the charges in the later docket occur before those in the earlier docket were resolved. If either of these timing conditions are met and the same prosecutor

in a final judgment.

[23] To classify the last name as matching, we require both last names to be populated and then either (i) a near perfect match on the last names, (ii) a high-quality match on the last names with the first letter of the first name matching, (iii) a high-quality match on the last name and a near perfect match on the non-missing first names, or (iv) a good match on the last names with the first letter of the first name matching and a near perfect match on the first names. After applying these rules we then further require that there is some way that the first letter of the first-name matches, either based on the names or the initials (since some people go by their middle names, the first and middle names sometimes didn't line up in predictable ways).

handles both dockets, we join the dockets into a single case. We always consider dockets handled by two different prosecutors as separate cases, even if the dates are proximate or the date ranges are overlapping.

Consolidated for judgment: We use the "consolidated for judgment" fields to join dockets that have been combined at sentencing for a single judgment. Of all offenses in the court records, 15% are consolidated with another offense at sentencing, and 37% of initial dockets have at least one consolidated offense.

Overlapping date ranges: When docket date ranges are proximate or overlapping, we join dockets with the same defendant that are handled by a single prosecutor. We consolidate 19.1% of all cases using common disposition weeks across dockets. We consolidate an additional 10.8% of cases using the case filing week. We consolidate an additional 2.14% of cases using the week the case was charged.

Organizing dockets into cases allows us to more accurately assess the time-line of cases and the decisions of prosecutors. In each case, the most severe lead charge determines the punishment under the state sentencing guidelines. The lead charge at arrest determines where defendants start in the sentencing guidelines and the lead charge at conviction determines where defendants land in the sentencing guidelines after the prosecutor has exercised her discretion. Organizing dockets into cases allows us to identify the lead charge at each stage and, thus, more accurately assess the time-line for each case and the prosecutor's decisions about whether or not to reduce the lead charge. Finally, since the punishments are served concurrently unless noted otherwise, organizing dockets into cases allows us to more accurately assess the punishment on each case.

A Note about Records of Probation Violations: In the North Carolina court records, violations of probation are typically recorded on the docket of the initial offense that led to the probation sentence. We split these probation violations into their own cases based on the first date that a probation violation appears on the docket. These violations amount to 11.8% of all charges. Probation violations are excluded from our analyses since prosecutors are rarely involved in these cases in North Carolina. Breaking off these probation violations from the initial offense is necessary to correctly date the case according to when it was first resolved. This is also essential for accurately assessing the initial punishment rather than the ultimate punishment that might be triggered by a probation violation.

C. Linking to Voter Records

We uniquely match the prosecutor to a voter record file in 88% of cases. We find a strong concordance with the survey information. Among prosecutors who identify as liberal on the survey, only 2% are registered Republicans. Similarly, only two prosecutors (1.5%) reported a different racial identity on the survey than in the voter records.

For the match of prosecutor identifiers to the North Carolina voter records, we require an exact match on the last name and first letter of the first name. We then use the rest of the first name, the middle initial/name, the suffix (if applicable), and the county to try to identify unique, high-quality matches. Finally, we use the voter's age and the prosecutor's years working in the voter records to identify unique, high-quality matches who would be active as prosecutors between the ages of 24 (after plausible law school graduation) and 64 (before retirement). Using this information, we identify a unique, high-quality match in 88% of cases and 93% of cases with a named prosecutor.