Consequences of Body-worn Cameras on the Court System

Katie Bollman

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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. In addition to monitoring policing, this tool generates large quantities of new data for criminal courts: footage of criminal defendants. These data can provide evidence pertinent to criminal cases, but do so at a cost of attorney time. To investigate whether body-worn camera adoption changes policing and court outcomes, I use rich criminal charge data from Virginia state courts from 2006-2020 and a new, self-collected body-worn camera data set. Leveraging the staggered adoption of body-worn cameras by local law enforcement through a difference-in-differences estimation strategy, I test for evidence of policing effects in changes to case filings. 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. I then use a set of case disposition, sentencing, and timing outcomes to test for evidentiary and attorney time use effects. I find that, contrary to expectations, both aggregate case processes and resolutions are unresponsive to the influx of data generated by body-worn camera footage.

1 Introduction

Defining and implementing 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.[1]

Although now commonplace, body-worn cameras are a recent technological advancement for law enforcement in the United States. Only a decade ago, in 2010, 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 body-worn camera use continues, and between 2020 and mid-2021 six states mandated 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, criminal justice practitioners warn of broader effects of body-worn cameras on policing and the courts. First, body-worn camera footage may increase the probability or consequences of a criminal conviction, leading to behavioral changes amongst police and members of the public. More specifically, body-worn cameras

[1] One recent poll shows 85 percent of Republicans and 94 percent of Democrats favor body-worn camera mandates (Kull, 2020).

may serve as a deterrent for police misconduct and certain visible criminal activities, changing the set of cases that enter the courts. Next, for those cases that are not deterred (i.e. that enter the courts), 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. While, alone, these data may be beneficial, accessing the evidentiary value of the 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 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), criminal responses to surveillance (Gómez, 2021; Piza et al., 2019; Gonzalez-Navarro, 2013), and criminal deterrence more broadly (Chalfin & McCrary, 2017). 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. Outside of criminal courts, Çubukçu et al. (2021) find evidence that body-worn camera footage provides influential evidentiary benefits for citizen complaints against police.

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. Because multiple law enforcement agencies can operate within a single court jurisdiction, the sampling structure for these data encourage law enforcement agency-level analyses rather than court-level analyses. 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 less 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 from 2008-2020.[2] I use these 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.

When analyzing these data I am conscious of the emerging econometric literature which

[2]These years reflect those for which I have both district court and circuit court data. I additionally use circuit court-only data dating back to 2006.

demonstrates some shortcomings of the traditional difference-in-differences 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 I discuss and implement both the traditional two-way fixed effects estimator and an alternative imputation estimator proposed by Borusyak, Jaravel, and Spiess (2021). In the case of body-worn cameras in Virginia, I find that the traditional two-way fixed effects estimator performs well, providing results congruous to those of the new imputation estimator.

Across two court levels and a variety of outcomes designed to measure changes in the number and composition of case filings, processes, and outcomes, I find a strong pattern of results. While body-worn cameras civilized a small subset of police interactions, reducing the prevalence of a set of charges encompassing assault on police, resisting arrest, and similar offenses, they do not appear to have altered police behaviors as a whole. The number of cases filed and the share of those cases involving multiple charges did not change after police began using body-worn cameras. Further, there was no evidence that body-worn camera-induced civilization effects systematically reduced the severity of the charges filed. Although body-worn cameras generate substantial data, and despite practitioner reports of an indigent defense system buckling under the weight of this data, I do not find evidence that cases systematically proceeded through the courts differently or resolved differently after police began using body-worn cameras. One potential explanation for this result could be that evidentiary benefits of footage offset the attorney time use effects for a net zero effect. However, I show that this explanation does not hold if attorneys substitute time across the cases they represent.

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 extensive 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

Nationally, body-worn cameras 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 amongst non-adopters to use the technology in the future. 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 nonetheless reported high rates of officer and community support for body-worn cameras.

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.[3] Because the LEMAS-BWCS samples law enforcement agencies and does not include adoptions that took place 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 the population exposed to body-worn cameras also increased rapidly. Figure 3 shows the size of the population in Virginia living in a court jurisdiction where a major law enforcement agency used body-worn cameras for each year from the earliest adoption in 2007 through 2020.

