Unequal Treatment Under the Law? Consequences of Body-worn Cameras on the Court System

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Abstract: In less than a decade, body-worn cameras rose from rarity to standard amongst local law enforcement in the U.S. as agencies sought to enhance trust, transparency, and accountability of officers. However, this policing tool also generates large quantities of a new source of data for criminal courts: footage of criminal defendants. This data can provide evidence pertinent to a criminal case, but at a cost of attorney time. Using rich case data from Virginia state courts from 2006-2020 and a new body-worn camera data set I investigate whether local law enforcement adoption of body-worn cameras changes court filings, criminal case dispositions, time to disposition, and other case outcomes. I find evidence that body-worn cameras affect interactions between police and members of the public, but that these effects are restricted to a small subset of cases. Once cases enter the courts, I find that in the aggregate, contrary to expectations, both case processes and resolutions are unresponsive to the influx of data generated by body-worn camera footage.
1 Introduction

Defining and implementing appropriate and effective policing remains one of the most salient political issues of the past decade. In the midst of sometimes contentious debate over policing policies, one policy with broad public support is expanding or mandating the use of body-worn cameras (BWCs) by law enforcement.\footnote{One recent poll shows 85 percent of Republicans and 94 percent of Democrats favor body-worn camera mandates (Kull, 2020).}

Although now commonplace, body-worn cameras are a recent technological advancement for law enforcement in the United States. Only a decade ago, in 2010, an estimated less than 5 percent of law enforcement agencies used body-worn cameras (LEMAS-BWCS, 2016). This changed when a police officer shot and killed eighteen-year-old Michael Brown in Ferguson, Missouri in August 2014. In the aftermath of the shooting, witnesses provided substantively conflicting reports of the event (Buchanan et al., 2015), and a grand jury decided not to indict the officer involved. This case spurred nationwide protests and highlighted tensions and distrust between the public and police in Ferguson and elsewhere. Under pressure to increase transparency, accountability, and public trust, Ferguson police soon outfitted officers with body-worn cameras (BBC News 2014).

In the following months and years, body-worn cameras swept across the country. To facilitate their expansion, in 2015 the Department of Justice announced a $75 million national grant program intended to fund 50,000 cameras over a three-year period (Department of Justice, 2015). Between 2013 and late 2016 the share of departments using body-worn cameras rose from approximately 12 percent to nearly half of general-purpose law enforcement agencies in the U.S. (LEMAS-BWCS, 2016). Pressure to increase police use of body-worn cameras continues, and between 2020 and mid-2021, six states mandated use of body-worn cameras for law enforcement (NCSL, 2021).

While advocates for body-worn cameras intend the technology to increase transparency and improve safety in police interactions, other court actors report unintended consequences of body-worn camera footage on their operations. Perhaps the most vocal of these actors are the attorneys representing low-income defendants in criminal courts. These lawyer-advocates point out that when law enforcement adopts body-worn cameras other actors in the criminal justice system receive an influx of recorded data that may be relevant to criminal cases. However, the evidentiary value of this body-worn camera footage data comes at a cost of already scarce attorney time. It is this tension between the additional labor demands of cases with body-worn camera footage and attorney time constraints that led the Executive Director of the Virginia Indigent Defense Commission to write of public defenders: “...we have significant concerns that our attorneys will not be able to continue to meet their ethical and professional responsibilities” (Compensation Board, 2018).

Despite extensive anecdotal evidence on the influence of body-worn camera footage on the courts and
court actors, there is minimal empirical research on these downstream effects. In this paper I extend the base of research on body-worn cameras as a policing tool to incorporate their aggregate effects on courts and defendant outcomes. Specifically, I use the timing of body-worn camera adoption by local law enforcement agencies across the Commonwealth of Virginia to study changes in criminal case outcomes and court processes after local law enforcement begin using body-worn cameras. I enumerate three primary avenues through which body-worn cameras can affect criminal courts, court actors, and criminal cases, which include: civilization (behavioral) effects, evidentiary effects, and attorney time use effects. The first of these – civilization effects – refers to the first order policing responses to body-worn camera adoption. My contributions build upon the existing work by criminologists including Ariel et al. (2015) and Yokum et al. (2017), as well as recent work in economics by Kim (2020). More broadly, by exploring civilization effects I contribute to literatures on police responses to oversight (Ba and Rivera, 2019) and criminal responses to surveillance (Gómez, 2021; Piza et al., 2019), and criminal deterrence more broadly. While previous body-worn camera studies often focus on changes in police use-of-force – an important but uncommon outcome – I check for civilization effects in more common events by measuring changes in the frequency and composition of charges that are filed in criminal courts.

Much less is known about if and how body-worn cameras affect court outcomes and processes once charges are filed. Two local impact evaluations – one in Washington D.C. and one in Phoenix– provide the best, but contradicting, evidence at this point. Yokum et al. (2017) do not find effects of body-worn cameras on case outcomes in Washington D.C., whereas Katz et al. (2014) highlighted prosecutorial changes that coincided with body-worn camera adoption. Additionally, Çubukçu et al. (2021) recently studied the effects of body-worn cameras on a different type of "court": citizen complaints against police, with results suggesting that in this type of court body-worn camera footage provides influential evidentiary benefits.

Although existing survey data, like that from the Law Enforcement Management and Administrative Statistics- Body-worn Camera Supplement (LEMAS-BWCS, 2016) can provide insights into body-worn camera adoption trends, these data are not comprehensive. Further, the typical body-worn camera evaluation is based on a single adopting agency. Together, these lead to a high representation of cities in body-worn camera evaluations and minimal evidence from small and mid-sized localities. To analyze effects of body-worn cameras on a range of courts, I collected a new body-worn camera adoption data set with the broadest coverage of Virginia law enforcement agencies to date, containing adoption and adoption timing information for all of the major law enforcement agencies in 90 percent of Virginia court jurisdictions. I also make use of a second data set containing the near-universe of criminal court cases in Virginia. I use this charge-level data to create a court-level panel, covering 102 Virginia circuit courts and 107 Virginia district courts. To my knowledge I am the first to use these case data in an academic research paper.

An emerging econometric literature demonstrates the shortcomings of the traditional difference-in-differences approach...
estimator under staggered treatment and in the presence of heterogeneous treatment effects over time. To
test for the effects of BWCs on case filings, processes, and outcomes using this data, I discuss and implement
an alternative imputation estimator proposed by Borusyak, Jaravel, and Spiess (2021). At this point, the
extent to which the shortcomings with the traditional estimator have affected accepted results within eco-
nomics is unknown; by including results using this new estimator and the traditional two-way fixed effects
estimator, I additionally contribute a case study to this growing literature.

The paper proceeds as follows. In Section 2 I provide context for this paper—context around both the
institutional details pertinent to Virginia courts and body-worn camera adoption, and an economic context
for understanding the consequences of court outcomes. In Section 3 I present a conceptual framework for
the three primary channels through which body-worn cameras may affect court cases: behavioral effects,
evidentiary effects, and attorney time use effects. Section 4 contains a description of the court and body-
worn camera adoption data that I use for my court-level analyses; Section 5 outlines the empirical strategies
that I use to analyze these data. Here I include estimates of the effects of body-worn cameras on policing as
measured through case filings, and estimates of the overall effects of locality body-worn camera adoption on
defendant outcomes. Despite pronounced practitioner reports of widespread effects of body-worn cameras
on criminal cases, I find no systematic effects of body-worn cameras on case processes or resolutions. Within
Section 6 I dissect this null result further and test for heterogeneity in treatment effects for cases more and
less likely to have body-worn camera footage. Because a common pool of attorneys litigates cases with and
without footage within a court, time use effects are expected to spill over into a broad base of cases, whereas
evidentiary effects are restricted to only cases expected to have footage. Finding no significant differences
across the two classifications of cases, I dismiss an "offsetting effects" hypothesis in which the three channels
for body-worn camera effects cancel in the aggregate. The national push for body-worn camera programs was
motivated by observed racial disparities in policing. For this reason, in this section I also test for evidence
of differential effects of body-worn cameras for black and non-black defendants, finding no aggregate effects
on case filings or outcomes. Section 7 concludes.

2 Background

2.1 Body-worn cameras

BWCs are personal video recording devices typically affixed to on-duty law enforcement officers’ clothing,
equipment, or accessories. Nationally, this technology became a commonplace tool for U.S. law enforcement
agencies in the latter half of the 2010s. A 2016 national survey of U.S. law enforcement agencies, the LEMAS-
BWCS, documented the rapid rise of this technology. This survey generated responses from nearly 4,000
law enforcement agencies and included comprehensive questions about body-worn camera adoption status,
expectations for the technology, and policies concerning body-worn camera use. The LEMAS-BWCS data shows broad adoption of body-worn cameras by 2016, but also high intentions to begin using the technology amongst agencies that had not yet adopted. Thirty-one percent of non-adopting agencies reported that they were likely or very likely to consider acquiring body-worn cameras in the next year. Even agencies that did not imminently intend to adopt body-worn cameras nonetheless reported high rates of officer and community support for the technology.

Within Virginia, respondents to the LEMAS-BWCS demonstrated similar adoption trends to the U.S. overall. Figure 1 shows 62% of the 85 responding Virginia law enforcement agencies adopted body-worn cameras by the time of the survey and adoptions in both Virginia and the U.S. peaked in 2015. Because the LEMAS-BWCS only includes data from a subset of law enforcement agencies, and no adoption information after 2016, I collected an updated and expanded body-worn camera adoption data set for Virginia agencies. I describe these data in detail in section 4.1, and Figure 2 shows these new data confirm the adoption trends evident in the LEMAS-BWCS. The pace of adoptions tapered after the 2015 peak but Virginia departments continued to routinely adopt body-worn cameras through 2018. Not only did the number of jurisdictions using body-worn cameras increase rapidly between 2014 and 2018, but Figure 3 shows the size of the population in Virginia living in a court jurisdiction where body-worn cameras were used also increased rapidly during this time.

The LEMAS-BWCS also helps to answer the questions of why and how the technology was implemented. Notably, although body-worn camera footage can be used in criminal courts, respondents indicated that
effects on the courts were not a primary consideration when evaluating the appropriateness of body-worn cameras for their departments. Instead, agencies emphasized expected benefits to policing including improvements in officer safety (21 percent), accountability (19 percent), and a reduction in/faster resolution of citizen complaints (15 percent) as the primary reasons they adopted body-worn cameras. Yet while agencies adopted body-worn cameras to affect policing rather than the courts, they did expect body-worn cameras to affect court cases as well; while less than 10 percent of agencies cited improvements to evidence quality (9.5 percent) or making cases more prosecutable (7.6 percent) as the primary reason they adopted body-worn cameras, the vast majority (78.8 and 69.8 percent, respectively) cited these as contributing factors. Those that did not obtain body-worn cameras cited the high costs to obtain and maintain the cameras, their footage, and related tasks – such costs were cited by many adopting agencies as an obstacle as well.

Once agencies adopt body-worn cameras they also must decide how to use the technology. This point is of particular importance as we consider the effects of body-worn camera footage on the courts; if officers wear body-worn cameras but rarely record or preserve the videos then body-worn cameras would be unlikely to affect criminal cases on a large scale. In practice, because officers typically must manually activate the cameras at the start of an interaction, adopting agencies almost always have some form of formal policy that outlines expectations for when body-worn cameras must be turned on. Of those agencies that set requirements, almost all (93 percent) required that the cameras be used during traffic stops and nearly 85 percent required officers to turn on body-worn cameras when executing arrest or search warrants, deploying

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2Percentages exclude respondents whose agency’s primary purpose was to conduct a pilot program.
firearms, and initiating contact with members of the public. Recordings typically must be preserved for between 1 month and 1 year, but in the event the footage is pertinent to an ongoing matter – such as a use of force incident, citizen complaint, or if used as evidence in a legal proceeding– may be retained longer.

2.2 Virginia Courts and Court Actors

For a member of the public, an interaction with a police officer carries the risk– small or large– of arrest or citation, and police interactions are common. The Bureau of Justice Statistics estimates that in 2018 over 60 million people, or about 24 percent of the U.S. population, had some contact with the police (Harrell and Davis, 2020). Typically these contacts do not result in criminal charges and, more often than not, are initiated by residents rather than officers. Nonetheless, in 2013 there were nearly 700,000 felony or misdemeanor filings in Virginia State General District Courts (Office of the Executive Secretary, 2014), and 190,000 in Virginia State Circuit Courts (Office of the Executive Secretary, 2014), the two primary venues for criminal litigation in Virginia. These courts largely share geographic jurisdictions, with approximately one circuit court and one district court in each county or independent city across the state.\(^3\) However, they differ in the scope of the cases they hear: district courts hold jurisdiction over misdemeanor cases whereas the circuit courts hear felonies.\(^4\) Oftentimes geographic court boundaries contain multiple law enforcement

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\(^3\)In a few places, multiple district courts operate within a single circuit court jurisdiction

\(^4\)Misdemeanors and felonies differ in the severity of the crime and the severity of the punishments if convicted: while a defendant can be sentenced to life in prison for a severe felony, the most severe misdemeanors carry a 12 month sentence.
jurisdictions. For example, both a county sheriff and a town police department may operate within a single county. Thus, courts can receive cases from multiple law enforcement agencies.

