SURVEILLING THE POLICE:

Do Body-Worn Cameras Prevent Police Bias from Becoming Systemic?*

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Abstract

Would surveilling the police reduce racial disparities in the criminal justice system? Using the staggered roll-out of police body-worn cameras (BWCs) in North Carolina, we find that BWCs reduce arrest rates for Black people — but have no significant impact for white people. These arrest effects are concentrated in low-level crimes that almost never lead to incarceration, and therefore, could not, on their own, generate large changes in incarceration. Nonetheless, we find that BWCs reduce incarceration rates for Black people by 11%. To unpack these downstream effects, we fielded an original survey of North Carolina prosecutors. We find that prosecutors with more exposure to BWCs believe that police are less reliable and arrests are more racially biased, suggesting that BWCs reveal a different reality than the one originally presented by police.

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Each year, seven million Americans are arrested, and over half a million end up incarcerated. Black Americans are disproportionately impacted: they are twice as likely to be arrested and over five times as likely to be incarcerated as white Americans (FBI, 2019; Carson, 2019). Although racial disparities in the criminal justice system are undeniable, their source is less clear. Disparities could reflect racial differences in crime. But they could also reflect racial bias among police officers, who make arrest decisions, or downstream actors like prosecutors and judges, who make sentencing decisions. Since downstream actors often rely on the police's account of the arrest, any bias in policing could distort ultimate sentencing decisions. Would surveilling the police change disparities in arrests and sentencing? How would changes in sentencing depend on downstream actors' beliefs about police — and would these beliefs shift if police were surveilled?

To study the effects of surveilling the police, we evaluate the introduction of police body-worn cameras (BWCs).[1] BWCs are designed to record police officers' interactions with the public and are worn on officers' uniforms, typically on their chest or belt.[2] This footage is shared routinely with prosecutors and defense attorneys, occasionally with judges, and rarely with the public.[3] Although sometimes blurry and incomplete, BWC footage provides an objective account of on-scene arrests — and so serves as a tool to surveil the police.

BWCs have been widely adopted over the last few decades: as of 2020, more than half of U.S. police departments had BWCs, compared to four percent in 2010 (BJS, 2016, 2020). Many hoped BWCs would hold police accountable, particularly in their interactions with Black people (e.g., Craven and Reilly, 2015). Despite their apparent promise, evaluations of BWCs have yielded mixed results for police misconduct, and small and inconclusive

[1] Our paper focuses on BWCs as a tool to surveil police, but they may also serve other purposes — for instance, protecting officers from false accusations and limiting the liability of police departments.

[2] Police departments with BWCs have policies about when officers must activate their cameras, typically requiring activation during arrests, calls for service, and other officer-initiated citizen contacts (BJS, 2016).

[3] In North Carolina, law enforcement agencies must share BWC footage with prosecutors for "review of potential criminal charges," "discovery" with the defense, or "any other law enforcement purpose." GS 132-1.4A(g). Civilians may also petition for BWC footage if they are in the recording.

effects on arrests (Lum et al., 2020; Williams Jr et al., 2021).[4] There is particularly limited evidence on how BWCs impact racial disparities in arrest and incarceration rates.[5]

In this paper, we provide quasi-experimental estimates of the effect of BWCs on disparities in per capita arrests and sentencing outcomes. Our design leverages the staggered roll-out of BWCs across North Carolina. We find that BWCs substantially reduce arrest rates for Black people — but have no detectable effect for white people. These effects are concentrated in low-level crimes that almost never lead to incarceration. Therefore, this change in arrests, on its own, could not produce large shifts in incarceration. Nonetheless, we find that BWCs reduce incarceration rates for Black people by 11 percent. Together, these results suggest a more nuanced relationship between police misrepresentation and downstream decision-making.

To better understand the relationship between police and downstream decision-makers, we conducted an original survey of North Carolina prosecutors. Coupling survey responses with administrative court records, we find that prosecutors who tend to take police reports at face value also tend to increase racial disparities in incarceration relative to more skeptical prosecutors, even within the same office and unit. Prosecutors' skepticism of police reports only predicts incarceration disparities in places without BWCs — where often the officer's word is the sole source of information. In places with BWCs, prosecutors' skepticism ceases to predict disparities, as prosecutors can cross-check the officer's narratives against the arrest footage. Finally, we find evidence that prosecutors who see

[4] A meta-analysis of BWC studies finds, on average, that BWCs reduce arrest rates by 3.9 percent but cannot rule out 5.8 percent increases or 12.7 percent decreases at the 5 percent level (Lum et al., 2020). Although research on the impact of BWCs on police misconduct yields conflicting results, recent large-scale studies find that they substantially reduce police use of deadly force (Miller and Chillar, 2022; Kim, 2024) and civilian complaints against police (Braga et al., 2022).

[5] Lum et al. (2020)'s systematic review of the literature on BWCs highlights the gap in research on its impacts on "racial and ethnic disparities in policing outcomes." Since then, a recent working paper studies the rollout of bodyworn cameras in Virginia (Bollman, 2021). The author finds that BWCs change the incidence of arrests arising from police interactions, including resist arrest. Conditional on arrest, Bollman finds no systematic changes in case outcomes for Black versus non-Black defendants. However, it is difficult to interpret these results given the changing composition of arrests. In addition, one criminology paper has studied the association between BWC activation, neighborhood racial composition, and the decision to make an arrest after initiating contact. Using data from Phoenix, AZ, Huff (2022) finds that the odds of arrest conditional on contact were 38% lower when officers activated BWCs in a majority Black neighborhood.

more unfiltered footage of arrests interpret Black people's arrests differently: those with more exposure to BWCs perceive police as less reliable and arrests as more racially biased. Suggestively, this learning matters for case outcomes: prosecutors with more prior BWC exposure tend to reduce incarceration disparities in their cases.

Our paper uses three primary datasets. First, we compile data on the timing of BWC adoption in North Carolina. We start with the Law Enforcement Management and Administrative Statistics (LEMAS) survey (BJS, 2016, 2020). Since the LEMAS only offers snapshots of BWC usage in a subset of agencies, we supplement LEMAS 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. Second, we link the timing of BWC adoption to administrative data on arrests and sentencing outcomes for three million cases. Finally, we link in surveys of 163 North Carolina prosecutors, whom we recruited in partnership with the elected head prosecutor ("District Attorney") of sixteen offices. Surveyed prosecutors collectively handled half a million cases.

BWCs have been gradually adopted across North Carolina. Some agencies applied for federal grants from an Obama-era program. Since only about a third of proposals could be funded each year, agencies often applied in multiple years before receiving federal funding (BJA, 2015). For local funding, there was also typically a multiple-year delay between the initial proposal and final implementation of BWCs. In North Carolina, BWC adoption does not coincide with police killings of civilians or with Department of Justice (DOJ) lawsuits against law enforcement agencies for unconstitutional practices. In addition, minutes from city council meetings almost always include BWCs as a free-standing agenda item — and not as part of other police reforms.[6]

We analyze the staggered adoption of BWCs in a difference-in-differences (DiD) design. To address econometric concerns with staggered DiD designs, we use a stacked estimator (Cengiz et al., 2019) and show robustness to alternative estimators.

[6]Nationally, other research has found that a specific agency's adoption of BWCs does not systematically overlap with its introduction of new programs, such as de-escalation or implicit bias trainings (Kim, 2024).

Our design relies on a parallel-trends assumption: that in the absence of BWCs, places that adopted would have followed similar trends as places that did not. We do several checks of this assumption. First, we examine pre-trends: reassuringly, adopters have similar trends in arrest and incarceration rates leading up to adoption as places that adopt BWCs later (or not at all).[7] We also show robustness to allowing earlier and later adopters to be on different linear trends in arrests and incarceration. Second, we find that BWCs' impacts are widespread across the twenty-six adopters, indicating that a confounding factor would have to be broadly coincident with adoption timing to explain our results. Third, our results are robust to focusing on adoption events that are less likely to be confounded by local policy shifts — specifically, those that were not preceded by the election of a new sheriff and ones that were federally funded, where adoption timing was not entirely under local control. Finally, we complement our primary strategy with an exposure design that leverages variation in the length of prosecutors' exposure to BWCs *within* a given place and year — and so does not rely on the same parallel-trends assumption.

We find that BWCs reduce per capita arrest rates for Black North Carolinians but have no significant effect on aggregate arrest rates for white North Carolinians. As a result, disparities in arrest rates fall by 9.1 percent (p-value $= 0.039$). The aggregate reduction in arrests is driven by arrests typically initiated by a police stop, such as drug possession and public-order offenses, with similar declines in police traffic stops. By contrast, arrest rates remain unchanged for serious violent crimes, like rape and homicide, where police usually respond to calls from victims or witnesses. Using data from Virginia, Bollman (2021) similarly finds that the BWC adoption only reduces police-initiated arrests.

The decline in police-initiated arrests disproportionately affects Black people. For these arrests, Black and white North Carolinians see comparable, significant declines in arrest rates in *percentage* terms (at 13 percent). However, since Black people were arrested for police-initiated charges at nearly three times the rate of white people in the pre-period,

[7] 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).

the same percentage reduction has a disproportionately large effect on Black people (a decline of 0.36pp for Black people versus 0.13pp for white people). In this case, a race-neutral policy — equipping *all* beat officers with BWCs — reduces arrest disparities due to the sheer difference in base rates of contact with police, and not necessarily because the policy has a different effect on officers' treatment of Black people and white people.

We also find that BWCs reduce per capita incarceration rates for Black people by 10.5 percent (0.029 pp) — with an insignificant increase of 2.0 percent (0.001 pp) for white people (p-value of difference = 0.0062).[8] These substantial changes in Black people's incarceration rates cannot be explained by BWCs' effects on arrest rates alone: indeed, over ninety percent of the decline in arrests of Black people comes from low-level charges that have less than a one percent chance of leading to incarceration. Thus, the decline in incarceration suggests a subtler effect on the information exchanged between police and the post-arrest system. BWCs may change how the police characterize Black people's crimes or how later actors interpret information from police.

To unpack the downstream effects of BWCs, we fielded an original survey of prosecutors, who are widely regarded to be the most powerful actors in the post-arrest system.[9] Prosecutors receive information from police about arrests, typically in the form of a police report. Based in part on this information, prosecutors choose what charges to pursue and what plea deal to offer. These choices, in turn, influence — and sometimes determine — sentencing outcomes. We link prosecutors' survey responses to their cases in the court records. We then estimate prosecutors' causal effects on incarceration disparities using the quasi-random assignment of cases to prosecutors within units of offices.[10]

We first use our survey to explore whether prosecutors' trust in the reliability of police

[8]There are similarly disparate declines in jail time and conviction rates.

[9]For legal scholarship on prosecutorial power, see, e.g., Pfaff (2017); Stith (2008); Davis (2007). A growing body of empirical work also recognizes the central role of the prosecutor in charge and sentence negotiation (see, e.g., Ulmer et al., 2007; Rehavi and Starr, 2014; Sloan, 2019; Tuttle, 2019; Agan et al., 2023).

[10]Three-quarters of surveyed prosecutors report that their units assign cases using a rotation system. We validate this approach by showing balance in case characteristics across prosecutors. We also use a mover design and show that when a prosecutor moves to a different office, her estimated disparate impact persists. Similar mover designs have been used in Chetty et al. (2014); Finkelstein et al. (2016); Fenizia (2022).

reports affects the persistence of racial disparities in the criminal justice system. For arrests that appear by the book, prosecutors may accept police reports at face value and so inadvertently pass through any embedded bias. To measure prosecutors' tendency to take police reports at face value, we ask prosecutors how often, after reading police reports, they feel certain about what the defendant did.[11] We find that prosecutors who put more trust in police reports tend to increase disparities in incarceration relative to more skeptical prosecutors, even within the same office and unit.

Prosecutors' trust in police only predicts their impacts on disparities in places without BWCs, and not in places with BWCs. In places with BWCs, all prosecutors — regardless of their reported trust in police — reduce disparities to a similar extent as the more skeptical prosecutors in places that have not yet adopted BWC surveillance technology. One explanation for this pattern is that a prosecutor's trust in police reports matters less once she can directly observe what happened. Another explanation is that, with BWCs, police misrepresent arrests less frequently, and so trust in police stops mattering as much, since the police themselves have become more trustworthy. Either way, this pattern suggests that surveilling the police acts as a substitute for prosecutor skepticism of police. Consistent with this interpretation, trust in the police only ever predicts disparities for arrests likely initiated by police — and not for arrests where victims and witnesses also likely played a role. Taken together, these survey findings suggest that, before BWCs, some prosecutors naively took police reports at face value and so inadvertently passed through earlier bias.[12] But then BWCs disrupted this dynamic by giving all prosecutors independent information about the ground truth.

Finally, we provide evidence that BWCs affect prosecutors' beliefs about police reliability and Black people's criminality. Without BWCs, repeat exposure to the police's account of events may warp prosecutors' beliefs. With BWCs, prosecutors may learn by directly

[11] We asked this question without referring to race and before mentioning race on the survey because we did not want to prime respondents to think about racial bias. Perhaps as a result of this choice, prosecutors' responses were largely unrelated to their demographics, including their race and politics.

[12] Seen through this lens, our finding supports models of naivete in games of strategic communication (Kartik et al., 2007; Kartik, 2009).

comparing the arrest footage to the officer's account.[13] In addition, prosecutors' beliefs about Black people's criminality may change because they start observing lower disparities in arrests after BWCs are adopted. Consistent with these hypotheses, we find that prosecutors with more exposure to BWCs report different beliefs about police and Black arrestees: they trust police reports less and think racial disparities are explained more by racial bias and less by Black people's criminality. Together, these patterns suggest that, although police respond to BWCs by making fewer arrests, particularly of Black people, prosecutors still come to believe that police are less reliable and Black people are less criminal after observing more objective signals of police-civilian interactions.

This analysis of BWCs' impacts on prosecutors' beliefs leverages the fact that prosecutors vary in the length of their exposure to BWCs at the time of the survey. This variation jointly depends on (i) when the prosecutor started working in the county and (ii) when that county adopted BWCs. As a result, we can compare prosecutors within the same county and year who differ only in their past exposure to BWCs, and not their current exposure or years of experience.

Using an analogous exposure design, we find that prosecutors with longer BWC exposure reduce incarceration disparities relative to prosecutors with less exposure, suggesting that some of BWCs' downstream impacts on incarceration may be driven by a shift in prosecutors' beliefs. Recent work has found that biased beliefs are important drivers of outcome disparities (e.g., Bordalo et al., 2016; Arnold et al., 2018). We contribute to this literature by showing where biased beliefs may come from: decision-makers' beliefs may be endogenous to misinformation from earlier actors.

Our paper has important limitations. First, for our BWC results, we cannot fully rule out that a contemporaneous change in crime, policing, or prosecution drove the observed change in arrests and incarceration in places that adopted BWCs. Second, our survey

[13] One prosecutor told us she had always taken police at their word when they cited observations like "furtive movements" to justify stops. However, once she could watch BWC footage, she began cross-checking police accounts and occasionally found discrepancies: for instance, when one officer claimed someone was "making furtive movements," the footage revealed that "the guy was just sitting there."

finding — that prosecutors who are more skeptical of police reports tend to reduce disparities — could reflect something else about these prosecutors that is correlated with both their skepticism and their disparate impacts. Yet, such a factor would need to predict disparities only in police-initiated arrests and only before BWCs are introduced.

Taken together, our paper provides a collage of evidence that surveilling police — or at least questioning their decisions — can meaningfully reduce disparities in the criminal justice system. Indeed, once a place adopts BWCs, arrest and incarceration rates precipitously decline for Black people. Although not definitive, BWCs' impacts on arrests suggest that, without oversight, police were over-arresting Black people and making too many low-level arrests across the board. And BWCs' disparate impacts on incarceration suggest that the post-arrest system was passing through police misinformation — and so was over-incarcerating Black people before BWCs were adopted.

Our paper proceeds as follows. Section I describes our data. Section II analyzes the rollout of BWCs, and Section III, our survey of prosecutors. Section IV concludes.

I Data

This section first describes how we construct arrest and sentencing outcomes using statewide court records from North Carolina. We then describe how we collected data on the timing of BWC adoption. Finally, we describe our survey of North Carolina prosecutors.

I.A Arrest and Sentencing Outcomes

Our analysis focuses on how BWCs impact the per capita incidence of arrests and incarceration sentences by race. For county c, year t, and racial group r, we calculate:

$$\begin{aligned} \text{Per capita arrest rate}_{ctr} &= 100 \cdot \frac{\text{\# Arrests}_{ctr}}{\text{Total Population}_{ctr}} \\ \text{Per capita incarceration rate}_{ctr} &= 100 \cdot \frac{\text{\# Incarceration Sentences}_{ctr}}{\text{Total Population}_{ctr}}. \end{aligned} \tag{1}$$

To construct the numerators, we aggregate case-level data from administrative court records, described below. To construct the denominators, we use population counts by race, year, and county (U.S. Census Bureau, a). Disparities in per capita outcomes reflect cumulative disparities in contacts with the criminal justice system: for example, disparities in per capita drug possession arrests capture any disparities in police stops, the probability of subsequent searches, and the likelihood of ultimate arrests. Similarly, disparities in per capita incarceration rates capture any disparities leading up to arrest as well as disparities in the treatment of arrests once they enter the court system.

Administrative Court Data. We construct counts of arrests and incarceration using data 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, which means we have all criminal cases for four years prior to the year the first place adopted BWCs (in 2014).[14] Since police officers are the charging agency in North Carolina, the AOC data contains the near universe of arrests, and each record details the arrest charges chosen by the police.[15] The court data also records the perceived race of the arrestee as well as the county and date of the arrest.

We estimate the number of arrests as the total count of new cases appearing in the court records for a given race in a given county and year. We also categorize arrests into different crime types, which allows us to focus on arrests where police have more discretion.[16] Following the categorization in LaFave et al. (2004), we define "police-initiated arrests" as arrests for drug, weapon possession, resist arrest, and public order offenses. We contrast these arrests with arrests for violent crimes, which tend to be initiated by victims or witnesses. We supplement our primary arrest outcomes with data on traffic stops (NCSBI, 2024; Baumgartner, 2024).[17] These data include annual counts of traffic stops by race

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

[15]Rose and Shem-Tov (2021) validates this by collecting jail booking records from the Charlotte-Mecklenburg sheriff's office: 93.3% of bookings appear in the AOC data, with the remaining bookings reflecting non-arrest events such as prison transfers.

