LEARNING ABOUT POLICE BIAS:

Prosecutors and Police Before and After Body-Worn Cameras *

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Abstract

Decision-makers often fail to correct for others' biases. We study this dynamic in the criminal justice system, where prosecutors rely on police for information about arrests but recently began receiving more objective information from body-worn cameras (BWCs). Using BWCs' staggered rollout in North Carolina, we find BWCs reduce incarceration disparities by 14% — only one-sixth of which is explained by reductions in arrests. To unpack mechanisms, we link an original survey of 203 prosecutors to their half-million cases. A back-of-the-envelope calculation suggests a quarter of the incarceration result reflects learning: prosecutors with more BWC exposure view police as less reliable and more biased.

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In many high-stakes settings, decision-makers rely on earlier actors for information: in labor markets, hiring managers rely on reference writers; in academia, editors rely on referees; and in the criminal justice system, prosecutors rely on police. In all of these contexts, the earlier actor may misrepresent the underlying reality — potentially in racially biased ways. This leaves the downstream decision-maker with a difficult inference problem: what reflects the truth, and what reflects shading or bias? There is growing evidence that decision-makers often fail to correct for earlier biases, and that these errors contribute to systemic disparities (Baron et al., 2024; Bohren et al., 2025).

The question remains as to *why* downstream decision-makers fail to correct for upstream bias — and, instead, pass it through.[1] We study this question in the context of the U.S. criminal justice system. Each year, police arrest seven million Americans, often following a discretionary stop (FBI, 2019), and, each year, more than half a million Americans are sentenced to prison (Carson, 2019). Prosecutors have considerable discretion over which arrests lead to prison.[2] To make these decisions, prosecutors typically rely on information provided by police, especially the police report's characterization of the crime. Prosecutors then face a difficult inference problem: how much do police reports reflect the truth, and how much do they reflect shading or bias?[3]

We model and test two behavioral mechanisms that could lead prosecutors to misprocess potentially biased signals from police. First, prosecutors may have inaccurate beliefs about the extent of police bias. Second, prosecutors may take information in police reports at face value: when confronted with the difficult task of distinguishing truth from bias, prosecutors may take a cognitive shortcut and instead act as if "what they see is all there is" (Kahneman, 2011; Enke, 2020). These two mechanisms could be linked — repeatedly accepting

[1]There is a rich literature on why decision-makers introduce new bias. Theories distinguishing between taste-based and statistical discrimination go back to Becker (1957) and Phelps (1972). Recently, researchers have focused on the role of biased beliefs (e.g., Bordalo et al., 2016; Arnold et al., 2018).

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

[3]Black Americans are over twice as likely to be arrested as white Americans, and research suggests that this disparity reflects both bias and actual differences in behavior (e.g., Knowles et al., 2001; Anwar and Fang, 2006; Tomic and Hakes, 2008; Jordan, 2021; Feigenberg and Miller, 2022).

biased information as fact may distort beliefs over time. Prosecutors who consistently read that Black people are more criminal may start to internalize this in their prior beliefs about racial differences in crime.

We first investigate how much disparities in criminal-justice outcomes reflect prosecutors' (mis)processing of potentially biased signals from police. The ideal experiment would randomly give some prosecutors additional unbiased signals about arrests: if this reduced disparities in incarceration and conviction rates, this would indicate that prosecutors were initially passing through police bias. We approximate this ideal using the rollout of body-worn cameras (BWCs). BWCs are designed to record police interactions with the public and are typically mounted to officers' chests or belts.[4] Footage is routinely shared with prosecutors and defense attorneys, occasionally with judges, and rarely with the public.

We evaluate how BWCs influence prosecutors' beliefs about police and the realized incarceration disparities in their cases. Although BWC footage can be blurry or incomplete, it provides a less filtered account of on-scene arrests.[5] This footage can change a prosecutor's view of a *specific* case — for example, by revealing that the defendant's conduct was less severe than the police report suggested. Over time, repeatedly comparing reports to footage may also reshape the prosecutor's beliefs about police bias and reliability across all cases, even when the footage itself offers no smoking gun evidence of misrepresentation.

We then unpack the two proposed mechanisms that may drive prosecutors' (mis)processing of information from police. To do this, we fielded an original survey of 203 North Carolina prosecutors in 2020, which we linked to the half a million cases that these prosecutors handled between 1995 and 2019. We measure prosecutors' beliefs about police bias by asking them how much disparities in the criminal justice system are driven by racial differences in crime — and how much these disparities are driven by Black people's crimes being per-

[4]BWCs have been widely adopted over the last few decades: as of 2020, more than half of U.S. police departments had BWCs, up from 4% in 2010 (BJS, 2016, 2020). Police departments with BWCs typically have policies requiring camera activation during arrests, calls for service, and other official actions (BJS, 2016).

[5]BWCs could lead some officers to scale back enforcement in Black neighborhoods for fear that footage might go viral and appear biased. In that case, BWCs may not necessarily yield less biased information.

ceived as more severe than the same crimes committed by white people.[6] We measure prosecutors' tendency to take police information at face value by asking them how often they feel certain about what the defendant did based on the information in the police report. These elicitations ground our model empirically and allow us to test the importance of the two proposed mechanisms.

We exploit the fact that body-worn cameras were rolled out at different times across North Carolina to estimate their effects on criminal-justice outcomes. Hand-collected city council minutes and local news coverage suggest that funding constraints, rather than local politics or policing priorities, drove adoption timing.[7] Typically, the question was *when*, not whether, to adopt BWCs, with funding and other logistics often generating multi-year delays.[8] 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.[9] We leverage this variation in timing in a stacked difference-in-differences (DiD) 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). In addition to the absence of detectable pre-trends, our results are robust to limiting to adoption events less likely to be confounded by contemporaneous policy shifts, such as the election of 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 see similar declines in disparities for other downstream outcomes including conviction rates and jail time. Using a Oaxaca–Blinder decomposition, we find that the decline in incarceration is driven primarily by changes in how arrests are handled

[6]Our survey did not ask about police bias more directly because, in our pilot, many prosecutors were unwilling to answer these questions.

[7]City council minutes also indicate that BWCs were almost always considered as stand-alone agenda items, rather than as part of broader policing or criminal justice reform package.

[8]On average, over two years elapsed between initial discussions of BWC adoption and implementation.

[9]We do not detect differential pre-trends, and pre-existing trends would have to be large —- and so very likely detectable — to explain our results (Roth, 2022).

by the court system, rather than by changes in arrests themselves. Holding fixed the mapping from arrest charges to incarceration outcomes, we find that only about one-sixth of the incarceration effect is attributable to changes in the number or composition of arrests. Although arrest disparities do fall, the decline is concentrated in low-level arrests that almost never result in incarceration. Therefore, the bulk of the decline in incarceration instead reflects changes in charging, dismissal, and sentencing decisions conditional on arrest.

To unpack why BWCs change downstream decision-making, we use our survey to investigate how prosecutors' beliefs about their cases shift with more exposure to BWCs.[10] We use a difference-in-difference design that leverages variation in BWC exposure based on when a prosecutor was hired and when her county adopted BWCs. We compare prosecutors hired in different years working in earlier adopting counties — who differ both in their tenure and BWC exposure — to analogous prosecutors working in later adopting counties — who only differ in their tenure. This design aims to isolate the effect of BWC exposure by netting out experience and county effects.

We find that prosecutors with more exposure to BWCs believe that police reports are less reliable and disparities in justice involvement are driven more by bias. This shift in beliefs likely does not reflect changes in prosecutor composition: turnover rates do not change around BWC adoption, and those with more BWC exposure are no more likely to be Black or politically liberal. Nor does this result seem to reflect a broader shift toward progressivism: BWC exposure does not predict prosecutors' beliefs about labor market discrimination or their support for affirmative action. The shift in prosecutors' beliefs suggests that BWCs help them update about police reliability and the data-generating process in their cases —- revealing a reality where arrest disparities are driven more by bias and less by racial differences in crime. Suggestively, this learning has real-world effects: prosecutors with more BWC exposure tend to have lower incarceration disparities relative to others in their unit, where cases are quasi-randomly assigned.[11] A back-of-the-envelope calcu-

[10] Our approach is similar in spirit to that of Agan and Starr (2018); Bohren et al. (2019); Chan (2022).

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

lation suggests that approximately a quarter of BWCs' aggregate impact on incarceration disparities reflects prosecutor learning.

We then use our survey to examine the second mechanism that leads to misprocessing: prosecutors simply take police reports at face value. Prior to BWC adoption, prosecutors who are more certain about police reports increase incarceration disparities relative to others in the same unit. This suggests that downstream disparities partly reflect prosecutors passively perpetuating police bias. After BWCs are adopted, however, disparities decline for all prosecutors and converge to the level previously realized by only the most skeptical prosecutors before BWCs. One interpretation is that BWCs incentivize police to write more accurate reports, and so faith in police matters less since police themselves have become more trustworthy. Another interpretation is that BWC footage allows prosecutors to cross-check police reports, making their baseline faith in police less consequential. Either way, surveilling police appears to substitute for prosecutor skepticism of police.

In a final exploratory analysis, we examine whether prosecutors who have greater faith in police reports update their beliefs more toward the reality presented to them by police. Among prosecutors who report high confidence in police reports, incarceration disparities increase sharply with experience in the years prior to BWC adoption. By contrast, disparities remain relatively stable among prosecutors who express lower confidence in police. Together, these patterns suggest that prosecutors who internalize police reports increasingly come to believe that racial differences in crime are larger than they initially thought.

Although we focus on prosecutors and police, these mechanisms likely operate in other high-stakes contexts. In many settings — including hiring, academic publishing, college admissions, lending, and child welfare — decision-makers must rely on information from earlier actors that is subjective but often appears objective. Relative to these other decision-makers, prosecutors may be especially well equipped to detect and correct for bias: they are highly educated professionals who have extensive experience interpreting criminal cases.

et al. (2016); Fenizia (2022) and show that prosecutors' estimated disparate impacts persist across offices.

Yet even prosecutors struggle to adjust for distortions in upstream information.

Our paper contributes to the literature on systemic discrimination by modeling and measuring how downstream decision-makers process potentially biased information. Economists have long recognized the challenge of identifying discrimination since relevant signals (like work history) may reflect past discrimination (Cain, 1986; Neal and Johnson, 1996; Blank, 2005).[12] Recent work has developed econometric tools to decompose ultimate disparities into contributions from each stage of a system (Baron et al., 2024; Bohren et al., 2025). We take a different approach: rather than assuming disparities can be cleanly attributed to different stages, we examine how disparities may result from later actors' (mis)interpretation of upstream decisions. Our paper provides a collage of evidence that some prosecutors mistake the disparities in police reports for the truth — and that BWCs allow them to update toward a different reality.

We also add to the growing body of evidence showing that biased beliefs can be important drivers of disparities (Bordalo et al., 2016; Agan and Starr, 2018; Arnold et al., 2018; Chan, 2022). Yet their origins are less clear. We provide evidence on the source of biased beliefs: suggestively, prosecutors' beliefs had been distorted by relying solely on information from police before they had BWCs. Simply put, biased beliefs may stem from earlier bias within the system itself.

Finally, we contribute to the growing literature on body-worn cameras. Most existing work has focused on BWCs' impacts on arrests and police use of force — often, but not always, finding reductions in both.[13] We focus on BWCs' impacts downstream of police, which has received far less attention.[14] Our survey of prosecutors further allows us to get under the

[12]There is also a large theoretical literature on systemic discrimination in sociology and law (Pincus, 1996; Powell, 2007; Pager and Shepherd, 2008; Feagin, 2013).

[13]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 in Virginia and Brazil finds, consistent with our results, that BWCs reduce the incidence of low-level arrests (Bollman, 2021; Barbosa et al., 2021). In addition, multiple recent large-scale studies find that BWCs substantially reduce police use of deadly force (Barbosa et al., 2021; Miller and Chillar, 2022; Kim, 2024) and civilian complaints against police (Braga et al., 2022).

[14]One exception is a working paper finding that BWCs reduce low-level arrests in Virginia but, conditional on arrest, have little effect on conviction or incarceration (Bollman, 2021). These results may differ from

hood of the system and better understand why BWCs affect downstream decision-making.

Section I of our paper provides background from interviews about prosecutors' use of BWC footage. Section II presents our model. Section III describes our data. Section IV analyzes the roll-out of BWCs, and Section V, our survey of prosecutors. Section VI interprets the results through the lens of the model, and Section VII concludes.

I Prosecutors' Use of Body-Worn Cameras

We interviewed North Carolina prosecutors and judges to learn about how they use body-worn camera footage. The lawyers we spoke with 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," 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.[15] Consistent with this, Jordan et al. (2025) finds that 72% of arrests have BWC footage in Chicago but that some "bad apple" officers have lower-activation rates.

The footage does not merely exist — it is watched. "I would say [prosecutors and defense attorneys] are watching 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 time to watch the video." A prosecutor in the same jurisdiction noted variation in *when* prosecutors watch the footage: some review it immediately, others after indictment, and others only once the case is set for trial. This particular prosecutor watches it early on, typically setting up two monitors with "the [police] report on one screen and the body-worn on the other."

Prosecutors told us that they watch the BWC footage because it can make or break their

ours due to the estimation strategy: conditioning on being arrested may introduce selection bias since BWCs reduce low-level arrests, which, all else equal, would increase incarceration rates conditional on arrest. Our focus on changes in per capita outcomes sidesteps this concern.

[15]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.

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, its the best evidence in my cases." Another echoed this view, saying that BWCs "help us all get to what actually happened. . . to the truth of the matter." He explained that the footage "can do one of two things": it either helps to "nail down the person's guilt" or is "disastrous to my case." "I want to see it no matter what, either way."

The footage can reveal information about the defendant. It may nail down guilt by showing a confession or a DWI defendant "falling into the side of the road."[16] In these sorts of cases, the footage can expedite the plea deal.[17] But the footage can also lead downstream actors to conclude that the defendant should be charged with a lesser crime or no crime at all. According to one judge, often the conduct in the "video is not as bad as what your mind is thinking when you hear a verbal version of it." The video can also provide additional *context* about the crime and "help you step into the shoes of the defendant." 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 one 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 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, he "fills out a dismissal and is done with it."

[16]One prosecutor explained that "A lot of times, defendants will confess on the body-worn. And having the officer testify the person confessed is different than watching the person confess on camera." Another prosecutor said in DWI cases with BWC footage, "It's not just the cop saying the person was falling down drunk. Now, we have evidence. . . and so the public defender is no longer spending time trying to convince the jury that the defendant is the guy to believe."

[17]As one prosecutor put it, "If I'm the PD [public defender], now I can show [my client] the video." Otherwise, "defendants have a lot of denial going on" that can prevent or prolong negotiations.

In this way, a judge said BWC footage can act as "an independent neutral arbiter of the truth" that helps both sides learn what actually happened in an individual case. But over time, it may also shift lawyers' views about defendant conduct and police officer reliability *in all their cases*, even ones 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."

Beyond helping prosecutors learn about the events leading up to the arrest, BWCs can also reveal an officer's use — or misuse — of force during the arrest itself. One prosecutor told us that he often gives defendants "a better deal" if he sees that "law enforcement beat [the person's] ass." Over time, repeat exposure to such footage may attune prosecutors to language in police reports that sanitizes an officer's use of force. According to this prosecutor:

> "If you've done this long enough, you know which language [in police reports] 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, lets look at the body-worn.' And then we see 'that means three officers brought him to the ground.'

He added that this kind of 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." In those moments, 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 more directly scrutinized. As one judge said, "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.[18] They may also change how they characterize what happened. One prosecutor told us, "With body-worn, I definitely think that cops change the way they

[18] One judge claimed that, especially for charges like "resist arrest," where, before BWCs, it would have been the officer's word against the arrestee's, BWCs may lead the officer to decline the arrest entirely.

write their report and what to charge." Another said he counseled police officers to "put everything in [the report]: the good, the bad, and the ugly. It'll all come out eventually."

II Model

We model the simplest system in which bias from an upstream actor could influence a downstream decision-maker. We describe our model in terms of police and prosecutors, but it applies to any system where earlier signals may be biased. In our context, a police officer first makes a potentially biased judgment about the severity of a person's crime. Then, a prosecutor faces the difficult inference problem of determining how much the officer's judgment reflects relevant information as opposed to racial bias. Our model applies the canonical statistical-discrimination model in Phelps (1972) to this type of sequential system. We then introduce two distinct behavioral mechanisms. First, in the spirit of Kahneman (2011), we allow prosecutors — at times — to bypass this hard statistical problem and, instead, to simply treat information from police as the ground truth. Second, we allow prosecutors to have biased beliefs about true racial differences in crime, and, over time, to mistakenly update their beliefs toward the reality described by police. We then consider a shock to prosecutors' information set, which helps to identify the role of these mechanisms.

Timing and Information Structure. Individual i commits a crime with latent severity, θ_i, drawn from an underlying type distribution, which 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 crime and the individual's race and then writes a report describing the incident.[19] This report is a noisy, potentially biased signal of i's type, $s_i = \theta_i + \gamma_{R(i)} + \epsilon_i$ where $\epsilon_i \sim N(0, 1/\tau_s)$ and $\gamma_{R(i)}$ reflects the officer's bias against a given racial group. This bias term could reflect taste-based discrimination or biased beliefs.[20]

[19] We assume that the officer always sends a report to the prosecutor. In reality, an officer may decide not to go forward with an arrest for crimes below a threshold of perceived severity. We abstract from this since we focus on incarceration outcomes, where low-severity cases play only a minor role.