Some charges proceed directly from the policing interaction to the district court if the police officer issues the defendant a Virginia Uniform Summons. Such is the case for many misdemeanors, and in these cases the defendant is not held in custody while awaiting court hearings. In the remaining cases, a local magistrate serves as an intermediate step between law enforcement arresting an individual and the case reaching the courts. Magistrates review sworn statements from a complainant (such as an arresting officer) to assess whether there is probable cause to proceed with a criminal charge. This standard of probable cause is much weaker than a standard to convict— the Virginia Magistrate Manual at one point describes this standard as that the magistrate be able to ascertain that "the charges are not capricious and are sufficiently supported to justify bringing into play the further steps of the criminal process" (Department of Magistrate Services, 2021).

Once a defendant is charged with a crime their outcomes can be influenced by three court actors: a judge, prosecutor, and defense attorney.5 I provide a basic case road map outlining the entities involved in various states of criminal litigation in Table 1. Broadly, the court actors can influence outcomes for the defendant ranging from the final set of charges to be ruled on in court to the outcomes of those charges, their sentencing, and even the pace at which the case is resolved. More specifically, prosecutors can alter, drop, or add charges to the case against the defendant; judges dismiss or rule on charges and determine sentences6; and both prosecuting and defense attorneys lobby for preferred dispositions and sentencing.

By design, both judges and prosecutors are always publicly funded government employees. In practice, defense attorneys often are too. A system of publicly funded attorneys ("indigent defenders") represent low-income defendants in order to fulfill the Constitutional right to counsel.7 In Virginia, these attorneys are either a) public defenders—salaried attorneys working in a state-funded law firm that represents indigent clients or b) assigned counsel—private attorneys compensated to represent indigent clients on a case-by-case basis.

The Virginia indigent defense system is organized such that full-time indigent defenders are likely to face binding time constraints. A commonly cited upper bound on the number of cases an attorney should work in a given year to allow time to fulfill their professional responsibilities for each case is 150 felonies or 400 misdemeanors (American Bar Association, 2009), however in FY 07/08, before body-worn cameras became

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5Statutorily, all three actors are involved in felony cases. Practically, all three actors are involved in misdemeanor cases as well, however for low-level misdemeanors and infractions a prosecutor and/or defense attorney may not be involved in the case.

6This is a simplification of the full role of judges: judges also rule on a variety of motions presented to the court and in some cases oversee jury trials wherein a jury rules on a case.

7Data on the precise share of defendants using indigent defenders vs. private counsel are hard to come by and indigency thresholds vary across states. However, estimates routinely place the share of indigent defendants in excess of 70% of state-court defendants (Harlowe, 2000) (Butcher et al., 2017).
widespread, public defenders in Virginia managed on average 320 cases per attorney per year (Kleiman and Lee, 2010).\textsuperscript{8} While assigned counsel are employed on a case-by-case basis, they too face time constraints: compensation is capped per charge such that an attorney representing common felonies in the circuit court would need to represent clients against 138 charges to reach the 10th percentile of attorney earnings nationally. If representing clients against misdemeanor charges in district courts, they would need to defend over 500 charges to reach this level of pay (Bureau of Labor Statistics 2021).\textsuperscript{9} This provides an additional avenue for body-worn cameras to affect court cases: by increasing the time intensity of attorney caseloads, which I describe in section 3.

<table>
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<th>Table 1: Case Roadmap</th>
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3 Literature and Conceptual Framework

Crime and the criminal justice system in the U.S. are both broad-reaching and multi-layered in their consequences. A rich literature spanning global contexts shows that the determinants of crime are similarly multifaceted. Crime determinants discussed within the academic literature range from inequality and other socioeconomic factors (Kelly, 2000; İmrohoroglu et al. 2000; Fajnzylber et al., 2002(a); Fajnzylber et al., 2002(b); Grogger, 1998; Buonanno & Montolio, 2008), to alcohol access (Heaton, 2012; Groönqvist & Niknami, 2014), incapacitation due to schooling, entertainment, and imprisonment (Dahl & DellaVigna, 2009; Jacob & Lefgren, 2003; Barbarino & Mastrobuoni, 2014), social networks (Damm & Dustman, 2014;)

\textsuperscript{8}I compare these caseloads to the standard in more depth in Appendix D.

\textsuperscript{9}There are some opportunities for fee waivers which would reduce this number however in Bollman, 2021 I calculate fee waivers to be rarely granted – given for only about 3.3% of charges.
Billings et al. 2019), family background or adverse childhood experiences (Doyle, 2008; Currie & Tekin, 2012; Eriksson et al., 2016) and more.

The resulting crimes are costly; victims of violent crime experience lower levels of mental well-being, as do non-victims with a higher probability of crime victimization measured through local crime rates (Cornaglia et al., 2014). Crime risks lower housing prices (Linden & Rockoff, 2008), and exposure to crime reduces educational attainment for youth (). Police can deter crime (Evans & Owens, 2007; Draca et al., 2011; Vollaard & Hamed, 2012; Chalfin & McCrary, 2018; Weisburd, 2021), but do so at substantial public cost: in 2018, states spent $119 billion on police (Urban Institute, 2021).

Even convicting criminal defendants comes with substantial and long-lasting consequences. Incarceration promotes public safety by incapacitating criminals, but does so at an annual average cost of over $30,000 per prison inmate each year across states (Mai and Subramanian, 2017). Incarceration and the peer networks formed within jails and prisons can spur additional criminal activity upon release as well as generate costs through uptake of public assistance programs (Bayer et al., 2009; Mueller-Smith, 2015). For defendants, incarceration and criminal records diminish economic self-sufficiency by posing barriers to formal employment (Agan and Starr, 2017; Dobbie et al., 2018), with stronger effects for defendants sentenced to longer incarcerations (Mueller-Smith, 2015).

Because body-worn camera programs introduce change to both policing and court processes, they have the potential to change the prevalence of criminal activities, conviction rates, or the sentencing terms if convicted. Body-worn cameras may act as a deterrent for criminal activity and police misconduct through civilization effects, altering the set of charges that reach the courts. Through distinct channels, body-worn cameras may affect case outcomes and court processes after cases enter the courts. Body-worn camera footage changes the set of information available to attorneys, judges, and juries (evidentiary effects) and can alter the intensity of attorney caseloads— in other words, change the amount of time required to litigate cases (attorney time use). In this section I detail each of these prospective channels, existing evidence of their roles, and the sometimes conflicting effects we may anticipate they will have on criminal court cases.

3.1 Behavioral/Civilization Effects

“That’s the beauty of these devices . . . everybody gets politer when the cameras are on.”

Norfolk Police Chief Michael Goldsmith

At least as far back as Becker’s influential crime model (1968), economists acknowledged that the “supply” of crimes should be inversely related to the likelihood a criminal is discovered and convicted. As an evidence-generating technology, body-worn cameras reduce some of the noise around allegations of criminal behavior

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\(^{10}\)Calculated using data available from 45 states
or professional misconduct – thereby increasing the likelihood, ceteris paribus, of conviction for offenses caught on camera. The most publicized instances of this occur with excessive force cases for police. For example, in one highly publicized case a jury convicted an officer of murder after body-worn camera footage contradicted the officer’s allegation that he shot into a car because it moved “aggressively” toward law enforcement (McCullough, 2018). However, offenses committed by members of the public that are caught on camera should also be easier to prosecute.

Because of this, it is possible that the mere presence of a body-worn camera is enough to alter court outcomes if police and members of the public know they are being recorded and thus adhere more closely to legal and social standards. At the most basic level, both police and members of the public should be less likely to engage in criminal behaviors in the presence of body-worn cameras. However, advocates and practitioners posit that these civilization effects are more comprehensive than simply disincentivizing criminal behavior and can foster milder interactions overall.

Established research into deterrence, criminal responses to surveillance, and police responses to oversight can be informative for predicting and understanding responses to body-worn cameras as evidence generators and accountability tools.

For defendants, numerous papers assess the effects of an alternate surveillance technology, stationary surveillance cameras called closed-circuit television (CCTV), on criminal behavior. Unlike body-worn cameras, CCTV cameras are affixed to a building or other fixed point from which they transmit video of whatever occurs within the camera’s frame. In contrast, body-worn cameras are mobile and record a constantly changing sight line intended to approximately reflect an officer’s field of vision. While CCTV cameras constantly transmit, often officers must activate their body-worn cameras. Like body-worn cameras, CCTV footage increases the likelihood that a criminal is caught and convicted if their offense is committed in view of a camera. Piza et al. (2019) conducted a meta-analysis of 76 CCTV studies within the criminology literature, finding an estimated 13 percent reduction in crime in CCTV areas compared to controls. However, their heterogeneity analysis within this paper suggested that the success of CCTV in reducing crime is context specific: while evaluations set in parking facilities showed strong evidence of crime reductions, cameras in housing complexes did not appear to affect crime rates. Beyond location, effect sizes across evaluations also varied by whether the CCTV was actively monitored. Monitoring can enable a real-time response to criminal activity and is a critical distinction between CCTV and body-worn cameras. Police, whether they use body-worn cameras or not, serve in the role of a monitor. If monitoring, rather than recording is the mechanism for CCTV crime reductions, then the crime reductions from CCTV are unlikely to be replicated with body-worn cameras. Gómez et al. (2021) provide new evidence on the role of monitoring using CCTV expansions in Medellín, Colombia that were not accompanied by expansions to monitoring capacities, finding that even without monitoring changes CCTV decreased the number of reported crimes and arrests.
For police, body-worn cameras enable additional oversight. The desired effects of police oversight include improved conduct amongst officers and sorting out low-quality officers from the ranks. A less desired theoretical outcome of heightened oversight is de-policing, or reduced interactions between police and members of the public. There is precedent for this response given economic theory (Prat, 2004), however while Ba and Rivera (2019) do find evidence of de-policing following oversight generated by public outcry, they do not find evidence for it when the oversight is generated within a policing organization.

Currently body-worn camera studies typically test for evidence of civilization effects in use of force and citizen complaint data. Within these studies, evidence for civilization effects is mixed. In an influential randomized controlled trial Ariel et al. (2015) found the rate of use-of-force incidents and officer complaints both declined for police assigned to use body-worn cameras, however an overlapping set of authors subsequently published a meta-analysis of 10 body-worn camera interventions that demonstrated no significant change in police use-of-force for adopters (Ariel et al., 2016). The following year Yokum et al. (2017) released results from a randomized controlled trial in Washington D.C. which showed no differences in either use of force or complaints between adopters and non-adopters. However, the interventions studied in these evaluations consisted of partial adoptances within single departments; it is possible that the estimates are attenuated due to spillovers into the interactions of non-BWC assigned police. For example, members of the public may be aware that police are using body-worn cameras but are unsure of whether the specific officers they interact with are using them. Additionally, officers may learn from their peer networks (Ouellett et al., 2019) – which do not necessarily directly coincide with their body-worn camera assignment groups. These concerns were also present in a non-randomized intervention which demonstrated a reduction in complaints against body-worn camera-wearing officers in Phoenix (Katz et al., 2014).

To bypass these limitations, Kim (2020) used a difference-in-differences strategy with a national sample of law enforcement agencies and found evidence that body-worn cameras do reduce police use-of-force. While this result suggests a civilizing effect on officers, he does not find any reductions in assaults where the victim was a police officer. Togeth

Together these findings suggest that officers – but not the public – are “civilized” by body-worn cameras and further may be indicative of null or limited changes in police use of discretion in charging.

While use of force is a salient and influential outcome to study, these events are relatively rare in policing. Complaints of excessive use of force are even more so: using data from Chicago police, Chalfin and Kaplan (2021) found that 84 percent of officers generated no use-of-force complaints over a 5-year period. If we consider that the “better behavior” caused by civilization effects more broadly reduces the likelihood that an interaction escalates either physically or verbally, then we can expect to find broader changes in the charges.