[16]When defendants have multiple charges, we focus on the most serious — or "lead" — arrest charge. To determine the lead charge, we use the offense classes from the state sentencing guidelines to rank severity.

[17]Under North Carolina state law (NCGS 114-10.01), all sheriffs' offices and almost all police departments

conducted by each law enforcement agency, which we aggregate to the county level to measure per capita traffic stops.

The court records also give us detailed information on conviction charges and sentencing outcomes. Our analysis of downstream outcomes focuses on the incidence of incarceration sentences,[18] although we also consider jail sentences and conviction rates.

Summarizing Outcomes. 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. They are about three times as likely to be arrested as white people, with 6.7% of Black people arrested each year compared to 2.3% of white people (see Column 1, Rows 2−3). Disparities in police-initiated arrests and traffic stops are similarly large (Rows 5−6 and 8−9). These initial disparities in contacts with the system persist to sentencing: Black people are about five times as likely to be incarcerated as white people (Rows 11−12). These disparities in arrests and incarceration parallel nation-wide disparities (FBI, 2019; Carson, 2019).

I.B Body-Worn Camera Adoption Timing

Different places in North Carolina adopted BWCs at different times, as shown in the map in Figure 1(a). To determine adoption timing, we started with data collected by the Bureau of Justice Statistics Law Enforcement Management and Administrative Statistics (LEMAS), which surveys a sample of law enforcement agencies (BJS, 2016, 2020).[19] The 2016 LEMAS survey asked agencies when they first used BWCs, while the 2020 LEMAS survey asked about current use. To identify the timing of BWC adoption for agencies that adopted between 2016 and 2020 — and to expand the sample of agencies — we hand-collected data by searching local news sources and cold-calling agencies. This additional

have been required to record and publish their traffic stops and searches since 2002.

[18] 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).

[19] The LEMAS does a census of agencies that employ a hundred or more full-time sworn officers and a nationally representative sample of smaller agencies.

data collection identified specific adoption dates for 58 agencies in the LEMAS sample,[20] and for 45 agencies not in the LEMAS sample (30 of which had adopted BWCs). We aggregate BWC treatment information to the county level since this is the level of our outcome measures.[21] We categorize a county as treated when at least half of its officers work in agencies that have adopted BWCs. We are able to identify the BWC adoption year for 48 counties and 77% of cases in the state court records.

Together, these data efforts give us precise BWC adoption timing for close to the entire state, and the administrative records allow us to estimate BWCs' impacts up to five years post-adoption. As a result, we are able to assess BWCs' impacts in a larger jurisdiction and over a longer time horizon than previous studies.[22]

Summarizing Differences across Adoption Groups. Our outcomes of interest — per capita arrest and incarceration rates — are nearly identical across BWC adoptions groups, both overall and by race (Rows 2−13 in Table 1).[23] Compared to later adopters, early adopters are more urban (Row 25), politically liberal (Row 26−27), and have a higher share of Black residents (Row 24). These demographic differences are especially pronounced for never adopting counties. We find that these county-level characteristics do not systematically change around BWC adoption (see Table A.2 and Section II.A).

I.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

[20] This data collection also revealed that the initial BWC year in the 2016 LEMAS often reflected the start of a pilot program — and not when the agency broadly adopted BWCs for its beat officers.

[21] We also do county-level aggregation because the court records include county identifiers but not law enforcement identifiers.

[22] 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 BWC's impact nationally but narrows the analysis to within one year around BWC adoption and primarily focuses on police use of force as the outcome.

[23] This also holds in the years before any NC county had adopted BWCs (2010−2013) (see Table A.1).

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 and the sources of racial disparities in the criminal justice system (described in Section III) as well as information about their personal backgrounds and attitudes.[24] 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 surveyed a total of 203 prosecutors, 90 of whom chose to debrief with us afterwards 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 each year. Together, surveyed prosecutors handled over half a million cases between 1995 and 2019. Since we fielded the survey in 2020, about three-quarters of prosecutors had already been exposed to BWC footage at the time they took the survey — and about about a quarter worked in offices that adopted after the survey.

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 A for details).

II 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.

[24] We asked about prosecutors' tenure, past experience as defense attorneys, law school, political ideology, race, age, and support for affirmative action.

II.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 arrest (or incarceration) rate, 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).[25] 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 (c) and year (t) to flexibly vary by sub-experiment (s) in:

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

We also analyze racial disparities directly:

$$Y_{rcts} = \beta^{B-W} \, \text{BWC}_{cts} \times \mathbb{1}[r = \text{Black}] + \beta^W \text{BWC}_{cts} + \mu_{rcs} + \mu_{rts} + \epsilon_{rcts}. \tag{3}$$

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 of residents in treatment and control counties would have followed parallel trends in the absence of different BWC timing. Un-

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

der this assumption, the difference-in-differences design helps to rule out potential confounding factors that may otherwise compromise a causal interpretation. 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 question our parallel trends assumption — and worry that counties in different BWC adoption groups would have been on different criminal-justice 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 better understand BWC's institutional context, we hand checked minutes of city council meetings that mention BWCs. These minutes almost always consider BWCs as a free-standing agenda item — and almost never in connection with other criminal-justice reforms.[26] In these meetings, the debate about BWCs typically centered on their timing and implementation — especially who would cover the costs — as opposed to whether they would ultimately be adopted. We therefore show robustness to limiting our analysis to places that eventually adopted BWCs but who differ in their timing. We also show robustness to limiting to places where adoption timing was more plausibly orthogonal to local policy shifts: specifically, those that did not elect a new sheriff in the election just before adopting BWCs 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.6), suggesting that BWCs were not adopted

[26]For example, in Charlotte and Raleigh — North Carolina's largest cities — both city councils approved BWCs as a free-standing agenda item (Charlotte City Council, 2015; Raleigh City Council, 2018). An exception to this general rule is Wilmington NC, where BWCs were expanded at the same time that the city increased funding for the police department's "text a tip" marketing campaign (Evans, 2015).

in response to local instances of police violence that could have prompted other policy changes.[27]

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 2.[28] 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.

In addition to concerns about parallel-trend violations, one may worry about spillover effects. BWC adoption in one county could impact neighboring counties, thereby tainting our control group and violating the stable unit treatment value assumption (SUTVA). One plausible source of spillovers is police departures: some officers may quit after BWCs are adopted and move to a neighboring county that has not yet adopted BWCs. We find some evidence to this effect. The number of sworn officers in a county declines by 2.5% after BWCs are adopted (Figure A.1(a), p-value $= 0.08$); and the number of officers in counties that neighbor adopting counties (but have not themselves adopted) suggestively increases by 0.6% (Figure A.1(b)). To ensure that officer moves do not bias our estimates, we show that our results are robust to excluding neighboring counties from our controls.

[27] We also that civil rights lawsuits and federal over-site of police departments do not coincide BWC adoption timing. In 2012, the DOJ sued the Alamance Sheriff's Office for a pattern and practice of violating the constitutional rights of Hispanics (DOJ, 2016). Alamance adopted BWCs eight years later in 2020, after our data period. In 2014, Fayetteville Police Department in Cumberland County began a one-year collaborative reform initiative with the DOJ (DOJ, 2014). The department adopted BWCs three years later in 2017.

[28] Our dynamic DiD design builds on Equation 2 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:

$$
\begin{aligned}
Y_{rcts} &= \sum_{e=-5}^{5} \beta_e^B \mathbb{1}[c = s] \cdot \mathbb{1}[t - e = t_s] + \mu_{cs}^B + \mu_{ts}^B + u_{rcts} && \text{if r is Black} \\
Y_{rcts} &= \sum_{e=-5}^{5} \beta_e^W \mathbb{1}[c = s] \cdot \mathbb{1}[t - e = t_s] + \mu_{sc}^W + \mu_{st}^W + v_{sct} && \text{if r is White.}
\end{aligned} \tag{4}
$$

II.B Results

II.B.1 Arrests

BWCs reduce arrest rates, especially for Black people. Table 3(a) compares changes in arrest rates in newly-adopting counties to the contemporaneous changes in control counties. After BWCs are introduced, overall arrest rates decline by 0.29 percentage points (pp) in Column 1. This decline is concentrated among Black people, whose arrest rates fall by 0.51 pp or 7.2% in Column 2 (p-value $= 0.063$). By contrast, there is an insignificant .07 pp change in aggregate arrests for white people (Column 3). As a result, disparities in arrest rates fall, eliminating 9.1% of the baseline gap of 4.5 pp (Column 4, p-value $=$ 0.039).

Figure 2 illustrates the dynamic effects of BWCs. Prior to BWC adoption, arrest rates for both Black and white people evolved similarly in counties about to adopt BWCs and counties not about to adopt BWCs. However, once BWCs are in use, arrests begin to fall in adopting counties, with reductions concentrated among Black people. We see similar dynamic patterns for arrests using alternative DiD estimators in Figure 2(c). Across all estimators, BWCs' impacts on arrest disparities grows over time. This compounding effect may partly explain why we find significant effects on arrests, while past studies, which tend to evaluate BWCs in a short window around adoption, do not.

BWCs' estimated effects on arrest disparities are robust to various checks on our design. In Table 3, Column 5 excludes never-adopters from the control group, and Column 6 excludes neighboring counties. In Column 7, which allows for adoption-group specific pre-trends, the point estimate is similar but less precise. Finally, these results are robust to excluding the ten counties that elect a new sheriff in the election preceding BWC adoption in Figure A.2(a$-$b), and also to limiting to the nine federally-funded adoption events in Figure A.3(a$-$b).

BWCs' impacts on arrests are concentrated in the types of arrests where police have more

discretion. Figure 3(a) illustrates the change in "police-initiated arrests," defined as arrests for drug possession, drug sales, weapon possession, public order offenses, and resisting arrest, which are all typically the result of a discretionary police stop (LaFave et al., 2004). BWCs reduce the rate of police-initiated arrests for Black people by 0.36 pp, off of a base of 2.7% (Column 2 in Table 3(b)). For white people, these arrests also significantly decline by 0.13 pp, off a base of 0.92% (Column 3). Despite differential declines in level terms, Black and white people see similar declines in percentage terms: 13.3% for Black people and 13.9% for white people. Thus, BWCs' disparate impact on police-initiated arrests may simply reflect disparities in baseline contact with police — as opposed to BWCs' differential impacts on officers' interactions with Black and white people. Consistent with this interpretation, BWCs reduce per capita traffic stops made by police to a similar extent in percentage terms for both races (by 13−14%) — but to a greater extent in absolute levels for Black people than white people (2.2 pp versus 0.85 pp, see Table 3(c)).[29]

We contrast police-initiated arrests with other types of arrests in Figure 3(b). Arrests for violent crimes, including homicide, sex crimes, and assaults — which tend to be initiated by witnesses rather than police —- do not significantly change for either race after BWCs are adopted.[30]

Arrest rates for low-level offenses. BWC's change in arrest rates is concentrated in minor offenses that almost never lead to incarceration. It is exceedingly rare for defendants to be incarcerated for offenses like loitering or simple drug possession; yet these are precisely the types of arrests that decline the most after officers start wearing BWCs. Figure 4 illustrates this pattern: arrest rates decline substantially for offenses with less than a 1% chance of incarceration, but they do not appreciably shift for offenses with a higher chance. Taking the point estimates at face value indicates that 96% of the aggregate decline in arrest rates for Black people is driven by arrests with less than a 1% chance of

[29]Results for police-initiated arrests and traffic stops are robust to omitting never-treated or neighboring counties and controlling for adoption-group specific linear trends (Columns 5–7, Table 3(b)-(c)).

[30]We also do not detect significant changes in police killings of civilians after BWCs are adopted, although we can not rule out large percentage changes in either direction (see Figure A.6).

incarceration (96% $= 100 \cdot 0.49/0.51$). Therefore, if arrest rates were the only outcome affected by BWCs, we would not expect an appreciable change in incarceration.

While BWCs' impacts on arrest rates could not, on their own, substantially change the incidence of incarceration, BWCs could still impact sentencing decisions by changing downstream actors' information sets. Once police have less control over arrest narratives, later actors may reach different conclusions about which punishments are warranted. We next turn to BWCs' impacts on incarceration and other outcomes downstream of arrest.

II.B.2 Incarceration

BWCs reduce incarceration rates for Black people, but have no significant effect for white people. Figure 5(a) illustrates this result. For both racial groups, incarceration rates follow similar trends in treated and control counties prior to BWC adoption. However, once BWCs are adopted, incarceration rates for Black people begin to decline in treated counties only. These patterns are consistent across alternative DiD estimators, as shown in Figure 5(c).

Table 4 presents the pooled difference-in-differences estimates, comparing the five years pre- versus post-BWC adoption. These estimates indicate that BWCs reduce Black people's incarceration rate by 0.029 pp or 10.5% in Column 2 of Table 4(a) (p-value $= 0.02$). Yet we see an insignificant effect increase of .001 pp for white people in Column 3. These differential effects lead to a meaningful decline in incarceration disparities in Column 4 and Figure 5(b) (p-value $= 0.0062$). Indeed, almost fourteen percent of the baseline disparity in incarceration rates is eliminated by BWCs. These estimates are similar when omitting never-adopting counties and neighboring counties from the control group (Columns $5-6$) and when allowing for differential pre-trends in incarceration rates by adoption group (Column 7). Finally, these results are robust to excluding counties with new sheriffs (in Figure A.2 ($c-d$)) and limiting to federally-funded adoption events (in Figure A.3($c-d$)).

BWCs similarly reduce disparities in other downstream outcomes, including jail time and

conviction rates (Table 4(b)-(c)). These effects may help to break cycles of criminal-justice contact (Agan et al., 2023; Stevenson et al., 2023) since jail time can lead to job loss (Dobbie et al., 2018) and criminal records can hamper job searches (Pager and Shepherd, 2008).

II.B.3 Probing Threats to the DiD Design

We further probe the validity of our design with additional tests for pre-trends. We find no evidence of differences in pre-trends for arrest disparities (p-value $= 0.85$) or for incarceration disparities (p-value $= 0.35$). These nulls are not due to a lack of statistical power to detect pre-trends. Figure A.4 uses the method proposed by Roth (2022) to compare our event-study results with the worst-case scenario trends that we have power to detect. Most of the event-study confidence intervals do not overlap with the worst-case trend that we have 50% power to detect, and many do not overlap with the worst-case trend we have 90% power to detect. Tests for pre-trends may be especially informative in this context since it typically takes multiple years to fund and implement BWCs. Given the length of time to fully roll out this technology, we would expect any confounding factors that cause a place to adopt BWCs earlier (versus later) to show up in differential trends before adoption. Yet we do not find any evidence of a divergence in arrests or incarceration between places about to adopt and those that will adopt later (or not at all).

Even with parallel pre-trends, our results could still be driven by contemporaneous policies (or other shocks) that coincide with the timing of BWC adoption. We find that BWCs' effects on both arrests and incarceration are broadly replicated across the different natural experiments, which indicates that confounders would have to broadly coincide with BWC timing to explain the observed patterns. Figure A.5 plots the distribution of the estimated effects for each of the twenty-six adoption events. For all sub-experiments, the effects on arrest and incarceration rates lie close to zero for white people. By contrast, the plurality of the estimates are negative for Black people.

III Survey

Why do BWCs reduce incarceration disparities, even though they have no impact on the incidence of arrests that could lead to incarceration? To unpack BWCs' downstream effects, we use our survey of prosecutors. Section III.A investigates the role of prosecutors' trust in police reports. Section III.B investigates whether prosecutors' beliefs about police reliability and Black people's criminality shift once they can watch arrest footage.

III.A Trust in Police Reports

Before BWCs, some prosecutors may increase incarceration disparities simply because they trust racially-biased decisions made at arrest. To assess this possibility, we examine whether prosecutors who place greater trust in police accounts of arrests increase incarceration disparities, and whether prosecutors' trust in police matters less once BWCs record police arrests. Section III.A.1 describes how we measure prosecutors' trust in police. Section III.A.2 details how we use the conditional quasi-random assignment of cases to prosecutors in North Carolina to relate prosecutors' beliefs about police to their effects on incarceration. Section III.A.3 presents the results.

III.A.1 Measuring Prosecutors' Trust in Police

The survey asked prosecutors in what percent of their cases they felt 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 (Figure 6(a) shows the question interface). While prosecutors almost never felt uncertain about the defendant's true conduct once the case reached its final disposition, prosecutors varied widely in their uncertainty after first reading the police report. As shown in the blue histogram in Figure 6(b), some prosecutors (toward the left) generally trust the police to provide accurate information, while others (toward the right) frequently question its reliability. On average, prosecutors say they remain uncertain of the defendant's true conduct after reading the police report in 42% of their cases, which implies that they trust the police report's accuracy most (58%) of the time.

Prosecutors' elicited skepticism about police reports is largely unrelated to their other characteristics, including their political affiliation and personal demographics (Figure 6(c)). 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, racial disparities, or racial bias. Prosecutors' beliefs are also not strongly related to their office: only about 8% of the variation can be explained by office, leaving considerable variation within each office (see Figure A.8).

III.A.2 Empirical Design

Our design compares racial disparities in incarceration outcomes across prosecutors who handle similar cases — but who have different beliefs about police reports. We estimate:

$$\text{Incarceration}_i = \kappa \text{Black}_i \times \text{Uncertainty about Police Reports}_{p(i)} + \mu_{p(i)} + X_i'\gamma + u_i, \quad (5)$$

where $\mu_{p(i)}$ 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 also control for the recommendation of the state sentencing guidelines, which is determined by the defendant's arrest charge and criminal record.[31] We show robustness to including fixed effects for the specific arrest charge and controls for defendant demographics.