[20] These bias terms could also reflect police strategically adjusting their signal based on how they expect prosecutors will respond to it for each racial group. This strategic mechanism magnifies the difference in these racial bias terms, $\gamma_B - \gamma_W = b$, if prosecutors are less biased than police against Black relative to white

The prosecutor receives this signal, s_i, and then chooses how much to press for an incarceration sentence in negotiations with the defense attorney. Although the prosecutor cannot directly pick the incarceration outcome, she can influence its probability.

The Prosecutor's Payoffs. The benefits of incarceration are increasing in the severity of the individual's crime, θ_i, and the prosecutor's taste-based bias, $\zeta_{R(i)}$.[21] The 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, θ_i, and the prosecutor's taste-based bias, $\zeta_{R(i)}$, up to a first order. The challenge for the prosecutor is that she cannot directly observe types and instead must form beliefs about them based on her priors and the signal from police.

The Prosecutor's Beliefs. A prosecutor should correct each signal given her belief about police bias for that individual's racial group, $s_i - \hat{\gamma}_{p,R(i)}$. The prosecutor knows the observed signals for a given race, $\mathbb{E}[s_j \mid R_j = R_i]$ reflect a combination of true type ($\mu_{R(i)}$) and police bias ($\gamma_{R(i)}$). As a result, her belief about police bias for each racial group can be pinned down by her prior belief about the average type for that group, yielding $\hat{\gamma}_{p,R(i)} = E[s_j|R_j = R_i] - \hat{\mu}_{p,R(i)}$. While the prosecutor's beliefs about types and police bias may not match the underlying reality, we assume for simplicity that she otherwise holds accurate beliefs about the type distribution.

Yet a prosecutor may not always perform this correction. Rather than doing the work of processing the police signal, she may take a cognitive shortcut and treat it as the true type ($s_i = \theta_i$) in a share of her cases, $\alpha_p \in [0, 1]$. In these cases, she also uses the police signal to update her prior about types for each racial group — and thus her beliefs about Black-white gaps in type. As the prosecutor sees more signals, her belief about Black versus white people's types, $\hat{\kappa}_{p,n}$, converges to the reality presented by the police.[22] The more often she

individuals; and it will reduce the difference in the bias terms, b, if prosecutors are relatively more biased against Black individuals than police. This directly follows from applying Kartik et al. (2007)'s model of strategic communication games to our setting.

[21] Prosecutors may care about crime severity because of e.g., retribution, deterrence, or incapacitation.

[22] Specifically, letting $\tau_{p,o}$ denote the precision of the prosecutor's baseline prior belief, her expected prior

takes police reports at face value, the faster the convergence. This mistaken learning will increase the prosecutor's updated prior about racial differences if police bias against Black relative to white people, $b \equiv \gamma_{B(i)} - \gamma_{W(i)}$, is greater than her initial belief about Black-white differences in types, $b > \hat{\kappa}_{p,o} - \kappa$.

The observed racial disparity in her incarceration rate is proportional to:

$$\underbrace{\kappa}_{\substack{\text{Race Gap}\\ \text{in Crime}}} + \underbrace{(\zeta_B - \zeta_W)}_{\substack{\text{Prosecutor}\\ \text{Taste-Based Bias}}} + \underbrace{\alpha_p b}_{\substack{\text{Taking Police}\\ \text{At Face Value}}} + \underbrace{(1-\alpha_p)(\hat{\kappa}_{p,n} - \kappa)}_{\substack{\text{Prosecutor Biased}\\ \text{Beliefs about Type}}}. \tag{1}$$

Without more information, the observed disparity could reflect any combination of the following: true racial differences (κ), the prosecutor's taste-based bias ($\zeta_B - \zeta_W$), and her misprocessing of signals from the police. The prosecutor may misprocess police signals either because she takes police signals at face value and so passes through any police bias ($\alpha_p b$) or because she has inaccurate beliefs about racial differences in types, $((1-\alpha_p)(\hat{\kappa}_{p,n} - \kappa))$, which could itself be endogenous to bias in police signals.

Information Shock. To isolate the misprocessing of police signals, we test what happens when the prosecutor receives an additional noisy — but unbiased — signal of type: $f_i = \theta_i + \nu_i$ where $\nu_i \sim N(0, 1/\tau_f)$. With additional, information at her disposal, it is less natural for her to treat the police signal as "all there is." After all, in any individual case, the unbiased signal may clearly deviate from the police signal. In addition, after receiving many unbiased signals, the prosecutor may learn that treating police signals at the ground truth would lead to systematic errors — and so be less likely to take this cognitive shortcut. For both reasons, we expect $\alpha_{p,f} < \alpha_{p,0}$. In addition, once police can be cross-checked against the new, unbiased signals, we expect the racial bias in police signals to fall, $b_f < b$.

after seeing n cases of both Black and white people is: $\mathbb{E}[\hat{\kappa}_{p,n}] = \kappa + \frac{\tau_{p,o}}{\tau_{p,o} + \alpha_p n \frac{\tau_s \tau_{p,o}}{\tau_s + \tau_\theta}} (\hat{\kappa}_{p,o} - \kappa) + \frac{\alpha_p n \frac{\tau_s \tau_\theta}{\tau_s + \tau_\theta}}{\tau_{p,o} + \alpha_p n \frac{\tau_s \tau_\theta}{\tau_s + \tau_\theta}} b$.

After the shock, the prosecutor's incarceration disparity is:

$$\underbrace{\kappa}_{\substack{\text{Race Gap}\\ \text{in Crime}}} + \underbrace{(\zeta_B - \zeta_W)}_{\substack{\text{Prosecutor}\\ \text{Taste-Based Bias}}} + \underbrace{\alpha_{p,f} b_f}_{\substack{\text{Taking Police}\\ \text{At Face Value}}} + \underbrace{(1-\alpha_{p,f})\tfrac{\tau_\theta+\tau_s}{\tau_\theta+\tau_s+\tau_f}(\hat{\kappa}_{p,n_f} - \kappa)}_{\substack{\text{Prosecutor Biased}\\ \text{Beliefs about Type}}} \tag{2}$$

The change in disparities after the shock (Equation 2 $-$ Equation 1) is proportional to:

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

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. If we observe disparities fall after the shock, we can conclude that *some form* of misprocessing must have contributed to the initial disparity. We use our survey of prosecutors to unpack specific mechanisms.

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).[23] 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.[24]

The court records allow us to reconstruct much of the information that the prosecutor sees when first opening a defendant's case file, including defendant demographics (race, age, and gender) and the defendant's most serious — or "lead" — arrest charge. The records also detail case outcomes, including conviction charges, jail time, and incarceration sentences.[25] As is typical in U.S. 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.[26] 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, as shown in the map in Figure 1(a). To determine adoption timing, we began with data collected by 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 supplement it by searching local news and cold-calling agencies. This research allows us to expand the sample and pinpoint the year BWCs were broadly rolled out to beat officers.[27] We aggregate BWC treatment information to the county level and categorize a county as

[23] Data from District Courts, which is primarily low-level misdemeanors, is only available after 2009.

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

[25] 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).

[26] Prosecutor identifiers let us group concurrent offenses into cases and so see a complete picture of prosecutors' decision-making. Appendix A details both cleaning steps.

[27] This additional data collection identified the adoption year for 103 additional agencies (45 of which were not sampled by LEMAS and 58 of which did not specify the specific adoption year). This additional data collection also revealed that the BWC adoption year in the LEMAS often reflected the rollout of a pilot program rather than broad adoption across officers.

treated when at least half of its officers work in agencies that have adopted BWCs.

These data efforts allow us to identify the BWC adoption year for 48 counties and 77% of cases in the state, and 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.[28]

Arrest and Sentencing Outcomes. BWCs could affect each stage of the criminal justice system — including at arrest, charging, and sentencing. As a result, we focus on the change in total per capita outcomes, rather than the change in the likelihood of each outcome conditional on reaching that stage. For example, we evaluate the change in per capita incarceration rates, not the likelihood of incarceration given arrest. Otherwise, our analyses would suffer from selection bias if the composition of people who make it to each stage endogenously responds to BWCs (Knox et al., 2020; Goncalves et al., 2025). In Section IV.B, we use a Oaxaca-Blinder decomposition to analyze the contribution of each stage.

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

$$\text{Per capita arrest rate}_{ctr} = 100 \cdot \frac{\text{\# Arrests}_{ctr}}{\text{Total Population}_{ctr}}, \tag{3}$$

and do the analogue to measure per capita conviction and incarceration rates. To construct the numerators, we aggregate case-level data from administrative court records. For incarceration, for instance, we measure the number of incarceration sentences imposed by the North Carolina court system for Black and white people whose cases are handled in that county and year.[29] To construct the denominators, we use population counts by race, year, and county (U.S. Census Bureau, a). We also use data on traffic stops by race (NCSBI, 2024; Baumgartner, 2024).

[28]Many past evaluations of BWCs have leveraged RCTs in single jurisdictions (e.g., Yokum et al., 2019; Groff et al., 2020; Braga et al., 2022). 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.

[29]We are not using per capita estimates of the *stock* of incarceration sentences.

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).[30]

Across BWC adoption groups, disparities in per capita arrest, conviction, and incarceration rates are nearly identical (Rows 1−9 in Table 1).[31] 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 the participating elected District Attorneys (DAs). Sixteen of the state's forty-three DAs chose to participate, including the DAs in the state's four largest cities — Charlotte, Raleigh, Greensboro, and Durham. Figure 1(b) shows a map of participating offices. We fielded the survey from May to 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) as well as their tenure, race, age, and political views. Prosecutors took the survey online, and, in most offices, the elected DA directly emailed links to prosecutors encouraging them to complete the survey. Support from elected DAs led to a 39% participation rate in participating offices. We

[30] Disparities in arrests and downstream outcomes parallel nationwide disparities (FBI, 2019; Carson, 2019).
[31] This also holds in the years before any North Carolina county had adopted BWCs (see Table A.1).

surveyed 203 prosecutors, and 90 chose to debrief with us after over Zoom. We linked 175 respondents to the court records (a 86% match rate), and of these, 163 answered the question about police reports. On average, each surveyed prosecutor handled 69 felony cases and 332 misdemeanor cases per year. Together, surveyed prosecutors handled over half a million cases between 1995 and 2019. Since we fielded the survey in 2020, about 80% of prosecutors had been exposed to BWC footage at the time they took the survey, and 10% had not been exposed to BWCs. For the final 10%, BWC adoption timing is unknown.

Surveyed prosecutors' experience, demographics, and politics are broadly representative of line prosecutors in North Carolina (see Table 2). To assess representativeness, we merged in data on prosecutors' reported demographics and political affiliation from North Carolina Voter Records (see Appendix B for details).

IV Body-Worn Cameras

This section estimates the effects of BWCs on racial disparities in arrests and sentencing. We first present and validate our empirical design and then discuss the results.

IV.A Empirical Design

Our empirical design leverages the staggered roll-out of BWCs across North Carolina counties from 2014 to 2019 (shown in Figure 1(a)). We use the variation in adoption timing to estimate how BWCs affect the per capita outcome, 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). 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).[32] We then vertically concatenate these datasets into a single "stacked" dataset, where we index observations by s since some observations appear multiple times as controls for different sub-experiments. We allow the fixed effects for county

[32]Each dataset contains the observations for the treatment group — i.e., the adopting county in the sub-experiment — and the observations of the control 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).

(c) and year (t) to flexibly vary by sub-experiment (s) in:

$$\begin{aligned} Y_{rcts} &= \beta^B \text{ Adopted BWC}_{cts} + \mu^B_{cs} + \mu^B_{ts} + \epsilon_{rcts} && \text{if } r \text{ is Black} \\ Y_{rcts} &= \beta^W \text{ Adopted BWC}_{cts} + \mu^W_{cs} + \mu^W_{ts} + u_{rcts} && \text{if } r \text{ is White.} \end{aligned} \tag{4}$$

We also analyze racial disparities directly:

$$Y_{rcts} = \beta^{B-W} \text{ Adopted BWC}_{cts} \times \text{Black}_r + \beta^W \text{Adopted BWC}_{cts} + \mu_{rcs} + \mu_{rts} + \epsilon_{rcts}. \tag{5}$$

We estimate these equations using weighted least squares, where the weights are the relevant population in a given county and year. We estimate pooled effects in the five years pre- or post-BWC 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 the criminal-justice outcomes in treatment and control counties would have followed parallel trends in the absence of different BWC timing. Under this assumption, the difference-in-differences design helps to rule out 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. For instance, if other state-wide policies impacted criminal-justice outcomes across the state, this would not bias our results.

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 city-council minutes across all adopting places that mention BWCs. BWCs were almost always discussed as a free-standing agenda item and debates centered on timing and funding, not adoption itself. On average, over two years elapsed between initial discussions of BWC adoption and implementation. The upfront cost of cameras — but even more the ongoing expense of data storage — slowed the pace of BWC adoption. Most places also trialed BWCs in small pilot programs before rolling them out to all beat officers, further contributing to delays between initial discussions and broader implementation. We therefore show robustness to limiting our analysis to places that eventually adopted BWCs but at different times. We also show robustness to limiting to places where adoption timing was more plausibly unrelated to local policy shifts: specifically, those that had not recently elected a new sheriff; those that did not adopt other contemporaneous 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 prior to BWC adoption (Figure A.8), suggesting that BWCs were not adopted in response to local instances of police violence that could have prompted other policy changes.

Finally, we check for deviations from parallel trends before BWC adoption. Visually, we do not see any deviations in pre-trends using a fully dynamic version of Equation 4.[33] 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.

[33] Our dynamic DiD design builds on Equation 4 by allowing BWCs' effects to evolve over time. Defining e as the event time relative to BWC adoption in the focal county of sub-experiment s, we estimate:

$$Y_{rcts} = \sum_{e=-5}^{5} \beta_e \mathbb{1}[c = s] \cdot \mathbb{1}[t - e = t_s] + \mu_{cs} + \mu_{ts} + u_{rcts} \qquad \text{for r is Black and r is white} \tag{6}$$

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 3(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.3(a)).

BWCs' impacts on arrests are heavily concentrated in low-level offenses that almost never lead to incarceration, as shown in Figure 2(c). The largest declines in arrests are for public-order offenses like loitering and drug possession, where it is exceedingly rare for defendants to be incarcerated. Indeed, 96% of the aggregate decline in arrest rates for Black people comes from charges with less than a 1% chance of leading to 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 there is an arrest.

While BWCs' impacts on arrest rates could not, on their own, substantially reduce incarceration, BWCs could still impact incarceration by changing downstream decision-makers' information sets and resulting choices. We next turn to BWCs' impacts on incarceration and other outcomes downstream of arrest.

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. However, once BWCs are adopted, rates for Black people begin to decline in treated counties only. These patterns are consistent across alternative DiD estimators (Figure 3(c)).

Table 3(c) presents the pooled difference-in-differences estimate for incarceration. BWCs reduce Black people's incarceration rate by 0.029 pp (or 10.5%, Column 2). Yet, for white people, incarceration insignificantly increases by 0.001 pp (Column 3), leading to a meaningful decline in incarceration disparities (p-value = 0.0062, Column 4). Indeed, about 14% of the baseline disparity in incarceration rates is eliminated by BWCs. We see similar patterns for conviction (Table 3(b)) and jail time (Table A.3(b)).

Decomposing the Incarceration Effect. We decompose the total change in incarceration into the relative contributions of the arrest and post-arrest stages, using a Oaxaca-Blinder decomposition (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.[34] Holding constant the relationship between arrest charges and incarceration, BWCs' effects on arrests 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). Therefore, the bulk of the incarceration effect for Black people (about 84%) can be explained by a change in post-arrest decision-making conditional on the arrest charge.

Robustness. BWCs' estimated effects on arrest and incarceration disparities are robust to various checks on our design. In Table 3, Column 5 excludes never-adopters from our controls, and Column 6 excludes neighboring counties to handle potential spatial spillovers from BWCs. In Column 7, we allow for adoption-group specific pre-trends, which leads to slightly attenuated but qualitatively similar results. The results are robust to excluding the ten counties that elected a new sheriff in the election preceding BWC adoption (Figure A.4), dropping those that had other contemporaneous policing reforms (Figure A.3), and limiting 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

[34] To isolate the contribution of the arrest stage, we first predict incarceration at arrest using charge-specific incarceration rates from counties that had not yet adopted BWCs. We then use our DiD (Equation 5) to estimate how these predicted rates change around BWC adoption, yielding β^{Arrest}. We estimate the post-arrest contribution as $\beta^{\text{Post-Arrest}} = \beta^{\text{Total}} - \beta^{\text{Arrest}}$.

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 roll-out to officers, which would likely lead any confounding factors that are coincident 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 the two survey questions we use to study beliefs about the sources of disparities in justice involvement (in Section V.B) and the tendency to take information from police at face value (in Sections V.C-V.D).

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.[35] The survey first presented prosecutors with facts about existing disparities in criminal-justice outcomes.[36] It then asked them to rate (on a scale from 0 to 100) the importance of five potential drivers of these disparities, as shown in Figure 4(a).

On average, prosecutors attributed 18% of disparities to biased perceptions of conduct —

[35] Our pilot of the survey suggested that this approach reduced prosecutors' reluctance to answer questions about race. Yet 25% of prosecutors 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).

[36] This question included the following preamble: "In North Carolina, and in the US more generally, an average black person is arrested more frequently and convicted of more felonies than an average white person. There are many theories about what drives these race gaps — and everyone seems to have an opinion. We are interested in your perspective on what generates these aggregate differences."

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 7% of incarceration disparities to Black defendants having more severe current conduct and 14% to Black defendants having more severe 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%, respectively).