11The global meta-analysis of local body-worn camera impact evaluations (Ariel et al. 2016) actually showed higher rates of assaults on police after adoption.
that reach the courts. More deferential defendants and officers should reduce the frequency of charges of officer-oriented offenses such as resisting arrest. And officers, who have a degree of discretion in issuing citations and making arrests, may be less likely to overcharge criminal defendants – however, they also may be disincentivized from displaying leniency if they anticipate that their footage will be reviewed. These alterations could affect defendants on both the intensive and extensive margins – in other words, civilization effects may reduce the probability an individual is accused of a first offense or that they are charged with multiple offenses.

When Katz et al. (2014) conducted an impact evaluation for the Phoenix Police Department they tested aspects of this broader view of civilization effects. The authors first surveyed police about how they expected body-worn cameras to affect officers discretion and the frequency of contacts with the public. In both cases, before adoption respondents expected body-worn cameras to reduce discretion and contacts. However, these concerns lessened after body-worn camera adoption. While the authors acknowledge some shortcomings that limit the strength of causal claims within the study– including substantial officer turnover in the pre-adoption period – they find in practice adopting squads actually significantly increased their daily arrests and the frequency of resisting arrest charges was not significantly changed after body-worn camera adoption.\textsuperscript{12} However, further study is needed to validate these findings outside of the Phoenix context.

### 3.2 Evidentiary Effects

Secondly, body-worn camera recordings can provide evidentiary value in court proceedings, affecting how judges and juries perceive the events that unfolded during a police interaction. Influentially, court actors resolve many cases outside of the courts through plea negotiations in which prosecutors and defendants (through their legal representation) agree upon a set of terms under which a defendant will admit culpability to the court– sometimes trading more lenient sentences or dropping charges in exchange for resolving the case without a time consuming trial. Guilty pleas are common and accounted for 88 percent of case resolutions in U.S. district courts in 2009 (Sourcebook, 2009).\textsuperscript{13} Rational plea negotiations will take into account the probability of conviction and the expected severity of sentencing if convicted (Butcher et al., 2021). Additional evidence can influence these plea negotiations by improving the bargaining position of one side. In the case of body-worn cameras, footage may reveal law enforcement error or abuse or may corroborate/undermine defendant or law enforcement accounts of events.

While exposure of law enforcement error or abuse clearly benefits defendants, other evidentiary effects are theoretically ambiguous in direction. We may anticipate that they lean against defendants on average

\textsuperscript{12}Since resisting arrest charges here were tested as a frequency instead of a share of arrests this does not rule out evidence of a civilization effect in this outcome

\textsuperscript{13}I too find a preponderance of cases that end with a guilty plea: within my sample, which I detail in section 4, 65 percent of cases at the circuit court level conclude with at least one guilty plea
if the typical police stop is merited and/or the core components of a typical police report align with body-worn camera footage. Ultimately, the balance of these elements determines whether additional evidence benefits or harms defendants on average— which is itself an empirical question. Although adjudication of complaints against officers falls outside of the criminal justice system, in a recent working paper Çubukçu et al. (2021) found evidence that body-worn camera adoption in Chicago significantly reduced complaint dismissals for insufficient evidence while increasing disciplinary actions due to substantiated complaints.

Just how often body-worn camera footage provides evidentiary value for a case is unclear due to scarce data. Katz et al. (2014) show survey results, qualitatively consistent with the LEMAS-BWCS results, that officers believe body-worn cameras provide evidentiary value and make cases more prosecutable. However, a concurrent staffing intervention within the department contaminated tests of the accuracy of this perception. Nonetheless, multiple sources show that a nontrivial share of cases in body-worn camera-using localities have related body-worn camera footage. The Katz et al. (2014) study comments on low compliance amongst officers, but also reports that footage was available for as high as 42 percent of calls in a month.

3.3 Time Use

“It’s a razor thin wire, because you’re looking to be sure your client’s due process rights are preserved. On the other hand, I have 120 other clients. I have to preserve their due process rights too.”

Newport News Public Defender Robert Moody (Albiges, 2019)

Another suggestive indicator of the evidentiary value of body-worn camera footage is the extent to which prosecutors and defense attorneys review the footage. Between 2016 and 2018 the Henrico County, Virginia Commonwealth’s Attorney’s office reportedly viewed footage for an average of over 2,000 cases each year with an average of just under an hour of footage viewed per case (Compensation Board, 2018), roughly the workload of a full-time employee. Although the Henrico County Commonwealth’s Attorneys’s (CA) office was the most detailed in its reporting, an October 2018 Commonwealth’s Attorney survey (Compensation Board, 2018) showed that 51 prosecutor offices reported receiving an estimated 180,000 hours of body-worn

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14 We can consider this in a signaling framework: when footage confirms some details of a party’s account of events this may strengthen the signal of the party’s reliability and thus lends credence to the elements of the party’s account that are not visible in the footage. Anecdotally attorneys report experiences consistent with this signaling. This signal may disproportionately advantage police accounts, particularly in those jurisdictions where law enforcement can review body-worn camera footage prior to writing an arrest report (NACDL, 2018). In the Katz et al. (2014) evaluation, the authors report that Phoenix police specifically required that their cameras have the capacity for in-field footage review.

15 An additional evidentiary consideration for body-worn cameras is the effect of not having body-worn camera footage of an incident when body-worn cameras are ubiquitous in an area and an officer is present. An officer may intentionally neglect to record an interaction for their own expected benefit or to preserve the privacy of a member of the public or may unintentionally neglect to record due to equipment malfunction or surprise.
camera footage over a 12 month period (or an average of about 300 hours per month per office.)

Lesser-paid indigent defenders also experience body-worn camera-induced workload increases. For assigned counsel, who operate under a capped pay schedule, an hour of work reviewing body-worn camera footage (the average per case in the Henrico County prosecutor’s office) comprises over 75 percent of the total compensated time on a misdemeanor case, or 20 percent on a typical felony. And, in a time use study, three Virginia public defender offices reported spending between 160 and nearly 3000 hours per month on body-worn camera related tasks—the workload of between 1 and 16 additional full-time employees, over baselines of six to nine attorneys (Gaub et al., 2019).\footnote{The offices employ non-attorney personnel as well.}

At this point the extent to which these body-worn camera hours crowd out non-BWC activities remains ambiguous and anecdotal evidence of enhanced workloads due to body-worn camera footage remains more prevalent than detailed data on time spent on tasks related to body-worn cameras. But, this anecdotal evidence is powerful and already influenced policymakers in Virginia (VACO, 2019). As more law enforcement agencies adopted body-worn cameras the Virginia Indigent Defense Commission raised an alarm about the expected workload impacts of this technology expansion. Regarding court-appointed attorneys, the Executive Director of the Commission wrote, “it is not hard to imagine that court-appointed attorneys will be faced with terrible choices, which will hurt their clients, hurt their practice, or potentially undermine both. Court-appointed attorneys will likely have to stop taking court-appointed cases; not watch all the body-worn camera footage, in violation of their ethical duties; or basically be forced to work for free” (Compensation Board, 2018). The Ethics Counsel for the Virginia State Bar echoed this sentiment on the prosecutorial side, stating “Existing prosecutors’ workloads will be significantly increased by the time taken to review footage derived from body-worn cameras. To comply with legal and ethical standards, Commonwealth’s Attorneys must staff more lawyers or decline handling cases. Breaching the legal and ethical standards is obviously not an option” (Compensation Board, 2018).

4 Data

To study the effects of body-worn cameras on court outcomes, ideally, we would be able to link body-worn camera footage records to each court case throughout Virginia. Unfortunately body-worn camera data, especially at a case-level, are scarce. Even at agency or locality levels, existing data sets have limited information about body-worn camera adoption. Within Virginia, these existing data sets included the LEMAS-BWCS survey with coverage of a sample of 85 local law enforcement agencies and the Virginia Commonwealth’s Attorney Survey that asked Commonwealth’s Attorneys about body-worn cameras in their localities but often generated missing or incomplete responses on questions pertaining to the timing of
body-worn camera adoption.

I fill this data gap by collecting a more comprehensive set of data on body-worn camera adoption by local law enforcement agencies in Virginia. I then use this to develop body-worn camera adoption indicators within geographic court jurisdictions, which may include multiple law enforcement agencies. Observing body-worn camera adoption at the court-level rather than the case level allows me to take a broader view of the effects of body-worn camera footage on cases in the court as a whole including spillover effects on non-BWC cases. For the following analyses I combine charge-level data from Virginia courts with court-level data on body-worn camera proliferation to form a quarterly court-level panel that I use to explore any changes in charging, case processes, and case outcomes after local law enforcement agencies begin using body-worn cameras.

4.1 BWC Data

To construct the court-level body-worn camera implementation data set, I used the 2016 Law Enforcement Agency Roster (United States Department of Justice, 2017) to identify the major law enforcement agencies within each geographic court jurisdiction in Virginia. I consider a court “treated” when the first major law enforcement agency based in its jurisdiction implements a body-worn camera program, excluding small-scale adoptions/pilot programs. I designate agencies as “major” if they employed at least 25 percent of the total officers in the court jurisdiction or if their policing jurisdiction included at least 25 percent of the court jurisdiction’s population.\(^{17}\) I detail this more thoroughly in appendix A.1 and describe an alternative 50 percent threshold and the robustness of my results to this threshold in appendix B.2.

To obtain body-worn camera implementation data from qualifying law enforcement agencies in Virginia I extended Freedom of Information Act (FOIA) requests to 157 agencies.\(^{18}\) In the exploratory stages of this project I also obtained information from an additional 32 Virginia agencies which either directly provided me with information about their body-worn camera programs or had highly publicized programs with information available on department websites and local media.\(^{19}\) Ultimately I obtained complete data for 106 circuit court jurisdictions (approximately 90 percent of the total possible) including 76 that adopted body-

\(^{17}\)Agencies had to have policing duties to be included, and omitted some sheriff’s offices that primarily handled jail and court security.

\(^{18}\)Within the FOIA requests I specifically asked for separate information for pilot programs, if applicable. It is common for departments to use a testing or pilot phase in which a limited number of officers are given body-worn camera to use for a short time period to provide feedback to a department considering or planning to adopt body-worn camera on a larger scale. For example, one large department of over 200 officers piloted the technology with eight officers who had temporary use of the cameras. Other departments do not formalize this as a “pilot program” but begin by outfitting very few officers with cameras before establishing a department program. I do not treat these preliminary programs as adoptions.

\(^{19}\)I am grateful to Nathan Fedorchak for his invaluable assistance navigating the Virginia FOIA process and to the numerous members of law enforcement agencies throughout Virginia and Michigan who shared their body-worn camera experiences with me.
worn cameras by 2019 and 111 district court jurisdictions including 78 that adopted body-worn cameras by 2019.\textsuperscript{20}

### 4.2 Criminal Case Data

I obtained charge-level data for criminal cases filed in Virginia circuit courts between January 2005 and March 2019 and Virginia district courts between January 2009 and March 2019 from Virginia Court Data, a repository developed by scraping Virginia court websites and maintained by a private Virginian citizen (Virginia Court Data, 2021). The precise variables included in the data differ by court type, however all courts report defendant demographic information including race and sex, the date the charge was filed, text variables containing information about the charge itself and the section of the Virginia Code that encompasses the charge, the charge disposition, and a series of sentencing outcomes. The most common dispositions are that a defendant is found guilty, the prosecutor drops the charge, or a judge dismisses the charge. Sentencing information can include time that someone is expected to serve in jail or prison as well as fines. I also observe whether a charge is amended (superceded by an alternative charge) after filing. For example, I observe multiple instances in which an initial charge of assault on a police officer is replaced with the lesser offense of obstructing justice. Amendments can correct inaccurate initial charges or may reflect plea negotiations.

Each individual charge represents an allegation of a single offense, however it is common for defendants to be charged with multiple offenses at the same time. These charges can operate as alternatives – i.e. providing a jury the opportunity to convict a defendant of either manslaughter or second degree murder (or neither, but not both), or can come out of related alleged crimes, like multiple instances of embezzlement activities discovered jointly or a domestic violence incident that ended in an altercation with a responding police officer.

When a defendant faces multiple charges at the same time is likely that charge characteristics, court processes, and outcomes of the individual charges are related to one another. To address this, within each court type I aggregate charges up to a case-level using a grouping algorithm described in Appendix A.2. I use these case-level data to apply sample selection criteria, define outcome variables, and then subsequently aggregate up to a court-level quarterly panel.