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 — in terms of defendants' demographics, arrest-charge severity, or criminal-record

[31] The state sentencing guidelines are organized as a grid, where each cell has a different punishment. The rows are determined by the "offense class" of the defendant's most severe charge, and the columns, by the defendant's criminal record "prior points," which are a weighted sum of prior convictions (e.g., a low-level felony is 2 points). We control for the guidelines cell with fixed effects for the "offense class" interacted with "prior points" and year. We infer each defendant's prior points using his past convictions in the records.

length (Figure A.9). Second, we show that when a prosecutor moves to a different office, her estimated disparate impact moves with her: the estiamted coefficient in predicting disparities across offices is 0.90 (Figure A.10). Since deviations from random case assignment would likely vary from office to office, this suggests that our estimates are not simply reflecting non-random case assignment.[32]

Under as-good-as-random case assignment, we can interpret κ in Equation 5 as the relationship between a prosecutor's skepticism of police reports and her causal effect on incarceration disparities. If some police are racially biased, we would predict κ to be negative, since prosecutors who are more skeptical of police reports would pass through less of this bias — and consequently reduce disparities relative to less skeptical prosecutors.

Heterogeneity in κ by the extent of police discretion. We investigate whether κ varies based on how much control the officer has over the arrest narrative. We first test whether κ differs in places without BWCs — where police reports are often prosecutors' only direct source of information about arrests — relative to places with BWCs — where prosecutors can directly cross-check the officer's narrative with footage of the arrest:

$$\begin{aligned}\text{Incarceration}_i =& \kappa^{\text{No BWC}}\text{No BWC}_{c(i)} \times \text{Black}_i \times \text{Uncertainty about Police Reports}_{p(i)} \\ &+ \kappa^{\text{BWC}}\text{BWC}_{c(i)} \times \text{Black}_i \times \text{Uncertainty about Police Reports}_{p(i)} \\ &+ \mu_{p(i),\text{BWC}_{c(i)}} + X_i'\gamma + u_i. \end{aligned} \tag{6}$$

We predict that $\kappa^{\text{No BWC}}$ is larger in magnitude than κ^{BWC}. With BWCs, prosecutors' trust in police reports may be less pivotal since they can see for themselves whether the officer's narrative diverges from the on-scene reality. Moreover, prosecutors' trust in police may matter less after BWC adoption if officers are less likely to make racially biased arrests when they are being recorded. Simply put, distrusting the police should do less to reduce

[32]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 prosecutor moves that span judicial districts, so any match effects with particular judges or defense attorneys would be disrupted.

disparities if police themselves have become less biased and any remaining bias is more detectable in arrest footage (so $\kappa^{\text{BWC}} \approx 0$). As a result, surveilling police may serve as a substitute for prosecutors' skepticism of police.

Second, we assess whether κ depends on police officers' control over the arrest given the type of offense. For drug possession and public-order arrests, police typically choose whether to initiate the arrest and are often the only witness. For other arrests, victims or witnesses will often play role by calling 911 and testifying about the crime. We estimate:

$$\begin{aligned}\text{Incarceration}_i =& \kappa^{\text{No BWC, Police-Init.}} \text{No BWC}_{c(i)} \times \text{Police-Initiated}_i \times \text{Black}_i \times \text{Uncertainty}_{p(i)} \\ &+ \kappa^{\text{BWC, Police-Init.}} \text{BWC}_{c(i)} \times \text{Police-Initiated}_i \times \text{Black}_i \times \text{Uncertainty}_{p(i)} \\ &+ \kappa^{\text{No BWC, Other}} \text{No BWC}_{c(i)} \times \text{Other Arrest}_i \times \text{Black}_i \times \text{Uncertainty}_{p(i)} \\ &+ \kappa^{\text{BWC, Other}} \text{BWC}_{c(i)} \times \text{Other Arrest}_i \times \text{Black}_i \times \text{Uncertainty}_{p(i)} \\ &+ \mu_{p(i),\text{BWC}_{c(i)},\text{Police-Initiated}_i} + X_i'\rho + v_i. \end{aligned} \tag{7}$$

We test whether $\kappa^{\text{No BWC, Police-Init.}}$ has the largest magnitude and whether skepticism of police does not matter for arrests not initiated by police ($\kappa^{\text{BWC, Other}}$, $\kappa^{\text{No BWC, Other}} \approx 0$).

Interpreting these results is complicated by the fact that we elicit prosecutors' beliefs about police reports in 2020 but observe their case outcomes in earlier years. We often elicit prosecutors' beliefs after BWCs have been adopted, but BWCs may themselves impact beliefs about police reliability (as discussed in Section III.B). If BWCs cause a similar shift in beliefs for all prosecutors in a unit, then our estimates of κ — the relationship between prosecutors' skepticism of police and their disparate impacts — would not be appreciably affected, since prosecutors' impacts are estimated *relative* to others in their unit. If, however, BWCs have idiosyncratic effects on prosecutors' beliefs — leading some, but not all, to become more skeptical of police reports — this would attenuate our estimate of $\kappa^{\text{No BWC}}$ in places that ultimately adopt BWCs.[33]

[33]We find similar, though more noisily estimated, results when restricting to counties that have not adopted BWCs by the time we conducted the survey.

III.A.3 Results: Prosecutors' Trust in Police & Their Disparate Impacts

Prosecutors who express more uncertainty about police reports have lower incarceration disparities in their cases compared to less skeptical prosecutors in the same office and unit. Figure 7(a) illustrates this result. Prosecutors who frequently question police reports tend to reduce disparities (the points to the right), while prosecutors who infrequently question police reports tend to increase incarceration disparities (the left-most point) after controlling for the unit of case assignment, the inputs to the state sentencing guidelines, and prosecutor fixed effects (as in Equation 5). Our estimate indicates that a prosecutor who always questions police reports would be 0.81 pp less likely to incarcerate Black defendants relative to white defendants compared to prosecutors who never question them (p-value $= 0.034$, Column 1, Table 5). Relative to the raw disparity in incarceration rates of 3.3 pp, this represents a 25% reduction in disparities.

Figure 7(b) analyzes the relationship between prosecutors' trust in police reports separately in counties with and without BWCs. In places without BWCs (the left panel), prosecutors who are more skeptical of police reports reduce disparities relative to prosecutors who trust the police more. By contrast, in counties with BWCs (the right panel), there is no systematic relationship between prosecutors' trust in police reports and their disparate impacts.[34] Consistent with our findings in Section II, we also see that incarceration disparities are lower in places with BWCs. Visually, *all* prosecutors in places with BWCs appear to reduce incarceration disparities to a similar extent as the *most* skeptical prosecutors in places without BWCs, suggesting that surveillance may act as a substitute for prosecutor skepticism. This substitution effect may also help to explain our findings in Section II: BWCs may reduce incarceration disparities partly because they make it less critical whether prosecutors question (potentially biased) arrest decisions.

Finally, we find that prosecutors' skepticism of police matters most when police have the

[34]The relationship between prosecutors' beliefs and their disparate impacts is significantly different in counties with versus without BWCs (p-value $= 0.056$, Column 1 of Table A.3).

most control over the arrest narrative, both because arrests are not recorded and because the arrest was likely initiated by police. For police-initiated arrests, prosecutors who always doubt police reports reduce incarceration disparities by 1.73 pp in places without BWCs relative to those in the same unit who always trust police reports (see Figure 7(c), p-value $= 0.0034$). This reduction nearly halves the baseline disparity in incarceration rates for police-initiated arrests (of 3.7 pp). Even more than in the analysis of all cases, it is clear that disparities in police-initiated cases are uniformly lower in places with BWCs — and that all prosecutors with BWCs end up with disparities as low as the most skeptical prosecutors without BWCs.[35] For other kinds of arrests (that are likely not initiated police), there is no clear relationship between prosecutors' skepticism of police and their disparate impacts, either with or without BWCs (Figure 7(d)).

Table 5 analyzes the robustness of these patterns to the inclusion of additional controls. The results are quantitatively similar with fixed effects for the specific arrest charge, controls for defendant demographics, and unit effects that vary by defendant race.

III.B Endogenous Beliefs

This section explores whether BWCs allow prosecutors learn and update their beliefs about police reliability and Black people's criminality. We then investigate whether prosecutor learning translates into effects on their case outcomes.

III.B.1 Exposure Design

To analyze the effects of exposure to BWCs, we leverage the fact that, *within* a given county and year, prosecutors differ in the duration of their exposure to BWCs based on (i) when they started working in the county and (ii) when their county adopted BWCs. We use this variation across prosecutors 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

[35] For police-initiated arrests, the relationship between prosecutor skepticism and disparate impacts is significantly different in places with BWCs versus without them (p-value $= 0.010$, Column 2 of Table A.3).

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. The difference in these differences aims to isolate variation in BWC exposure by netting out experience and county effects.

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

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

$$\text{Belief}_p = \phi \text{Years with BWC}_p + \mu_{c_p} + \mu_{\tau_p} + \epsilon_p. \tag{8}$$

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, the following assumption must hold: 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 using the set of surveyed prosecutors who were hired before their county adopted BWCs — and so who all had the *the same* years of exposure to BWCs despite differing in their tenure.[36] Consistent with our identifying assumption,

[36]Consider Greensboro, which adopted in 2014, and Raleigh, which adopted in 2018. Among all pre-2014

we find that tenure does not differentially predict beliefs across adoption groups among prosecutors hired before their county adopted BWCs (see Figure A.11).

We estimate the analogue of Equation 8 in the court records to evaluate how a prosecutor's exposure to BWCs predicts the incarceration disparities in her cases:

$$\begin{aligned}\text{Incarcerated}_i = \gamma \text{Years with BWC}_{p(i)} \times \mathbb{1}[r_i = \text{Black}] + \theta \text{Years with BWC}_{p(i)} \\ + \mu_{r(i),c(i),t(i)} + \mu_{r(i),\tau_{p(i)}} + \mu_{\text{unit}(i)} + \zeta_i. \end{aligned} \tag{9}$$

The coefficient of interest, γ, aims to isolate BWCs' cumulative effect on prosecutors, which may reflect learning from watching unfiltered footage in more cases. By including race-county-year fixed effects ($\mu_{r(i),c(i),t(i)}$), this design absorbs any direct effects of BWCs that would impact *all* prosecutors working in counties with BWCs.[37]

Since this analysis leverages variation within a given county and year, it relies on a different identifying assumption than our baseline BWC analysis in Section II. For γ to capture the causal effect of cumulative exposure to BWCs on prosecutors' disparate impacts, the relationship between prosecutors' tenure and their disparate impacts must have been similar in places that adopted earlier and later in the absence of different BWC timing. Under this assumption, in the years before *any* county adopted BWCs (from 1995 to 2014), we would expect the relationship between tenure and disparate impacts to be similar in counties that would (versus would not) adopt BWCs in the future. We find empirical support for this: prosecutor tenure does not differentially predict incarceration disparities across counties in the years before any county used BWCs (see Figure A.12).

hires, those hired earlier versus later do not differ in their BWC exposure in either county and so, under our identifying assumption, the relationship between tenure and beliefs would not differ across the two.

[37] BWCs' direct effects on all prosecutors may include, for instance, the impacts of directly observing BWC footage and any anticipatory changes in police arrests.

III.B.2 Results

Beliefs. We test whether prosecutors with more exposure to BWCs hold different beliefs about the sources of racial disparities in the criminal justice system. To elicit these beliefs, we first present prosecutors with statistics about existing racial disparities in criminal-justice outcomes and then ask them to rate (on scales from 0 to 100) the importance of potential drivers of these disparities.[38] Figure 8(a) shows the interface for this question.

We find that prosecutors with more exposure to BWCs tend to believe that Black people are less criminal and the system is more biased. Figure 8(b) illustrates this result. Prosecutors with more BWC exposure believe that disparities are driven less by racial differences in past or current conduct (shown in light and dark orange) compared to prosecutors with less exposure in the same county. Each year of additional BWC exposure is associated with a 3.9 pp decline in the percent of disparities prosecutors attribute to differences in past conduct (p-value $= 0.026$) and a 3.0 pp decline for current conduct (p-value $= 0.028$). Prosecutors with more BWC exposure also believe that disparities are driven more by bias — i.e., by the same conduct being *perceived* differently when the defendant is Black rather than white (shown in green, coefficient $= 4.4$ pp, p-value $= 0.044$). These patterns suggest that BWC footage shows prosecutors a reality different than the one previously presented by police — a reality where disparities in arrests are driven less by Black people's conduct and more by racial bias. Consistent with this potential learning mechanism, prosecutors with more BWC exposure are also more skeptical of the reliability of police reports (shown in blue, coefficient $= 4.5$ pp, p-value $= 0.09$).

We probe the robustness of these findings in three ways. First, we do placebo checks using beliefs about potential drivers of disparities that one would not expect to be affected by

[38] We modeled the questions on the approach taken by the General Social Survey (GSS), which first presents respondents with facts about racial disparities and then asks respondents about the drivers of disparities. Our piloting suggested that this approach helped to reduce prosecutors' reluctance to engage with questions about race. Despite these efforts, a quarter of prosecutors still did not answer this question. Non-responses are not correlated with BWC exposure: an additional year of BWC exposure was associated with an insignificant 2.2 pp increase in the likelihood of answering the question (p-value $= 0.44$).

BWC exposure. We find that prosecutors' BWC exposure is orthogonal to their beliefs about racial differences in the quality of legal representation (in dark gray in Figure 8) and racial differences in criminal record "prior points" (in light gray).[39] Second, we show that these patterns are unlikely to be driven by changes in the selection of prosecutors: after a county adopts BWCs, there is not a significant change in the share of prosecutors who leave or are hired in a given year, the share who are Black, or the share who are politically liberal (the last four rows of Table A.2). Finally, the shift in prosecutors' beliefs is similar after controlling for prosecutor's race and politics (in Table A.4).

Disparate Impacts. We find suggestive evidence that prosecutors with more exposure to BWCs reduce incarceration disparities compared to other prosecutors in the same county and year, who have the same contemporaneous exposure to BWCs. Figure 8(c) illustrates this result. The x-axis plots a prosecutor's number of years of prior BWC exposure based on her start date and the year her county adopted BWCs. The y-axis plots the racial disparities in the prosecutor's incarceration rates relative to other prosecutors in her county and year who handle similar cases in the same unit, as in Equation 9. Each additional year of exposure to BWCs is associated with a 0.33 pp reduction in incarceration disparities (p-value $= 0.044$), which represents a 10% reduction in incarceration disparities (relative to the baseline raw disparity of 3.3 pp).

These patterns suggest that BWCs' impacts on prosecutors' beliefs and decisions compound over time as prosecutors see more unfiltered footage of arrests. Consistent with this interpretation, we see that BWCs' impacts on per capita incarceration disparities grow over time (in Figure 5).[40] Taken together, these results suggest that BWCs' aggregate impacts on incarceration partly reflects prosecutors updating their beliefs about the data-generating process for their cases.

[39]The contribution of disparities in past convictions to disparities in current sentencing outcomes is an empirical reality that prosecutors may (or may not be) fully aware of. It is unlikely that watching more BWC footage would reveal the extent to which Black defendants often have longer criminal records.

[40]This growing effect may also reflect improvements in BWC technology or wider distribution to officers.

IV Conclusion

We use the staggered adoption of body-worn cameras (BWCs) in North Carolina to study the impacts of surveilling police officers. We find that BWCs reduce per capita arrests and incarceration for Black people, but have no significant impact for white people. To unpack BWCs' downstream effects, we fielded an original survey of North Carolina prosecutors and asked them about their trust in police reports and their beliefs about the source of racial disparities in the criminal justice system. We find that prosecutors who more frequently question police reports reduce incarceration disparities relative to others in the same office and unit, and that this skepticism no longer matters once BWCs provide prosecutors with more objective information about arrests. We also find evidence that prosecutors with more exposure to BWCs believe that police are less reliable and arrests are more racially biased — and that this shift in beliefs partly explains BWCs' effects on incarceration disparities.

Our results are consistent with police and prosecutors playing a strategic communication game that is then disrupted by BWCs (Kamenica and Gentzkow, 2011). Disparities in arrests and incarceration decline once police know that prosecutors (and other downstream actors) may observe any misrepresentation of arrests. Without BWCs, prosecutors may find themselves in a catch-22. Prosecutors may know that police tend to misrepresent Black people's arrests, but police may be strategic and not lie so frequently that prosecutors disregard their accounts. If prosecutors cannot discern whether the officer is biased in any given case, they may be better off relying on officers' account, despite knowing that they are biased on average. As a result, prosecutors may rationally, though unwillingly, pass through police misrepresentation before BWCs — and then BWCs shut down this strategic miscommunication.

In addition to a rational game, our results are consistent with behavioral biases perpetuating initial disparities. Our first survey result — that prosecutors' tendency to trust police reports magnifies their pass through of arrest disparities — is consistent with naivete

in strategic communication games (Kartik et al., 2007; Kartik, 2009). Our second survey result suggests that prosecutors' beliefs may be biased by misinformation from police. Before receiving more objective signals of the ground truth, prosecutors' worldviews may have been warped by biases in arrests.

Our paper focuses on police and prosecutors. Yet similar dynamics exist in any setting where later decision-makers rely on information from earlier actors — for instance, in labor markets, where hiring managers rely on the impressions of screening interviewers, or education, where college-admissions officers rely on the views of recommenders. In these other contexts, the initial actor may misrepresent the underlying reality, potentially in discriminatory ways. Future work could explore whether the dynamics we find are at play in other settings. Concretely, one could investigate whether monitoring screening interviewers would reduce disparities in ultimate hiring decisions. Or one could assess whether college-admission officers' beliefs about the reliability of recommenders predicts race or gender disparities in their admission decisions. These analyses could further reveal whether asymmetric information perpetuates discrimination — and whether surveillance could reduce the disparities generated by sequential systems.