Prosecutors' beliefs about the sources of racial disparities predict the incarceration disparities in their own past cases. Because cases are assigned by rotation within most crime units (e.g., the drug unit in Raleigh), we can estimate prosecutors' impacts on incarceration disparities and relate these impacts to prosecutors' beliefs. (Section V.C discusses this design and validates quasi-random case assignment.) Prosecutors who attribute disparities more to biased perceptions of conduct significantly reduce disparities relative to other prosecutors in the same unit (the green point in Figure A.9). Suggestively, prosecutors who believe that disparities are driven more by differences in conduct tend to increase disparities relative to others in the same unit (the orange points in Figure A.9).[37] These beliefs remain predictive after controlling for prosecutor race and politics.

Our survey elicited prosecutors' tendency to take police reports at face value by asking about the share of cases they recalled feeling uncertain about the true severity of the defendant's conduct (a) after first reading the police report and (b) at the case's final disposition (see Figure 4(b) for the question interface). While prosecutors almost never felt uncertain at the final disposition, they varied widely in their uncertainty after first reading the police report. Some prosecutors generally believe police reports provide accurate and complete information, while others frequently question their reliability (shown in the histogram in Figure A.10(a)). On average, prosecutors say they are uncertain of the defendant's true conduct after reading the police report in 42% of their cases, indicating that they are certain of the officer's version of events 58% of the time.

[37]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.

Prosecutors' confidence in police reports predicts the incarceration disparities in their past cases: those who are more certain about police reports have larger disparities than less certain prosecutors working in the same unit (the blue point in Figure A.9). Prosecutors' confidence in police reports is not significantly related to their race, politics, or beliefs about the sources of disparities in justice involvement (Figure A.10(b)). Thus, controlling for these factors has no impact on the relationship between prosecutors' certainty in police reports and their incarceration disparities (Figure A.9). The fact that the question was not politically or racially polarizing may reflect its relatively neutral phrasing and the fact that we included this question *before* any questions about race or bias.[38]

V.B Inaccurate Beliefs

This section explores whether prosecutors held inaccurate beliefs about police bias and reliability by testing whether exposure to BWCs shifts prosecutors' beliefs about police.

Exposure Design

Our design leverages the fact that, *within* a given county and year, 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-date com-

[38]Only about 8% of the variation in prosecutors' certainty can be explained by their office (Figure A.11).

parison 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 counties, and τ prosecutor tenure, we estimate:

$$\text{Belief}_p = \phi \text{Years of BWC}_p + \mu_{c_p} + \mu_{\tau_p} + \epsilon_p. \tag{7}$$

County fixed effects, μ_{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, μ_{τ_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, in the absence of BWCs, the relationship between prosecutors' beliefs and tenure would have been similar in counties that adopted earlier versus later. We test the validity of this assumption by limiting to prosecutors hired before their county adopted BWCs — who all have *the same* years of exposure to BWCs despite differing in their tenure. Consistent with our identifying assumption, tenure does not differentially predict beliefs across earlier and later adopters among prosecutors all hired before BWC adoption (Figure A.16).

We estimate the analogue of Equation 7 in the court records to evaluate how a prosecutor's exposure to BWCs predicts her effects on incarceration disparities in her cases (see Section V.C for how we estimate and validate prosecutors' disparate impacts). Here, we include race-county-year fixed effects to absorb any direct effects of BWCs that impact *all* prosecutors.[39] This design relies on an analogous identifying assumption as above.

[39] We estimate:

$$\text{Incar}_i = \gamma \text{ Years of BWC}_{p(i)} \times \text{Black}_i + \theta \text{ Years of BWC}_{p(i)} + \mu_{\text{Black}_i, c(i), t(i)} + \mu_{\text{Black}_i, \tau_{p(i), t(i)}} + \mu_{\text{unit}(i)} + \zeta_i. \tag{8}$$

The coefficient of interest, γ, aims to isolate BWCs' cumulative effect on prosecutors, which may reflect learning from watching unfiltered footage in more cases.

Reassuringly, we find that prosecutor tenure does not differentially predict incarceration disparities across counties in the years before any county adopted BWCs (Figure A.17).

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 conduct after controlling for county and tenure (as in Equation 7). Each year of additional 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 (shown in the first and third points in Figure 4(b)). These patterns suggest that BWC footage shows prosecutors a reality different from the one previously presented by police: a reality where disparities in arrests are driven more by racial bias and less by Black people having worse criminal conduct.

Reassuringly, we find that prosecutors' BWC exposure is unrelated to their beliefs about drivers of disparities that one would *not* expect to be revealed by BWC footage: racial differences in the quality of legal representation and the length of defendants' criminal records (their "prior points"), both shown in gray in Figure 4(b).

Beliefs about Police Reports. Prosecutors with greater BWC exposure view police reports as less reliable. Each additional year of BWC exposure is associated with a 4.5 pp decline in the share of cases where prosecutors feel certain about what the defendant did after reading the police report (p-value = 0.09). On the one hand, this shift is unsurprising since BWC footage can allow prosecutors to see when police reports diverge from the on-scene reality (as discussed in Section I). On the other hand, one could have imagined the opposite effect if BWCs incentivize police to characterize arrests much more accurately. Our results suggest that prosecutors' updating about police is dominated by their increased awareness of police misrepresentation — rather than by increased police reliability.[40]

Alternative Explanations. We find that the shift in prosecutors' beliefs is unlikely to be driven by changes in the selection of prosecutors. Prosecutor turnover is similar before and after a county adopts BWCs (rows 9−10, Table A.2). In addition, prosecutors with more BWC exposure are not significantly more (or less) likely to be Black or liberal (Figure A.14), and the shift in beliefs is similar after controlling for prosecutor race and politics (Table A.8). Our results likely do not simply reflect a more general shift toward progressivism that happens to coincide with BWC exposure length. Specifically, prosecutors with more BWC exposure are not more (or less) likely to believe that discrimination drives economic disparities or to support affirmative action (Figure A.14). Instead, the shift in beliefs seems specific to prosecutors' beliefs about the data-generating process in their cases.

Implications for Incarceration Disparities. To understand the real-world consequences of the shift in prosecutors' beliefs, we use our exposure design to compare the incarceration disparities of prosecutors within the same county who vary in their past exposure to BWCs — but who have the same access to BWC footage in their current cases. This design helps us isolate how past exposure to BWCs affects decisions, such as through learning about police reliability *in all cases* — while holding constant any direct effects of BWCs, such as watching the footage and changing one's mind about what happened *in the individual case*.

Prosecutors with more past exposure to BWCs reduce incarceration disparities after controlling for county and tenure. Each additional year of exposure to BWCs 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 (in the right-most point in Figure 4(b)) and a 10% reduction of the raw race disparity in incarceration rates.

We can also approximate the real-world implications of prosecutor learning using the estimated shift in prosecutors' beliefs more directly. If we make the strong (or, perhaps, heroic)

[40] It is unsurprising that prosecutors' certainty about the defendant's conduct at the final resolution of the case does not shift with BWC exposure, given their high baseline degree of certainty (see Figure A.10).

assumption that the relationship between prosecutors' beliefs and their disparate impacts is causal, we can rescale 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.[41]

Using analogous reasoning, we can also 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 bias in justice involvement (see Figure 4). Taken together, these results suggest that BWCs' aggregate impact on incarceration reflects prosecutors updating their beliefs about their cases.

In addition to changing prosecutors' beliefs about the data-generating process, BWCs may change the data-generating process itself: monitoring police may make them more reliable and less biased. If this were true, we would expect this change to matter most for prosecutors who have a tendency to take police reports at face value.

V.C Certainty in Police Reports

This section analyzes how prosecutors' certainty in police reports predicts their incarceration disparities before and after BWC adoption. We interpret the results cautiously since beliefs are elicited after many prosecutors had viewed BWC footage and after all cases in our sample were resolved. The timing of elicitations, therefore, blurs the distinction between beliefs as inputs to decisions and beliefs as outputs of past experiences. Even so, the pattern of results in this section would be difficult to reconcile with reverse causality alone.

Empirical Design. We estimate disparities in incarceration outcomes across prosecutors who handle similar cases — but who have different beliefs about police reports:

[41] We then 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.

$$\text{Incarceration}_i = \lambda \text{Black}_i \times \text{Certainty in Reports}_{p(i)} + \alpha \text{Black}_i + \mu_{p(i)} + X_i' \psi + \nu_i, \tag{9}$$

where Certainty in Reports$_{p(i)}$ 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), $\mu_{p(i)}$ are prosecutor fixed effects, and X_i is a vector of case characteristics used to determine case assignment. Cases are typically quasi-randomly assigned within a unit of an office: 77% of surveyed prosecutors report that cases are assigned based on a rotation system, either in their office overall or within their unit (e.g., Charlotte's drug unit). X_i includes controls for the "unit" — defined by the case's office, year, and crime type.

We validate quasi-random assignment in two ways. First, we show that the composition of a prosecutor's caseload is not related to her estimated impact on incarceration disparities (Figure A.12). Second, we show that when a prosecutor moves to a different office, her estimated disparate impact moves with her (across-office coefficient of 0.90, Figure A.13). This suggests our results are not driven by non-random case assignment, which would likely vary by office — or by match effects with local judges or defense attorneys.[42] Under as-good-as-random case assignment, we can interpret λ in Equation 9 as the relationship between a prosecutor's certainty in police and her causal effect on incarceration disparities.

We estimate the relationship between certainty and disparities with and without BWCs:

$$\begin{aligned}\text{Incarceration}_i =& \gamma \text{No BWC}_{c(i),t(i)} \times \text{Black}_i \times \text{Certainty}_{p(i)} + \delta \text{No BWC}_{c(i),t(i)} \times \text{Black}_i \\ &+ \kappa \, \text{BWC}_{c(i),t(i)} \times \text{Black}_i \times \text{Certainty}_{p(i)} + \alpha \text{BWC}_{c(i),t(i)} \times \text{Black}_i \\ &+ \psi \text{No BWC}_{c(i),t(i)} \times \text{Certainty}_{p(i)} + \nu \text{BWC}_{c(i),t(i)} \times \text{Certainty}_{p(i)} + \mu_{p(i),\text{BWC}_{c(i),t(i)}} + X_i' \Theta + u_i.\end{aligned} \tag{10}$$

Before BWCs are adopted, we expect certainty in police reports to affect incarceration disparities ($\gamma \gtrless 0$). Whenever the prosecutor is certain in the police report — and so

[42]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.

treats it as the ground truth of what happened in the case — she will inherit the police's account and ignore her own prior belief about racial differences in crime. Thus, certainty in police reports increases disparities when police signals are more racially disparate than prosecutors' priors — and reduces disparities when the reverse is true. After BWCs, we expect certainty in the police to be less predictive of disparities ($|\kappa| < |\gamma|$), either because BWCs reduce bias in police reports or because the footage sometimes allows prosecutors to directly see that police mischaracterized arrests of Black people.

We also expect certainty in police reports to play a more limited role in arrests for violent and property crimes, where victim and witness accounts can provide independent information. In these cases, prosecutors will often be less reliant on the officer's version of events, so certainty in police reports should matter less both before and after BWCs.

This leaves us with two placebo checks: that prosecutors' certainty will matter less (1) after BWCs are introduced and (2) in cases with victims or witnesses. We use these checks below to help validate that our results reflect how prosecutors process signals from police — rather than reflecting reverse causality.

Results. Figure 5 shows the relationship between prosecutors' certainty in police reports separately in counties with and without BWCs. In counties that have not yet adopted BWCs, prosecutors who are more certain in 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 in police reports increases disparities by 1.52 pp compared to a prosecutor who always questions them (p-value = 0.048, Column 4, Table 4). This represents about a 50% reduction in incarceration disparities, given a raw disparity of 3.1 pp. By contrast, in counties with BWCs, there is no systematic relationship between prosecutor certainty and their disparate impacts.[43] 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. Taken together, these patterns suggest that surveil-

[43]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.4).

lance acts as a substitute for prosecutor skepticism. Table 4 shows robustness to controlling for the defendant's specific arrest charge, criminal record, and demographics as well as allowing unit effects to vary by defendant race. These results are also robust to controlling for prosecutor race and politics (Table A.5).

The fact that prosecutors' beliefs about police reliability predict disparities only before BWC adoption — and not afterward — is hard to reconcile with a reverse-causality explanation. Suppose, for example, that beliefs about police reliability were formed by exposure to BWC footage. In this case, prosecutors who observe more discrepancies between footage and police reports for Black defendants may both update their beliefs about police reliability and adjust their charging or sentencing recommendations in those cases. As a result, elicited beliefs should be *more* predictive of disparities after BWC adoption, not less predictive. More generally, if prosecutors' beliefs primarily reflect learning from past cases, we would expect those beliefs to continue to predict outcomes in their most recent cases, many of which occur with BWCs.[44]

We also find that prosecutor certainty in police reports *only* matters for arrests that were likely initiated by a discretionary police stop, including public-order, drug, and weapon possession arrests (in blue in Figure 5(b)). By contrast, prosecutor certainty does not predict disparities in violent and property arrests, where victims and witnesses play a larger role — either with or without BWCs (in gray). This placebo check helps assuage other concerns about causality. Suppose, for instance, that prosecutors' beliefs about police were correlated with their taste-based racial preferences. In that case, those beliefs would predict their disparities across *all* cases — both when police have more and less discretion and in cases before and after BWCs. More generally, if the relationship between certainty and disparities is spurious, it would be hard to explain why beliefs predict disparities *only* before BWCs are adopted and *only* when police have greater discretion. Yet this is exactly the pattern we find.

[44] As an additional check, we estimate the relationship between prosecutor certainty and disparities, but restricting to counties that had not adopted BWCs by the time of our survey. The results are similar, though estimated with less precision.

V.D Mistaken Learning

If a prosecutor repeatedly reads in police reports that Black defendants commit more serious offenses than white defendants — and believes these reports to be accurate — she may come to view observed disparities as reflecting real differences in crime. In a final exploratory analysis, we therefore examine how incarceration disparities evolve as prosecutors gain experience in the years prior to BWC adoption, and whether this evolution differs depending on prosecutors' certainty in police reports:

$$\begin{aligned}\text{Incarceration}_i =& \omega \text{Black}_i \times \text{Certainty in Police Reports}_{p(i)} \times \text{Tenure}_{p(i),t(i)} + \delta \text{Black}_i \times \text{Tenure}_{p(i),t(i)} \\ &+ \tau \text{Tenure}_{p(i),t(i)} + \delta \text{Certainty}_{p(i)} \times \text{Tenure}_{p(i),t(i)} + \mu_{p(i),\text{Black}_i} + X_i' \gamma + v_i. \quad (11)\end{aligned}$$

The coefficient of interest, ω, captures whether the disparities of more certain prosecutors increase more (or less) with tenure than those of less certain prosecutors. Defendant race by prosecutor fixed effects, $\mu_{p(i),\text{Black}_i}$, net out fixed 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 (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 incarceration disparities by 1.1 pp, more than one-third of the raw race disparity. This pattern is consistent with prosecutors who repeatedly treat police reports as the ground truth coming to view racial differences in crime as larger than they had thought. In this sense, biased beliefs may originate within the criminal justice system itself.

Table A.6 tests this relationship using a continuous interaction between certainty and tenure (Equation 11). We show that similar effect sizes across a variety of specifications, with disparities increasing significantly more with tenure for more certain prosecutors throughout.[45] This pattern is also robust to controlling for prosecutor race and politics (Table A.7).

VI Theoretical Implications

We draw out the theoretical implications of our empirical results under the assumptions in the model in Section II. The table below summarizes these implications for the model's parameters: prosecutors' beliefs and police bias.

We first find that BWCs reduce incarceration disparities. Through the lens of the model, this result indicates that prosecutors had been misprocessing police signals before BWCs. One possibility is that prosecutors' priors about racial differences in crime had been biased up ($\bar{\bar{\kappa}}_{p,n} > \kappa$), and then BWCs allow them to update toward the true difference, causing disparities to fall. Another possibility is that prosecutors had sometimes taken biased police reports at face value ($\alpha_p > 0$), and BWCs weakened this channel either by reducing prosecutors' trust in police ($\alpha_{p,f} < \alpha_p$) or by making police more trustworthy ($b_f < b$).

Our Result	**Model Implication**
BWCs reduce downstream disparities	$\bar{\bar{\kappa}}_{p,n} - \kappa > 0$ and/or $\alpha_{p,f} < \alpha_p$ and $b > \bar{\bar{\kappa}}_{p,n} - \kappa$ and/or $b_f < b$ and $\alpha_p \geq \alpha_{p,f} > 0$
Before BWC, prosecutors who are more certain in police increase incarceration disparities	$b > \bar{\bar{\kappa}}_{p,n} - \kappa$
Before BWC, prosecutors who are more certain in police increase disparities more with tenure	$b > \bar{\bar{\kappa}}_{p,o} - \kappa$
Prosecutors with more exposure to BWCs believe true racial differences in conduct are smaller	$\bar{\bar{\kappa}}_{p,n} - \kappa > 0$

$\alpha_p \equiv$ Share of cases the prosecutor treats police signals as the truth, $\alpha_{p,f} \equiv$ share after BWCs
$b \equiv$ Bias in police signal, $b_f \equiv$ bias in signal post-BWC
$\kappa \equiv$ True racial differences in conduct
$\bar{\bar{\kappa}}_{p,n} \equiv$ Prosecutors' average prior after $n \geq 0$ pre-BWC signals

We use our survey of prosecutors to investigate these possibilities. We find that prose-

[45]For the most skeptical prosecutors, incarceration disparities decline with tenure (the tenure-by-race interaction in row 2). One interpretation is that these prosecutors learn from less biased sources than police reports — such as witness testimony — leading them to revise their priors about racial differences downward.

cutors with more exposure to BWCs believe disparities in the system are driven more by racial bias and less by true differences in crime. This shift in beliefs is consistent with BWCs revealing a reality where disparities are less justified by differences in crime than prosecutors had believed before gaining access to more objective information ($\bar{\bar{\kappa}}_{p,n} > \kappa$).