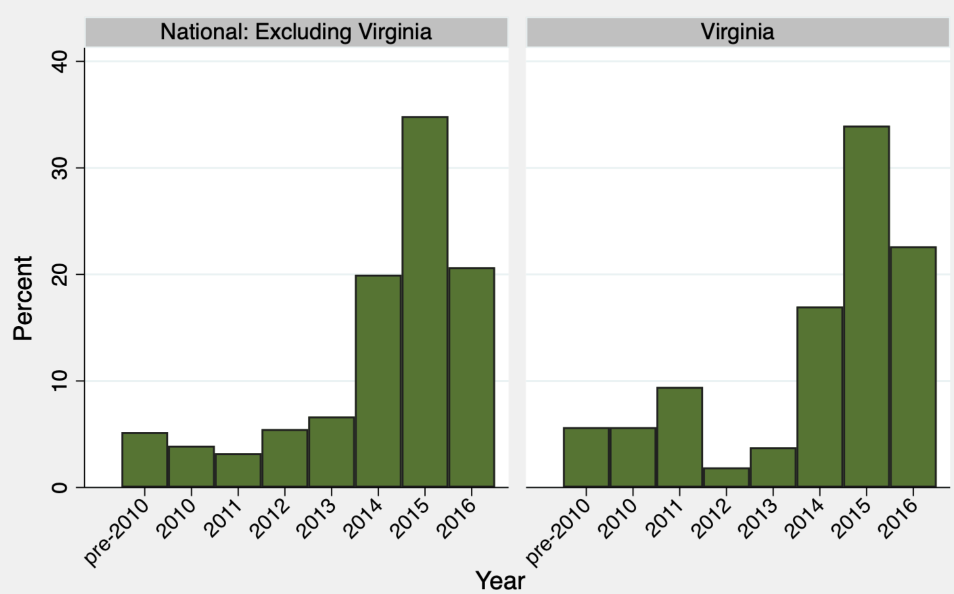

Figure 1: Year of BWC adoption for law enforcement agencies in LEMAS-BWCS (adopters only)
National adopters: n = 1862 (out of 3843 respondents)
Virginia adopters: n = 53 (out of 85 respondents)

Respondents to the LEMAS-BWCS also clarified why and how they implemented body-

[3]Respondents for LEMAS surveys are drawn from the Department of Justice's Law Enforcement Agency Roster (2016) which shows 293 agencies within Virginia.

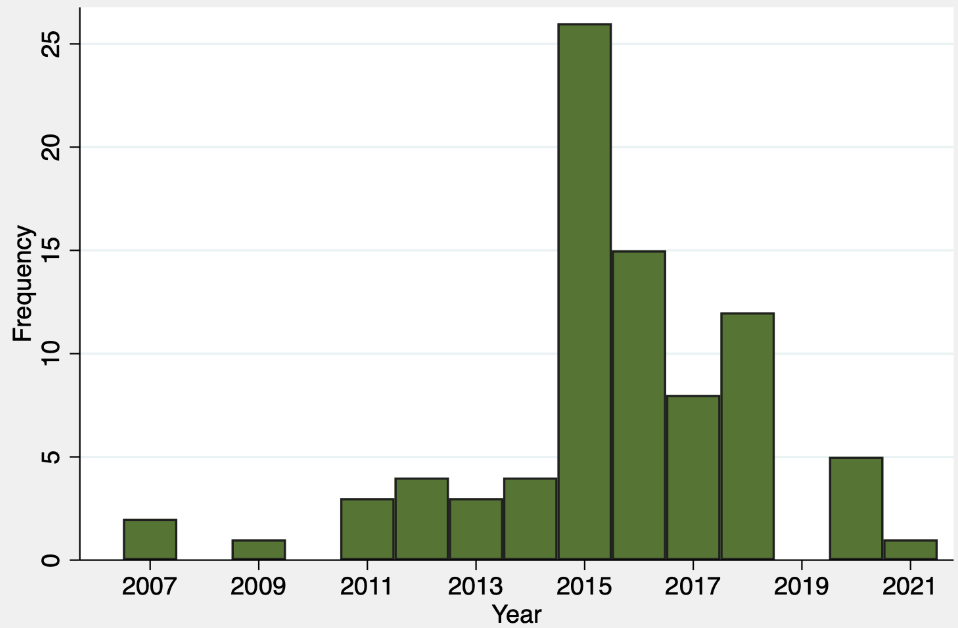

Figure 2: Year of BWC adoption by district court jurisdiction, Virginia
Source: author's data collection

worn cameras. Although body-worn camera footage can be used in criminal courts, agencies rarely based adoption decisions on expected effects on the courts. Instead, agencies cited 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)[4] as the primary reasons they adopted body-worn cameras. Yet while expected effects in the courts rarely drove adoption decisions, agencies reported that they *did* anticipate that body-worn cameras would affect court cases; 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 but most (78.8 and 69.8 percent, respectively) cited these as contributing factors. Agencies that did not obtain body-worn cameras by the time of the survey 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

[4]Percentages exclude respondents whose agency's primary purpose was to conduct a pilot program.