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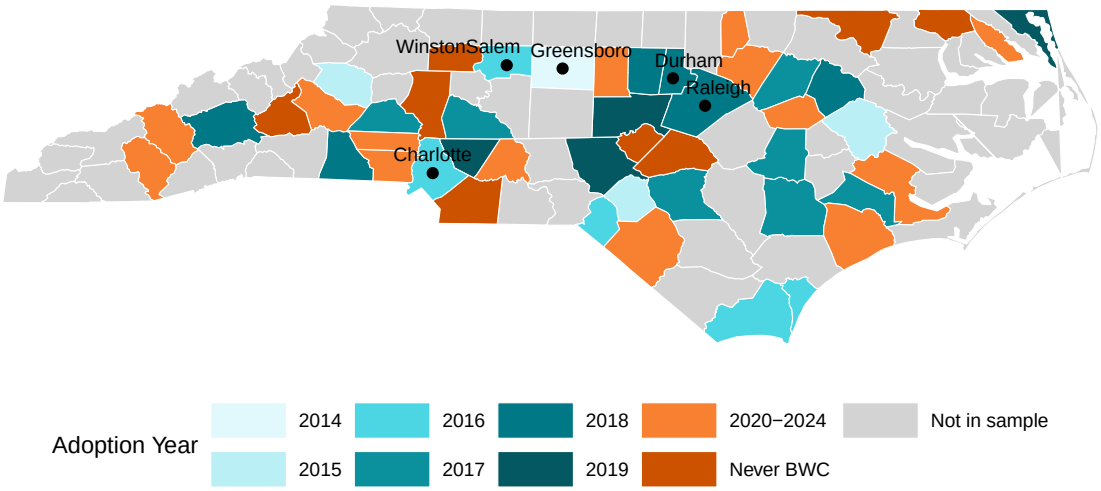
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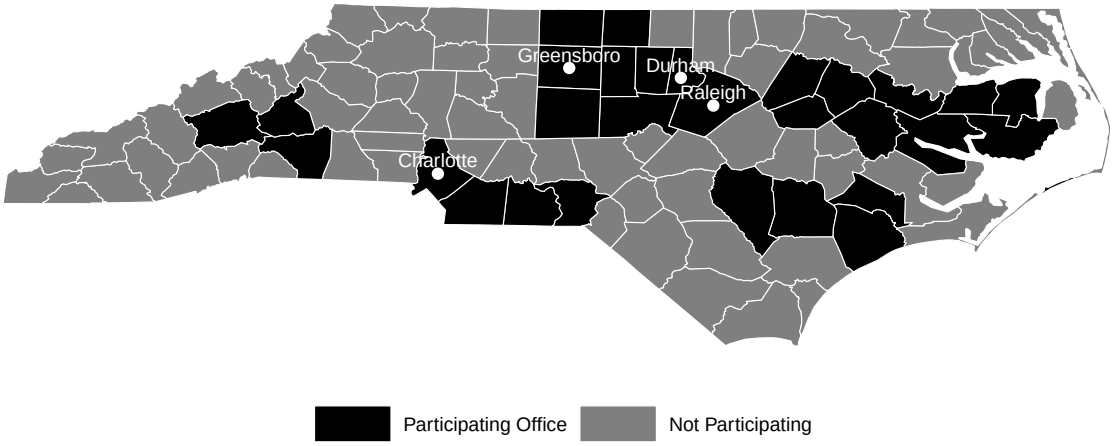
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Figure 1: Geographic Coverage of Analyses

(a) Body-Worn Camera Adoption Across North Carolina

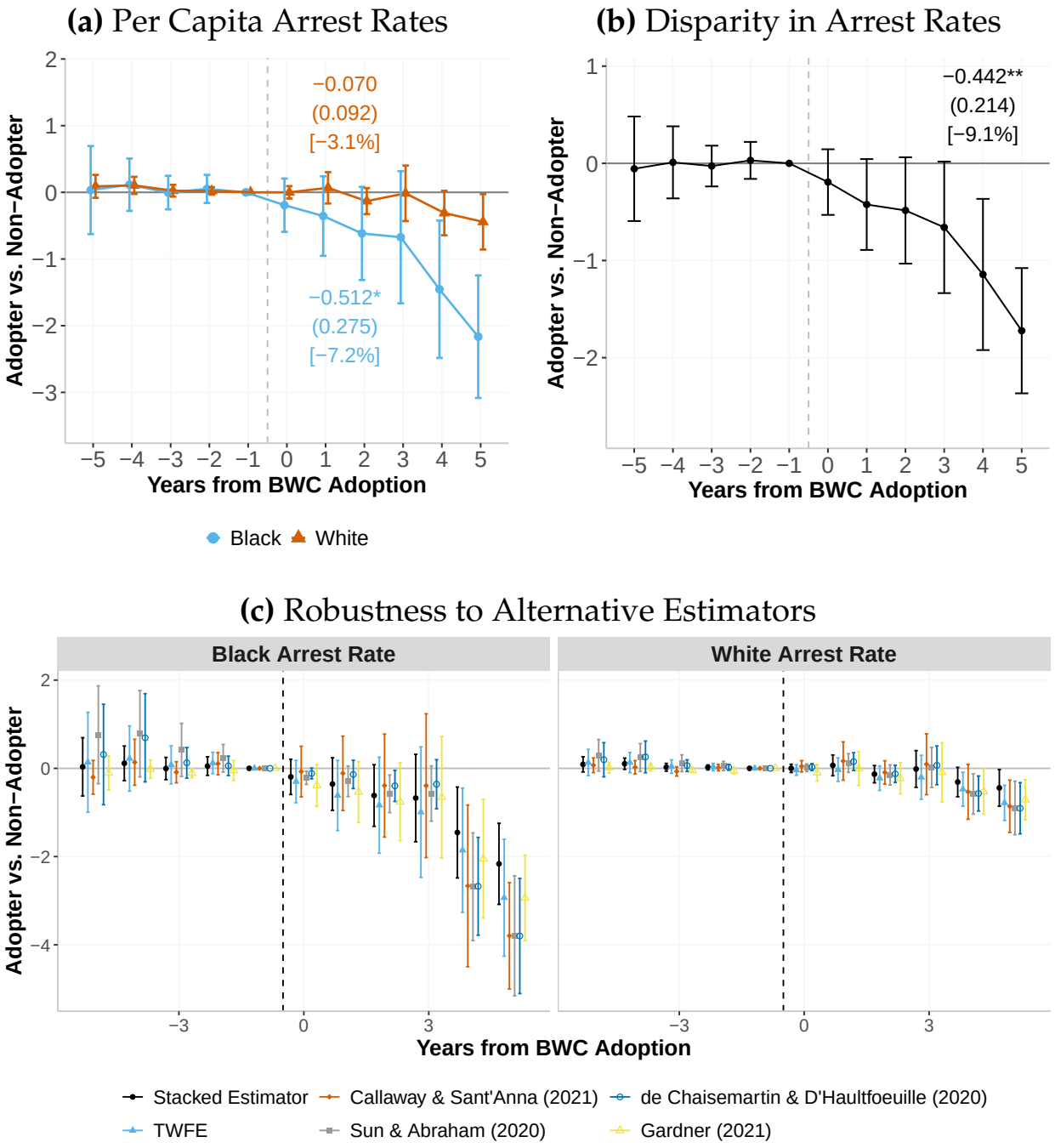


(b) Map of Offices Participating in our Survey



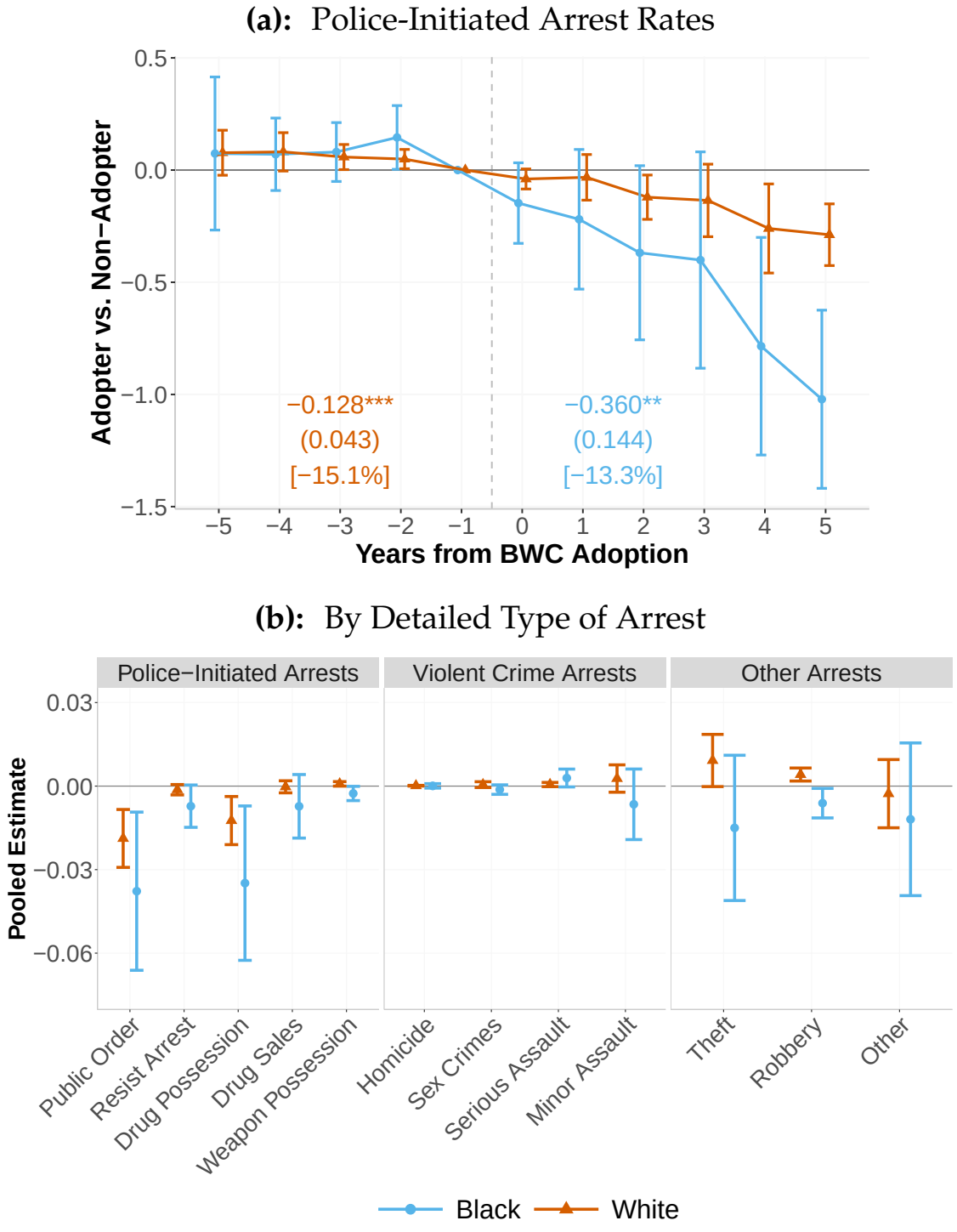
Notes: This figure illustrates the geographic coverage across North Carolina for our two analyses. Map (a) illustrates the staggered timing of body-worn cameras (BWCs) adoption across North Carolina counties. Section I.B describes how we determined the year of BWC adoption, by supplementing existing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

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



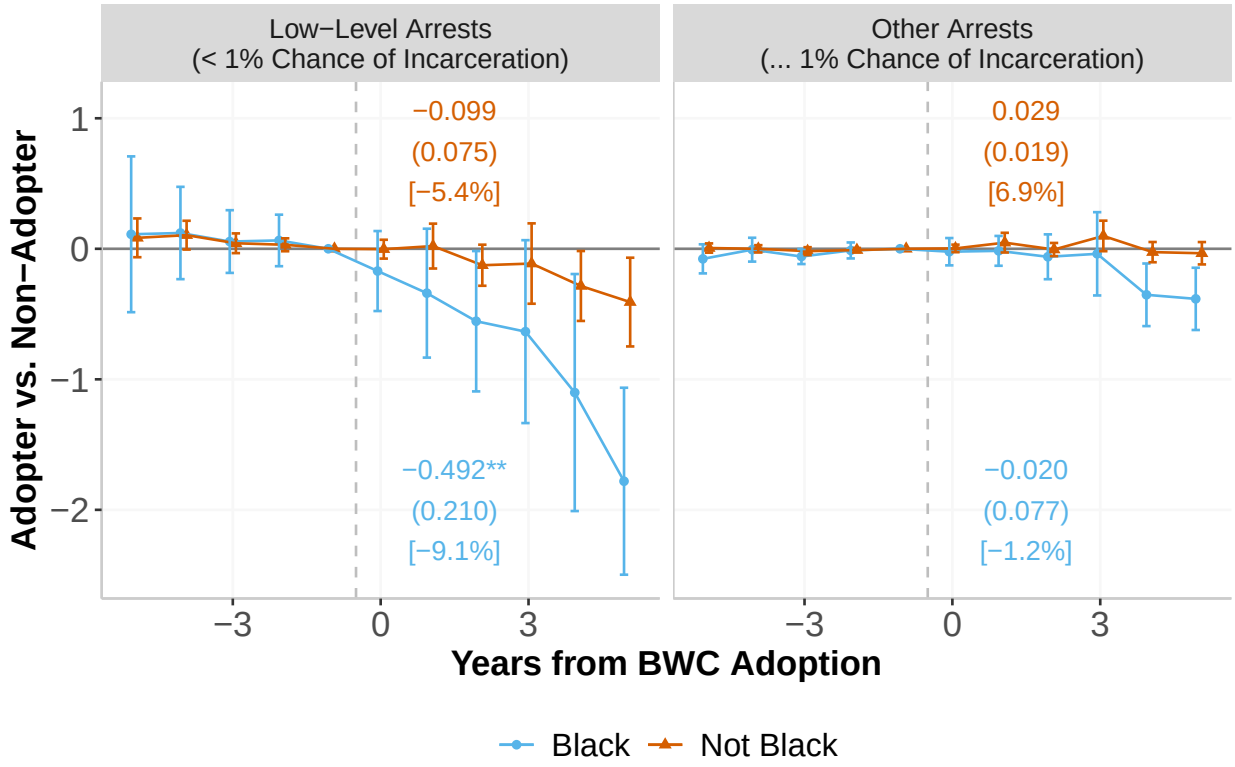
Notes: This figure illustrates the changes in arrests rates in North Carolina counties that adopt BWCs relative to control counties that have not yet adopted BWCs (or never do). Panel (a) plots per capita arrest rates by race, and panel (b), disparities in these rates. Both show estimates from Equation 4, with the year before BWC adoption as the reference year. All specifications are weighted by population. The annotated coefficients are pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years before; the bracketed numbers show the pooled estimates as percentages of the dependent mean. Panel (c) shows per capita arrest estimates using alternative DiD estimators: our preferred stacked approach (also shown in panel (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. To calculate per capita arrest rates, we divide the total number of observed arrests for each race by Census population counts, as in Equation 1. *p<0.1; **p<0.05; ***p<0.01.

Figure 3: Body-Worn Cameras' Effects on Police-Initiated Arrests



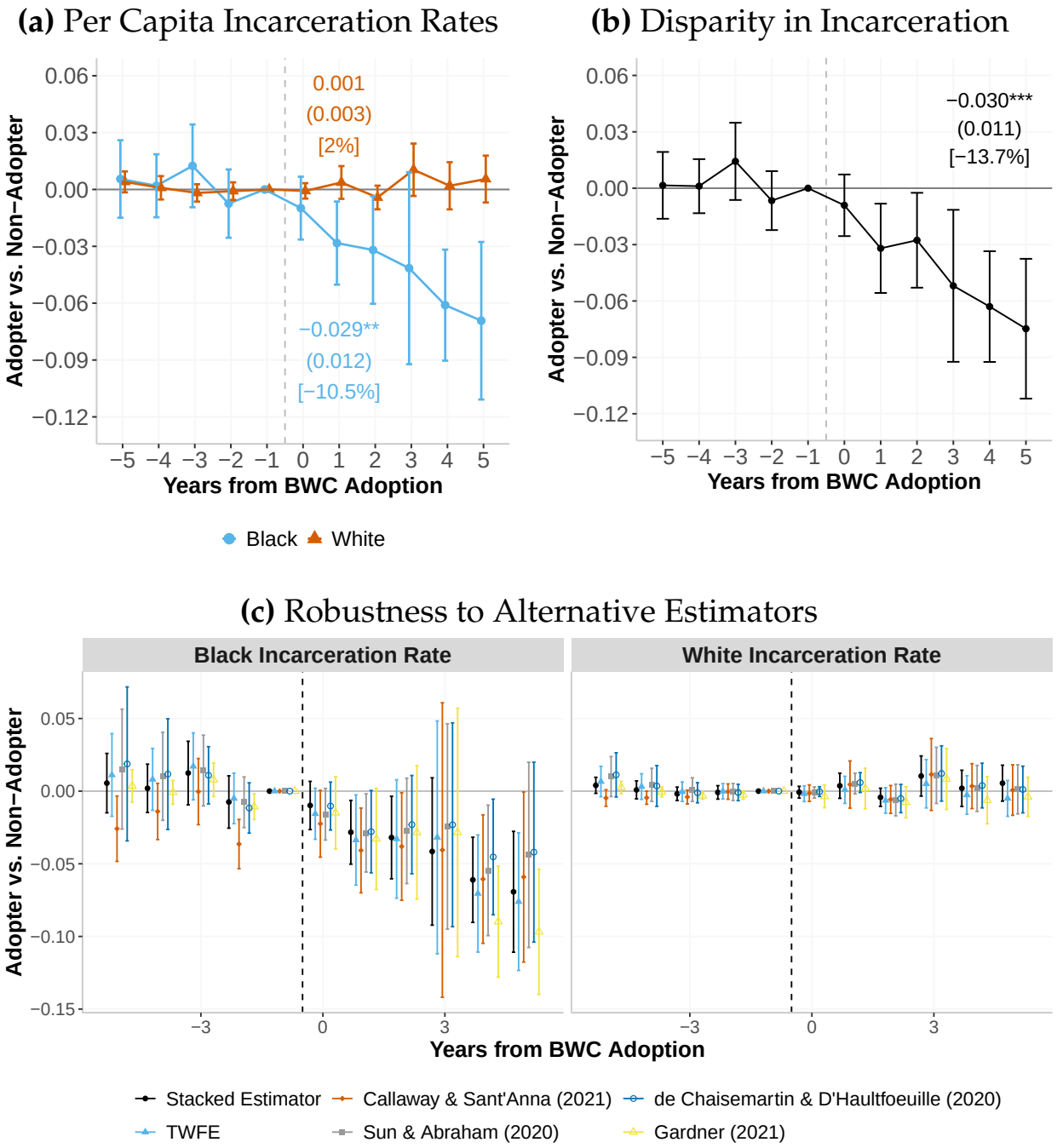
Notes: This figure illustrates the impact of body-worn cameras (BWCs) on per capita arrest rates. Panel (a) focuses on police-initiated arrests, defined as drug possession, drug sales, public-order offenses, weapon possession, and resist arrest as in LaFave et al. (2004). The event study is estimated with Equation 4. Panel (b) shows the pooled estimates for all crime types separately. The annotated coefficients and estimates reflect pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years prior to BWC adoption. The bracketed numbers show the coefficients as percentages of the dependent mean. Throughout, error bars represent 95% confidence intervals with standard errors clustered by county. All specifications weight by population. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure 4: Body-Worn Cameras' Effects By Severity of Arrest