Once charges are aggregated into cases, they may carry multiple dispositions and multiple sentences. For example, a three-charge case could end with one charge dismissed and two five-year prison sentences for the remaining two charges. I define disposition variables for cases by whether any of the charges in the case received a certain disposition. In this example, the case would be recorded as having both a "dismissed" and

\textsuperscript{20}While some of the localities for which I do not include data did not respond to FOIA requests, there were also multiple localities where a major law enforcement agency had incomplete records and thus could not be included.
"guilty" disposition. Overall for analyses, I focus on the dispositions of "guilty", "dismissed", and "dropped" where charges are "dismissed" if a judge determines they should not proceed while charges are "dropped" if a prosecutor determines they should not proceed. I discuss the sentencing data in more detail and show results for additional sentencing outcomes in appendix B.4., but in the main results I show a simple binary measure of whether an individual was sentenced to serve a nonzero amount of time in a jail or prison. 21

An institutional difference between the two courts introduces a selection consideration inherent in the data: charges filed against a defendant should appear in the district court data set with few opportunities for unobserved attrition, which is not true for circuit court cases. Because Virginia uses a magistrate as an intermediary step between arrests by police and charges filed in the district courts, it is typical for charges to be filed against a criminal defendant without a prosecutor initiating the charges. As a result, while individuals can be arrested without ultimately being charged, my data set is inoculated from selection issues arising from prosecutorial discretion or attorney intervention. Subsequent changes to the case that might reflect prosecutorial discretion, like dropped, amended, or added charges, will be observable to the researcher. In contrast, there are numerous off-ramps for cases before advancing to the circuit courts: a grand jury can decline to advance the charges at the district court level, a prosecutor could drop the charges, or the defense could accept a plea deal. As such, circuit court cases are an inherently selected set of court cases.

<table>
<thead>
<tr>
<th>Table 2: Outcomes and Channels</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Case Filing</strong></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Number of cases filed</td>
</tr>
<tr>
<td>Number of &quot;civilization effect&quot; cases filed</td>
</tr>
<tr>
<td>Share of cases with multiple charges</td>
</tr>
<tr>
<td><strong>Case Process</strong></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td><strong>Case Outcome</strong></td>
</tr>
<tr>
<td></td>
</tr>
</tbody>
</table>

Notes: Shaded cells indicate channels through which BWCs may directly affect criminal cases. However, as discussed in the paper, compositional changes in case filings through civilization effects could also carry through to influence aggregate case process and outcome measures.

21 Some defendants who are sentenced to serve time according to this measure actually forgo incarceration by adhering to certain requirements set by the judge in their case. I ignore this in the definition of this variable, but include supplementary results showing the use of suspended sentences after body-worn camera introduction in appendix B.4.
4.3 Sample and Outcome Variables

The three channels through which body-worn cameras can affect criminal defendants and the courts occur at two different stages in the criminal justice process. Civilization effects arise in police interactions, whereas evidentiary and attorney time use effects arise within the courts. Because of this distinction, I use different sample selection criteria for analyses of policing-based and court-based effects.

4.3.1 Case Filing Sample

While much of the existing literature on civilization effects looks for changes in police use of force or complaints against officers, these are rare outcomes to observe. The vast majority of police interactions do not entail use of force and do not result in a complaint against the officer. So, while these are important and salient outcomes to evaluate, it is also worthwhile to test for civilization effects in more routine police interactions. Civilization effects could change how police exercise discretion when issuing citations/making arrests, how likely it is for interactions to (even nonviolently) escalate, or the prevalence of certain charges. In this way, civilization effects could influence the number and composition of cases that enter the courts.

When testing for civilization effects, I test for changes in charging after body-worn cameras are introduced. To do so I include all recorded filings when creating the quarterly case filing panel data sets. If interactions are less likely to escalate after body-worn cameras are introduced, we may see a reduction in these filings both on the extensive and intensive margins. That is, we would expect to observe a reduction in the total number of cases entering the courts and, conditional on any charges being filed, a reduction in the share of cases that carry multiple charges. Within each of the two case filing samples (one each for district and circuit courts), I calculate as outcome variables the number of cases filed in each court in a given quarter, and the share of those cases which include more than one charge.

A third outcome I define at this stage pertains to the prevalence of a subset of charges I will call “civilization effect charges”. These are charges that originate or escalate in the presence of a police officer. We might expect these charges—such as disorderly conduct, eluding police, resisting arrest, and assault or another offense specifically directed toward a police officer—to be the first to show evidence of civilization effects.

To identify these charges I use both the code section and charge fields within my data. These two pieces of information typically complement one another: the code section describes which specific provision of the Virginia legal code the defendant is accused of violating, while the charge field provides a textual, and sometimes finer, description of the offense. For example, assault and battery is listed under 18.2-57 in the Virginia code, but the corresponding charge field might contain something like "A/B - LEO", which designates that the defendant is charged with assault and battery against a law enforcement officer. To
develop the set of civilization effect charges I used both code sections and text from the charge field to capture more of the true civilization effect charges.\textsuperscript{22}

Finally, I will restrict the sample frame for main analyses to only those cases filed by Q1, 2019. This is not necessary if my intention is simply to quantify the changes to case filings after body-worn camera introduction, but is important for using these first stage results to interpret subsequent changes to aggregate case processes and outcomes. Because cases take time to resolve, my sample frame for these subsequent outcomes is shortened. I discuss this process in the next section. However, I also show results using a longer sample window in appendix 3.1.

Table 3 shows descriptive statistics for each of these outcomes within the civilization effect panels: treated localities tend to have more cases overall than untreated localities at the circuit court level, but this reverses at the district court level.

\begin{table}[h]
\centering
\caption{Comparison of treated and untreated localities at early-sample baseline}
\begin{tabular}{|c|c|c|c|c|}
\hline
\textbf{Panel A: Case Filings} & & & & \\
\hline
\textbf{District} & Untreated & Treated & Untreated & Treated \\
\hline
Cases & 4,424.8 & 4,090.7 & 80.2 & 189.3 \\
Civilization Effect Cases & 27.6 & 44.2 & 3.6 & 9.4 \\
Multi-charge Cases & 17.2\% & 18.9\% & 49.1\% & 46.3\% \\
\hline
\textbf{Count Localities} & 33 & 76 & 28 & 76 \\
\hline
\textbf{Panel B: Case Processes \\
\textsc{&} Outcomes} & Misdemeanors & Felonies & Felonies \\
\hline
\textbf{District} & Untreated & Treated & Untreated & Treated \\
\hline
Female & 28 & 29 & 22 & 24 & 21 & 22 \\
Black & 21 & 33 & 26 & 43 & 27 & 45 \\
Multi-charge & 17 & 18 & 57 & 54 & 47 & 43 \\
% Public Defender & 33 & 43 & 33 & 43 & 25 & 46 \\
% Cases with amended charge & 9 & 9 & 4 & 4 & 2 & 2 \\
Avg Sentence Time (days) & 21.7 & 28.0 & 67.1 & 82.3 & 2404.2 & 2518.9 \\
% Sentenced to time & 18 & 22 & 27 & 28 & 66 & 71 \\
% Received fine & 65 & 65 & 13 & 13 & 11 & 12 \\
% Cases with charge dismissed & 19 & 17 & 10 & 10 & 7 & 4 \\
% Cases with charge dropped & 11 & 11 & 38 & 40 & 21 & 24 \\
% Cases with guilty charge & 73 & 75 & 34 & 33 & 72 & 72 \\
Avg Num. Cases & 999.7 & 1182.0 & 91.7 & 132.2 & 53.7 & 124.3 \\
Count Localities & 33 & 69 & 33 & 69 & 28 & 68 \\
\hline
\end{tabular}
\end{table}

\textit{Note: 2009 District court case characteristics, 2006 Circuit court case characteristics from unweighted locality-level panel. The treated group are localities that adopted by Q2, 2018 for the case processes and outcomes sample and localities adopting through Q4, 2018 for the case filing sample. Case filing panels are not case-type specific.}

\textsuperscript{22}It is possible that there are times when an offense is directed at a law enforcement officer but this element of the charge is not indicated in either the code section or charge fields. If such misclassifications represent classical measurement error, my estimates will be less precise than they would be with perfect charge classifications but the measurement error does not introduce bias.
4.3.2 Court Process and Resolution Sample

I refine my sample more when exploring effects on court processes and resolutions. Throughout these analyses, a trade-off for the timeliness of this paper is a shortened window of post-intervention periods to evaluate. Because cases can take weeks, months, or even years to be fully resolved, I have a shorter window of useful observations for case resolutions than case initiations. Additionally, cases will proceed and resolve differently based on characteristics of the case. Most notably, the set of potential outcomes and their likelihoods differ for misdemeanors and felonies.

Because of this, I tailor the samples for the district and circuit court analyses based on the types of cases over which each court has primary jurisdiction. In contrast to the civilization effects panels, I create three panels for the court process and resolution analyses: a circuit court panel, district-felony panel, and district-misdemeanor panel. I restrict circuit court sample cases to those that include at least one felony. For all three samples I drop charges such as probation violations, bond violations, etc. which arise as a result of previous engagement with the criminal justice system.

I also restrict my sample to include only cases for which all charges were filed by March 12, 2019. This is to give all charges in my data set at least one year to resolve prior to the onset of the coronavirus pandemic. Even so, some cases were not resolved by the end of the data collection period. This is particularly true for circuit court cases, which encompass more severe charges and often more intense litigation. Without correction, longer and more complex cases would systematically drop from the sample in later periods. To mitigate the effects of this attrition, I condition outcome variables for circuit court cases on having been observed within 1 year of the filing date. For example, rather than examining the share of cases for which at least one charge is amended, I use the share of cases for which at least one charges is amended within 1 year of filing.

District court cases tend to be simpler and faster-moving than circuit court cases, mitigating the end-of-sample attrition issue for district court cases without any intervention. In appendix B.1 I discuss the attrition issue in more depth.

The March 2019 end date for my court samples highlights another sample consideration: law enforcement began using body-worn cameras relatively recently; 15 percent of adoptions before 2019 took place in 2018. This means that the window of body-worn camera adoption does not neatly fit within my court data sample since I require at least two years of pre-adoption case data and 1 year of post-adoption data in order for a locality to be included in my primary analyses. I drop the 2018 adopters from the court process and resolution panels, but do include them in supplementary analyses in Appendix C.1. At the other end, the

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23Because courts and attorneys suspended and/or substantially modified their operations after the onset of the 2020 coronavirus pandemic, cases after this time did not have a “normal” year to be resolved – the Governor of Virginia declared a state of emergency on March 12, 2020.

24In base year 2006, 83 percent of circuit court cases in my sample were disposed within 1 year of filing.
earliest adopters began using the technology in 2007. My district court data begins in 2009, so I omit from the district court analyses those localities that adopted before 2011. Such early adoptions are rare, comprising less than 4 percent of the localities that adopted body-worn cameras by 2019.

When faced with a criminal charge, a natural sequence of questions to ask would be "will I be found guilty?" and "what happens if I am?" In line with this, the most basic outcomes of interest when studying the effects of body-worn cameras on criminal cases are the share of defendants who are convicted of an offense and the share that are sentenced to incarceration. Incarceration is a common but severe outcome, and is not the only possible sentence for a guilty verdict. Misdemeanors commonly carry a sentence of a fine rather than jail time and so I also include this outcome measure.

However, we are also interested in how these outcomes come to pass. While the ultimate effect on the defendant is the same, a verdict of "not guilty" after a case is presented to the court is different from a dropped charge or case dismissal, which may happen due to lack of evidence, plea negotiations, or demonstrated error. To capture changes in these case processes, I test for changes in the share of the share of cases with dropped charges. Case dismissals and amended charges, while important aspects of the courts system, are rare case outcomes, particularly for felonies where they each occur less than 5 percent of the time on average in my court panels. In contrast, prosecutors drop charges routinely in both district and circuit court cases, particularly for felony cases.

Additionally, insofar as ongoing criminal litigation is disruptive for defendants, the amount of time a case is active within the courts is an outcome of interest. I leverage the timing data within the circuit court sample to see whether cases took longer to resolve after body-worn camera introduction using the share of cases that were disposed within 1 year of filing. Because my circuit court sample conditions on case outcomes being observed within 1 year of filing this will fulfill the dual purposes of describing changes to case duration and also alerting me to compositional changes in my sample stemming from this timing criteria. For district court cases, which resolve more quickly than circuit court cases, I also use the share of cases disposed within 3 months. The disposition date is unavailable in circuit court data, so here I substitute the date of the latest hearing for disposed cases. This will cause an overestimate of the time to case resolution, particularly for cases for which a defendant was sentenced to probation, but should serve as an effective proxy.