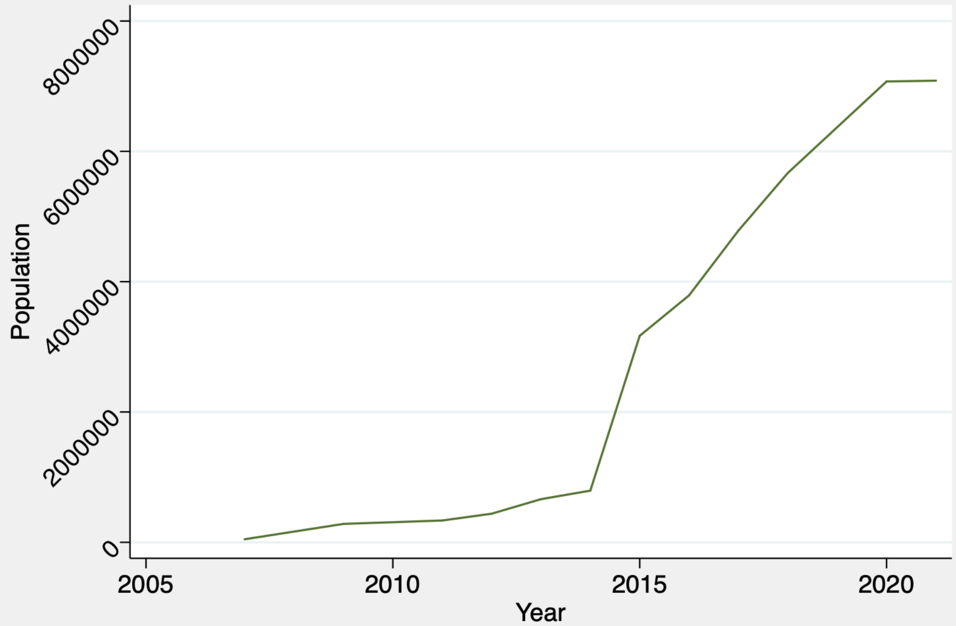

Figure 3: Population living in a district court jurisdiction where BWCs are used
source: author's data collection, intercensal population estimates (described in data appendix)

then the 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, the LEMAS-BWCS shows that 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 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, 2014a), and 190,000 in Virginia State Circuit Courts (Office of the Executive Secretary, 2014b), 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.[5] However, they differ in the scope of the cases they hear: district courts hold jurisdiction over misdemeanor cases whereas the circuit courts hear felonies.[6] Oftentimes geographic court boundaries contain multiple law enforcement 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.

Cases enter the courts through two primary mechanisms. Police officers can issue a Virginia Uniform Summons, which initiates a filing in the district court. These summonses are common in misdemeanors and do not require that a defendant be held in custody while awaiting court hearings. Alternatively, police can arrest defendants. When this happens a local magistrate serves as an intermediate step between law enforcement and 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 describes that the magistrate needs only 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).

After a summons is issued or the charges advance from the magistrate's office, the outcomes of the charges can be influenced by three court actors: a judge, prosecutor, and defense attorney.[7] I provide a basic case road map outlining the entities involved in various states of

[5] In a few places, multiple district courts operate within a single circuit court jurisdiction.

[6] 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.

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

Table 1: Case Roadmap			
Stage	Case Activity	Entities Involved	Activity Summary, *Body-worn camera role*
1	(Alleged) Offense	Defendant Police	Defendant allegedly commits offense; may or may not be seen by police. *Video may be captured*
2	Summons issued/ Arrest and booking	Defendant Police	Officer may release defendant with summons for later court date or arrest defendant. *Video captured*
3	Warrant/ Charge issued	Defendant Police Magistrate	If arrested, defendant appears before a judicial officer to determine whether charges proceed. *None*
4	Court hearings, preparation	Defendant Judge Attorneys	Court hears evidence, disposes charges, pronounces sentences. Attorneys negotiate plea agreements, argue for preferred dispositions and sentences. *Video reviewable*

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 sentences[8]; and both prosecuting and defense attorneys lobby for preferred dispositions and sentencing.[9]

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.[10] 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.