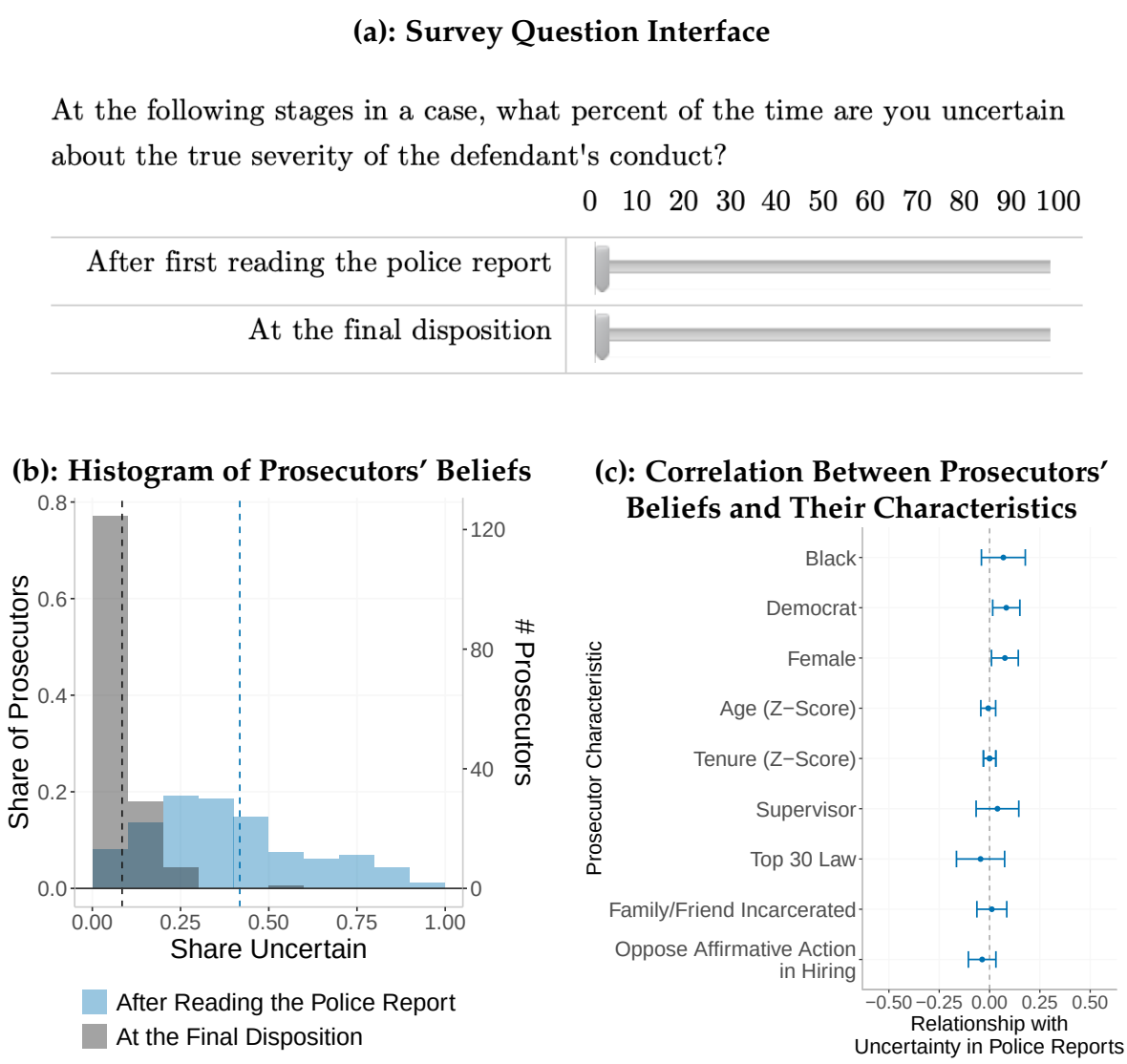
Notes: This figure illustrates the impact of body-worn cameras (BWCs) on by the severity of the arrest offense. The left panel includes low-level arrests — i.e., charges with less than a 1% chance of incarceration — and the right panel includes all other arrests. Each event study plot shows estimates from Equation 4. The annotated coefficients reflect pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years prior to BWC adoption. The bracketed numbers show the coefficients as percentages of the dependent mean. Throughout, error bars represent 95% confidence intervals with standard errors clustered by county. All specifications weight by population. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure 5: Body-Worn Cameras' Effects on Incarceration Rates



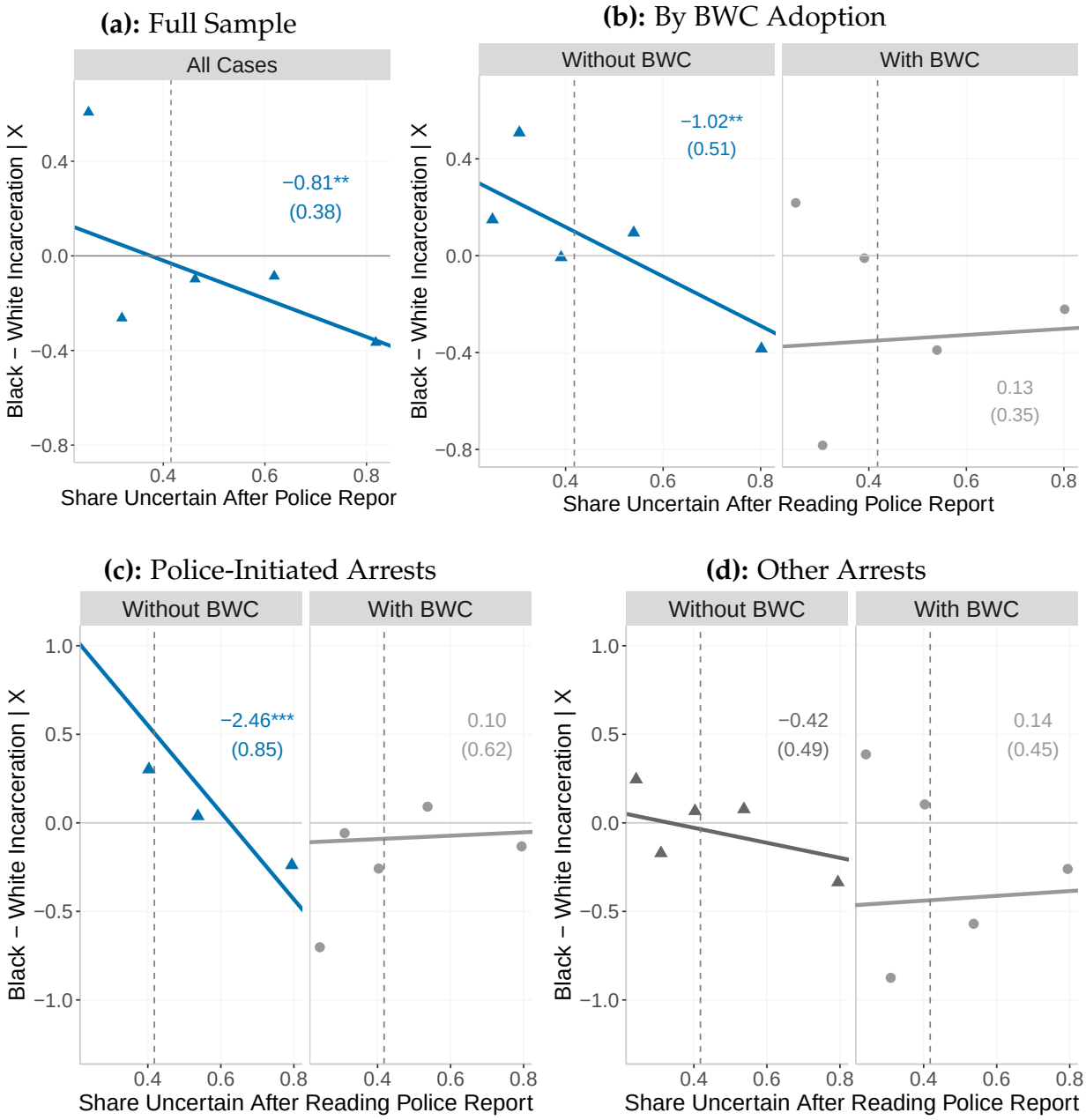
Notes: This figure illustrates the changes in incarceration rates in North Carolina counties that adopt BWCs relative to control counties that have not yet adopted BWCs (or never do). Panel (a) plots per capita incarceration rates by race, and panel (b), disparities in these rates. Both show estimates from Equation 4, with the year before BWC adoption as the reference year. The annotated coefficients are pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years before; the bracketed numbers show the pooled estimates as percentages of the dependent mean. Panel (c) shows per capita incarceration estimates 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. All specifications weight by population. To calculate per capita incarceration rates, we divide the total number of people of each race sentenced to prison by Census population counts, as in Equation 1. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure 6: Prosecutors' Beliefs about the Reliability of Police Reports



Notes: This figure summarizes surveyed prosecutors' beliefs about the reliability of police reports. Panel (a) shows the question interface from the survey. Panel (b) shows the distribution of surveyed prosecutors' trust in police reports — i.e., the share of cases in which they recall feeling uncertain about the true severity of the defendant's conduct after reading the police report and at the final disposition of the case. Panel (c) shows the coefficients from separate regressions that regress prosecutors' belief about police reports on the prosecutor characteristic or attitude. Error bars represent 95 percent confidence intervals.

Figure 7: Prosecutors' Uncertainty About Police Reports and their Disparate Impacts



Note: This figure shows the relationship between surveyed prosecutors' beliefs about the reliability of police reports and the racial disparities in incarceration rates in their cases. Panel (a) includes the full sample and is estimated using equation 5. Panel (b) compares prosecutors in counties without access to body-worn cameras (BWCs) to those in counties with access to BWCs and is estimated with Equation 6. Panels (c) and (d) compares counties with and without BWCs for police-initiated arrests and for all other arrests, and is estimated with Equation 7. Police-initiated offenses include drug, public order, and weapon possession offenses. All lines and annotated coefficients reflect linear fits estimated at the case-level. Standard errors are clustered by prosecutor. Table 5 tests the robustness of these estimates to the inclusion of additional controls, and Table A.3 shows the relationship between prosecutor skepticism and disparate impacts is significantly different in counties with BWCs versus without BWCs and for police-initiated arrests versus other arrests. Prosecutors' beliefs about police reliability come from our survey (see Figure 6(a) for the question interface), and incarceration outcomes come from North Carolina administrative records from 1995 to 2019. *p<0.1; **p<0.05; ***p<0.01.

Figure 8: Exposure to BWCs, Prosecutors' Beliefs, & Disparate Impacts

(a): Question Interface for Sources of Racial Disparities

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 gaps in sentencing outcomes for black and white defendants -- and everyone seems to have an opinion. We are interested in *your* perspective on what generates these aggregate differences.

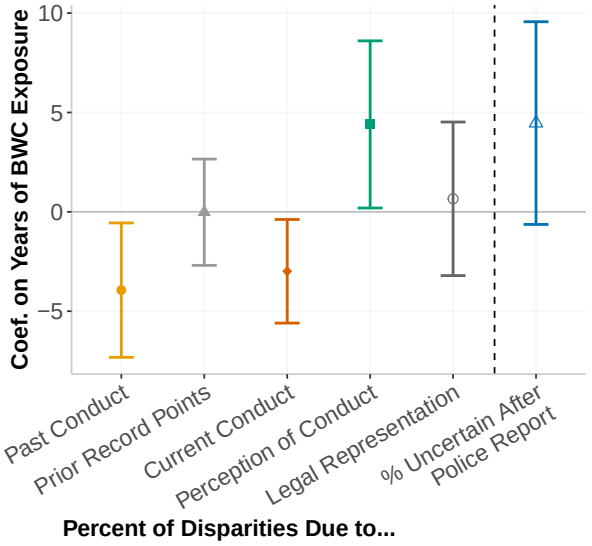
Among all defendants who are arrested for any felony offense, an average black defendant receives an active sentence more frequently than an average white defendant. In your view, how important are the following potential explanations in generating this difference? (Note, if you believe a potential explanation is incorrect, please place the corresponding slider at 0.)

0 10 20 30 40 50 60 70 80 90 100

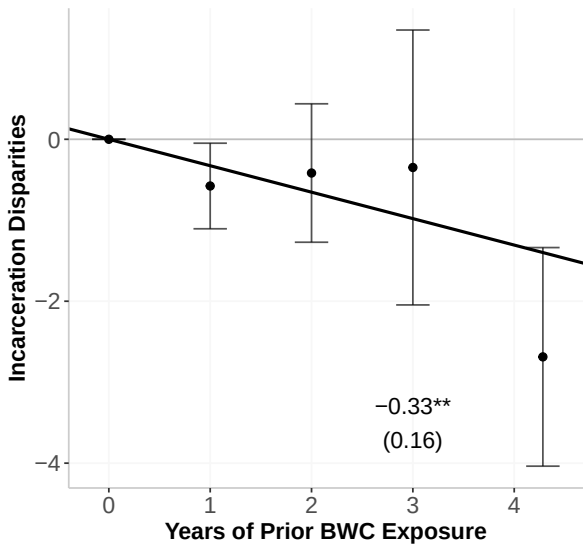
Black defendants tend to have more severe past criminal conduct.	
Black defendants tend to have more prior points.	
Black defendants tend to have more severe current criminal conduct.	
The current conduct of a black defendant is often perceived to be more serious than the same conduct committed by a white defendant.	
Black defendants tend to have lower quality representation.	

Colors added for clarity

(b): Shift in Beliefs



(c): Shift in Disparate Impacts



Note: This figure illustrates the relationship between the length of prosecutors' exposure to body-worn cameras (BWCs) and their beliefs and disparate impacts on incarceration. Panel (a) shows the interface for the survey question about the sources of racial disparities, where we have highlighted each of the sliders in colors to match the colors used in panel (b). Panel (b) shows the relationship between the length of prosecutors' BWC exposure and their elicited beliefs, where each point is estimated according Equation 8. The first five points use the beliefs elicited from the five sliders in panel (a), where we rescale if the prosecutor's responses to all five sliders summed to more than 100. The right-most point uses the question about police reports (shown in Figure 6(a)). Panel (c) shows the relationship between prosecutors' BWC exposure and the disparities in incarceration rates in their cases, where the annotated coefficient is estimated according to Equation 9. Error bars represent 95% confidence intervals. Standard errors are clustered by prosecutor. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 1: Summary Statistics of the North Carolina Court Records

			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
% Police-Initiated Arrests	1.13	1.15	1.31	1.05	1.12	0.89
Black	2.53	2.51	2.57	2.53	2.46	2.26
White	0.78	0.76	0.81	0.67	0.86	0.65
% Traffic Stops	7.66	7.67	9.51	7.73	5.47	4.12
Black	14.55	14.56	16.50	15.57	11.01	9.32
White	5.45	5.45	6.61	5.39	4.20	3.31
% 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
Outcomes in the Court System						
% Incarcerated (> 6mo)	6.4	6.4	6.3	6.8	6.1	6.2
% Conviction	44.8	44.1	39.4	46.6	46.9	46.6
% Trial	0.7	0.7	0.7	0.7	0.7	0.6
Defendant Characteristics						
% Black Defendant	45.3	48.7	56.1	50.3	36.6	36.4
% Female Defendant	27.5	27.5	26.6	27.5	29.0	28.1
Defendant Age (in Years)	31.3	31.1	30.6	31.4	31.4	31.3
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). Traffic stop data comes from the NC State Bureau of Investigation (NCSBI, 2024; Baumgartner, 2024)

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. 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: Body-Worn Cameras' Effects on Arrests and Stops

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(a) Per Capita Arrests						
	Overall	Black	White	Disparity			
Black x BWC				−0.44**	−0.39*	−0.44**	−0.36
				(0.21)	(0.21)	(0.22)	(0.23)
BWC	−0.28**	−0.51*	−0.07	−0.07	0.04	−0.06	−0.06
	(0.13)	(0.28)	(0.09)	(0.09)	(0.09)	(0.09)	(0.10)
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 Police-Initiated Arrests						
	Overall	Black	White	Disparity			
Black x BWC				−0.23**	−0.22*	−0.23**	−0.26**
				(0.11)	(0.11)	(0.11)	(0.12)
BWC	−0.22***	−0.36**	−0.13***	−0.13***	−0.08*	−0.12***	−0.09
	(0.07)	(0.14)	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)
Baseline Dependent Mean	1.32	2.70	0.85	1.32	1.32	1.32	1.32
Baseline Disparity				1.86	1.86	1.86	1.86
Percentage Change	-17.0%	-13.3%	-15.1%	-12.5%	-11.7%	-12.4%	-13.7%
	(c) Per Capita Traffic Stops						
	Overall	Black	White	Disparity			
Black x BWC				−1.31*	−1.22*	−1.04	−2.06***
				(0.70)	(0.71)	(0.64)	(0.67)
BWC	−1.20**	−2.17**	−0.85**	−0.85**	−1.05***	−0.76**	−1.03***
	(0.57)	(1.03)	(0.39)	(0.39)	(0.38)	(0.35)	(0.35)
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%
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 per capita arrests, police-initiated arrests, and traffic stops in newly-adopting BWC counties compared to contemporaneous changes for not-yet or never adopting counties in North Carolina. Columns 1−3 estimate Equation 2, where each observation is a county in a given year (and sub-experiment), and the dependent variable is the per-capita rate for all people in Column 1, Black people in 2, and white people in 3. Columns 4−7 estimate Equation 3, which includes observations for both Black and white people. Column 5 excludes never adopting counties, Column 6 excludes neighboring counties, and Column 7 allows for adoption group specific pre-existing trends in arrests. 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 4: Body-Worn Cameras' Effects on Sentencing and Conviction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(a) 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%
	(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%
	(c) 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%
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	16,068	8,034	8,034	16,068	12,740	15,008	16,068

Notes: This table analyzes the changes in per capita incarceration, jail time, and convictions in newly-adopting BWC counties compared to contemporaneous changes for not-yet or never adopting counties in North Carolina. Incarceration is defined as imprisonment of at least 6 months, typically served in state prison. Jail includes any stint behind bars. Conviction includes any misdemeanor or felony conviction. Columns 1−3 estimate Equation 2, where each observation is a county in a given year (and sub-experiment), and the dependent variable is the per-capita rate for all people in Column 1, Black people in 2, and white people in 3. Columns 4−7 estimate Equation 3, which includes observations for both Black and white people. Column 5 excludes never adopting counties, Column 6 excludes neighboring counties, and Column 7 allows for adoption group specific pre-existing trends. The baseline mean is the average in treated counties before they adopt BWCs. In Columns 1−3, the percentage change divides the coefficient on BWC by this baseline mean. In Columns 4−7, the baseline disparity is the percentage-point race gap in outcomes, and the percentage change divides the coefficient on Black x BWC by this gap. Standard errors are clustered by county. *p<0.1; **p<0.05; ***p<0.01.