Overall, treated and untreated localities demonstrate many baseline similarities in case processes, but vary in the court and defendant characteristics and some case outcomes. Shown in table 3, case dispositions are similar across the treatment groups, including the share of cases with a guilty disposition, dropped or amended charges. One difference in case processes is evident at the circuit court level, where untreated localities on average take longer to reach case resolutions. However, at the district court level, cases across groups tend to resolve at similar rates. At the district court level, fines are levied across groups at similar

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25 I summarize each of these variables in table 2, along with their expected responses to body-worn camera adoption
rates, although they are slightly less common in untreated courts at the circuit court level. Across all three samples, incarceration outcomes are more common/more severe in treated courts. Treated localities are more likely to be in populous areas with more active courts, a higher share of black defendants, and are more likely to be served by a public defender’s office.

5 Methods and Results

I use the rollout of body-worn cameras across law enforcement agencies in Virginia to test for effects of body-worn cameras on the courts. There are potential pitfalls toward identifying these effects for which I will be particularly mindful: in particular, we may be concerned that localities that adopt body-worn cameras systematically differ from those that don’t. This could happen if, for example, law enforcement agencies with low conviction rates implemented body-worn cameras to improve the evidence available in cases. However, multiple pieces of evidence suggest that this was not the case, and I argue that police body-worn camera adoption is plausibly exogeneous to court processes. First, the LEMAS-BWCS survey showed that body-worn camera adoption was driven by police in pursuit of improvements to police interactions rather than desired changes in court processes or resolutions. Of the 53 adopting agencies in Virginia, only three cited evidentiary or prosecutorial reasons as the primary reason they began using body-worn cameras. Second, while adopting and non-adopting localities do demonstrate baseline differences across certain demographic characteristics and sentencing outcomes, they are overall similar in court processes, as discussed in the previous section. Finally, as I detail in appendix B.1., in the years before body-worn camera adoption most variables of interest evolve similarly over time in treated and untreated localities.

Even so, because the plausible exogeneity of the treatment is so critical for interpreting results I will supplement my primary analyses, which use all treated units with sufficient pre- and post- treatment observations, with analyses using only units treated during my sample period. This strategy weakens the exogeneity assumption: now I do not require that adoption itself is exogeneous but only that the timing of adoption is plausibly exogeneous. Deshpandi and Li (2019) use this strategy in their study of the effects of social security office closings on disability program participation, and Kim (2020) demonstrated and exploited the exogeneous timing of body-worn camera adoption for his national study of the effects of body-worn cameras on policing. I detail the specific methods used to test for body-worn camera effects as well as results in the next sections.

5.1 Methods

There have been numerous recent developments in the econometric literature pertaining to two-way fixed effects (TWFE) models with differential treatment timing and/or heterogeneous treatment effects. In short,
this emerging literature clarifies how classic TWFE estimates under staggered treatment timing reflect
weighted averages of many comparisons across groups that may not reflect the intentions of the researcher.
For example, some comparisons can be given a negative weight in the treatment effect aggregation, and
the “forbidden comparison” of newly treated to previously-treated groups are not excluded from the treat-
ment effect calculation (Borusyak, Jaravel, and Spiess, 2021; Goodman-Bacon, 2021). In line with this, the
traditional TWFE estimator can under-perform in the presence of treatment effect heterogeneity. Multiple
new and modified estimators emerged in recent years to address these shortcomings (Borusyak, Jaravel, and
Spiess, 2021; Callaway & Sant’Anna, 2020; Sant’Anna and Zhao, 2020; Chaisemartin & D’Haultfoeuille 2020;

In my context, law enforcement adopt body-worn cameras at different points in time, and it is theoreti-
cally highly plausible that body-worn camera effects are heterogeneous over time due to salience effects for
criminals and police, and attorney adaptation within the courts. To implement a difference-in-differences
strategy under these conditions I choose to use the modified event study framework developed by Borusyak,
Jaravel, and Spiess (2021) for my main results. This imputation estimator (BJS), implemented within Stata
using code generously provided by the authors, uses untreated observations to estimate unit and time fixed
effects, which are subsequently used to impute counterfactual untreated outcomes for treated observations.
The difference between the observed outcomes and their imputed counterfactuals gives a unit and time
specific treatment effect which can then be aggregated into the desired treatment effect. That is, I estimate

\[ Y(0)_{it} = A'_{it}\lambda_i + X'_{it}\delta + \epsilon_{it} \]

, and use these estimates, \( \hat{\lambda}_i \) and \( \hat{\delta} \), to calculate \( \hat{\tau}_{it} = Y_{it} - Y(0)_{it} \) for each locality in each quarter. I
aggregate these in two ways; one showing an overall ATT across all treated unit-quarters and one showing
estimated average treatment effects for each of the four periods following implementation. In doing so, I
can discuss short term treatment dynamics. For robustness, I show alternative estimators including the
traditional difference-in-differences in appendix C.3.

The key underlying assumption for this analysis is that of parallel trends. In a recent working paper, Roth
(2021) demonstrated that common pre-trends tests used to validate the plausibility of the parallel trends
assumption can inadvertently introduce a survivor bias to estimates that pass these tests. To mitigate this
issue, BJS (2020) propose an alternative parallel trends test that complements their imputation estimator.
In this test, the researcher estimates an expanded version of the imputation model from the main analyses,

\[ Y(0)_{it} = A'_{it}\lambda_i + X'_{it}\delta + W'_{it}\gamma + \tilde{\epsilon}_{it} \]

using only only untreated observations and where \( W_{it} \) is a vector of indicator variables for some determined
number of periods before treatment. After obtaining estimates \( \hat{\gamma} \) of \( \gamma \), the researcher conducts a joint
significance test (F-test) of the null hypothesis that \( \gamma = 0 \). Using this procedure, I show results in appendix table B.1 for all of the main case filing, process, and resolution models using indicators for the (a) four and (b) eight periods prior to body-worn camera adoption. Overall, while certain variables of interest do show evidence of differential trends between the untreated and the treated units, there does not appear to be evidence of systematic violations of the parallel trends assumption. In the cases where the test does raise concerns regarding trends, I will discuss results in light of the expected biases from such a violation.

5.2 Case Filing Effects

I begin by testing for effects of body-worn cameras on the quantity and types of cases that enter the courts. Doing so provides two primary benefits: first, since changes to the cases that enter the courts would be indicative of behavioral changes to police or the public, I contribute directly to the developing consensus on the effects of body-worn cameras on policing. Secondly, these analyses are critical for interpreting estimates on outcomes that occur later in the criminal justice process. If the cases entering the courts appreciably change after body-worn camera introduction due to policing changes, then estimates of body-worn camera effects on case outcomes may reflect not only the evidentiary or time use effects that are introduced in the courts but also these case changes. For example, if police make fewer marginal arrests after body-worn camera introduction then we could erroneously attribute higher conviction rates to evidentiary or time use channels when in reality the cases that entered the courts were stronger simply on the basis of police forgoing the weaker arrests.

There are four primary measures I use to examine whether the set of cases entering the courts changed after body-worn cameras were introduced. First, I look at broadly at the number of cases that are filed in each quarter after body-worn cameras are introduced. Because of heterogeneity in court activity, I define this outcome variable \( Y_{it} = \ln(\text{case\_counts}_{it}) \) at the district court level and \( Y_{it} = \ln(\text{case\_counts}_{it} + 1) \) at the circuit court level, since some\(^{26}\) less active circuit courts hear no qualifying cases in certain quarters throughout the sample period. Using the log of the case count variable allows me to approximately interpret estimates in percent change terms. Similarly, when examining changes to the outcome defined by the number of "civilization effect cases" I use \( Y_{it} = \ln(\text{civ\_case\_counts}_{it} + 1) \) for both the district and circuit court samples.\(^{27}\) These two outcomes provide information about the number of cases that enter the courts. The

\(^{26}\)3 courts in a total of 10 quarters.

\(^{27}\)While adding 1 before taking a log is an oft-used technique, the selection of the value to add can alter results at times. In practice, I do not find that to be a problem in this context. At the district court level, the results are similar whether I add 1 or 0.1 to the civilization case count before taking the log; both estimates are statistically significant at the 5% level and the point estimates vary from -0.110 to -0.115. Similarly, at the circuit court level, the estimates for the overall case count outcome variable are virtually unchanged between these two variable definitions, ranging from -0.064 to -0.065, although precision changes result in statistical significance only at the \( \alpha = 0.10 \) level for the outcome with 0.1 added, compared to
third and fourth outcomes, the shares of cases with (3) multiple charges and (4) cases with misdemeanor charges will provide information about the composition of the cases that end up in the courts. While I am indifferent about the types of cases that qualify for case counts (in particular, I include infractions at this stage), I calculate the misdemeanor share using only felonies and misdemeanors and do this only for the district court. I provide estimates of the overall ATT for each court level and each outcome in table 4.

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Case Count</th>
<th>Civilization Case Count</th>
<th>Share Multicharge Cases</th>
<th>Share Misdemeanor</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Effect</td>
<td>-0.065†</td>
<td>-0.091*</td>
<td>-0.007</td>
<td>—</td>
</tr>
<tr>
<td>Circuit Court</td>
<td>(0.033)</td>
<td>(0.037)</td>
<td>0.009</td>
<td>—</td>
</tr>
<tr>
<td>Mean</td>
<td>145.3</td>
<td>6.9</td>
<td>0.486</td>
<td>—</td>
</tr>
<tr>
<td>Observations</td>
<td>5,472</td>
<td>5,472</td>
<td>5,463</td>
<td>—</td>
</tr>
<tr>
<td>Treatment Effect</td>
<td>0.015</td>
<td>-0.115**</td>
<td>-0.004</td>
<td>-0.002</td>
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<td>(0.037)</td>
<td>(0.005)</td>
<td>(0.003)</td>
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<td>4,141</td>
<td>4,141</td>
<td>4,141</td>
</tr>
</tbody>
</table>

Cluster-robust standard errors in parentheses

**p<0.01, * p<0.05, † p<0.1

Note: Means of columns 1 and 2 reflect the average counts for the variable but the outcome used is \( \ln(\text{count}+1) \) for civilization case counts in both courts and case counts in the circuit court, and \( \ln(\text{count}) \) for the remainder.

I find limited evidence that body-worn cameras affected the cases entering district courts. Body-worn cameras do not appear to have changed the overall quantity of cases, the share with multiple charges, or the share of criminal cases that included a misdemeanor. Were civilization effects widespread, we would expect fewer cases to enter the courts, and those within the courts to be less severe. However, I find no evidence for widespread substitutions between felonies and misdemeanors after body-worn camera introduction. I do find suggestive evidence for civilization effects in the narrower set of civilization effect charges. I find a statistically and economically significant reduction in civilization effect cases of 11.5% after police begin using body-worn cameras. This would be equivalent to a reduction of 3.8 civilization effect cases per quarter from the mean number of cases across courts. However, pre-trend violations suggest that this estimate likely underestimates the true effect of body-worn cameras on civilization effect charges. Within the adopters-only sample (table x), which does not suffer from a pre-trends violation for the civilization effect charge outcome, I find a less extreme estimate for the effects of body-worn cameras on civilization effect cases at the district court level – a 6.1% reduction, or a reduction of 2.3 cases per quarter from the mean. Although this is a sizeable point estimate, I am unable to statistically distinguish it from zero. However, I do find that two of the first five quarterly treatment effect estimates are significantly different from zero at \( \alpha = 0.10 \). I show these estimates in figure 4.

(barely) \( \alpha = 0.05 \) for the outcome with 1 added. The choice is more influential at this level for civilization effect cases, but the overall result is still the same; I find a statistically significant reduction of -0.091 (\( \alpha = 0.05 \)) or -0.114 \( \alpha = 0.10 \)

28 The treated-only sample also supports the null effects on intensive margin outcomes. Notably, the point estimates for the
At the circuit court level I find limited evidence for short-term reductions in both the overall number of cases entering the courts and the number of these cases that include civilization effect charges. While the estimated treatment effects for both of the extensive margin outcomes are statistically significant and negative, as expected under civilization effects, the civilization effects outcome is highly volatile after body-worn camera introduction. In the adopters-only sample, which covers a slightly truncated time period compared to the full sample, the point estimate is statistically and economically null. Similarly, the estimated 6.5% reduction in circuit court cases – a sizeable reduction amounting to 9.4 fewer cases per quarter compared to the mean – appears to be driven by a small number of late-adopting localities. Using the adopter-only sample, I find no evidence of civilization effects at the circuit court level.