We also find that, before BWCs, prosecutors who report being certain in police reports more frequently (higher α_p) increase incarceration disparities. In our model, these prosecutors place greater weight on police signals relative to their priors. Thus, for these more certain prosecutors to increase disparities, police signals pre-BWC must have been more racially disparate than prosecutors' priors ($b > \bar{\bar{\kappa}}_{p,n} - \kappa$). Given that prosecutors come to believe that disparities in crime are smaller after seeing BWC footage ($\bar{\bar{\kappa}}_{p,n} > \kappa$), these two findings together imply that police reports had been racially biased — that the disparity in their reports exceeded both prosecutors' priors and the truth ($b > \bar{\bar{\kappa}}_{p,n} - \kappa > 0$).

Finally, we use our survey to investigate whether prosecutors' beliefs were distorted by police reports themselves. In the model, prosecutors who treat police reports as the ground truth use them to update their priors. Consistent with this mistaken learning, we find that among those most certain in police reports, incarceration disparities rise sharply with experience in the years before BWCs. For this to hold, police must have characterized crimes as more racially disparate than prosecutors had initially believed ($b > \bar{\bar{\kappa}}_{p,o} - \kappa$).

VII Conclusion

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

We use the shock of body-worn cameras to show that prosecutors had previously misprocessed information from police. We then unpack the mechanisms using an original survey of North Carolina prosecutors, linked to their administrative court records. Together, our results suggest that officers wrote racially biased police reports before being monitored by BWCs. Our main contribution is to unpack *why* prosecutors let police bias affect downstream disparities. Our results suggest that some prosecutors treated police reports as the ground truth and, over time, internalized the biased reality these reports presented.

Although we focus on prosecutors and police, our findings have implications for other settings where people rely on others' potentially biased judgments. Similar dynamics may arise at other stages of the criminal justice system, as well as in hiring decisions, credit assessments, and college admissions. In these contexts, an agent's mental model of upstream discretion may be a key determinant of the disparities produced by the system. This dimension of systemic discrimination suggests new strategies to combat bias. Existing evidence on more direct interventions such as implicit bias trainings show limited impacts on disparities (Forscher et al., 2019). Monitoring earlier actors — or simply making later decision-makers more aware of the potential for earlier bias — may be more effective.

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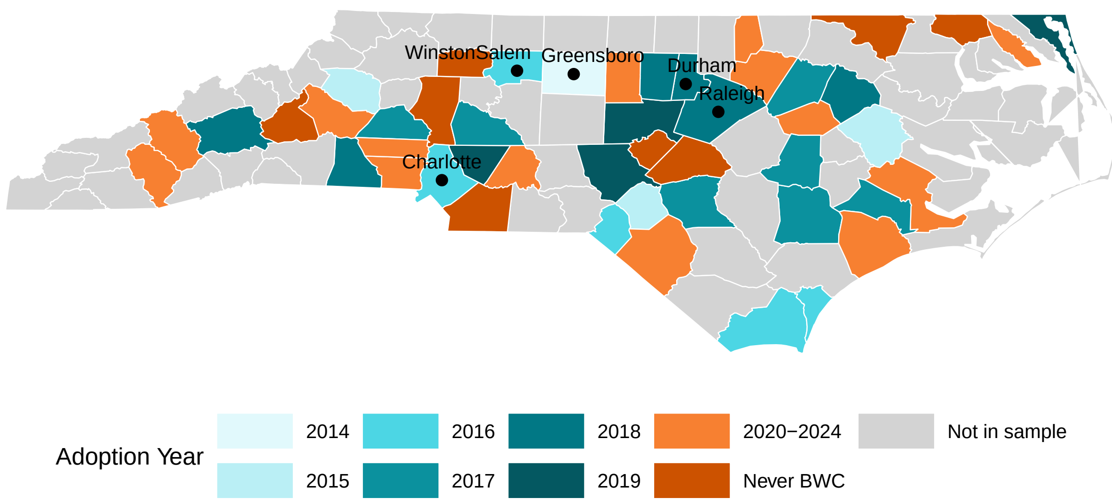
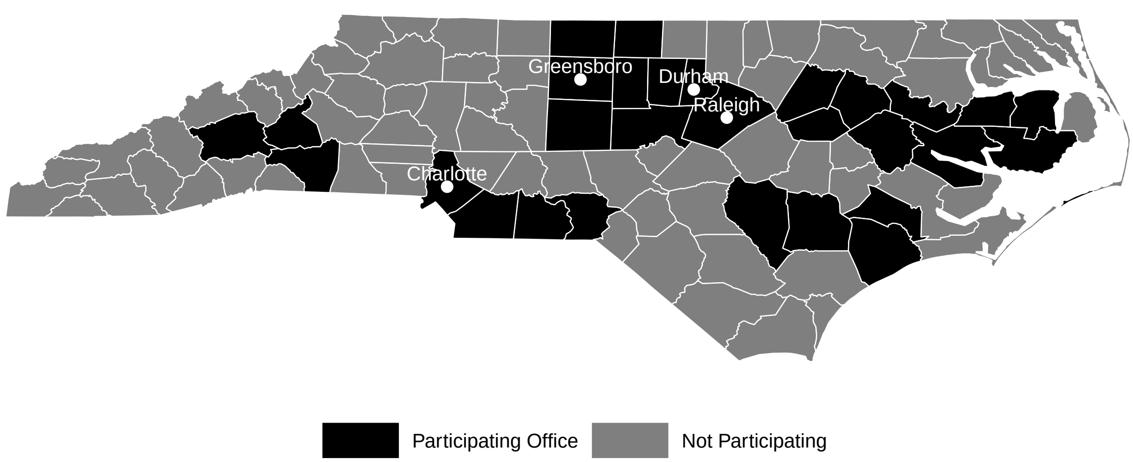
Figure 1: Geographic Coverage of Analyses

(a) Body-Worn Camera Adoption Across North Carolina

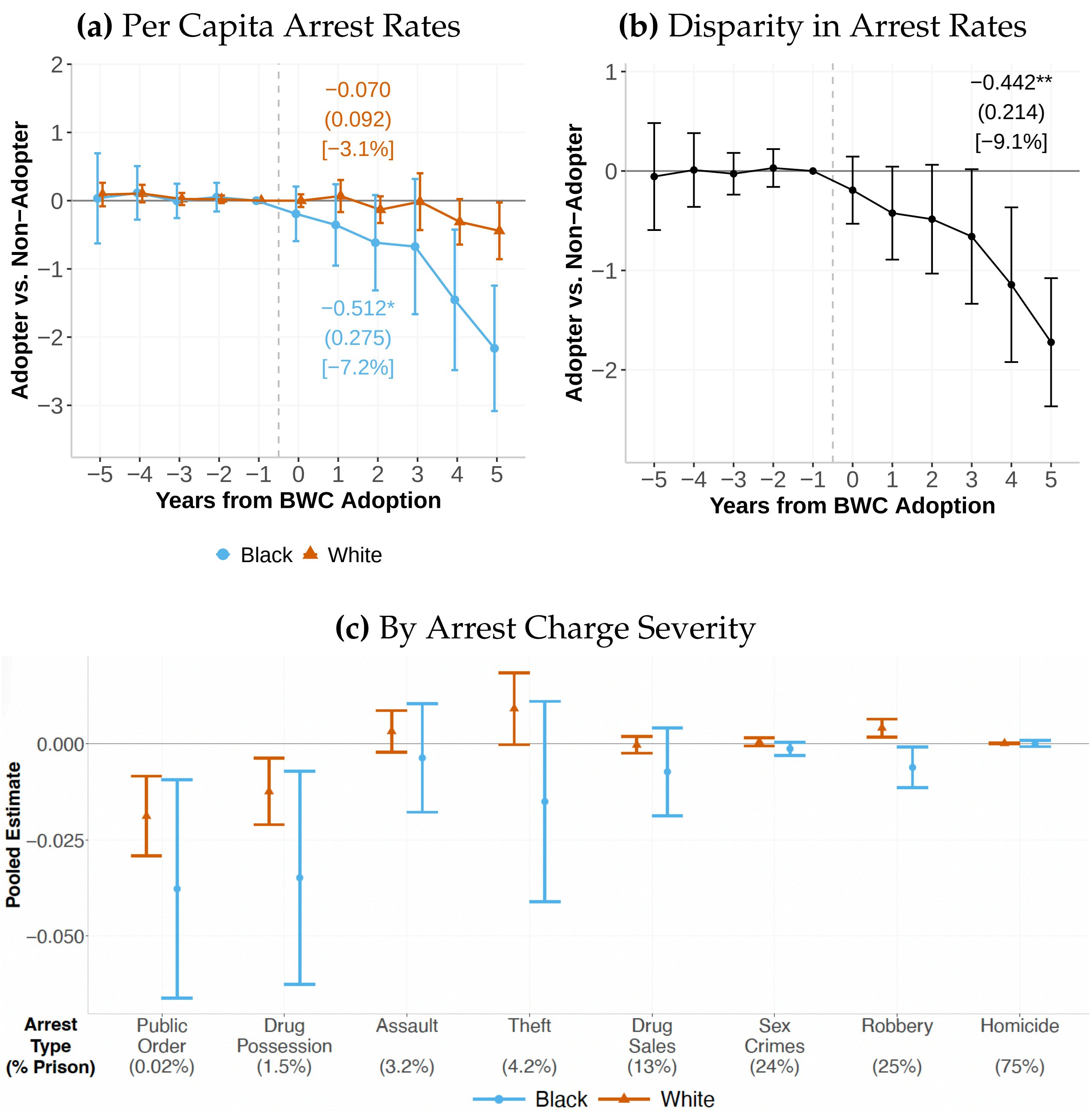


(b) Map of Offices Participating in our Survey



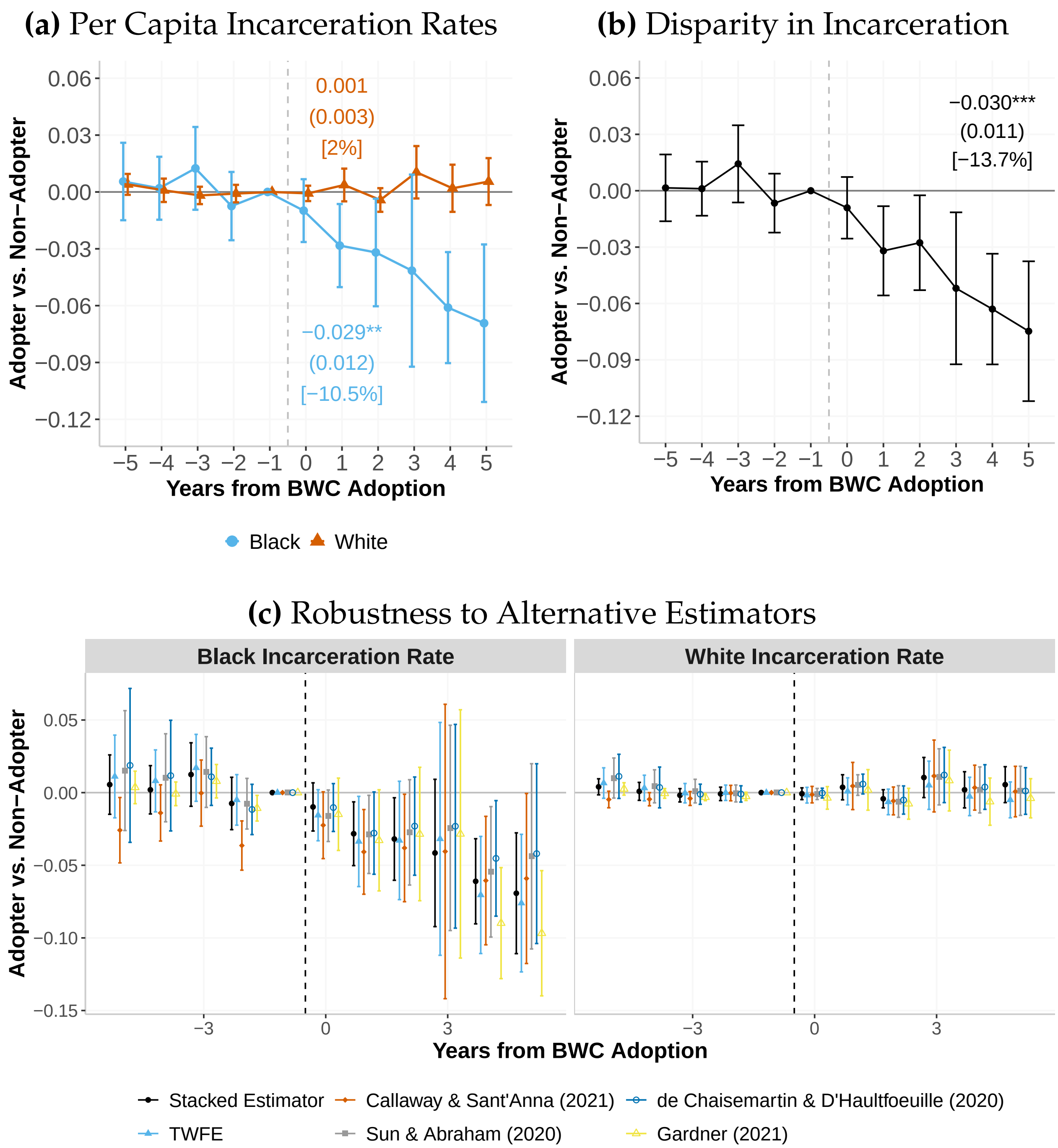
Notes: This figure illustrates the geographic coverage of North Carolina for our two analyses. Map (a) illustrates the staggered timing of body-worn cameras (BWCs) adoption across North Carolina counties. Section III.B describes how we determined the year of BWC adoption, by supplementing existing surveys (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 6, with the year before BWC adoption as the reference year. All specifications are weighted by population. The annotated coefficients are pooled estimates from Equation 4, 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.

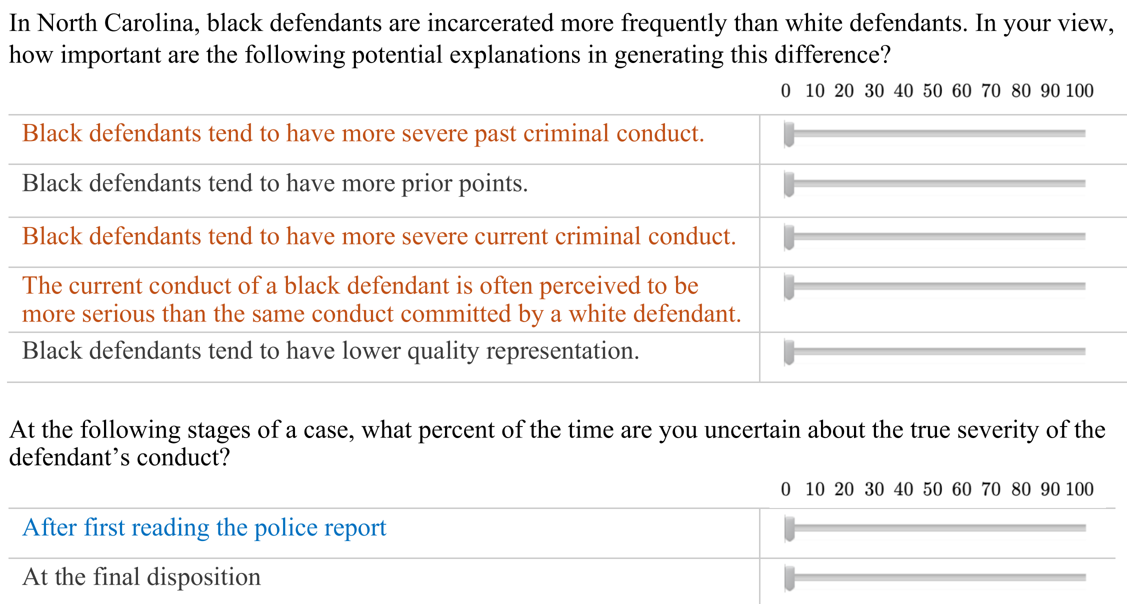
Figure 3: Body-Worn Camera Effects on Incarceration Rates



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 6, with the year before BWC adoption as the reference year. The annotated coefficients are pooled estimates from Equation 4, 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 3. *p<0.1; **p<0.05; ***p<0.01.

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

(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?