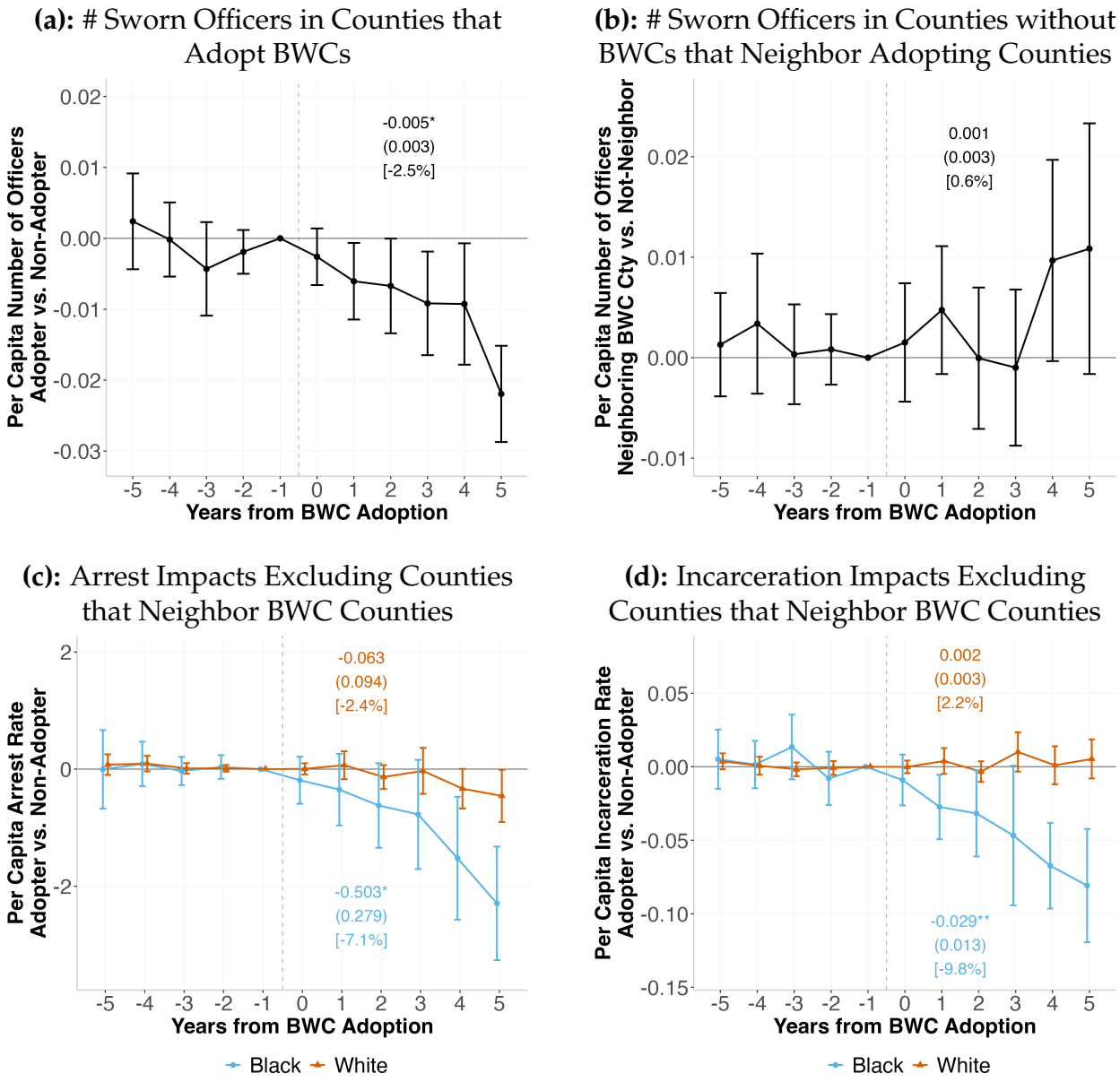
Table 5: Prosecutors' Uncertainty About Police Reports and Their Incarceration Disparities

	% Incarcerated >6mo								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black x Share Uncertain about Police Report	−0.81**	−0.84**	−0.77**						
	(0.38)	(0.39)	(0.39)						
No BWC x Black x Share Uncertain				−1.02**	−1.08**	−1.15**			
				(0.51)	(0.53)	(0.56)			
BWC x Black x Share Uncertain				0.13	0.05	0.35			
				(0.35)	(0.35)	(0.25)			
Police-Init. x No BWC x Black x Uncertain							−1.73***	−1.74***	−1.95**
							(0.58)	(0.58)	(0.87)
Police-Init. x BWC x Black x Uncertain							0.07	−0.04	0.36
							(0.43)	(0.43)	(0.39)
Other Arrest x No BWC x Black x Uncertain							−0.53	−0.65	−0.61
							(0.59)	(0.60)	(0.54)
Other Arrest x BWC x Black x Uncertain							0.14	0.06	0.34
							(0.53)	(0.55)	(0.45)
Office Crime-Unit 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 Uncertain	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.42	0.42
Std. Dev. in Share Uncertain	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	0.41	0.43	0.45	0.42	0.43	0.46	0.42	0.43	0.46
Adjusted R^2	0.39	0.40	0.42	0.40	0.41	0.43	0.40	0.41	0.43

Notes: This table analyzes the relationship between prosecutors' beliefs about the reliability of police reports and the racial disparities in incarceration rates in their cases, also shown in Figure 7. Column 1−3 estimate Equation 5 for the full sample of cases handled by prosecutors who participated in the survey; column 4−6 estimate Equation 6 for cases handled by surveyed prosecutors in counties where we can determine BWC adoption timing; and column 7−9 estimate Equation 7, also for surveyed prosecutors in counties with known BWC adoption timing. 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. The survey question interface for prosecutors' beliefs about police reports is shown in Figure 6(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.

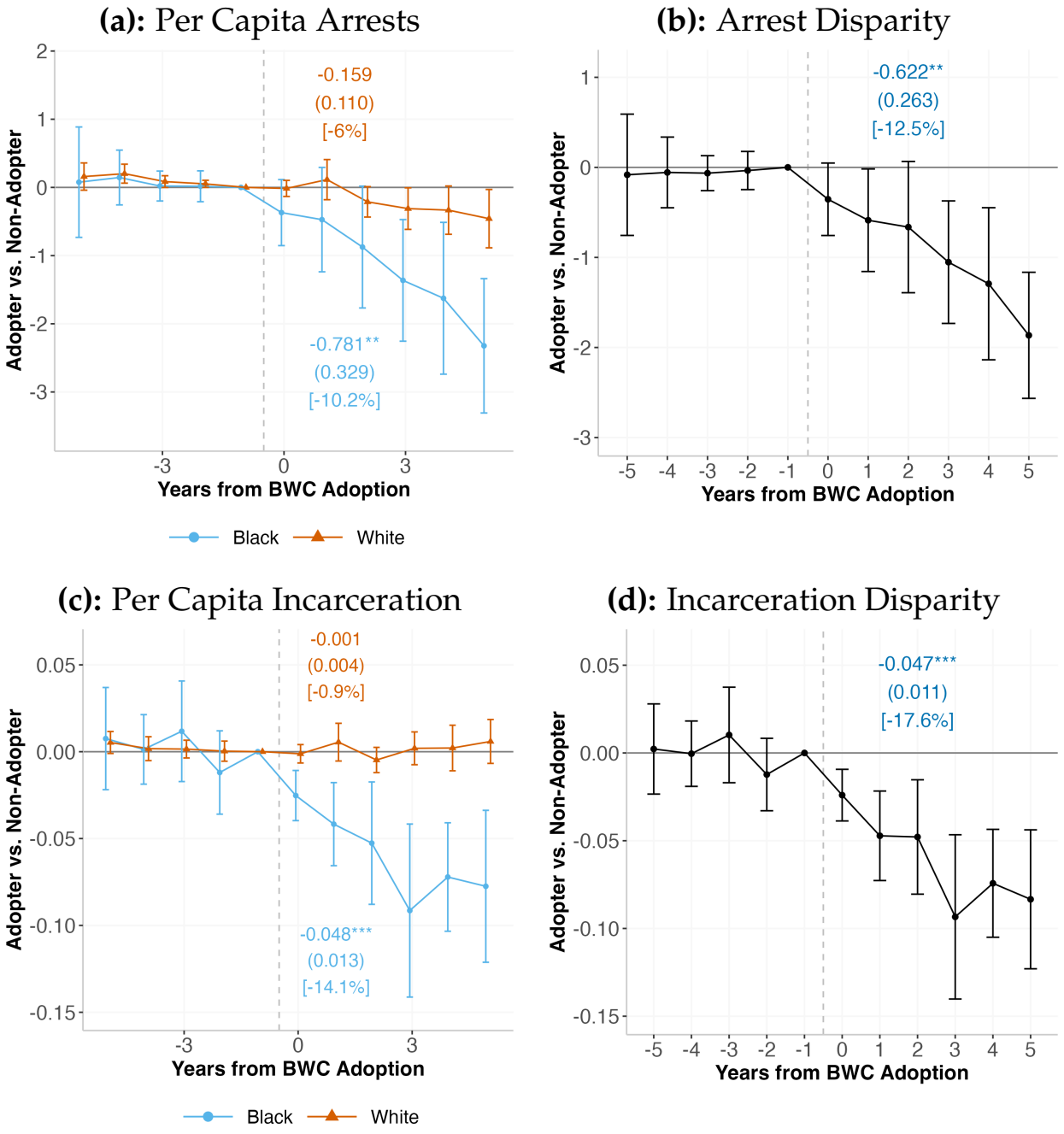
A. Online Appendix

Figure A.1: Potential Spillovers of BWCs onto Neighboring Counties



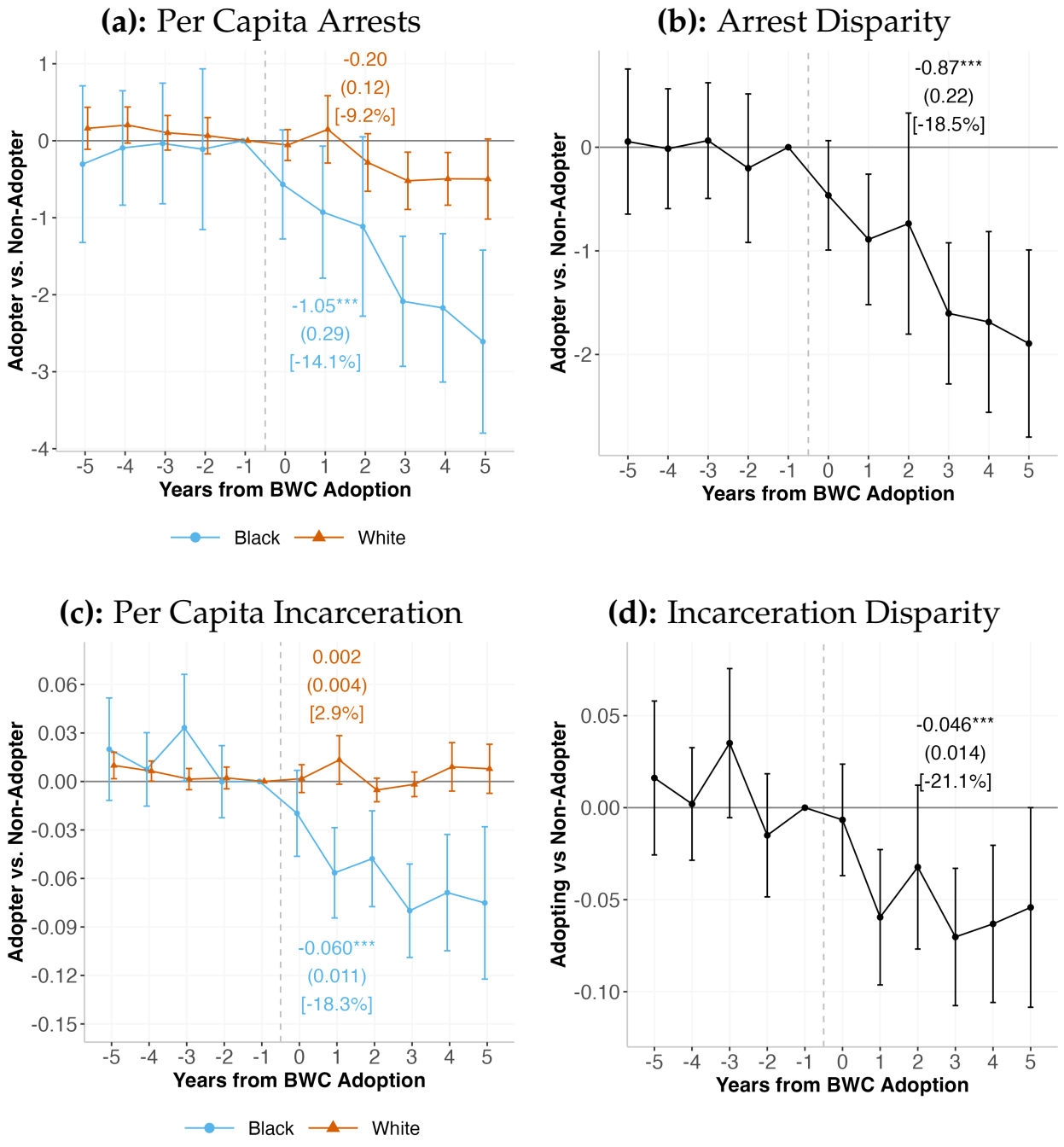
Notes: This figure illustrates potential spillover effects of BWCs on neighboring counties. Panel (a) shows the change in the per capita number of sworn officers after a county adopts BWCs. Panel (b) shows the change in the per capita number of sworn officers in counties that have not yet adopted BWCs but that neighbor a county that adopts BWCs. Panels (c) and (d) show BWCs' impacts on per capita arrest and incarceration rates excluding these neighboring, potential spillover counties. All point estimates come from Equation 4, and the error bars represent 95% confidence intervals with standard errors clustered by county. The annotated coefficients reflect pooled estimates from equation 2, comparing the five years post-BWC adoption to the five years prior to BWC adoption. The bracketed numbers show the coefficients as percentages of the dependent mean. Annual data on the number of sworn officers in each law enforcement agency come from the North Carolina State Bureau of Investigation (NCSBI). *p<0.1; **p<0.05; ***p<0.01.