In interpreting these results it is useful to keep in mind that circuit court cases are relatively more severe than district court cases and that, unlike district court filings, circuit court filings are a selected set of cases that persisted past district court-level off-ramps. Most civilization effect cases never reach the circuit courts to begin with. For this reason, that the reduction in civilization effect cases at the district level does not carry through to the circuit court level is understandable: the cases that we would expect to be marginally altered by civilization effects in policing are likely less severe cases and would be less likely to have entered the circuit courts to begin with. For example, we may expect a body-worn camera to more strongly dissuade a disorderly conduct violation than an assault violation by a member of the public. That I do not find

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effects of body-worn cameras on the share of multi-charge cases and the share of misdemeanors were nearly identical across specifications.
compelling evidence of broader case changes—either in quantity or characteristics—is more surprising. Since the evidentiary and time use channels for effects have the potential to alter plea negotiations (by changing the information set available for a case and the time required for an attorney to represent all of her clients) we may expect to see some case sorting and re-classifications across case types, yet I do not find evidence of this taking place.

5.3 Case Processes and Resolutions

I next proceed with an estimation of the effects of body-worn cameras on case processes and resolutions. Because (excluding the small set of civilization charges in the district courts) the set of case filings does not appear to systematically change after body-worn camera introduction, I can interpret these estimates as evidence of combined evidentiary and attorney time use effects uncontaminated by persistent civilization effects from altered policing. I show two case process results of interest (share of cases in which a prosecutor drops a charge and the share of felony cases that are certified to the circuit court from the district court) as well as two case resolution outcomes of interest (whether the defendant is found guilty and whether they are sentenced to time) in table 5. Additionally, I show results for the share of cases that are resolved within one year of filing. This provides not only provides a case process measure but also will serve as a check on the influence of the circuit court-level case timing condition on the composition of cases for which I calculate these case process and outcome variables. If body-worn cameras caused more cases to resolve more than one year after their filing dates, I would need to alter the analyses of case process and resolution outcomes to account for this composition change.

First, I note that cases in the circuit court do not show evidence of slowing down to an economically or statistically significant degree as measured by the disposition timing variable. This rules out any concerns that there are influential compositional changes in the cases that meet the circuit court sample one-year timing condition after police begin using body-worn cameras. Moreover, I do not find evidence that body-worn cameras altered case processes or outcomes. Across 13 models, only one treatment effect estimate was statistically significant: at the circuit court level, prosecutors dropped charges in three percentage points fewer cases after body-worn camera introduction than before. This did not correspond to a significant increase in the share of cases in which a defendant was found guilty, nor the share of defendants sentenced to incarceration and the point estimates for these treatment effects were negative. This suggests that if prosecutors did drop charges less frequently after body-worn camera adoption at the circuit court level, they dropped individual charges rather than cases as a whole but maintained stable conviction rates.

29I provide supplementary results for an extended set of court processes and case outcomes in appendix B.2.
6 Heterogeneity Analyses

The preceding null results in a variety of case outcomes calculated across multiple case types and courts are both robust and surprising: practitioners report and data support the narrative that inputs to the criminal justice system changed when police began using body-worn cameras. An influx of camera footage added data to the courts and an additional job responsibility for attorneys. The Commonwealth of Virginia created a committee to document these court changes, and even introduced legislation limiting the number of cameras per prosecutor. And yet, I find no aggregate effects of body-worn cameras on the courts. This disconnect raises additional questions about how these null results came to be.

It could be that body-worn cameras are simply a smaller shock to the system than practitioners and advocates perceive them to be: case processes and resolutions could be sticky and unresponsive to changes in attorney time use or the noise reduction produced by body-worn cameras. Relatedly, body-worn cameras could be strongly influential only in a small subset of cases that do not change aggregate processes and outcomes. An alternative explanation for the null result is that various channels of body-worn camera effects could offset one another. In this section I delve deeper into two subsets of the data to further shed light on these null results. First, I use variations in the likelihood an offense is captured on body-worn camera video to test the offsetting effects hypothesis. Then, I test for heterogeneity in effects based on a key defendant demographic characteristic: race. body-worn camera adoption in the U.S. is tightly linked to broader concerns with racial disparities in policing and the criminal justice process. I test whether black
defendants specifically experienced changes to case filings, processes, or resolutions after police began using body-worn cameras.

6.1 Revisiting Evidentiary and Time Use Channels

In the primary results, certain case filing outcomes provide evidence that body-worn camera civilization effects do not induce widespread changes in caseloads or characteristics. However, subsequent case process and resolution outcomes reflect the combined influence of evidentiary and attorney time use channels. One driver of the null result could be that evidentiary benefits offset the time costs for attorneys to review this footage. To provide evidence toward this counteracting effects hypothesis, I leverage variation in the likelihood that footage is available for a case. Cases with body-worn camera footage will introduce the possibility of both evidentiary effects and time use effects as attorneys access the body-worn camera data. However, while evidentiary effects only pertain to cases with footage, attorneys are likely to work multiple cases at a time. If, as expected, attorneys substitute their hours across cases to the case activities with the highest marginal benefit then at times they will substitute their work hours from cases without body-worn camera footage toward a case with footage. If this happens systematically then after law enforcement begin using body-worn cameras the outcome paths of cases with body-worn camera footage and without should diverge.

I cannot observe whether body-worn camera footage was available for or used in a given case so instead I classify a subset of charges that are likely to take place in the presence of an officer as a “more likely treated” (MLT) group. Such charges include the civilization effect charges from before, but also DUI/DWI, concealed weapons, and possession of weapons or drugs. I include DUI/DWI based off of the input of numerous law enforcement agents throughout Virginia who independently volunteered this as an example of a charge that is likely to be affected by body-worn camera footage. An individual receives a concealed weapon or possession charge because an item was observed in the presence of an officer, and thus if the officer is wearing a camera there is likely to be footage associated with the charge. However, these differ from the civilization effect charges from before because the alleged offense would have been initiated prior to interaction with an officer.

Under this partitioning, localities have an average of 40.6 MLT and 50.8 non-MLT cases per quarter in the circuit court; 49.9 MLT and 64.4 non-MLT felony cases in the district court; and 157.6 MLT and 824.2 non-MLT misdemeanor cases in the district courts. I calculate for each locality-quarter the differences in the shares of each outcome across MLT and non-MLT cases, as well as differenced covariates such as the share of

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30It’s important to keep in mind the attorney will be maximizing a utility function that incorporates both her defendant’s well-being and her own professional and personal well-being. Some activities which yield lower returns to defendant outcomes may nonetheless have higher returns in this framework. For example, even if a defender is convinced that, no matter what they do, a defendant will be convicted and sentenced harshly they must nonetheless complete certain tasks in order adhere to professional standards.
female defendants, black defendants, and certain case class shares. I then use the same imputation estimator and model on this differenced panel and show results in table 6. While some differenced outcomes are noisy, leading to large error bands around point estimates, I do not find compelling evidence that case processes or resolutions for cases that were more likely to have body-worn camera footage diverged from those less likely to have footage. It is unlikely that the counteracting effects hypothesis holds in the aggregate; body-worn camera footage appears to have minimal effects on the case processes and outcomes as a whole.

6.2 BWCs and Race

The discussion around body-worn cameras in the U.S. is tightly linked to a broader discussion around race and the criminal justice system. Black adults in the U.S. persistently express less confidence in the police than do white adults, and polls show that this gap grew throughout the 2010s (Jones, 2020). The fallout from the events in Ferguson, viewed by many as an inflection point in the push for body-worn cameras, was so great not simply because an officer killed a member of the public but because of the racial context in which this took place. The U.S. Department of Justice investigated and released a report on Ferguson police practices in 2015, finding that “African Americans experience disparate impact in nearly every aspect of Ferguson’s law enforcement system”, and evidence of “intentional discrimination” (Department of Justice,
Outside of Ferguson, numerous studies document racial discrepancies and discrimination in the criminal justice system, including in policing (Antonovics and Knight, 2009; Fryer, 2019; Horrace and Rohlin, 2016; Luh, 2020), pretrial release (Arnold et al, 2018), convictions and jury deliberations (Abrams et al, 2012; Anwar et al, 2012; Bjerk and Helland, 2020; Flanagan, 2018), and sentencing (Alesina and Ferrara, 2014). Additional research has shown that some policies intended to ameliorate these disparities and their effects can unintentionally exacerbate disparities (Doleac & Hansen, 2020). Because of this, in addition to testing for effects of body-worn cameras on case outcomes and processes overall I also look specifically for differential effects of body-worn cameras on black defendants. To do so I employ the same techniques used in the primary analyses in this paper, but now apply these techniques to a modified quarterly court-level panel for case processes and outcomes consisting of the difference in average outcomes between black and non-black defendants.\(^\text{31}\) I also extend the case filing analysis to include a variable representing the share of case filings with black defendants at both the district and circuit court levels.

First, I note that the unconditional mean differences across the two racial groups are small within this sample. For most outcomes, the difference between average black and non-black defendant outcomes across courts are less than one percent. One notable exception to this rule is the difference in the share of defendants sentenced to serve time for misdemeanors at the district court level, where across the sample courts black defendants are sentenced to time 3.5 percent less often than non-black defendants. This difference diminished after police began using body-worn cameras: Table 7 shows that the difference in the share of cases concluding with a positive sentence time declined by 1.3 percentage points after body-worn camera implementation. Apart from this decline, outcomes between black and non-black defendants were overall stable after law enforcement began using body-worn cameras. The share of cases filed that listed a black defendant did not significantly change, and the processes and resolutions of these cases did not diverge for black defendants compared to non-black defendants.

### 7 Conclusion

Body-worn cameras have become a key tool in a public push for transparency and accountability for police officers. However, while law enforcement agencies equipped their officers with this recording technology, attorneys and other court actors grew concerned about unintended consequences of the data influx from body-worn cameras. The results of this study may ameliorate these concerns. Using a rich data set containing

\(^{31}\)A challenge for this analysis with my data structure stems from the racial homogeneity within many rural localities. For example, at the circuit court level, black defendants make up 45% of cases while white defendants comprise 53%. However, less populous localities routinely show in excess of 90% non-black defendants (sometimes over 99%) making within locality decompositions difficult.
detailed charge-level information for criminal charges filed in Virginia courts between 2006 and 2020 and accounting for the selection of police into body-worn camera programs, it appears that body-worn cameras have an overall limited civilizing effect on police interactions as measured by district court filings. While a subset of charges that are initiated in the presence of a police officer—such as assault on an officer or eluding police—become less prevalent after police begin using body-worn cameras, cases overall do not change in quantity or composition, measured by the share of district court cases including a misdemeanor and the share of cases at both court levels which include multiple charges.

At the next stage in the criminal justice process, I find that body-worn camera adoption does not adversely affect criminal defendants. Defendants are found guilty and sentenced to incarceration at similar rates before and after police start to use body-worn cameras. This finding cannot be attributed to compositional changes in cases stemming from changing case characteristics, and is robust to the inclusion of various charge characteristic controls. This result is surprising: body-worn cameras can generate hours of evidence, and attorneys in Virginia report that they view this footage at substantial time costs. However, neither the evidentiary value of the footage nor the reallocation of attorney time within and across cases to view the

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Robust standard errors in parentheses

**p<0.01, *p<0.05, † p<0.1

*Time to disposition is approximated using time to latest hearing in district court

Controls included in regressions include share female, black, and of case classes. No controls are used in the first column.

Case filing panels were not separated based on case type, so District (A) includes results for the share of black defendants for all district court filings. For case processes and outcomes, District (A) shows results for the misdemeanor sample and District (B) shows results for the felony sample.

Pre-trends p-values are given for a joint significance test using the 8 quarters prior to adoption.
footage appears to affect the body of cases as a whole. While body-worn cameras were adopted due to concerns over racial disparities in policing, I also do not find evidence of differential policing or court effects for black defendants.

Overall my results suggest that BWC effects on policing and the courts are exceptions rather than the norm. Existing research suggests that the benefits, such as reduced use of force, in these exceptional cases can nonetheless provide benefits exceeding the costs of obtaining and maintaining cameras (Williams Jr. et al., 2021). Even so, the policing effects I find and that are described in the previous literature and even rich criminal case data provide merely a snapshot of the broader costs and benefits borne by actors in the criminal justice system. The attorney concerns described in this paper eventually culminated in legislative efforts to increase funding for prosecutors offices in Virginia– which may prove to exacerbate funding differentials between indigent defenders and the prosecutors to whom they serve as a balance in the years to come. My counterintuitive finding of null effects illustrates that engaging a broad base of community stakeholders and researchers early on in criminal justice policy decisions may mitigate the unintended consequences of seemingly simple policy changes.
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Ariel, Barak, Alex Sutherland, Darren Henstock, Josh Young, Paul Drover, Jayne Sykes, Simon Megicks, and Ryan Henderson. 2016. "Wearing body cameras increases assaults against officers and does not reduce police use of force: Results from a global multi-site experiment". European Journal of Criminology. 13(6): 744-755


Chalfin, Aaron and Jacob Kaplan. 2021. "How many complaints against police officers can be abated by incapacitating a few 'bad apples'?". Criminology & Public Policy.