[8]This 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.

[9]"Dispositions" are the rulings or resolutions to cases, for example "guilty".

[10]Data 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).

A key motivation for this paper is the reports from indigent defenders warning that the marginal time required to review body-worn camera footage exceeds attorney time constraints. With this in mind it is valuable to note that prior to body-worn camera adoption, full-time indigent defenders in Virginia *were* likely to face binding time constraints for their caseloads. In FY 07/08, before body-worn cameras became widespread, public defenders in Virginia managed on average 320 cases per attorney per year (Kleiman and Lee, 2010). I provide evidence in Appendix D that these caseloads exceed the American Bar Association's recommendation of a maximum of 150 felonies *or* 400 misdemeanors (American Bar Association, 2009) annually. While assigned counsel are employed on a case-by-case basis, they too face time constraints in the form of compensation caps. An assigned counsel attorney would need to represent clients against 138 felony charges in Virginian circuit courts or over 500 misdemeanor charges in district courts to reach merely the 10th percentile of national attorney earnings (Bureau of Labor Statistics 2021).[11]

3 Literature and Conceptual Framework

Criminal deterrence is a widely studied and politically salient topic because crime and the criminal justice system in the U.S. are both broad-reaching and multi-layered in their consequences. Criminal activity not only harms victims, but imposes costs on society as a whole. Victims of violent crimes experience lower levels of mental well-being, as do non-victims with higher levels of crime in their communities (Cornaglia et al., 2014). Local crime risks inhibit wealth accumulation by lowering housing prices (Linden & Rockoff, 2008) and exposure to crime reduces academic performance for youth (Schwartz et al., 2016).

Economic theory dating back at least to Becker's influential crime model (1968) suggests that crime reduction may come about by increasing the expected costs or reducing the expected benefits to engaging in criminal activity. Empirically there is evidence that the determinants of criminal activities range from inequality and other socioeconomic factors (Kelly, 2000; İmrohoroğlu et al. 2000; Fajnzylber et al., 2002(a); Fajnzylber et al., 2002(b);

[11] There are some opportunities for fee waivers which would reduce this number. However in a companion paper (Bollman, 2021) I calculate fee waivers to be rarely granted, given for only about 3.3% of charges. I also find that attorneys rarely report working fewer than the maximum compensated hours per case.

Grogger, 1998; Buonanno & Montolio, 2008), to alcohol access (Heaton, 2012; Groönqvist & Niknami, 2014), social networks (Damm & Dustman, 2014; Billings et al. 2019), family background or adverse childhood experiences (Doyle, 2008; Currie & Tekin, 2012; Eriksson et al., 2016) and more. Even schooling and entertainment can affect crime by incapacitating would-be offenders (Dahl & DellaVigna, 2009; Jacob & Lefgren, 2003).

The most direct lever through which governments seek to reduce crime is through policing. Police can directly disrupt criminal activities and increase the costs of criminality by bringing defendants into the courts, where criminal convictions and punishments carry both short and long-term consequences. Studies repeatedly show that police are also effective in deterring criminal activity (Evans & Owens, 2007; Draca et al., 2011; Vollaard & Hamed, 2012 ; Chalfin & McCrary, 2018; Weisburd, 2021), and in 2018 states spent $119 billion on police to do so (Urban Institute, 2021). Upon conviction, some offenders are incapacitated from criminal activities through incarceration (Barbarino & Mastrobuoni, 2014; Mastrobuoni, 2019), costing U.S. states an average of over $30,000 per prison inmate each year (Mai and Subramanian, 2017).[12] While states incur financial costs for housing inmates, convicted defendants pay for their criminal activities in both time and future opportunities. 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). Through these channels and the peer networks formed within jails and prisons convictions introduce future costs to society by spurring additional criminal activity upon release and increasing reliance on public assistance programs (Bayer et al., 2009; 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 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

[12] Calculated using data available from 45 states.

of attorney caseloads– in other words, change the amount of time required to litigate cases (attorney time use).