Figure A.2: Robustness to Excluding Counties with New Elected Sheriffs



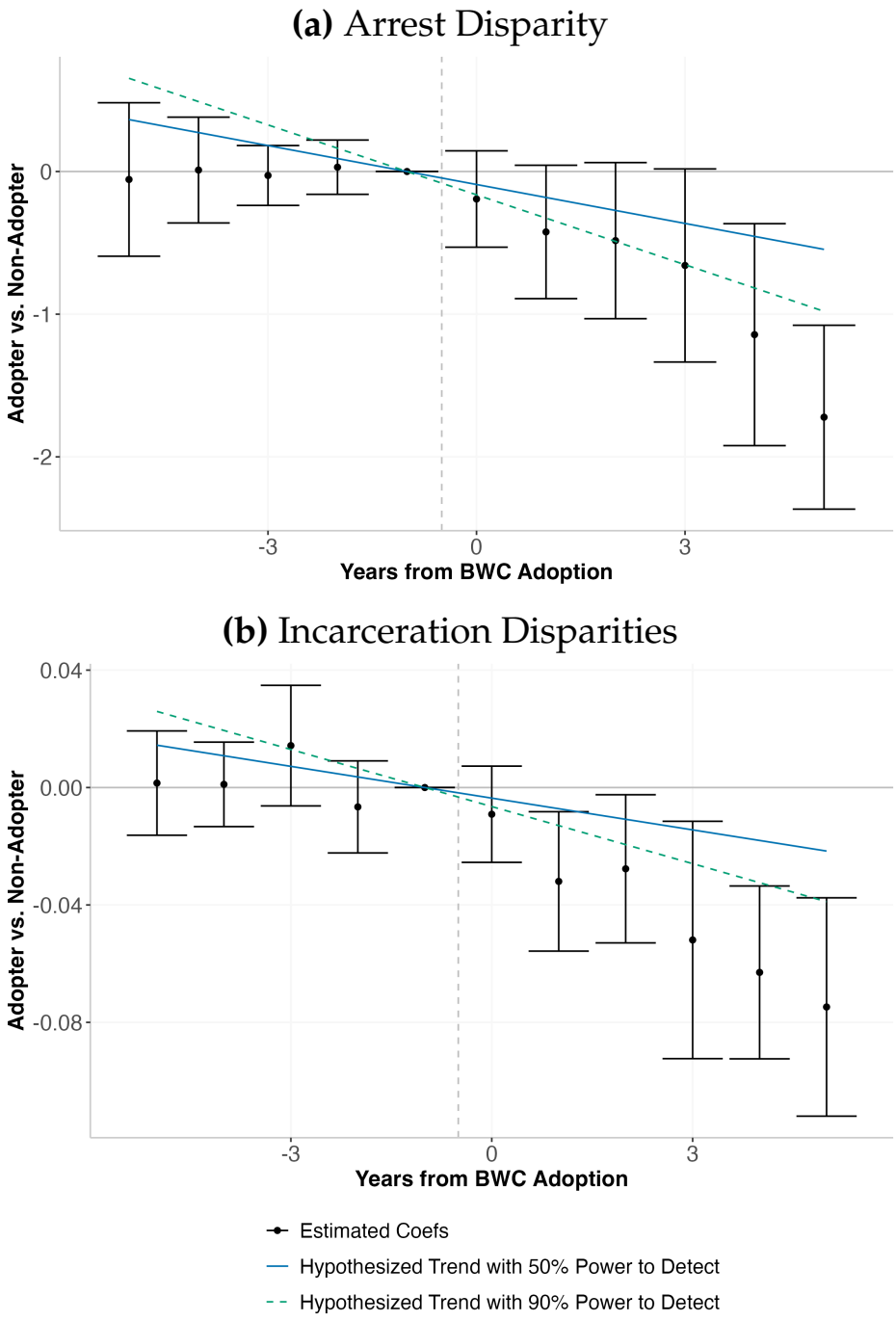
Notes: This figure illustrates the changes in arrests and incarceration rates in NC counties that adopt body-worn cameras (BWCs) relative to counties that have not yet adopted BWCs (or never do) — but restricts to counties whose sheriffs do not change in the election preceding the adoption of BWCs. For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

Figure A.3: Robustness to Limiting to Federally Funded BWCs



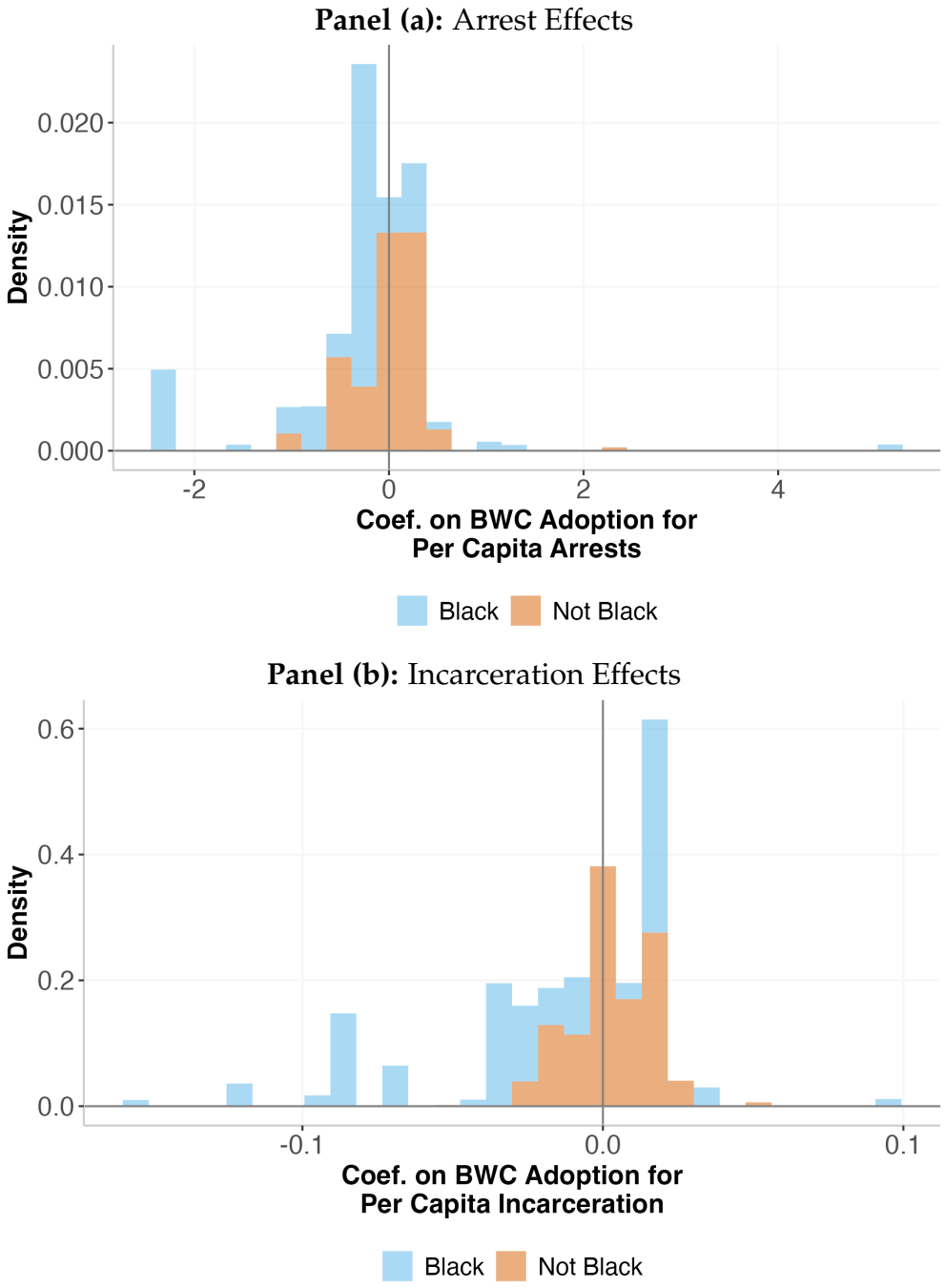
Notes: This figure illustrates the changes in arrests and incarceration rates in North Carolina counties that adopt BWCs using federal grants relative to control counties that have not yet adopted BWCs (or never do). Nine of the twenty-six BWC adoption events were federally funded. Panel (a) plots per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

Figure A.4: Roth (2022) Robustness to Differential Trends



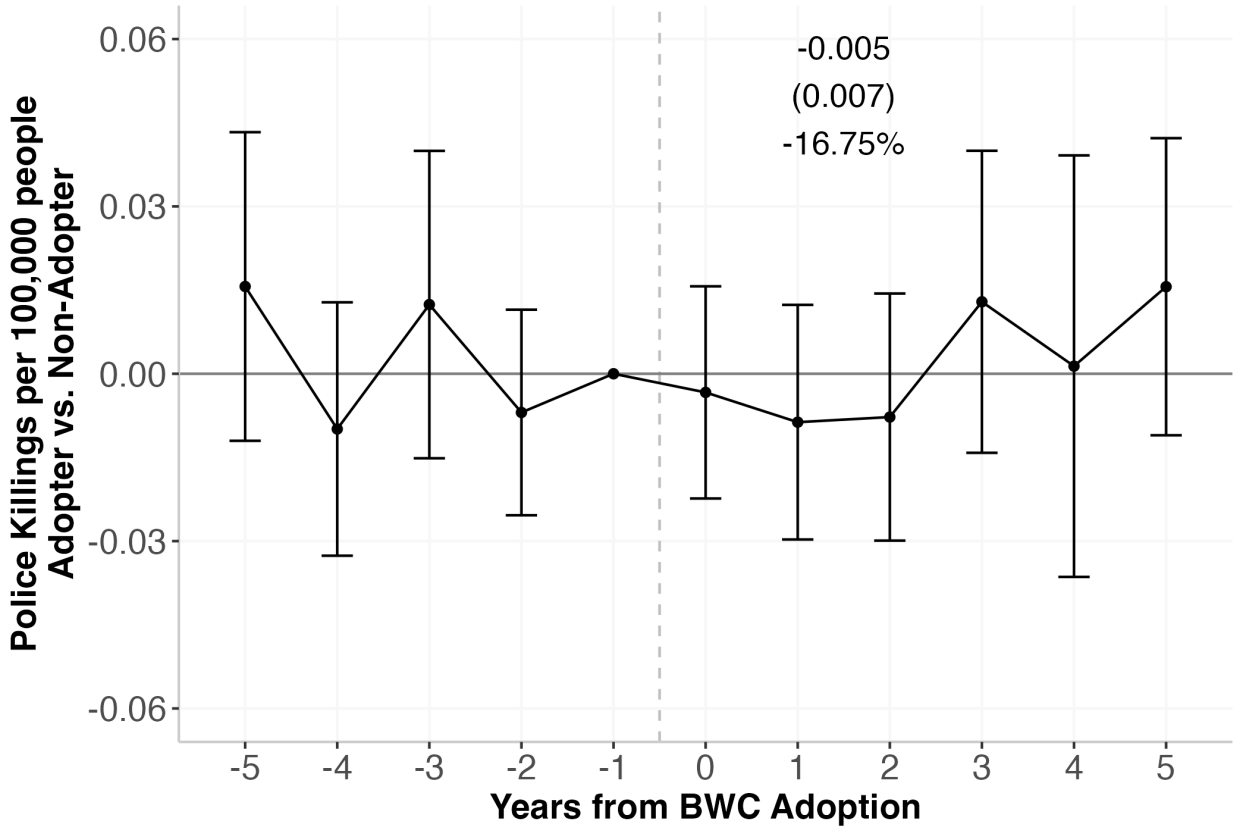
Notes: This figure investigates the robustness of the results to plausible pre-existing trends in adopting versus non-adopting counties. The tests proposed by Roth (2022) juxtaposes the estimated event study coefficients (in black) with worst-case pre-existing trends that we would have 50% power to detect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

Figure A.5: Distribution of Changes in Per Capita Arrest Incarceration Rates around Body-Worn Camera Adoption



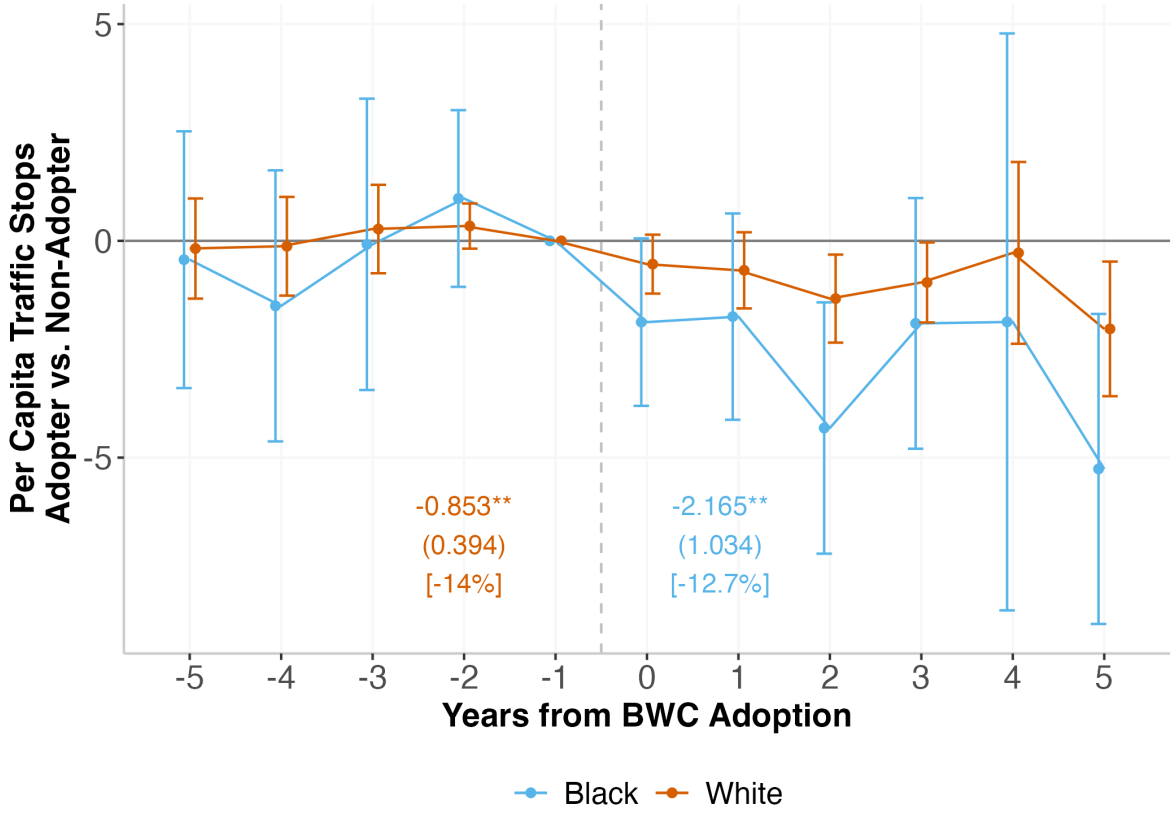
Notes: This figure shows the distribution of estimated effects of BWC adoption on per capita arrest and incarceration rates for Black and non-Black residents. Each estimate comes from Equation 2 and compares one of the treated, adopting counties to all the not-yet adopters.

Figure A.6: Changes in Police Killings Around BWC Adoption in North Carolina



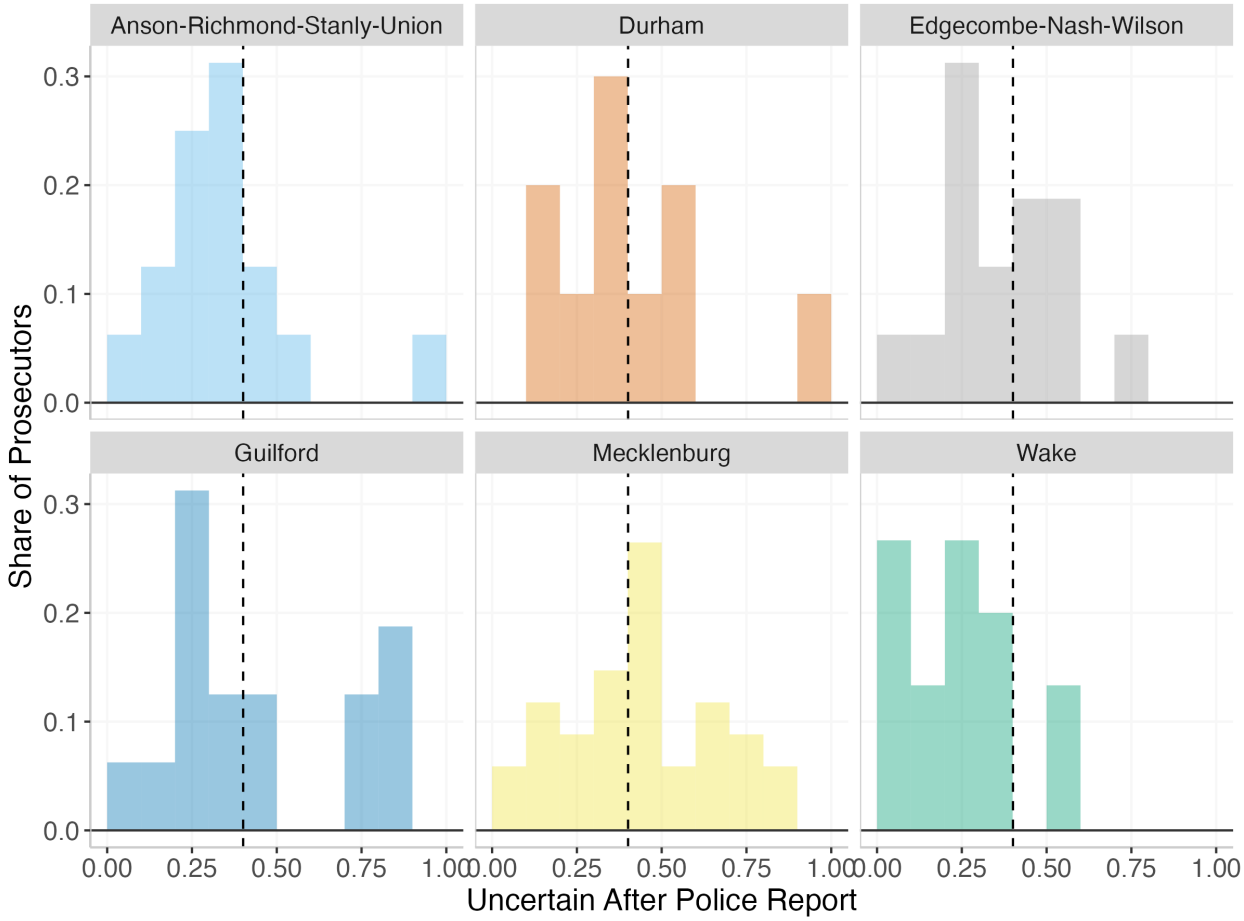
Notes: This figure illustrates the impact of body-worn cameras (BWCs) on police killings of civilians in North Carolinia. The error bars represent 95% confidence intervals with standard errors clustered by county. The annotated coefficients reflect pooled estimates from Equation 2, comparing the five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

Figure A.7: Body-Worn Cameras' Effects on Traffic Stops Conducted By Police Officers