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A Data

In this appendix I further describe the data sources used in this paper as well as the decision rules I applied in preparing the data for analysis.

A.1 BWC Data

I constructed a body-worn camera adoption dataset using multiple sources of information on the timing of body-worn camera adoption by local law enforcement throughout Virginia. The three primary sources I used to construct this dataset were, 1) FOIA requests 2) Local news and agency websites 3) Non-FOIA personal contact with departments. From these sources I obtained information about body-worn camera implementation for 166 agencies throughout Virginia.

This set of 166 agencies is not exhaustive: there are hundreds of local law enforcement agencies throughout Virginia. Oftentimes multiple agencies operate within a single court jurisdiction. Since these agencies typically vary in force size and the size of the populations they serve, their individual influence on local courts also varies. For example, according to the 2008 Census of State and Local Law Enforcement Agencies, 15 Virginia departments had only one full time sworn officer while 35 departments had over 100. As such I defined a court jurisdiction to be treated when the first “major” local law enforcement agency operating in the court jurisdiction began using body-worn camera, excluding small scale pilot adoptions.

Defining “major” law enforcement agencies can at times be somewhat arbitrary. My primary specification used throughout the paper considers a law enforcement agency “major” within its locality if it is a policing organization which has jurisdiction over at least 25% of the locality’s population or employs at least 25% of the locality’s full time sworn officers amongst agencies with policing mandates. I used two sources of information to determine which agencies would meet these criteria, detailed subsequently.

Defining “major” law enforcement agencies can at times be somewhat arbitrary. My primary specification used throughout the paper considers a law enforcement agency “major” within its locality if it is a policing organization which has jurisdiction over at least 25% of the locality’s population or employs at least 25% of the locality’s full time sworn officers amongst agencies with policing mandates. I used two sources of information to determine which agencies would meet these criteria, detailed subsequently.

Law Enforcement Force Size and Characteristics: I use policing role indicators and force size measures from the 2016 Law Enforcement Agency Roster (LEAR). The LEAR itself includes variables pulled from other sources. Thus the LEAR 2016 officer counts I use are counts from the 2008 Census of State and Local Law Enforcement Agencies (CSLLEA 2008). The population served by an agency is pulled from the 2014 UCR Population as listed in the FBI Police Employee Data from the same year.

The LEAR variable indicating policing activities is not always fully reflective of the mandate of an agency. Particularly in large and medium sized cities, it is common for both a police and sheriff’s department to operate within city limits. However, the sheriff’s department may be tasked with court security, civil processes, and jail security in contrast to the police department which engages in patrol and investigations. In many of these cases the LEAR population variable is missing, and the officer count may be substantially greater than the true number of officers engaging in policing activities. I omitted such agencies.
**Locality Population Size:** I use intercensal population estimates from the Weldon Cooper Center for Public Service, Demographics Research Group for estimates of the 2014 locality level population size. I developed crosswalks matching the counties and cities in these population estimates to the courts with jurisdiction over them. One city in my sample is split across two circuit court jurisdictions, so in this case I applied half of the estimated population of the city to each relevant court jurisdiction.

I used intercensal population estimates rather than a sum of LEAR population estimates to head off potential issues with double counting in shared jurisdictions as well as missing data issues which could respectively inflate and deflate the denominators of the calculated shares. However, as a data check I compared the population shares calculated using a sum of LEAR populations to my primary share measure (using intercensal estimates). The departments classified as “major" were unchanged.

### A.2 Case Data

As described in the body of the paper, I obtained charge data from VirginiaCourtData.com. To provide more clarity on the form this data takes, I include here an example of the web-based case information that the owner of this repository scrapes (Schoenfeld, 2017). To maintain the privacy of the defendant, I redacted information that could be used to identify this specific record online.

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<th>Military</th>
<th>Traffic Fatality:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
</tr>
</tbody>
</table>

---

I aggregated this charge-level data to a case level before forming the court-level panel. To identify which charges were associated with a common case, I began by using the "case number" defined by the court. In reality, these should be considered charge numbers, since the values provided for each charge in a given
case are generally speaking related but unique. While the District Court Clerk’s Manual (2021) recommends a common method for assigning case numbers \((\text{case type})+\text{year}+(\text{sequential number})+\text{suffix})\), the Circuit Court Clerk’s Manual (2020) acknowledges variations in numbering conventions across courts. For charges within each court, I first group charges into cases based on the criteria that charges are treated as a single case if they belong to the same defendant and the last 4 non-suffix digits of the case number are either identical or sequential. I then expand those groupings to include any additional charges that were filed against the same person on that same date—even if the case numbers appear unrelated.

In the included example, all six entries represent charges against the same individual. However, they are grouped as four distinct cases. The first three would be grouped together on either the case number or filing date criteria: since 4309, 4310, and 4311 are sequential these are treated as one case and they also were all filed on the same date. In contrast, none of the remaining charges show related case numbers or identical filing dates, so they are treated as separate—even though two of the charges were filed only two weeks apart.

<table>
<thead>
<tr>
<th>casenumber</th>
<th>fileddate</th>
<th>person_id</th>
<th>case_id_ba-s</th>
</tr>
</thead>
<tbody>
<tr>
<td>4310-00</td>
<td>21138</td>
<td>100000000000005</td>
<td>11</td>
</tr>
<tr>
<td>4311-00</td>
<td>21138</td>
<td>100000000000005</td>
<td>11</td>
</tr>
<tr>
<td>4309-00</td>
<td>21138</td>
<td>100000000000005</td>
<td>11</td>
</tr>
<tr>
<td>8756-00</td>
<td>21319</td>
<td>100000000000005</td>
<td>13</td>
</tr>
<tr>
<td>2897-00</td>
<td>21404</td>
<td>100000000000005</td>
<td>14</td>
</tr>
<tr>
<td>3327-00</td>
<td>21418</td>
<td>100000000000005</td>
<td>15</td>
</tr>
</tbody>
</table>

For the analyses in which I omit probation violations and similar offenses, I exclude these charges before grouping the cases. For example, if an individual was sentenced to probation due to a charge on Jan 1, 2015 and then on Jan 1, 2016 was charged with violating that probation and another offense, they would appear in the data as having two separate cases, one stemming from the 2015 event and the other from the 2016 event.
B Supplementary Analyses for Main Tables

In the main body of the paper I show overall ATTs for all outcomes, and intermittently show event study plots for outcomes in which I wanted to highlight some aspect of the heterogeneity of results over time. In this appendix I show additional event study plots and results from tests that indicate the plausibility of the parallel trends assumption. I also show results from restricting the analysis to only those courts that adopted body-worn cameras during the sample period, with the 2018 cohort serving as a control. Lastly, the sentencing outcome that I use in my main results is a coarse measurement: I only look at whether someone was sentenced to serve a positive amount of time or not. I show here additional results under various sentencing outcome measures.

<table>
<thead>
<tr>
<th>Table B.1: BJS estimates, parallel trends tests</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of pre-periods</td>
</tr>
<tr>
<td>------------------------</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Cases</td>
</tr>
<tr>
<td>Civilization Effect Cases</td>
</tr>
<tr>
<td>Multi-charge Cases</td>
</tr>
<tr>
<td>Share Misdemeanor</td>
</tr>
<tr>
<td>Prosecutor Dropped Charge</td>
</tr>
<tr>
<td>Case Certified</td>
</tr>
<tr>
<td>Guilty</td>
</tr>
<tr>
<td>Sentenced to Time</td>
</tr>
<tr>
<td>Disposition: 1 year</td>
</tr>
<tr>
<td>Circuit</td>
</tr>
</tbody>
</table>

Note: Values shown are the p-values for a test of parallel trends in the 4 and 8 periods prior to BWC implementation. The test used is described in Borusyak, Jaravel, and Spiess (2021). Case filing panels were not separated based on case type, and so District (A) includes p-values for the tests on all district court filings. For case processes and outcomes, District (A) shows results for the misdemeanor sample and District (B) shows results for the felony sample.

B.1 Adopter Only Results

We may be concerned that the untreated group systematically differs from the treated group in ways that will bias the estimates presented in the main results. From the descriptive statistics presented in table 3, we can observe some level differences across these two groups. At the circuit court level, treated courts saw over twice as many cases filed than did untreated courts. Additionally, treated courts were more likely to have a
salaried public defender office to represent indigent clients, and defendants received longer sentences. At the same time, the courts showed numerous similarities including in the rates at which fines were imposed and case dispositions. While it appears that the communities in which the courts are situated differed between adopters and non-adopters, the cases within the courts appeared to proceed similarly regardless of whether the court was in a treated jurisdiction or not. And, the parallel trends test results show that even where courts differed in levels they did not tend to differ significantly in trends. For these reasons, I presented my primary specifications using both not-yet-treated units and never-treated units to impute counterfactual outcomes for treated observations.

Nonetheless, police during this time had to opt into body-worn camera programs, and so at least at the policing stage it is very reasonable that there would be meaningful differences across the adopting and non-adopting groups, some of which I would not be able to observe. Additionally, we may be concerned that there are similar differences across adopting and non-adopting courts that could result in biased estimates of the effects of body-worn camera adoption on case filings, processes, and outcomes. To address this concern, here I show results using only data from those courts for which law enforcement adopted body-worn cameras during the sample period. I include the 2018 adopters exclusively as a control group, and accordingly shorten the analysis window to only those cases filed by the end of 2017. I show case filing results from this exercise in table B.2.a, case process and outcome results in B.2.b., and results from parallel trends tests in B.2.c.

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Case Count</th>
<th>Civilization Case Count</th>
<th>Share Multicharge Cases</th>
<th>Share Misdemeanor</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Effect</td>
<td>0.022</td>
<td>0.002</td>
<td>-0.034**</td>
<td>--</td>
</tr>
<tr>
<td>Circuit Court</td>
<td>0.050</td>
<td>0.063</td>
<td>0.010</td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>175.8</td>
<td>8.2</td>
<td>0.477</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>3,952</td>
<td>3,952</td>
<td>3,952</td>
<td></td>
</tr>
<tr>
<td>Treatment Effect</td>
<td>0.055†</td>
<td>-0.061</td>
<td>-0.004</td>
<td>-0.001</td>
</tr>
<tr>
<td>District Court</td>
<td>(0.028)</td>
<td>(0.042)</td>
<td>(0.004)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Mean</td>
<td>3,578.0</td>
<td>38.2</td>
<td>0.192</td>
<td>0.924</td>
</tr>
<tr>
<td>Observations</td>
<td>2,736</td>
<td>2,736</td>
<td>2,736</td>
<td>2,736</td>
</tr>
</tbody>
</table>

Cluster-robust standard errors in parentheses

* * * p<0.01, * p<0.05, † p<0.1

Note: Means of columns 1 and 2 reflect the average counts for the variable but the outcome used is ln(count+1) for civilization case counts in both courts and case counts in the circuit court, and ln(count) for the remainder.

I first note that in both the main results and the adopters only sample, some outcomes do not have "well-behaved" pre-adoption trends. For example, in the adopters-only sample, there is an upward trend to the district court case count variable prior to body-worn camera adoption. As such, although the estimated coefficient on the body-worn camera treatment variable for adopters only differs from the main (null) result, we should be cautious about assigning causality to this statistically significant ($\alpha = 0.10$) 5.5% increase in district court case filings; the differing trends will cause an overestimate in this case.

We do not have the same concerns with the parallel trends assumption for the other district court
outcomes, and in these cases I find similar results to the main specification. I do not find evidence for a change in the share of multi-charge cases after body-worn camera adoption, and I do find evidence suggesting a meaningful reduction in the number of civilization effect cases. However, the magnitude of this estimate is reduced from -11.5% to -6.1% in the adopters only sample and is no longer statistically distinguishable from zero.

At the circuit court level, the estimates vary more meaningfully. Where previously I found a statistically significant reduction in both case filings and civilization effect cases, under the the treated-only sample I find no evidence of such reductions. For civilization effect charges, this is at least partly attributable to prior concerns with parallel trends violations in the main specification, which would result in an underestimate of the true effect.