And, while police are tasked with reducing crime amongst the public, misconduct, criminality, and general levels of use-of-force amongst police themselves are also salient policy concerns. Public advocacy for body-worn cameras grew against the backdrop of a police shooting in Ferguson, Missouri that ignited large-scale protests (BBC News 2014) and a series of highly publicized incidents of police misconduct caught on camera. These incidents are costly to budgets, trust in criminal justice institutions, and social well-being. In 2019 the City of Chicago, on behalf of the Chicago Police Department, paid nearly $47 million in settlements and court awards stemming from police misconduct (City of Chicago, 2020).[13] Indirect exposure to police violence has been shown to reduce academic attainment amongst high school students (Ang, 2021) with disparate effects across racial groups. There is also a correlation between perceptions of police use-of-force and trust in police across racial groups; a 2015 survey showed black individuals were both more likely to view police violence as a problem and less likely to trust the police than white or Hispanic individuals (AP-NORC, 2015). In light of this, body-worn cameras are intended to enhance accountability and trust of police officers and induce civilization effects for police as well as members of the public.

In the following subsections I detail all three prospective channels through which body-worn cameras can affect policing and the courts, present the existing evidence of their roles, and describe 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

As Becker (1968) noted, the "supply" of crimes should be inversely related to the likelihood a criminal is discovered and convicted. As an evidence-generating technology, body-worn

[13] For context, this is approximately equal to the City's budget for senior service programs through the Department of Family and Support Services in the same year (City of Chicago, 2019).

cameras reduce some of the noise around allegations of criminal behavior 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 "aggressive(ly)" 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.[14] 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

[14] Body-worn cameras are worn on the outside of an officer's uniform, typically affixed to clothing, equipment, or accessories. Because of this, they are observable to members of the public when interacting with police.

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. The role of monitoring, which can enable a real-time response to criminal activity and is empirically shown to deter crime (Gonzalez & Komisarow, 2020), 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.

Another technology, the Lojack car recovery device, lends empirical support to the theory of criminal deterrence in Becker's model. Lojacks are easily concealed location transmitters placed in cars to aid in the recovery of stolen vehicles. Unlike CCTV or body-worn cameras, this technology has limited ability to link a particular defendant to an offense. However, it does increase the probability of discovery and, perhaps more importantly, reduces the expected value of the stolen goods by increasing the likelihood that the stolen vehicle is recovered. In practice, Lojack adoption by a car manufacturer in Mexico deterred an estimated 48 percent of car thefts among Lojack-equipped vehicles (Gonzalez-Navarro, 2013).

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 adoptions 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.[15] 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

[15] The global meta-analysis of local body-worn camera impact evaluations (Ariel et al. 2016) actually showed higher rates of assaults on police after adoption.

physically or verbally, then we can expect to find broader changes in the charges 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.[16] 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

[16] Resisting arrest charges here were tested as a frequency instead of a share of arrests so this does not rule out evidence of a civilization effect in this outcome.

U.S. district courts in 2009 (Sourcebook, 2009).[17] 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 if the typical police stop is merited and/or the core components of a typical police report align with body-worn camera footage. [18] Ultimately, the balance of these elements determines whether additional evidence benefits or harms defendants on average– which is itself an empirical question.[19] 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

[17]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

[18]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

[19]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.

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 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).[20]

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

[20] The offices employ non-attorney personnel as well.

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 coverage. Within Virginia the LEMAS-BWCS data included sample of 85 local law enforcement agencies and a survey of Virginia Commonwealth's Attorneys 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 data 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 broad view of the effects of body-worn camera footage on cases in the court as a whole, encompassing direct effects of body-worn cameras on cases with footage and spillover effects on cases without footage. I combine this court-level data on body-worn camera proliferation with charge-level data from Virginia courts aggregated 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.[21] I detail this designation 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.[22][23] 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 depart-

[21] Only agencies with policing duties were included and I omitted some sheriff's offices that primarily handled jail and court security.

[22] 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.

[23] 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.

ment websites and local media. Ultimately I obtained complete data for 111 district court jurisdictions including 78 that adopted body-worn cameras by 2019 and 106 circuit court jurisdictions, 76 of which adopted by 2019. These comprise nearly 90 percent of state district and circuit courts in Virginia.[24]

4.2 Criminal Case Data

I use charge-level data for criminal cases filed in Virginia district courts between January 2009 and March 2019 and Virginia circuit courts between January 2005 and March 2019 obtained 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, the charge disposition, a series of sentencing outcomes, and text variables containing information about the charge itself and the section of the Virginia Code that encompasses the charge. Defendants most often receive dispositions of guilty, charge dropped by the prosecutor, or charge dismissed by a judge. Sentencing information can include the amount of time that someone is sentenced to serve in jail or prison as well as fines incurred. 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 allegations, 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

[24] 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.

defendant faces multiple charges at the same time it 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 "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. [25]

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.