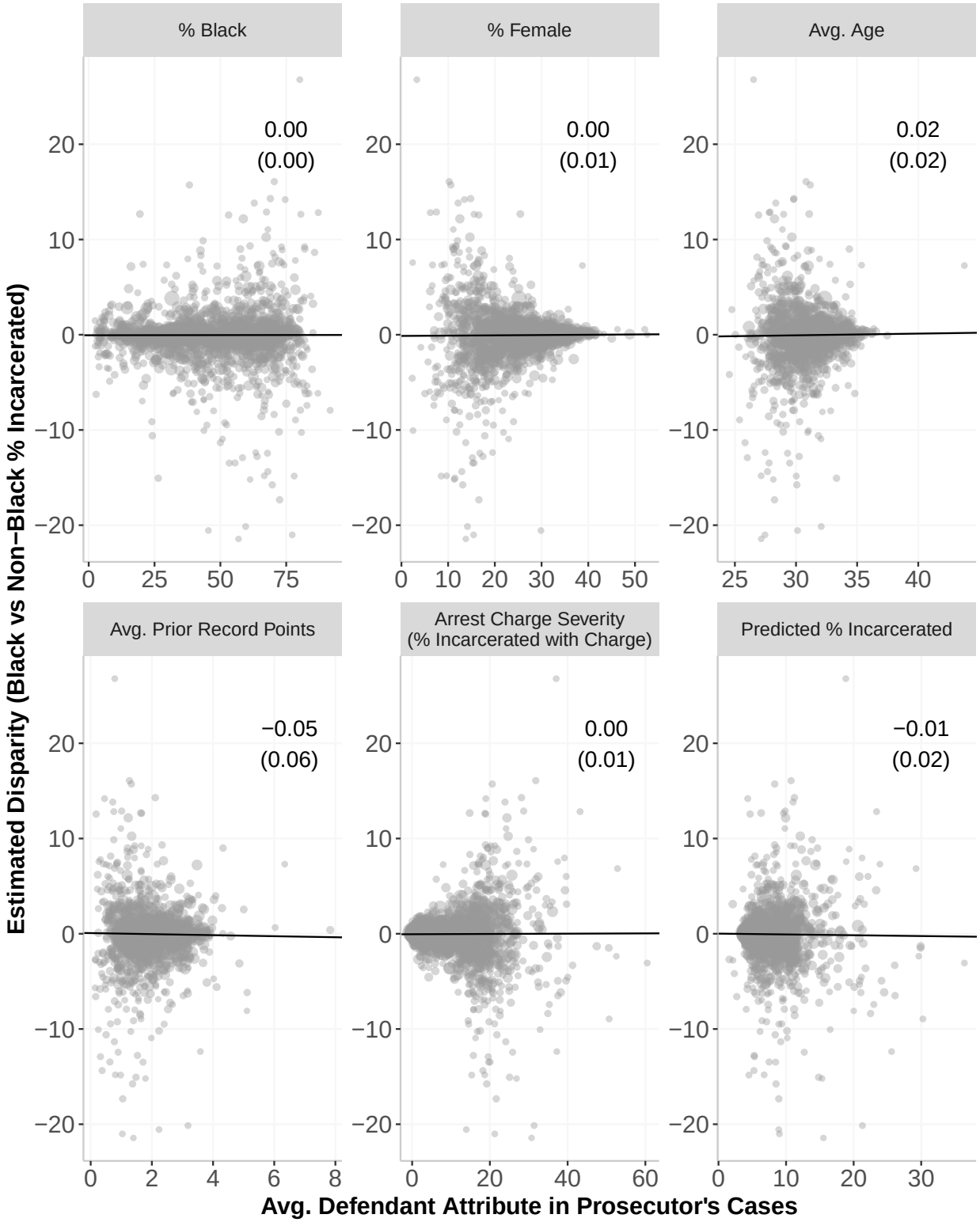
Notes: This figure illustrates the impact of body-worn cameras (BWCs) on per capita traffic stops conducted by police officers for Black and white North Carolinians. Each plot shows estimates from Equation 4. The error bars represent 95% confidence intervals with standard errors clustered by county. The annotated coefficients reflect pooled estimates from Equation 2, comparing the five years post-BWC adoption to the five years prior to BWC adoption. The bracketed numbers show the coefficients as percentages of the dependent mean. We use traffic stop data from NCSBI (2024); Baumgartner (2024) to sum up the total number of people of each race who are stopped in their car in a given year and county. We then divide this number by Census population counts by race (U.S. Census Bureau, a) in that year and county. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Figure A.8: Uncertainty Varies Widely Within Office



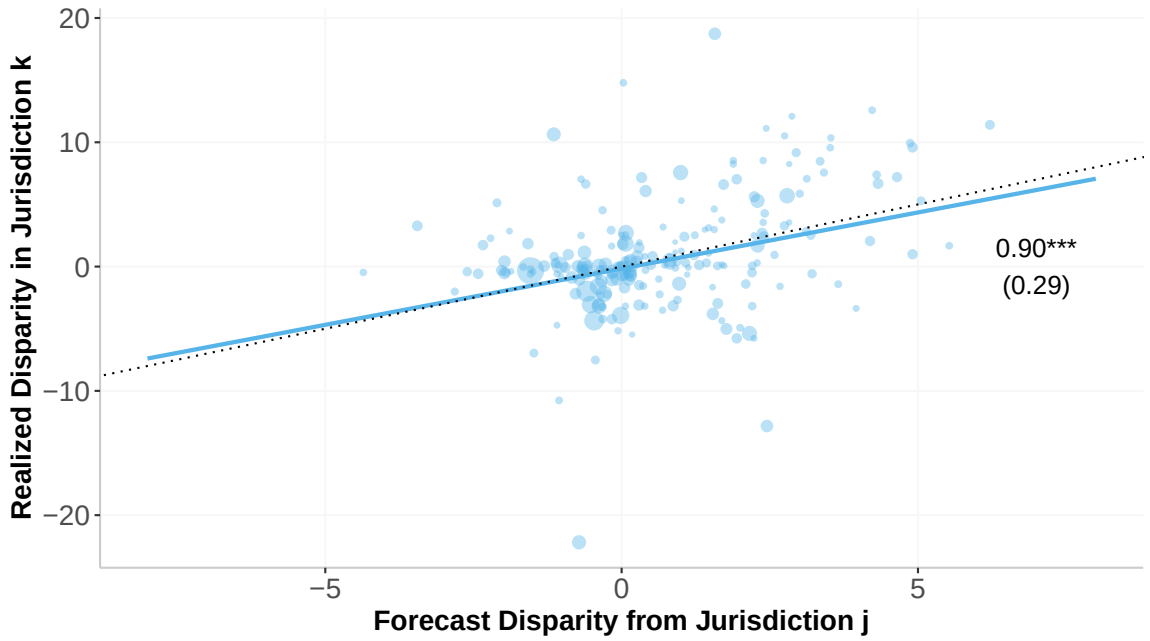
Notes: This figure shows the empirical distribution of prosecutors' uncertainty about the accuracy of police reports across the six largest participating offices in the survey. The adjusted R^2 indicates office explains 8% of variation in uncertainty.

Figure A.9: Balance in Case Characteristics Across Prosecutors with Different Disparate Impacts



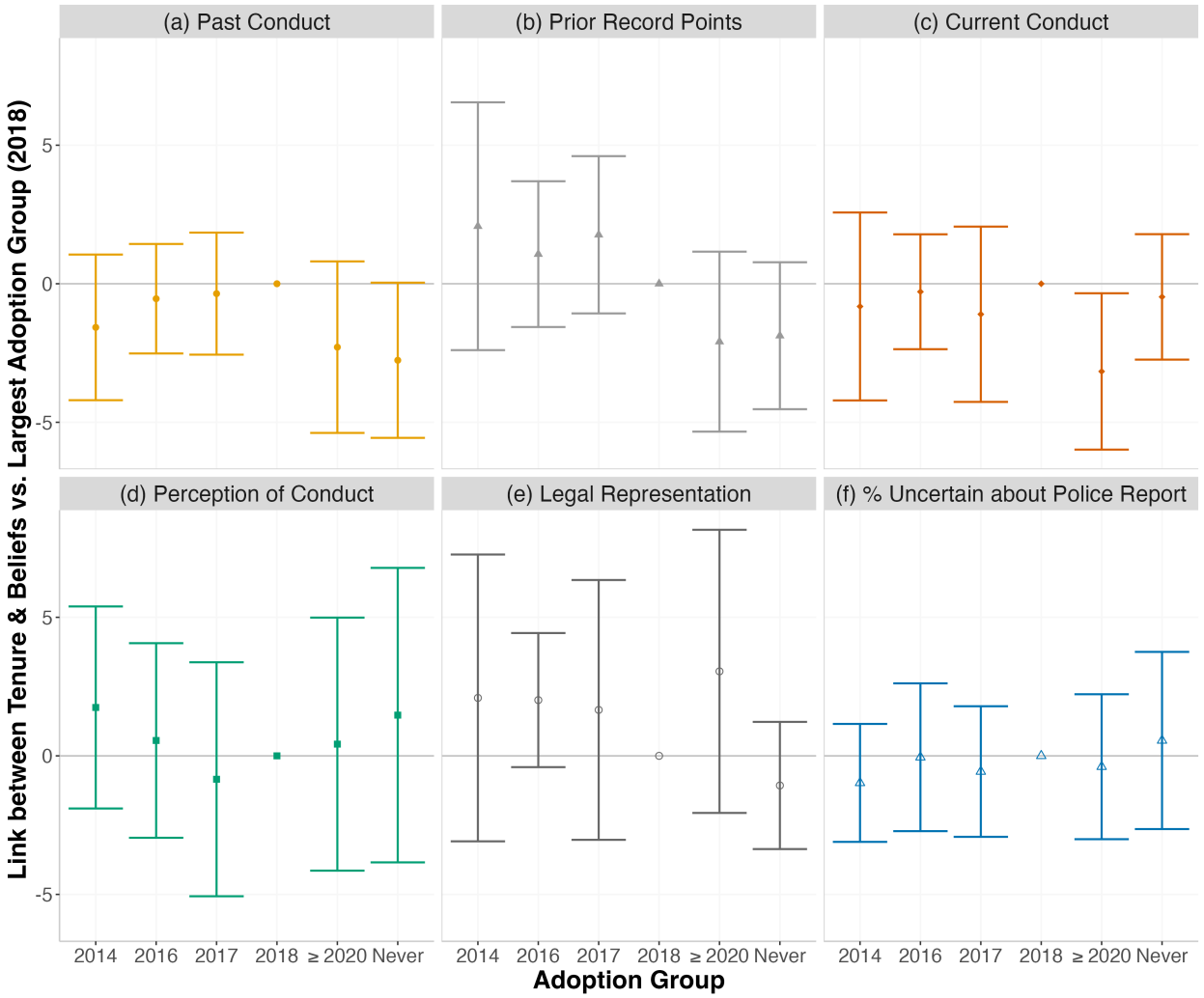
Notes: This figure illustrates the balance in case characteristics across prosecutors with different estimated impacts on racial disparities in incarceration rates. The x-axis plots an attribute of a prosecutor's cases, such as the percent of her cases with a Black defendant or a female defendant. The 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.10: Mover Validation of Prosecutors' Disparate Impacts



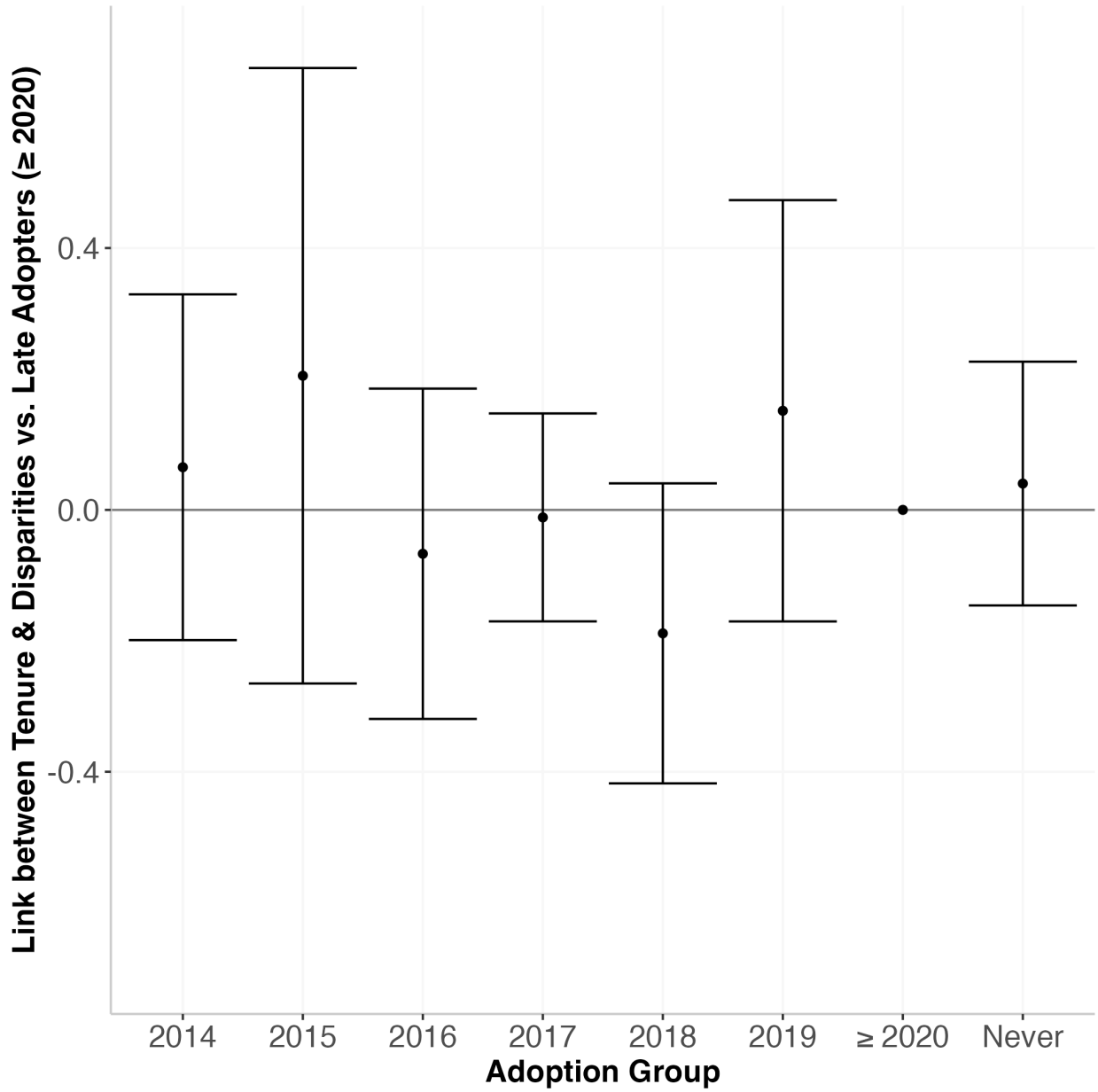
Notes: This figure illustrates the mover design to validate prosecutors' estimated effects on racial disparities in incarceration rates, after residualizing by unit and sentencing guidelines recommendations. The figure tests the validity of the design, using the 87 prosecutors who moved across judicial 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.11: 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 8(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 6(a) for the question interface).

Figure A.12: 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 of the NC Court Records, 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
% Police-Initiated Arrests	1.50	1.54	1.82	1.37	1.47	1.15
Black	3.29	3.34	3.52	3.29	3.21	2.94
White	1.01	1.00	1.13	0.85	1.10	0.84
% Traffic Stops	8.45	8.46	10.63	8.05	6.01	5.22
Black	15.68	15.70	18.01	16.15	11.80	10.54
White	6.04	6.04	7.46	5.64	4.60	4.31
% 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
Outcomes in the Court System						
% Incarcerated (> 6mo)	2.9	2.9	2.7	3.2	2.7	3.0
% Incarcerated | Felony Arrest	13.9	14.1	13.3	15.8	12.9	12.7
Incarceration/ Jail (Months)	1.4	1.4	1.3	1.5	1.4	1.4
% Felony Conviction	7.6	7.3	6.8	7.6	7.7	8.5
% Conviction	40.1	39.6	35.8	42.7	40.8	38.9
% Trial	0.3	0.3	0.3	0.3	0.3	0.2
Defendant Characteristics						
% Black Defendant	44.2	47.5	54.5	49.2	34.5	34.5
% Female Defendant	28.8	28.9	28.1	28.9	30.3	29.4
Defendant Age (in Years)	31.1	30.9	30.3	31.2	31.1	31.2
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
% Elected Prosecutor Democrat	58.5	61.5	52.3	76.1	59.5	37.4
Prosecutor Demographics						
% Registered Democrat	38.3	40.0	41.3	40.7	40.1	26.1
% 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
Prosecutor Tenure (in Years)	9.3	9.2	9.4	8.9	9.2	9.1
# 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. Per capita incarceration and arrest rates are constructed from the court records and Census population counts by race (U.S. Census Bureau, a). Traffic stop data comes from the NC State Bureau of Investigation (NCSBI, 2024; Baumgartner, 2024)

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

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

Table A.3: 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 Without BWC x Black x Share Uncertain		-1.80^{**}	-1.20^{**}
		(0.69)	(0.51)
Police-Initiated x With BWC x Black x Share Uncertain			-0.07
			(0.75)
Police-Initiated x Black x Share Uncertain		0.07	
		(0.43)	
Without BWC x Black x Share Uncertain	-1.15^{*}		-0.53
	(0.60)		(0.59)
With BWC x Black x Share Uncertain			0.14
			(0.53)
Black x Share Uncertain about Police Report	0.13		
	(0.35)		
Other Arrest x Without BWC x Black x Share Uncertain		-0.67	
		(0.72)	
Other Arrest x Black x Share Uncertain		0.14	
		(0.53)	
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' uncertainty about the reliability of police reports and the racial disparities in incarceration rates 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 and other arrests, where police-initiated offenses include drug, public order, and weapon possession offenses. Column 3 tests heterogeneity for police-initiated arrests versus other arrests separately for places with BWCs and without BWCs. Each column includes prosecutor fixed effects and our preferred controls for units within offices and the inputs to the sentencing guidelines (the offense class of the most severe arrest charge and criminal record prior points). Standard errors are clustered by prosecutor. *p<0.1; **p<0.05; ***p<0.01.

Table A.4: 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 8, 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 8(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 6(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. 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 percent are registered Republicans. Similarly, only two prosecutors (1.5 percent) 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 percent of cases and 93 percent of cases with a named prosecutor.