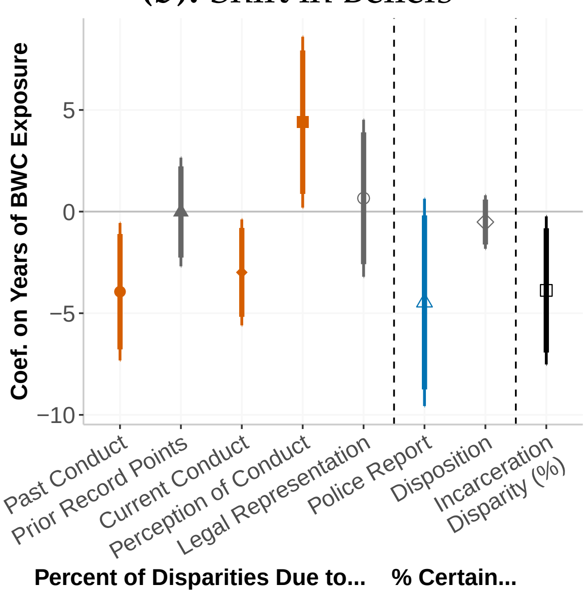
	0 10 20 30 40 50 60 70 80 90 100
Black defendants tend to have more severe past criminal conduct.	
Black defendants tend to have more prior points.	
Black defendants tend to have more severe current criminal conduct.	
The current conduct of a black defendant is often perceived to be more serious than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

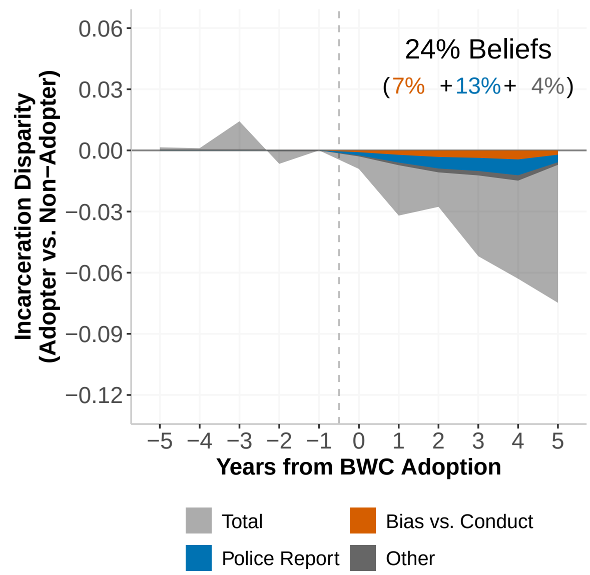
	0 10 20 30 40 50 60 70 80 90 100
After first reading the police report	
At the final disposition	

Colors added for clarity

(b): Shift in Beliefs

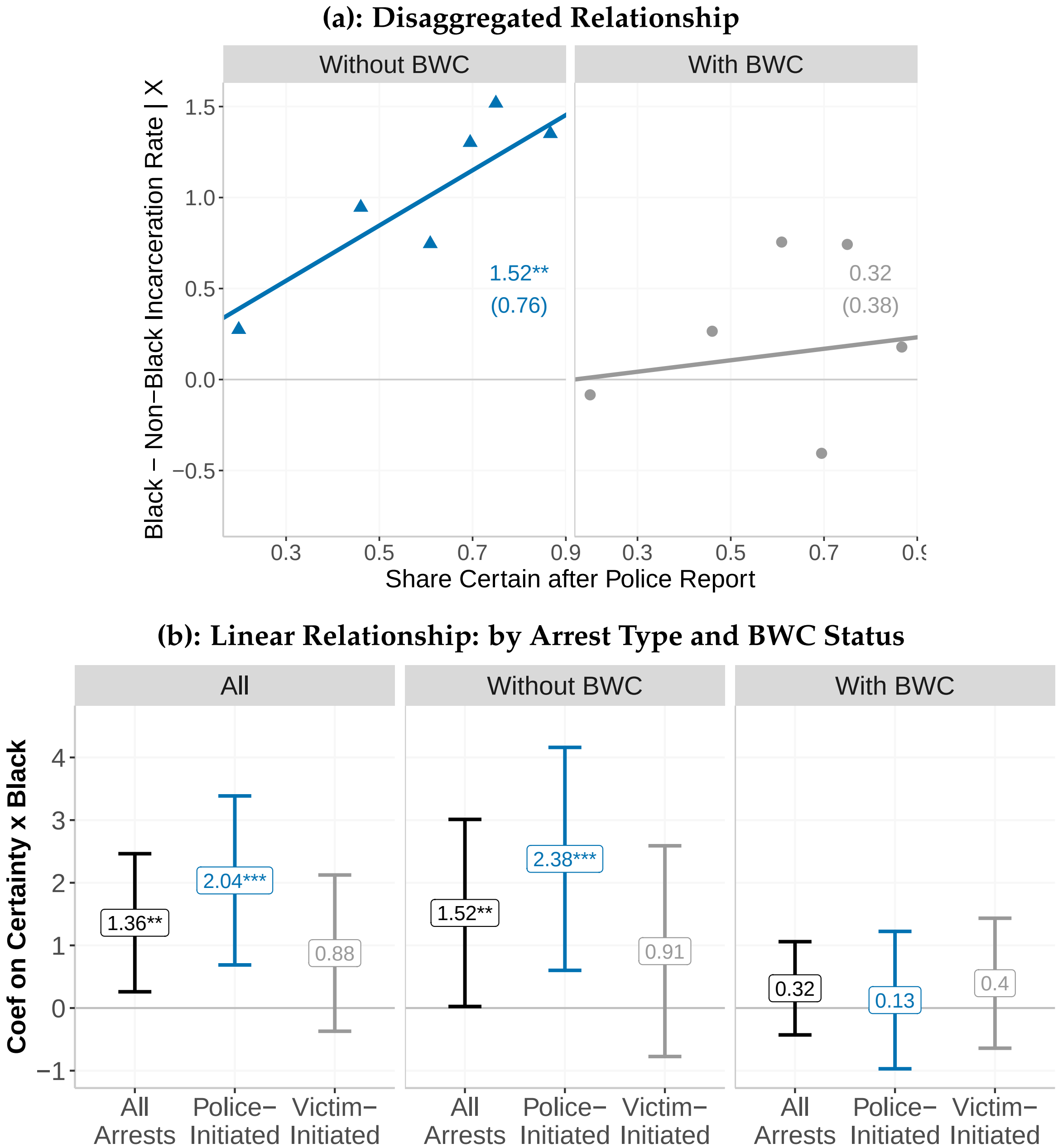


(c): Contribution to BWC Effects



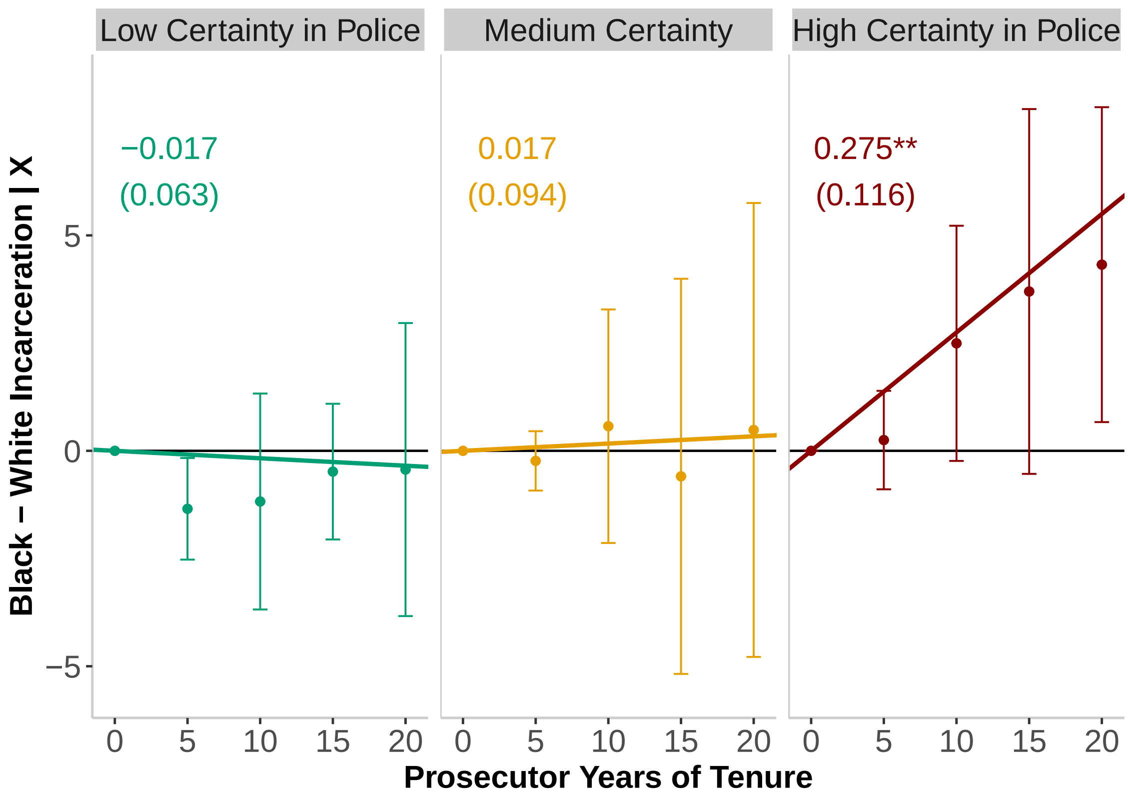
Note: This figure illustrates the relationship between prosecutors' years of exposure to body-worn cameras (BWCs) and their beliefs about their cases as well as their impacts on incarceration disparities. Panel (a) shows the interfaces for our two survey questions. The first question asked prosecutors' about the sources of racial disparities in criminal-justice outcomes, and the second asked about prosecutors' certainty about the defendants' conduct at different stages in the case. We have highlighted each of the sliders in colors matching those 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 7. The last point reflects the percent change in prosecutors' incarceration disparities with years of exposure to BWCs, using Equation 8, 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



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 9), 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 10). 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 11, 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.6). 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

			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	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: 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 3: 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 4, where each observation is a county in a given year (and sub-experiment).Columns 4−7 estimate Equation 5, 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.

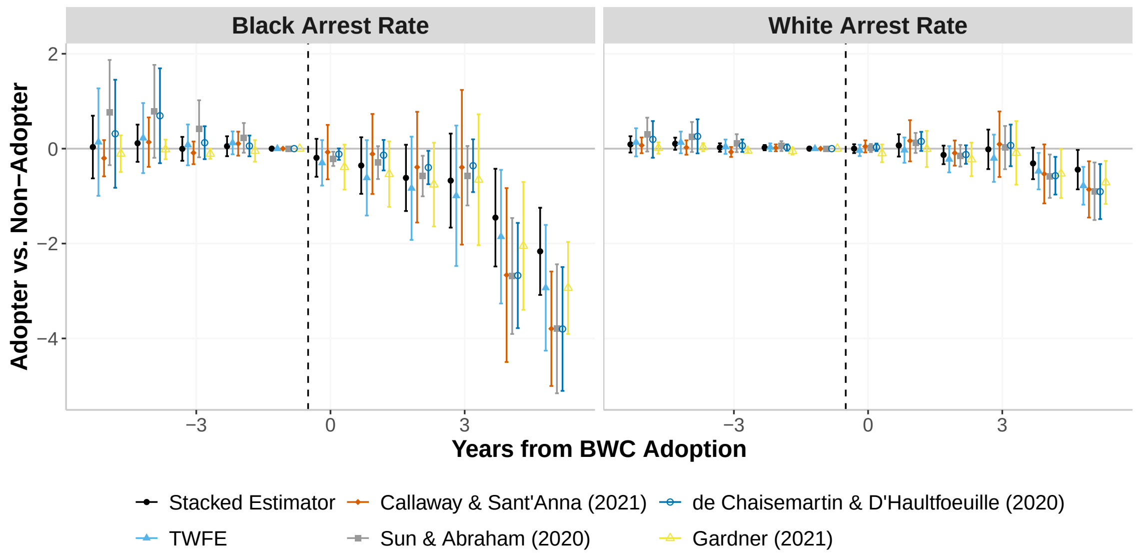
Table 4: The Relationship Between Prosecutors' Certainty After Reading Police Reports and the Incarceration Disparities in Their Cases

	% 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 9 for the full sample of cases handled by prosecutors who participated in the survey; column 4−6 estimate Equation 10 for cases handled by surveyed prosecutors in counties where we can determine BWC adoption timing; and column 7−9 estimate Equation 10 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.5 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. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

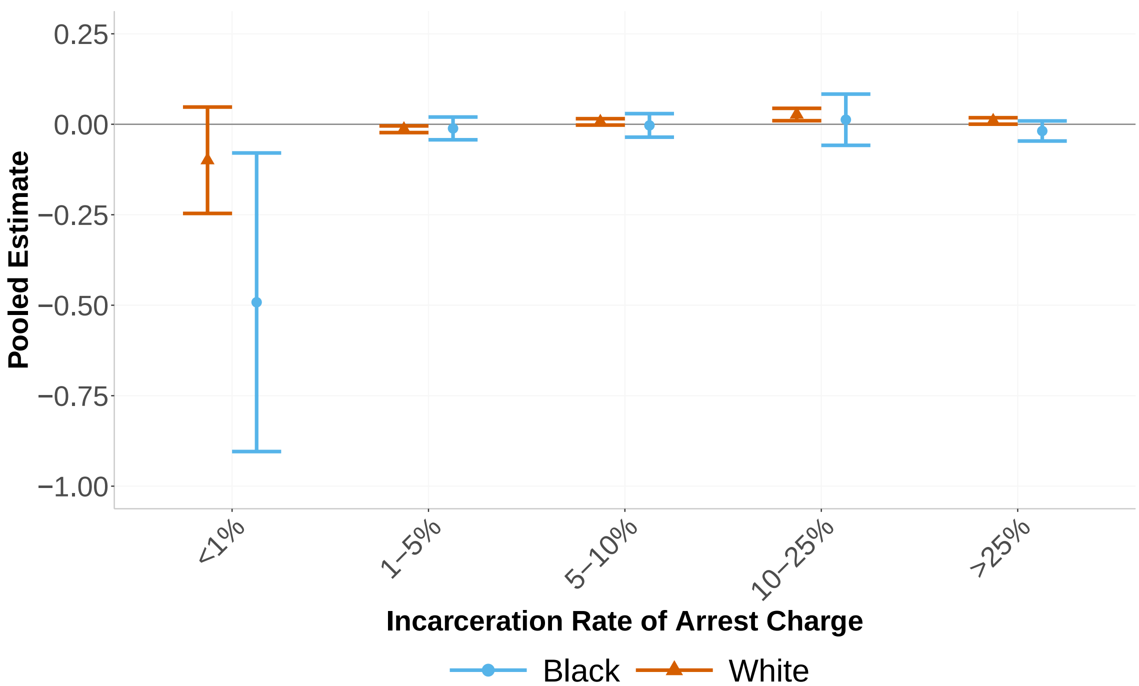
A. Online Appendix

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



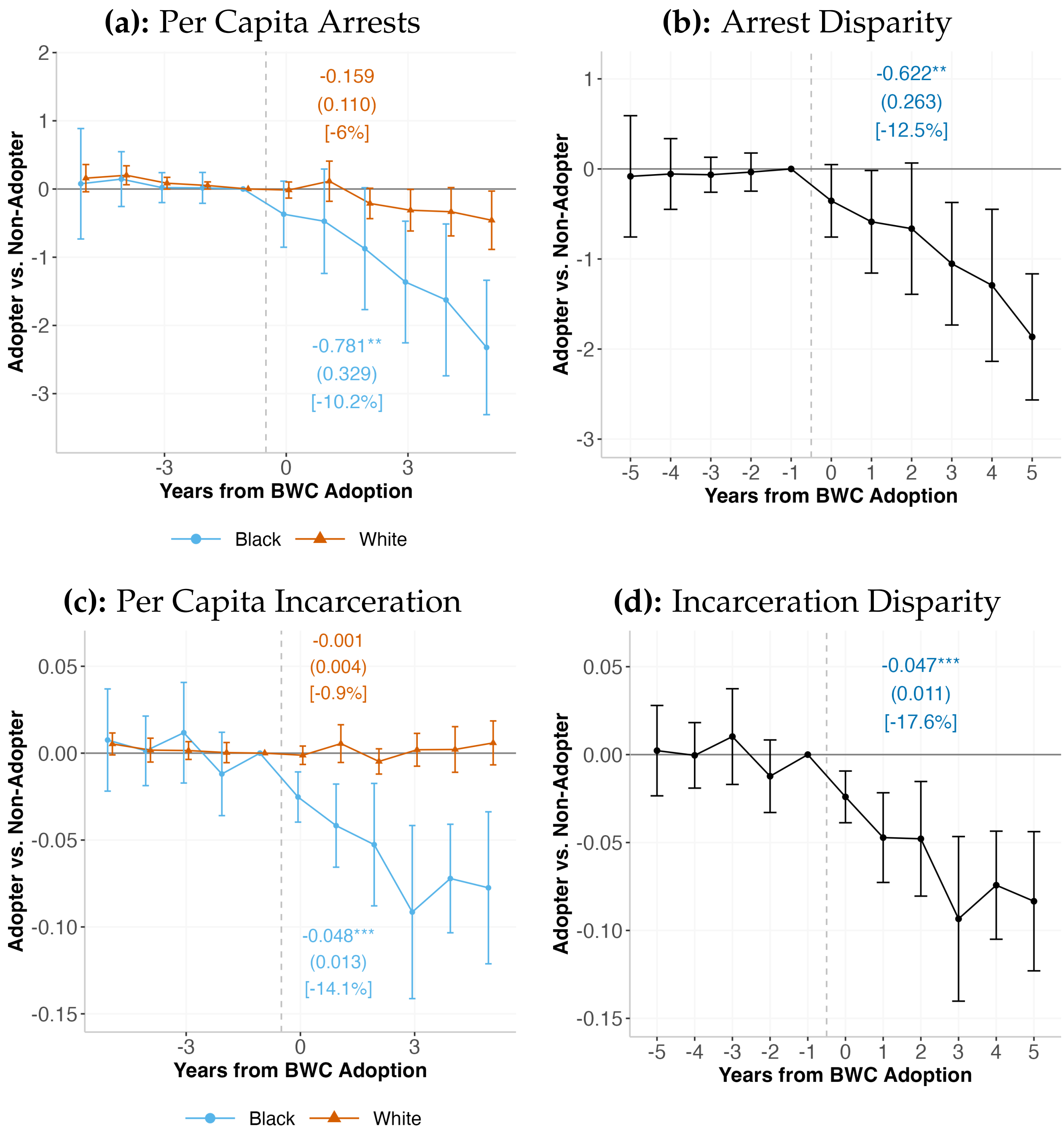
Notes: This figure illustrates the estimated effects of BWCs on arrests rates using alternative DiD estimators: our preferred stacked approach (also shown in Figure 2(a)), a two way fixed effects (TWFE) estimator, and estimators proposed by Callaway and Sant'Anna (2021), Sun and Abraham (2021), De Chaisemartin 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