[25] 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

4.3.1 Case Filing Sample

I test for civilization effects in routine police interactions, expanding upon the existing literature that emphasizes the rare (but influential) outcomes of police use of force and complaints against officers. Oversight from body-worn cameras could change how police exercise discretion when issuing citations and 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.[26]

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.

Table 3: Comparison of treated and untreated localities at early-sample baseline

Panel A: Case Filings	District				Circuit	
	Untreated	*Treated*			*Untreated*	*Treated*
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%
Count Localities	33	76			28	76
Panel B: Case Processes & Outcomes	***Misdemeanors***		***Felonies***		***Felonies***	
	Untreated	*Treated*	*Untreated*	*Treated*	*Untreated*	*Treated*
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

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.

[26] 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 and bond violations that 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.[27] 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*.[28] 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

[27] Because 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.

[28] In base year 2006, 83 percent of circuit court cases in my sample were disposed within 1 year of filing

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 because 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 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 district court data, so here I substitute the date of the latest hearing for disposed cases. [29] 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 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. To do so I will use a difference-in-differences strategy. An emerging econometric literature demonstrates shortcomings of the traditional

[29] I summarize each of these variables in table 2, along with their expected responses to body-worn camera adoption

two-way fixed effects (TWFE) estimator in the presence of differential treatment timing and heterogeneous treatment effects. Because of this, I will supplement these TWFE results with those estimated using an alternative imputation estimator proposed by Borusyak, Jaravel, and Spiess (2021).

5.1 Methods: Two-way Fixed Effects

5.2 Methods: Imputation Estimator

Under staggered treatment timing, classic TWFE estimates 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 treatment 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 ; Sun & Abraham,2020 ; Wooldridge, 2021).

In my context, law enforcement adopt body-worn cameras at different points in time, and it is theoretically highly plausible that body-worn camera effects are heterogeneous over time due to changes in salience at the policing stage 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 $\hat{\tau}_{it} = Y_{it} - Y(\hat{0})_{it}$ 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.

5.3 Parallel Trends Tests

The key underlying assumption for difference-in-differencs 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 γ, 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 discuss results in light of the expected biases from such a violation.

5.4 Treatment Exogeneity

Parallel trends tests provide evidence toward the suitability of comparison units for causal inference. These tests assuage concerns that adopting localities do so because they differ from those that don't, which can bias difference-in-differences estimates. For example, parallel trends tests can reveal if localities with declining conviction rates adopted body-worn cameras. Passing these tests is not, the same as exogeneous assignment of the technology. Instead, police departments will select into using the technology but I argue that this choice is plausibly exogeneous to the courts. And, while I do not claim that police adoption decisions are exogeneous, I can estimate an adopters-only model that requires the weaker exogeneity assumption of exogeneous adoption timing to produce valid causal estimates.

The LEMAS-BWCS survey suggests exogeneity of body-worn cameras to the courts because it shows 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.

5.5 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, because 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 $Y_{it} = ln(case\_counts_{it})$ at the district court level and $Y_{it} = ln(case\_counts_{it} + 1)$ at the circuit court level, because some[30] 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 $Y_{it} = ln(civ\_case\_counts_{it} + 1)$ for both the district and circuit court samples.[31] These two outcomes provide information about

[30] 3 courts in a total of 10 quarters.

[31] 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 (barely)

the number of cases that enter the courts. The 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 4: BJS estimates, case filing effects

VARIABLES	Case Count	Civilization Case Count	Share Multicharge Cases	Share Misdemeanor
Treatment Effect	-0.065†	-0.091*	-0.007	--
Circuit Court	(0.033)	(0.037)	0.009	
Mean	145.3	6.9	0.486	
Observations	5,472	5,472	5,463	
Treatment Effect	0.015	-0.115**	-0.004	-0.002
District Court	(0.030)	(0.037)	(0.005)	(0.003)
Mean	3,578.8	33.1	0.188	0.927
Observations	4,141	4,141	4,141	4,141

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 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-

$\alpha = 0.05$ 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 $\alpha = 0.10$

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. [32]