Using the limited sample, I find no statistically significant changes in civilization effect cases or cases overall. In the latter case, the formerly statistically significant reduction of 7.4 percent is now an insignificant change.

Using both methods I find no significant changes to the number of cases overall or the share of these cases carrying multiple charges after body-worn cameras are introduced. Under this method, in all cases, the parallel trends tests do not show statistically significant evidence of violations in the 4 or the 8 quarters before body-worn camera implementation although the overall case count in the district court and the share of multi-charge cases in the circuit court only narrowly miss an $\alpha = 0.1$ cutoff.
<table>
<thead>
<tr>
<th></th>
<th>District (A)</th>
<th>District (B)</th>
<th>Circuit</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>4</td>
<td>8</td>
<td>4</td>
</tr>
<tr>
<td>Cases</td>
<td>0.080</td>
<td>0.167</td>
<td>0.664</td>
</tr>
<tr>
<td>Civilization Effect Cases</td>
<td>0.380</td>
<td>0.440</td>
<td>0.881</td>
</tr>
<tr>
<td>Multi-charge Cases</td>
<td>0.452</td>
<td>0.744</td>
<td>0.176</td>
</tr>
<tr>
<td>Share Misdemeanor</td>
<td>0.351</td>
<td>0.585</td>
<td>--</td>
</tr>
<tr>
<td>Prosecutor Dropped Charge</td>
<td>0.947</td>
<td>0.574</td>
<td>0.150</td>
</tr>
<tr>
<td>Case Certified</td>
<td>--</td>
<td>--</td>
<td>0.902</td>
</tr>
<tr>
<td>Guilty</td>
<td>0.630</td>
<td>0.768</td>
<td>0.469</td>
</tr>
<tr>
<td>Sentenced to Time</td>
<td>0.324</td>
<td>0.704</td>
<td>0.471</td>
</tr>
<tr>
<td>Disposition: 1 year</td>
<td>0.833</td>
<td>0.767</td>
<td>0.732</td>
</tr>
</tbody>
</table>

*Note: Values shown are the p-values for a test of parallel trends in the 4 and 8 periods prior to BWC implementation. The test used is outlined in Borusyak, Jaravel, and Spiess (2021). Case filing panels were not separated based on case type, and so District (A) includes p-values for the tests on all district court filings. For case...*
B.2 Selected Event Study Plots

Figure x: District Court Event Study, Multi-charge cases

Figure x0: Circuit Court Event Study, Multi-charge cases
B.3 Additional Sentencing Outcomes

In addition to a binary measure of whether someone was sentenced to serve time, we may be interested in the sentence length. Here I show results for various binned sentence length variables as well as the average sentence length. Additionally, I show an indicator for whether any of this sentence was suspended: some defendants are given an option to forgo jail or prison as long as they meet some conditions established by the court. Should these defendants fail to meet the conditions, the suspended portion of the sentence comes into full effect.

These supplementary sentence length variables are constructed using both regular and suspended sentences, so a separate suspended sentence variable provides additional clarity to the actual impact on defendants. Another key issue to note with the sentence length variables is that I treat the lengths as additive. In other words, for this analysis I assume all sentences are served consecutively. While my data set does include a variable describing whether sentences are concurrent or consecutive, it is often (≈ 60% of cases with multiple positive sentence times) missing, and this missingness is not uniformly distributed across localities. When reported, sentences were consecutively assessed rather than concurrently twice as often.

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Sentenced to 30 days</th>
<th>Sentenced to 6 months</th>
<th>Sentenced to 1 year</th>
<th>Sentenced to 3 years</th>
<th>Sentenced to 5 years</th>
<th>Sentence Time</th>
<th>Sent. Suspended</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Effect</td>
<td>-0.005</td>
<td>-0.009</td>
<td>-0.012†</td>
<td>--</td>
<td>--</td>
<td>-5.5</td>
<td>-0.007</td>
</tr>
<tr>
<td>Circuit Court</td>
<td>(0.012)</td>
<td>(0.008)</td>
<td>(0.007)</td>
<td>--</td>
<td>--</td>
<td>(4.3)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Mean</td>
<td>0.241</td>
<td>0.136</td>
<td>0.119</td>
<td>--</td>
<td>--</td>
<td>73.8</td>
<td>0.228</td>
</tr>
<tr>
<td>Observations</td>
<td>4,129</td>
<td>4,129</td>
<td>4,129</td>
<td>--</td>
<td>--</td>
<td>4,129</td>
<td>4,129</td>
</tr>
<tr>
<td>Treatment Effect</td>
<td>-0.011*</td>
<td>-0.003</td>
<td>--</td>
<td>--</td>
<td>--</td>
<td>-1.9†</td>
<td>-0.011†</td>
</tr>
<tr>
<td>District Court (Fel.)</td>
<td>(0.006)</td>
<td>(0.002)</td>
<td>(1.1)</td>
<td>--</td>
<td>--</td>
<td>(0.006)</td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>.183</td>
<td>0.036</td>
<td>27.4</td>
<td>0.190</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>4,182</td>
<td>4,182</td>
<td>4,182</td>
<td>4,182</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Race, sex covariates</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td></td>
</tr>
<tr>
<td>Offense year, quarter FE</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td></td>
</tr>
<tr>
<td>Locality FE</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td></td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

**p<0.01, * p<0.05, † p<0.1
C Extended Analyses for Main Tables

Here I show the robustness of my results to varying a) sample selection criteria b) treatment status/threshold c) the outcomes measured and d) the estimators used. In particular, I include results from alternative estimators proposed in the emerging staggered difference-in-differences literature.

C.1 Sample Restrictions

In the body of the paper I use data only from those localities for which I observe at least 8 quarters of pre-adoption case data and at least 4 quarters of post-adoption data. I additionally restrict my case data to allow adequate time for cases to be disposed. As a result, my choice of the final period for case data affects which courts are included when I calculate the effects previously presented. In particular, district court cases conclude more quickly than circuit court cases and it is possible that my decision to use only cases filed by Q1 2019 is overly conservative for the district courts.

In this section I show district court results under a less conservative timing threshold, using cases filed by the end of 2019. For the misdemeanor subsample, 0.38 percent of cases filed in Q1 2019 are missing disposition information for at least one charge. In contrast, by Q4 2019, this grows exponentially to 3 percent. For felonies, the shares are 0.76 percent and 5.5 percent, respectively. The growth in disposition missingness is shown in figures B.1.a and B.1.b.
C.2 Treatment Status/Threshold

In the primary analyses for this paper I use a 25 percent population or officer threshold to determine which local law enforcement agencies, if they implement a body-worn camera program, would qualify their local court jurisdiction to be classified as “treated”. However, one may think that this threshold is too low and so to check the robustness of my results to this selection, I additionally apply a 50% threshold.

In practice, whether I apply the 50% or the 25% threshold infrequently changes whether and when a locality is classified as treated, shown in tables B.2.a and B.2.b. Most court localities are served by only one major law enforcement agency—like a city police department or county sheriff—which satisfies both threshold criteria. However, in approximately 1/3 of localities there is another candidate department. At both the circuit and district court levels, the treatment status is the same regardless of which threshold I use for more than 96 percent of the localities for which I observe treatment status under both thresholds. Additionally, there are a few localities for which I know treatment status under the 50 percent threshold but do not have information for all of the law enforcement agencies between the 25 and 50 percent thresholds. Similarly, amongst adopters, the timing of adoption is largely unchanged when I adjust the threshold; only 5 localities change treatment quarter. However, these 5 show substantial timing differences with the smaller adopting agencies initiating their programs at least 2 years earlier.

I show results in tables C.2.a and C.2.b for modified case filing and case process/outcomes when the 50% threshold is used instead of my primary specification. Overall I find that these results tell the same story as that contained in the main body of the paper; body-worn cameras have a limited effect on case filings but do not appear to change aggregate case outcomes or case processes. Results using both thresholds
show no effects on the share of multi-charge cases, but a moderate reduction in civilization effect cases at both the district and circuit court levels.

### Table B.2.a: Comparison of treatment status by threshold

<table>
<thead>
<tr>
<th>Threshold</th>
<th>Circuit Court</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Untreated</td>
<td>Treated</td>
<td>Unclassified</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Untreated</td>
<td>30</td>
<td>0</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treated</td>
<td>4</td>
<td>68</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unclassified</td>
<td>3</td>
<td>1</td>
<td>14</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

### Table B.2.b: Comparison of treatment status by threshold

<table>
<thead>
<tr>
<th>Threshold</th>
<th>District Court</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Untreated</td>
<td>Treated</td>
<td>Unclassified</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Untreated</td>
<td>33</td>
<td>0</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treated</td>
<td>4</td>
<td>70</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unclassified</td>
<td>2</td>
<td>1</td>
<td>15</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

### Table C.2.a: BLS estimates, case filing effects 50% threshold

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Case Count</th>
<th>Civilization Case Count</th>
<th>Share Multicharge Cases</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Effect</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Circuit Court</td>
<td>-0.064</td>
<td>-0.110**</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.009)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Mean</td>
<td>147.4</td>
<td>7.1</td>
<td>0.487</td>
</tr>
<tr>
<td>Observations</td>
<td>6,420</td>
<td>6,420</td>
<td>6,410</td>
</tr>
<tr>
<td>Treatment Effect</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>District Court</td>
<td>-0.017</td>
<td>-0.097**</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.006)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Mean</td>
<td>3,513.6</td>
<td>33.7</td>
<td>0.187</td>
</tr>
<tr>
<td>Observations</td>
<td>4,972</td>
<td>4,972</td>
<td>4,972</td>
</tr>
</tbody>
</table>

Cluster-robust standard errors in parentheses

**p<0.01, * p<0.05, † p<0.1

*Note: Means of columns 1 and 2 reflect the average counts for the variable but the outcome used is ln(count + 1) for civilization case counts in both courts and case counts in the circuit court, and ln(count) for the remainder.*
C.3 Alternative Estimators

In the body of the paper I use the BJS imputation estimator to estimate the effects of law enforcement body-worn camera implementation on criminal courts. I discuss the benefits of this estimator over the traditional TWFE estimator, and also list alternative estimators that have emerged in recent years to fill similar econometric gaps. Here I show alternative results using the standard difference-in-differences.

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Case Count</th>
<th>Civilization Case Count</th>
<th>Share Multicharge Cases</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Effect</td>
<td>-0.050</td>
<td>-0.091**</td>
<td>-0.009</td>
</tr>
<tr>
<td>Circuit Court</td>
<td>(0.009)</td>
<td>(0.033)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Mean</td>
<td>148.8</td>
<td>7.2</td>
<td>0.486</td>
</tr>
<tr>
<td>Observations</td>
<td>6,240</td>
<td>6,240</td>
<td>6,230</td>
</tr>
<tr>
<td>Treatment Effect</td>
<td>-0.010</td>
<td>-0.101**</td>
<td>-0.002</td>
</tr>
<tr>
<td>District Court</td>
<td>(0.027)</td>
<td>(0.034)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Mean</td>
<td>3548.6</td>
<td>34.3</td>
<td>0.188</td>
</tr>
<tr>
<td>Observations</td>
<td>4,796</td>
<td>4,796</td>
<td>4,796</td>
</tr>
</tbody>
</table>

Cluster-robust standard errors in parentheses

**p<0.01, *p<0.05, † p<0.1

Note: Means of columns 1 and 2 reflect the average counts for the variable but the outcome used is ln(count + 1) for civilization case counts in both courts and case counts in the circuit court, and ln(count) for the remainder.
D  Back of Envelope Caseload Calculations

In the body of the paper I reference a data point showing the average caseload for Virginia indigent defenders before body-worn camera adoption was 320 cases and cite this as evidence toward attorneys facing binding time constraints. A simple back-of-the-envelope calculation shows why this is the case. I show in Figure D.1 an attorney’s production possibilities frontier under ABA guidelines, as well as the possible combinations of felony and misdemeanor cases that an attorney can take to total 320. Attorneys representing 320 cases can do so while adhering to ABA guidelines if their case combination lies on or under the ABA Guidelines curve (shown in green). This will only happen if they represent 48 or fewer felonies (15 percent of their caseload). However, the same report shows that felonies comprise over 30 percent of the cases overall, and a 3:4 ratio of felonies to misdemeanors when case types such as parole violations are excluded. Thus, it is reasonable to conclude that the caseloads faced by public defenders in Virginia before body-worn camera adoption lie outside the ABA production possibilities frontier and thus indicate a binding time constraint under the ABA guidelines.

![Figure D.1: PPF for defense attorneys under ABA guidelines and caseloads for VA public defenders](image)