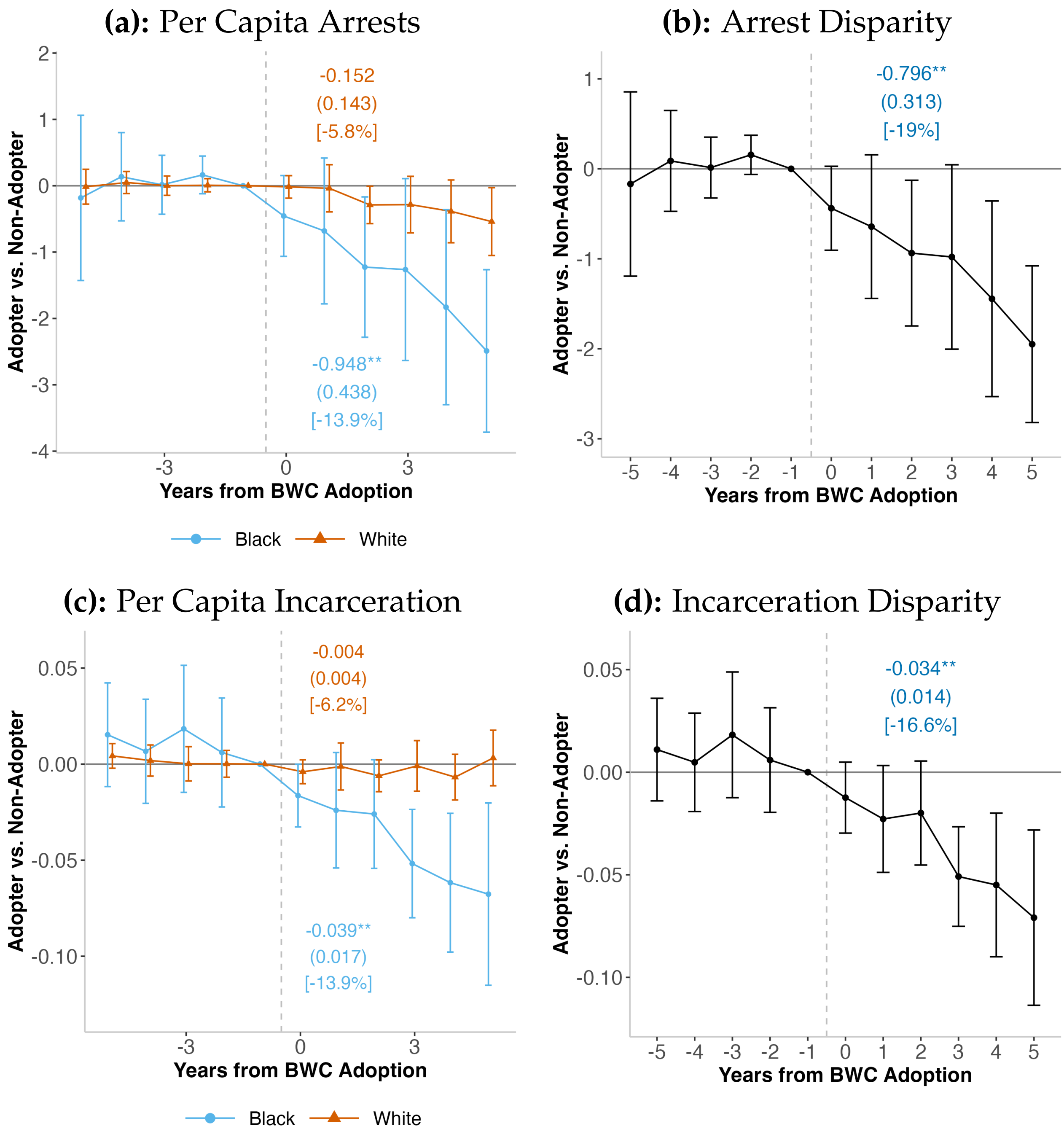
Notes: This figure illustrates the impact of body-worn cameras (BWCs) on per capita arrests rates, separately for arrest charges that have different percent chances of leading to incarceration. All points reflect pooled estimates from Equation 4, comparing the five years post-BWC adoption to the five 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



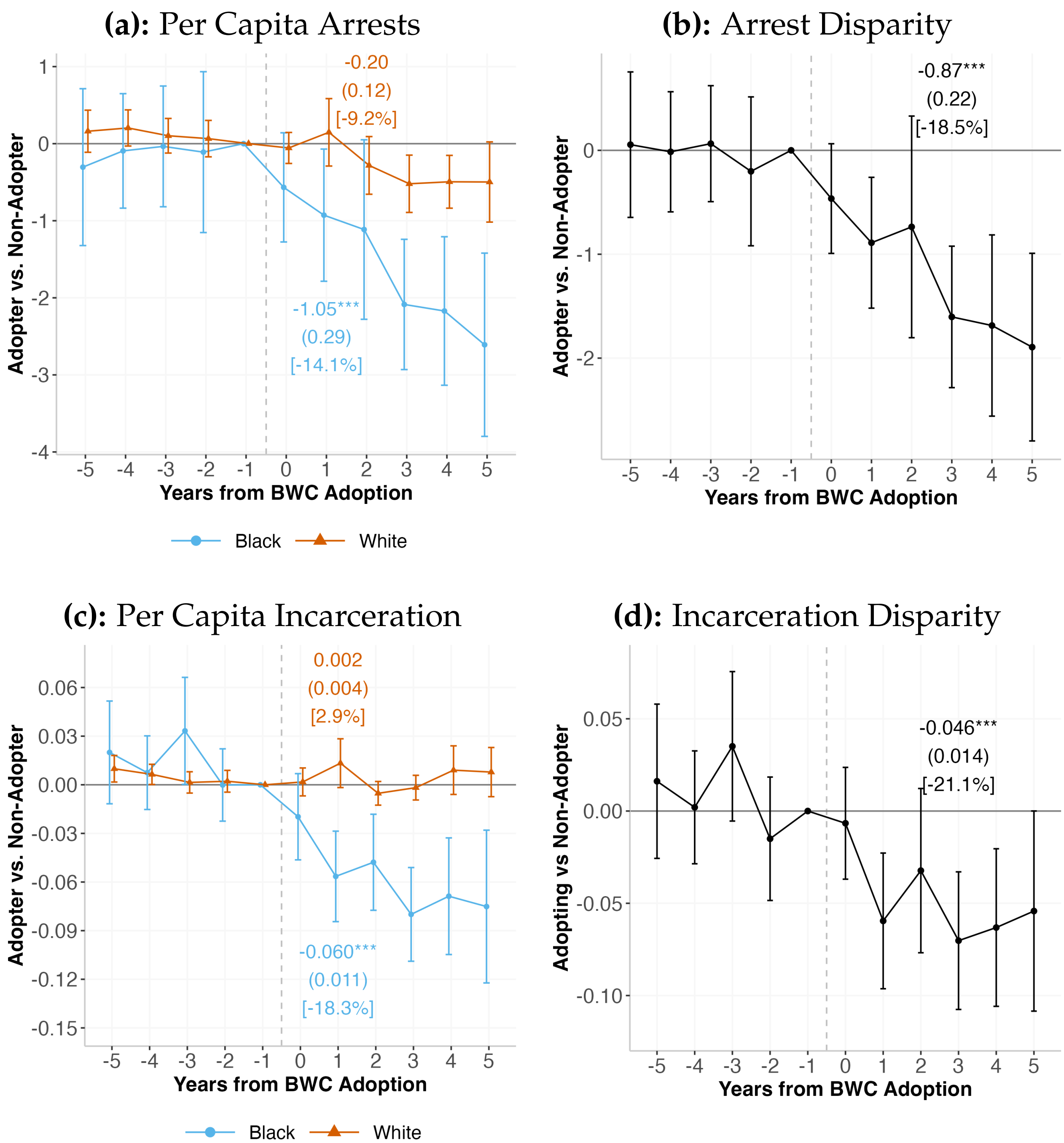
Notes: This figure illustrates the changes in arrests and incarceration rates in NC counties that adopt body-worn cameras (BWCs) relative to counties that have not yet adopted BWCs (or never do) — but restricts to counties whose sheriffs do not change in the election preceding the adoption of BWCs. For 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 6, with the year before BWC adoption as the reference. All specifications are weighted by population. The annotated coefficients are pooled estimates from Equation 4, 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 6, with the year before BWC adoption as the reference. All specifications are weighted by population. The annotated coefficients are pooled estimates from Equation 4, 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.

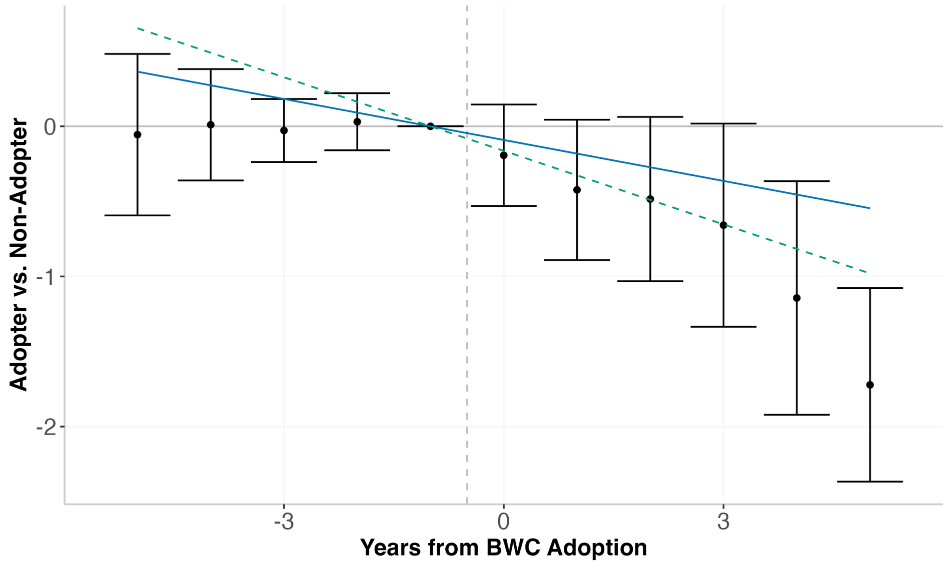
Figure A.5: Robustness to Limiting to Federally Funded BWCs



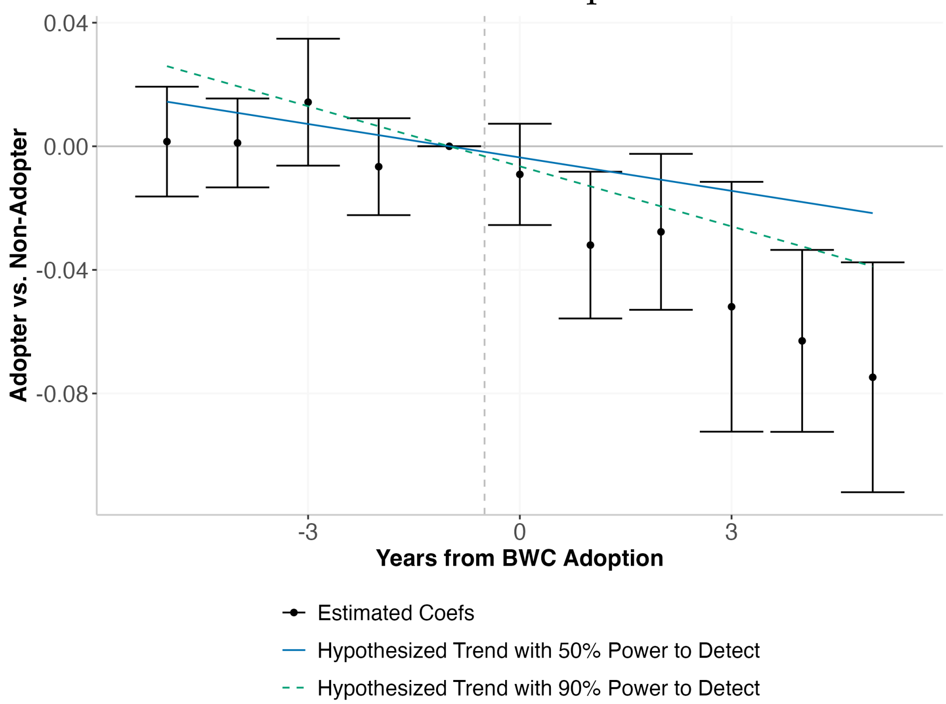
Notes: This figure illustrates the changes in arrests and incarceration rates in North Carolina counties that adopt BWCs using federal grants relative to control counties that have not yet adopted BWCs (or never do). Nine of the twenty-six BWC adoption events were federally funded. Panel (a) plots per 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 6, with the year before BWC adoption as the reference year. All specifications are weighted by population. The annotated coefficients are pooled estimates from Equation 4, 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 3. *p<0.1; **p<0.05; ***p<0.01.

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

(a) Arrest Disparity



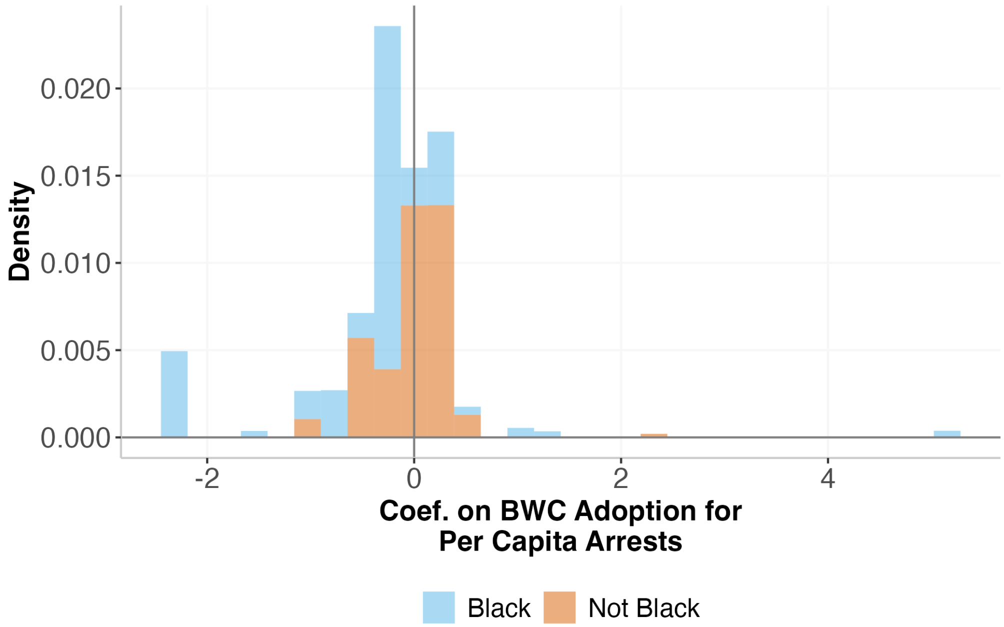
(b) Incarceration Disparities



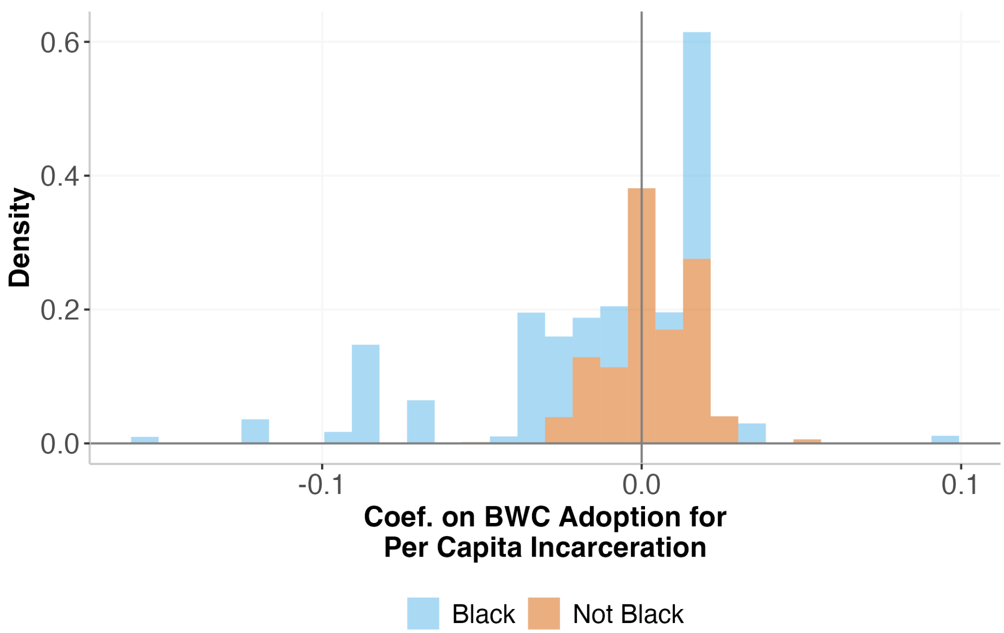
Notes: This figure investigates the robustness of the results to plausible pre-existing trends in adopting versus non-adopting counties. The tests proposed by Roth (2022) juxtaposes the estimated event study coefficients (in black) with worst-case pre-existing trends that we would have 50% power to detect (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