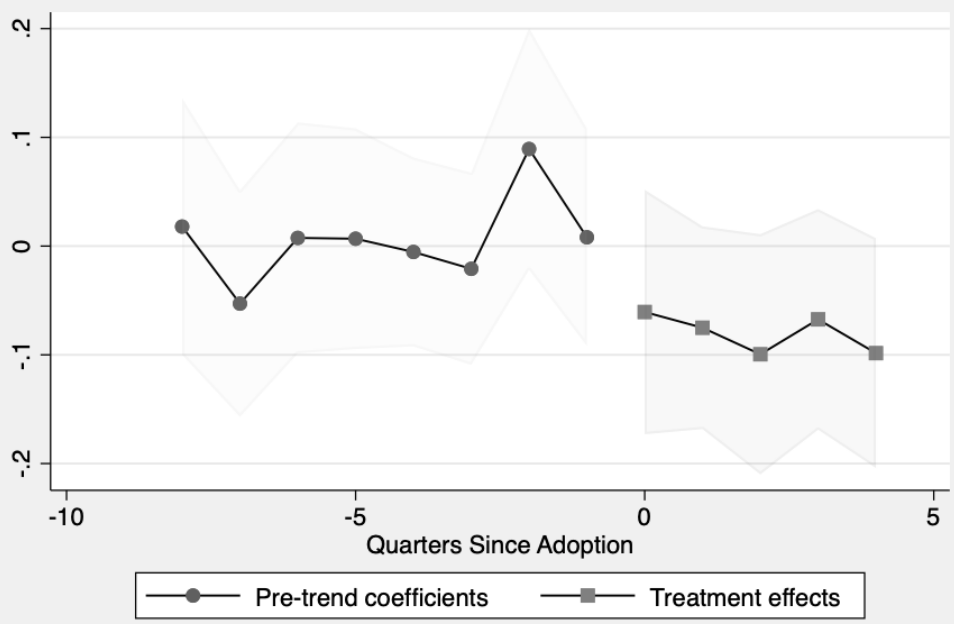

Figure 4: District court civilization effect charge quarterly estimates, adopters-only sample

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

[32]The treated-only sample also supports the null effects on intensive margin outcomes. Notably, the point estimates for the effects of body-worn cameras on the share of multi-charge cases and the share of misdemeanors were nearly identical across specifications.

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 compelling evidence of broader case changes– either in quantity or characteristics– is more surprising. Because 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.

5.6 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.[33] 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.

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

[33]I provide supplementary results for an extended set of court processes and case outcomes in appendix B.2.

Table 5: BJS estimates, case processes and resolutions					
VARIABLES	Prosecutor Dropped Charge	Case Certified	Guilty	Sentenced to Time	Disposition: 1 year°
Treatment Effect	-0.030*	--	-0.006	-0.018	-0.006
Circuit Court	(0.012)		(0.011)	(0.014)	(0.011)
Mean	0.258		0.720	0.708	0.825
Observations	5,439		5,439	5,439	5,439
Treatment Effect	-0.004	0.007	.005	-.006	-0.010
District Court (Fel.)	(0.014)	(0.016)	(0.15)	(.014)	(0.006)
Mean	0.401	0.593	0.296	0.246	0.964
Observations	4,047	4,047	4,047	4,047	4,047
Treatment Effect	-0.002	--	-0.001	-0.011	-0.001
District Court (Misd.)	(0.005)		(0.008)	(0.007)	(0.005)
Mean	0.116		0.739	0.199	0.956
Observations	4,100		4,100	4,100	4,100

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

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[34] 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

[34] It'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.

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 female defendants, black defendants, and certain case class shares. For example, if in court A in quarter 1 MLT cases received guilty dispositions 50 percent of the time and non-MLT recieved guilty dispositions 45 percent of the time, the differenced guilty outcome would be equal to 5 percentage points. The interpretation of the treatment effect estimates under the differenced-outcome are intuitive in that a statistically and economically significant point estimates would indicate a divergence in the outcomes across case type that is attributable to body-worn camera implementation. Delving into the underlying model for this outcome, it is practically similar to implementing a fully interacted model in a typical difference-in-differences framework i.e. including an interaction term of $MLT * variable$ for all right hand side variables for a regression on the earlier used, non-differenced outcome variables. However, in using differenced outcomes I place different restrictions on the relationship between the outcome of interest and covariates across MLT and non-MLT groups. These relationships are unrestricted across MLT and non-MLT groups except through standard functional form assumptions in the fully interacted model, but are assumed to be equivalent between MLT and non-MLT groups in the differenced-outcome model. Using the differenced outcomes, I apply the same imputation estimator and model from the main results to these new panels 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.