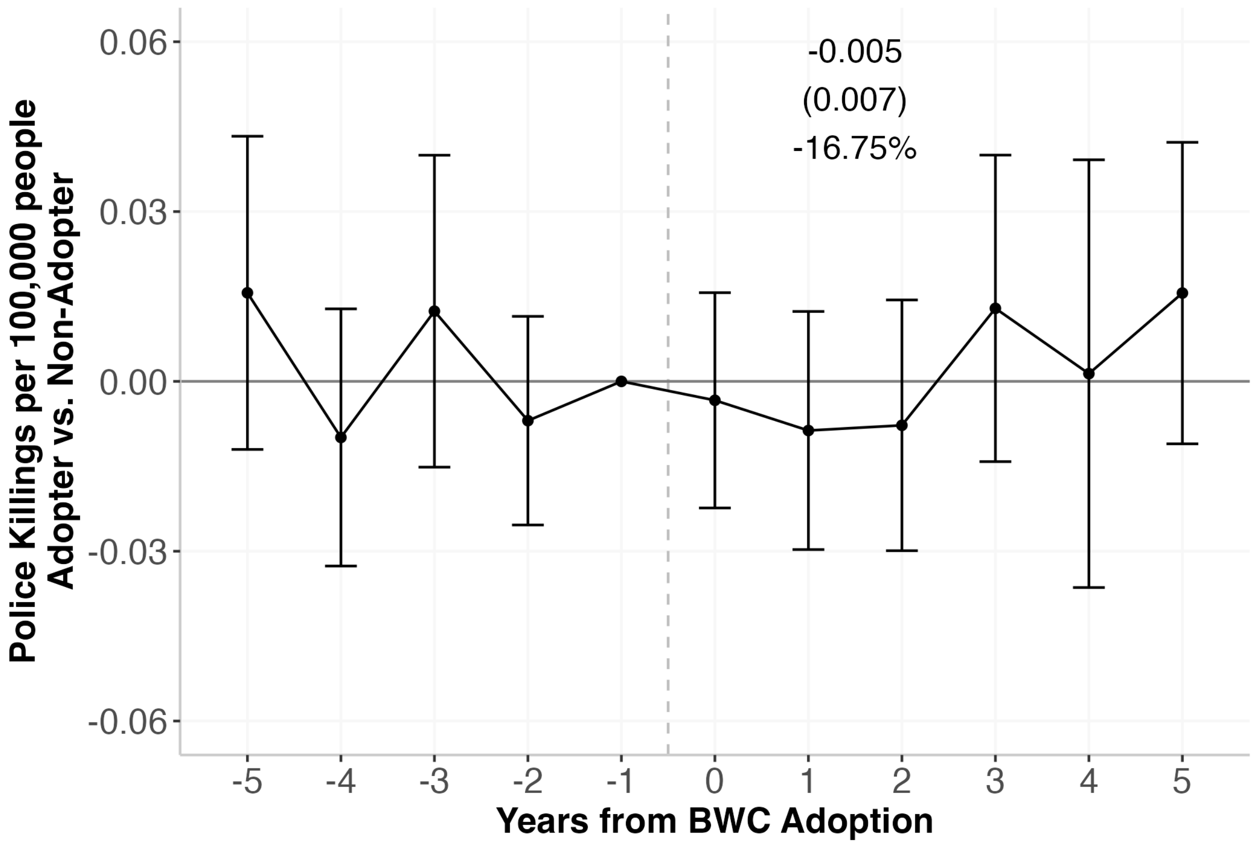


Panel (b): Incarceration Effects



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 4 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 4, 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?

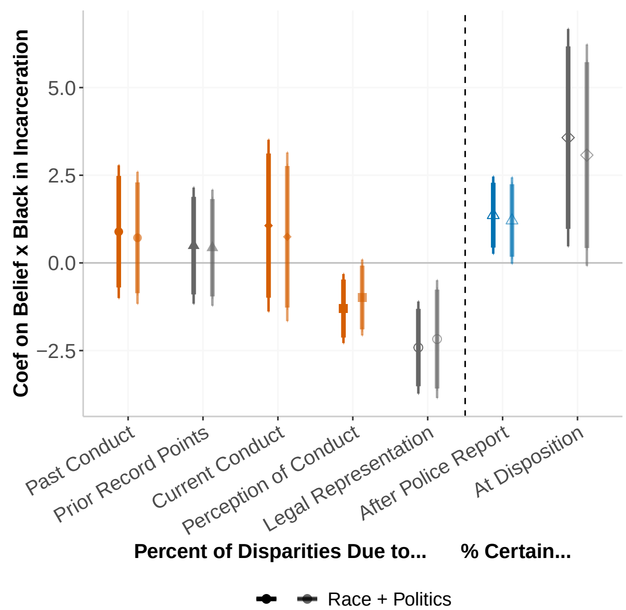
	0 10 20 30 40 50 60 70 80 90 100
Black defendants tend to have more severe past criminal conduct.	
Black defendants tend to have more prior points.	
Black defendants tend to have more severe current criminal conduct.	
The current conduct of a black defendant is often perceived to be more serious than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

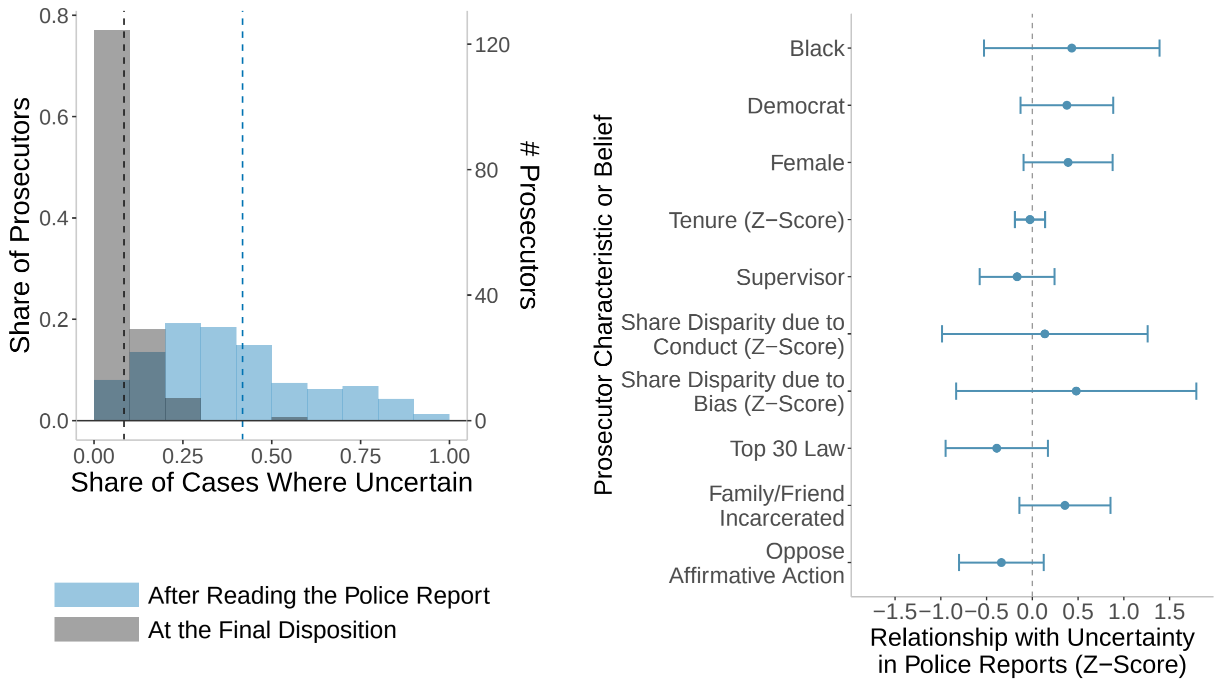


Note: This figure illustrates the relationship between prosecutors' elicited beliefs in our 2020 survey and their estimated impacts on incarceration disparities in their cases from 1995 to 2019. Panel (a) shows the interface for two survey questions: one about the sources of racial disparities in criminal-justice 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 9. 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

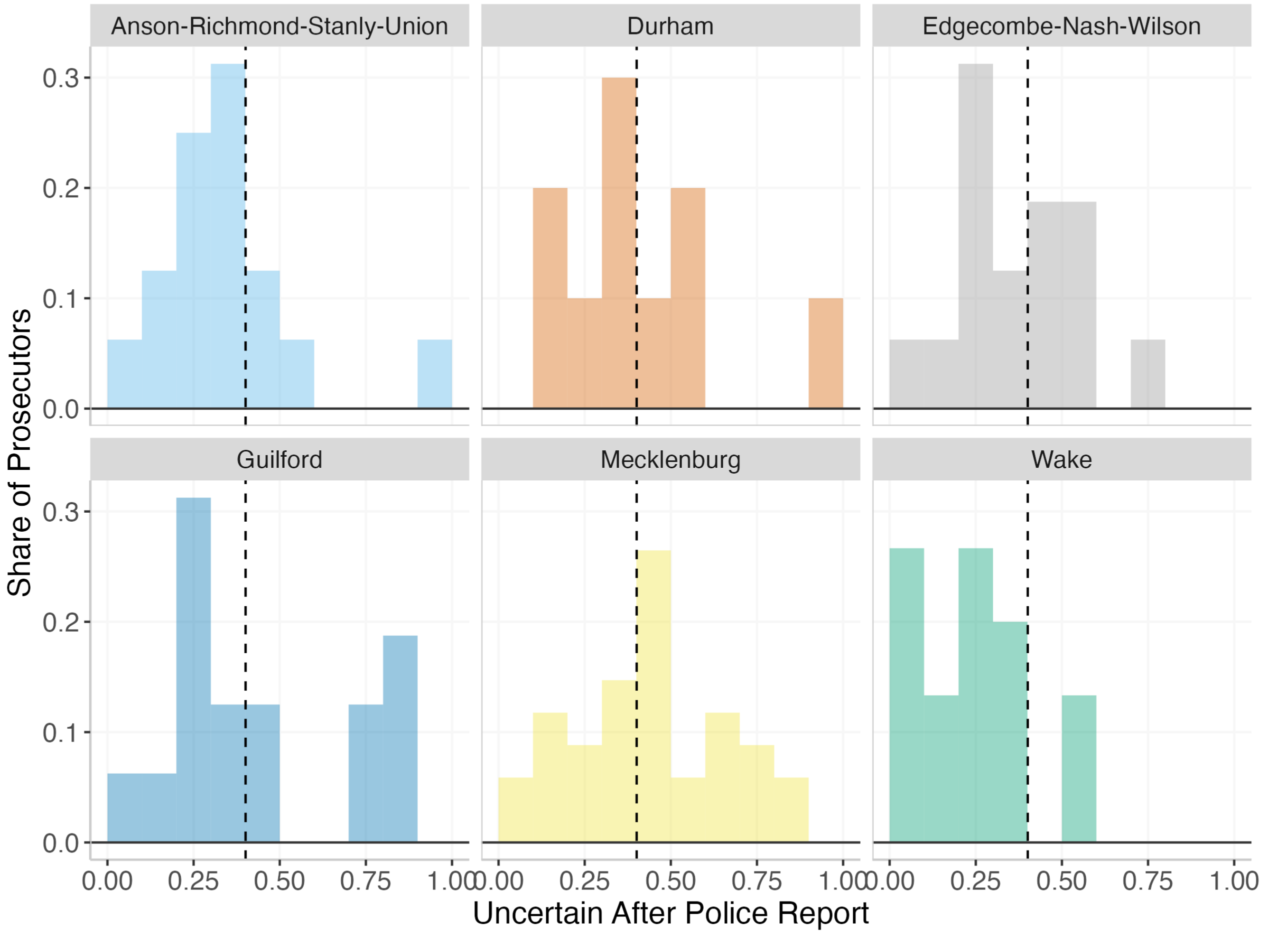
(a): Distribution of Prosecutor Certainty

(b): Relationship With Prosecutor Characteristics and Beliefs



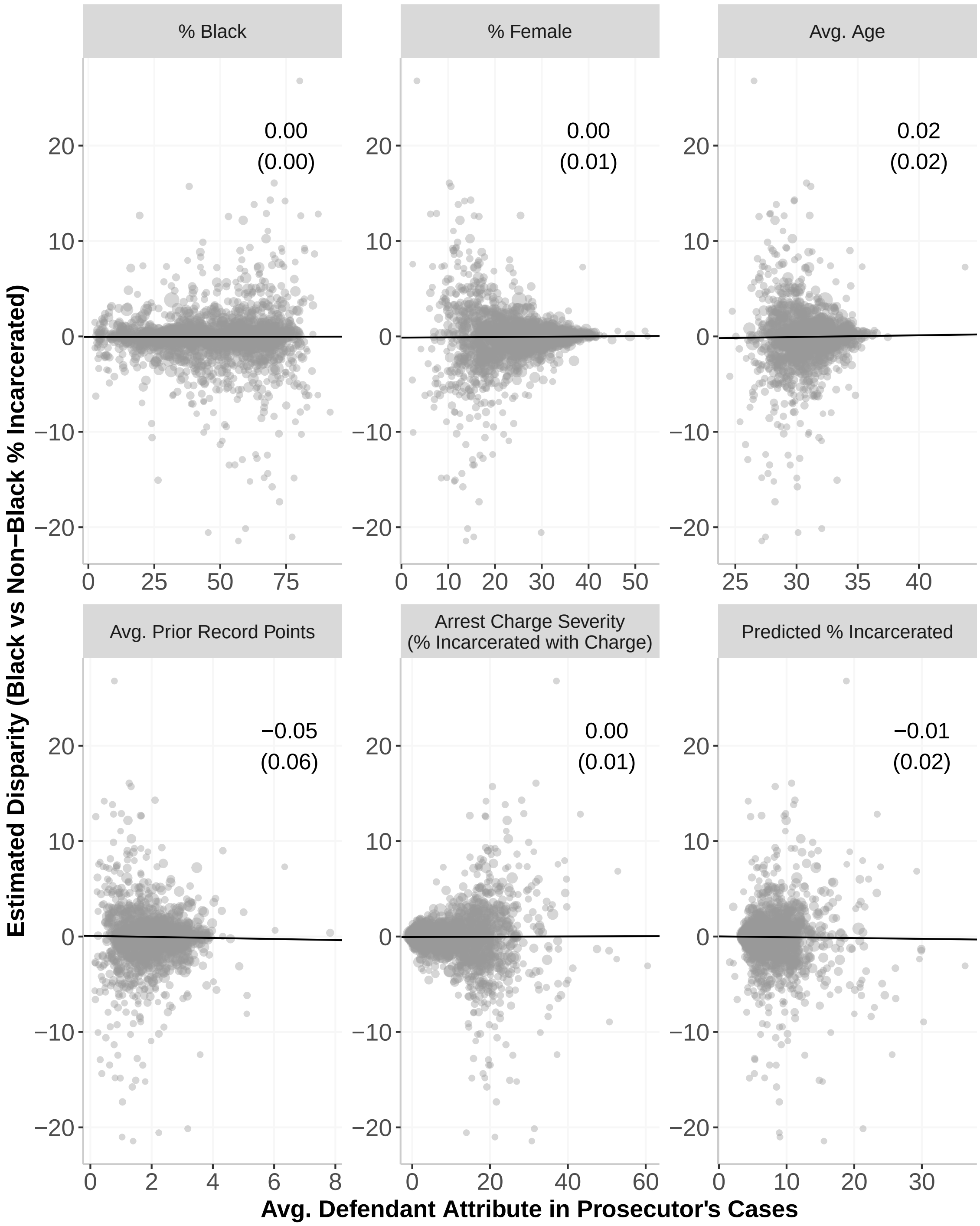
Notes: This figure summarizes prosecutors' degree of certainty about the defendant's conduct after reading the police report. Panel (a) shows the distribution of responses among the 163 prosecutors who answered the question. Panel (b) shows the coefficients from separate regressions that regress the 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.15 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



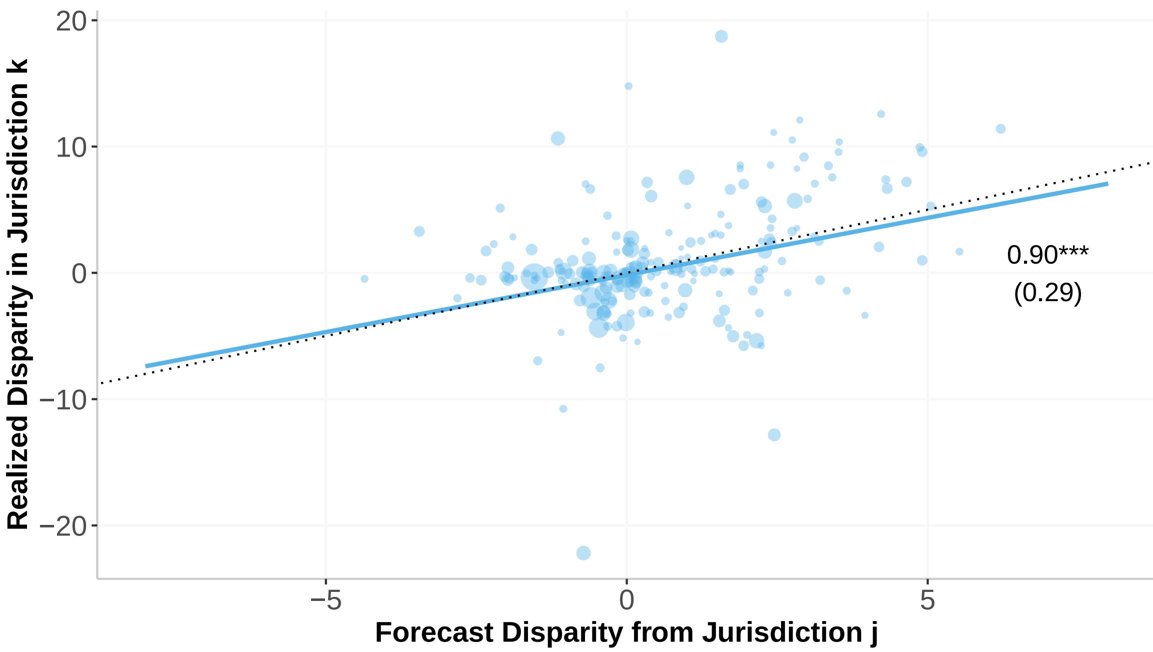
Notes: This figure shows the empirical distribution of prosecutors' uncertainty about the accuracy of police reports across the six largest participating offices in the survey. The adjusted R^2 indicates office explains 8% of variation in uncertainty.

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



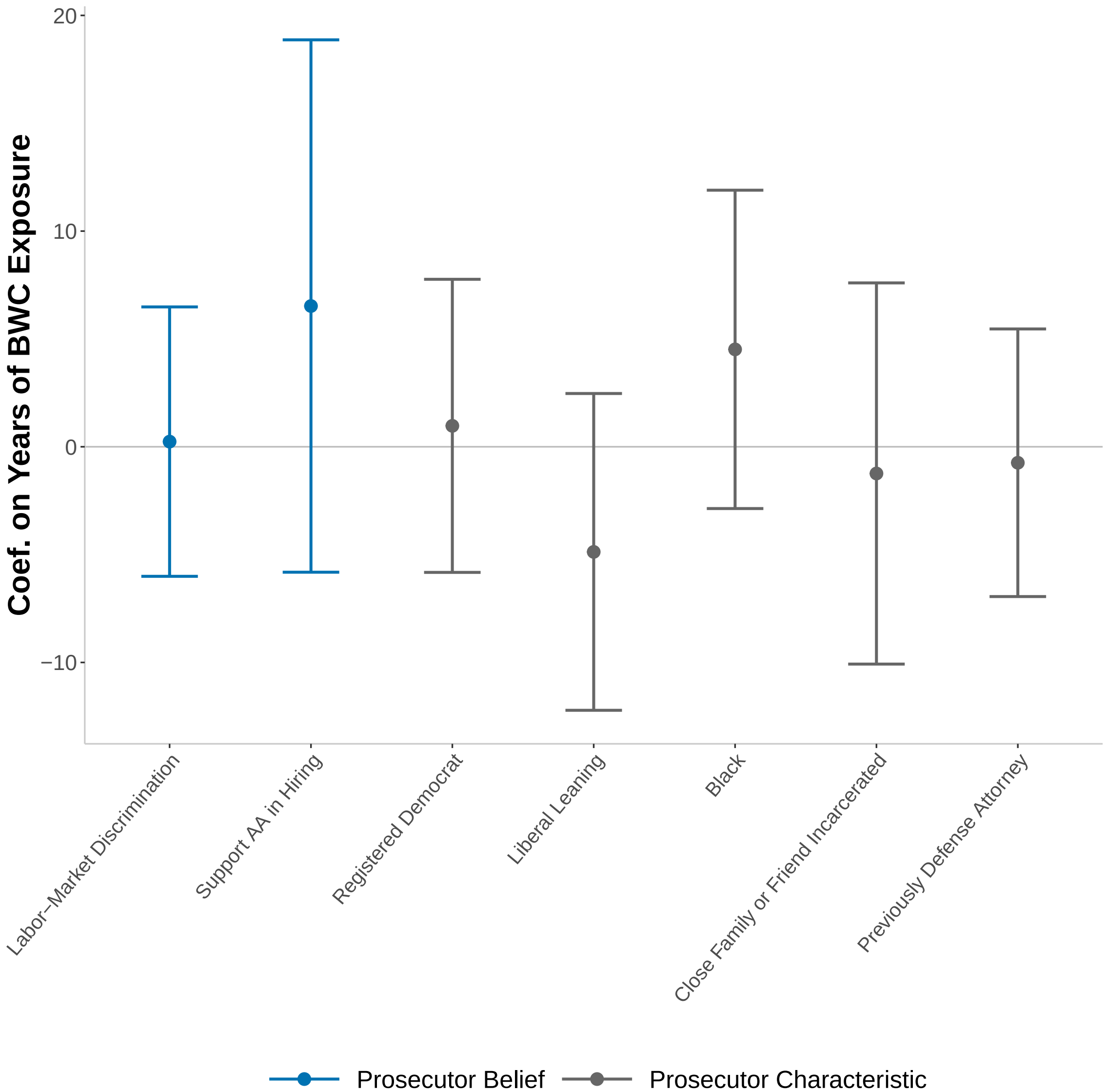
Notes: This figure illustrates the balance in case characteristics across prosecutors with different estimated impacts on racial disparities in incarceration rates. The x-axis plots an attribute of a prosecutor's cases, such as the percent of her cases with a Black defendant or a female defendant. The 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



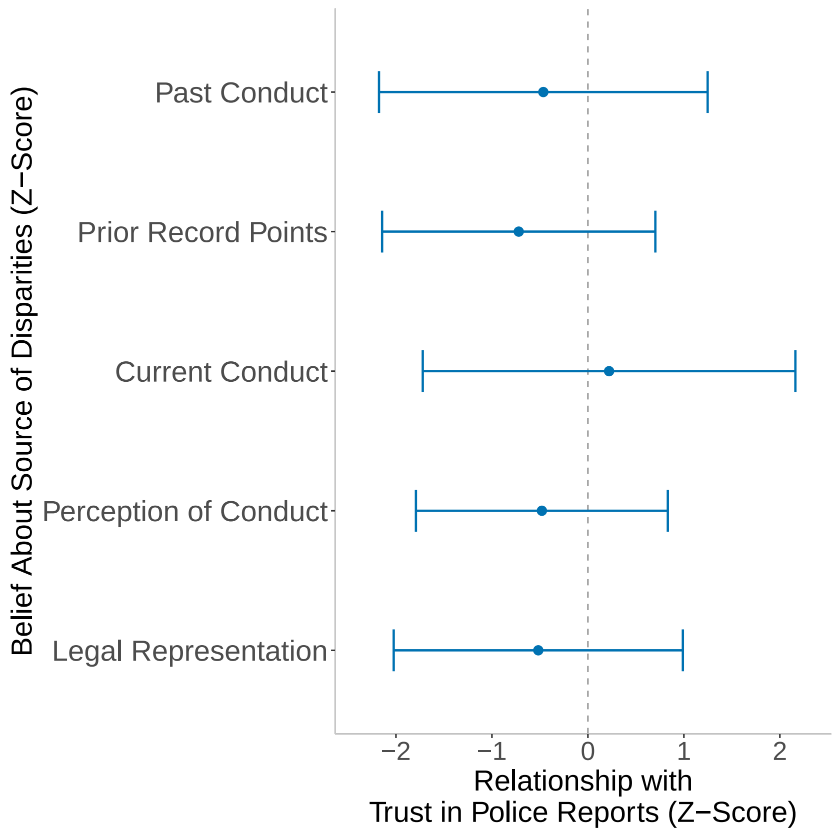
Notes: This figure illustrates the mover design to validate prosecutors' estimated effects on racial disparities in incarceration rates, after residualizing by unit and sentencing guidelines recommendations. The figure tests the validity of the design, using the 87 prosecutors who moved across judicial 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: Placebo Check: Relationship Between BWC Exposure & Prosecutor Beliefs and Characteristics



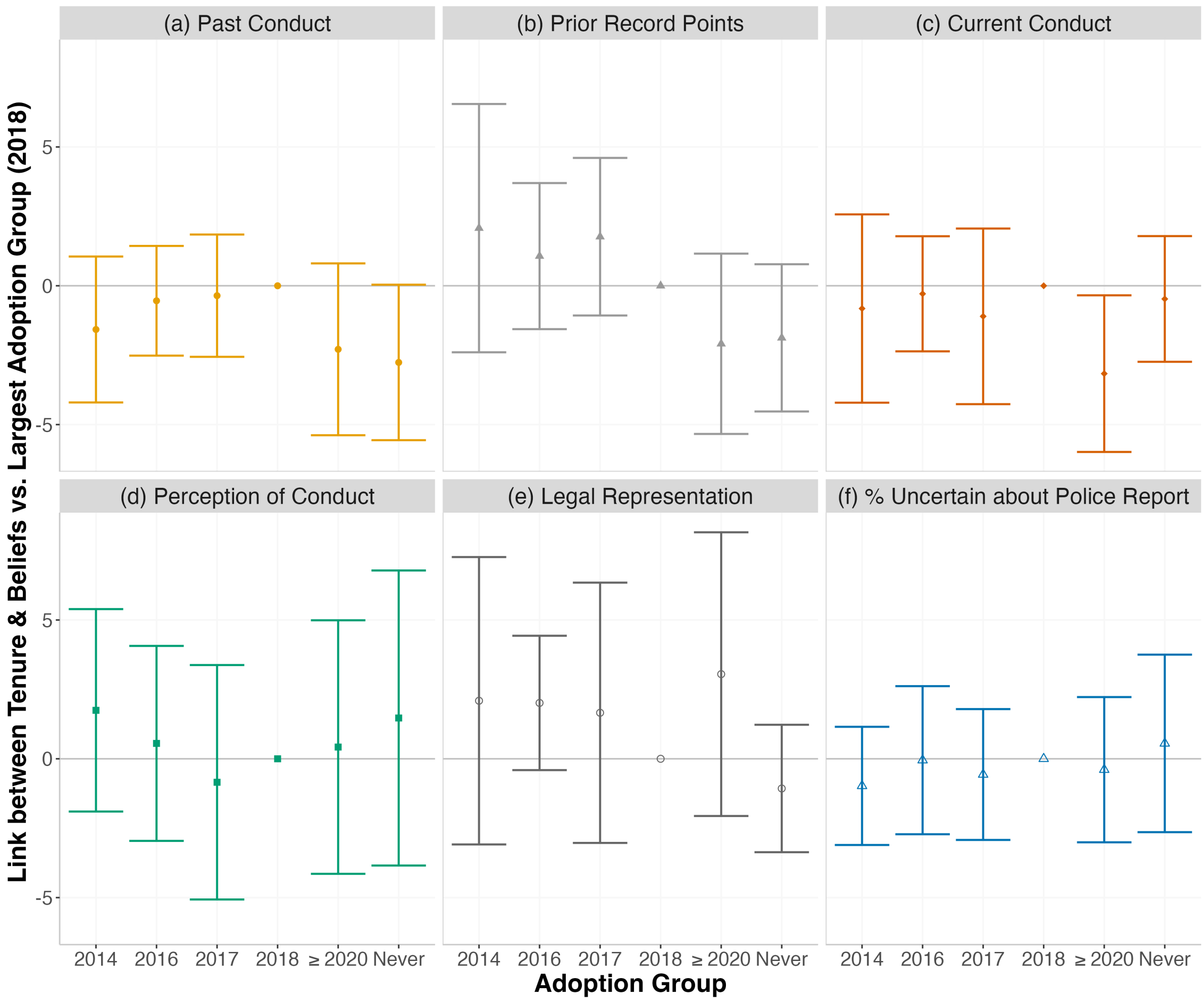
Notes: This figure illustrates the relationship between the length of prosecutors' exposure to BWCs and their beliefs and characteristics. Each point is estimated from a separate regression using Equation 7. Error bars represent 95% confidence intervals. Standard errors are clustered by prosecutor. Prosecutors' beliefs and characteristics come from our survey of North Carolina prosecutors in 2020. Among the 153 surveyed prosecutors in this analysis, 56% said that labor-market discrimination played a role in driving economic disparities, and 54% said that they support affirmative action in hiring. 42% of surveyed prosecutors are registered as democrats in the North Carolina Voter Records, and 55% said that the leaned politically liberal in our survey. 12% were Black, 39% had a close friend or family member who had been incarcerated, and 30% had previously worked as a defense attorney. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure A.15: The Relationship Between Prosecutors' Certainty in Police Reports and Their Beliefs About the Sources of Disparities



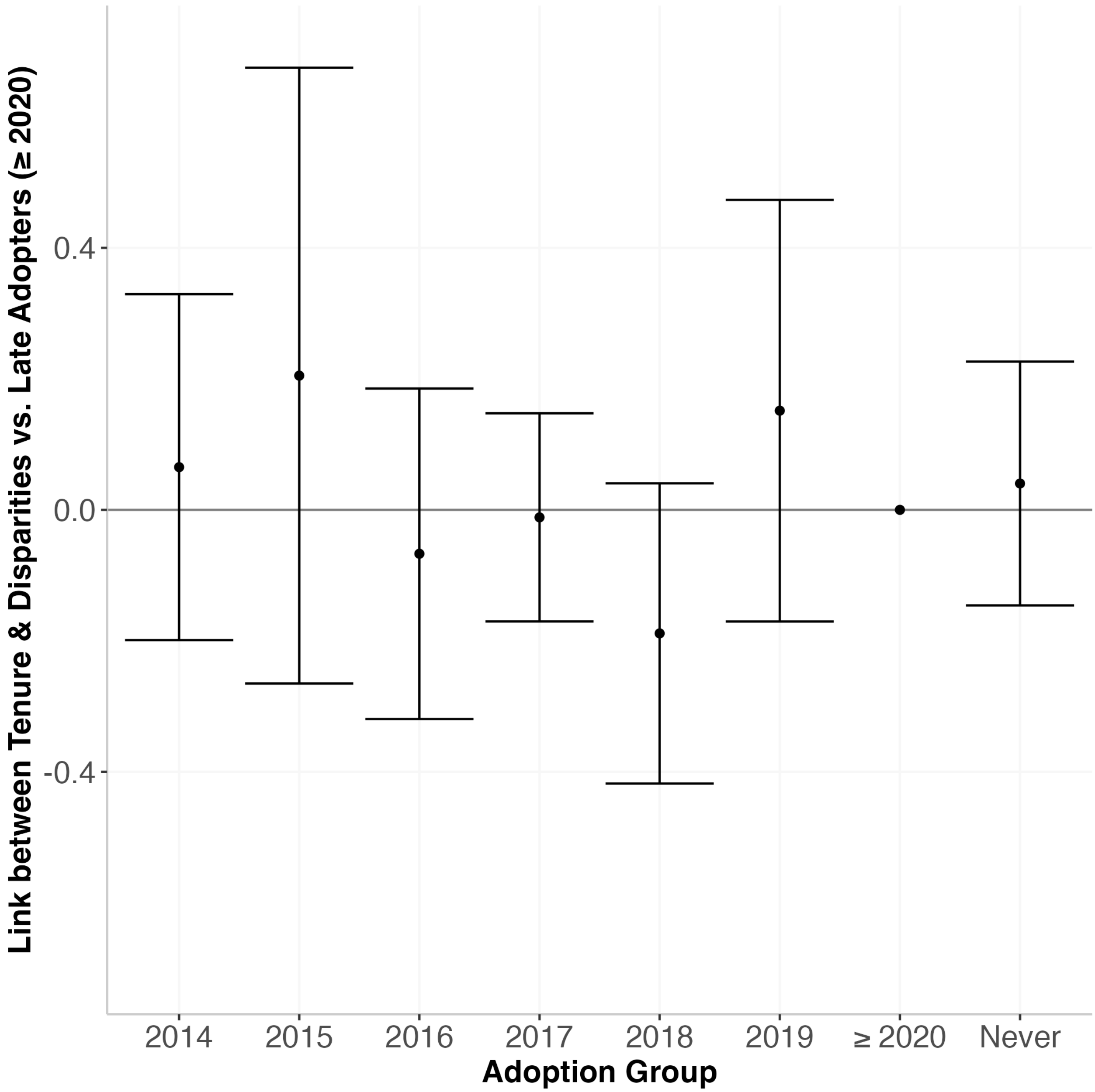
Notes: This figure illustrates the relationship between prosecutors' certainty in police reports and their beliefs about the sources of racial disparities. Both questions come from our survey of prosecutors. The interface for the question about certainty in police reports is shown in Figure A.10(a) and 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.16: 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.17: 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 4, 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: 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. Conviction includes any misdemeanor or felony conviction. Incarceration is defined as imprisonment of at least 6 months, typically served in state prison. Jail includes any stint behind bars. Columns 1−3 estimate Equation 4, 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 5, 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.4: 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 9. Standard errors are clustered by prosecutor. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.5: 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 4, 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.6: 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 11. Columns 1-2 uses more parsimonious controls, and Columns 4-7 add more extensive controls. Standard errors are clustered by prosecutor. Table A.7 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.7: 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.6. For reference, Column 1 replicates the final column of Table A.6 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.8: 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 7, 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. 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".[46] Identifying 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.

A.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.[47] 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.

[46]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 in a final judgment.

[47]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).

A.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 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.

B. 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.