Table 6: BJS estimates, case processes and resolutions; MLT

VARIABLES	Prosecutor Dropped Charge	Case Certified	Guilty	Sentenced to Time	Disposition: 1 year°
Treatment Effect	0.007	--	0.016	0.017	0.013
Circuit Court	(0.011)		(0.013)	(0.013)	(0.010)
Mean	0.012		-0.034	-0.031	-0.079
Observations	5,330		5,330	5,330	5,330
Pre-test p-value	0.022		0.299	0.277	0.794
Treatment Effect	-0.001	-0.003	0.003	-0.002	0.002
District Court (Fel.)	(0.014)	(0.016)	(0.016)	(0.014)	(0.006)
Mean	0.111	0.126	0.054	0.024	-0.009
Observations	3,976	3,976	3,976	3,976	3,976
Pre-test p-value	0.612	0.752	0.453	0.376	0.888
Treatment Effect	0.002	--	0.006	0.015	-0.006
District Court (Misd.)	(0.008)		(0.011)	(0.012)	(0.009)
Mean	0.214		-0.008	0.398	-0.074
Observations	4,098		4,098	4,098	4,098
Pre-test p-value	0.837		0.470	0.830	0.133

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. Outcome variables and controls are expressed here in differences, and so a point estimate of 0.01 would be interpreted as a 1 percentage point increase in the outcome gap between MLT and non-MLT cases. Pre-trend p-values are for tests of the 8 quarters prior to BWC adoption.

6.2 BWCs and Race

Body-worn cameras in the U.S. are tightly linked to a broader national 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, 2015).

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 previous section of this paper, but now apply them to a modified panel where cases are aggregated to the quarterly court level within two racial groups: black and non-black defendants.[35] That is, for court A in quarter 1 if black defendants received guilty dispositions 50 percent of the time and non-black defendants received guilty dispositions 45 percent of the time, the guilty outcome would equal 5 percentage points. I also introduce a new outcome variable representing the share of case filings with black defendants to the case filing analysis.

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.

[35] 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.

VARIABLES	Share of Filings (Black)	Prosecutor Dropped Charge	Case Certified	Guilty	Sentenced to Time	Disposition: 1 year°
Treatment Effect	0.005	0.005	--	-0.014	-0.011	0.005
Circuit Court	(0.007)	(0.012)		(0.012)	(0.012)	(0.011)
Mean	0.365	0.011		0.006	0.006	0.018
Observations	5,463	5,072		5,072	5,072	5,072
Pre-trends p-value	0.333	0.143		0.277	0.170	0.397
Treatment Effect	-0.000	0.022†	0.007	-0.010	-0.009	0.010
District Court (A)	(0.003)	(0.013)	(0.017)	(0.013)	(0.011)	(0.006)
Mean	0.257	-0.002	0.002	-0.006	-0.000	0.004
Observations	4,141	3,751	3,751	3,751	3,751	3,751
Pre-trends p-value	0.638	0.080	0.147	0.049	0.041	0.236
Treatment Effect	--	0.002	--	-0.003	-0.013*	-0.003
District Court (B)		(0.004)		(0.007)	(0.006)	(0.003)
Mean		0.005		0.016	0.035	0.001
Observations		4,045		4,045	4,045	4,045
Pre-trends p-value		0.564		0.531	0.493	0.471

Table 7: BJS estimates, racial heterogeneity

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.

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 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 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 footage appears to affect the body of cases as a whole. While concerns over racial disparities in policing have been an integral part of the public discourse around body-worn camera adoption, I also do not find evidence of differential policing or court effects for black defendants.

Overall my results suggest that body-worn camera effects on policing and the courts are exceptions rather than the norm. Existing research shows 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). Combining this prior result with my own findings that body-worn cameras do not substantially alter outcomes in the courts, it appears that expanded body-worn camera adoption will produce net benefits to the criminal justice system with both the costs and benefits accruing primarily to law enforcement and law enforcement interactions.

Although I use rich criminal case data, there are two considerations necessary to place this paper in it's proper context. Just as I find body-worn cameras affect only a small subset of police interactions, it is possible that body-worn cameras are deeply influential in a small subset of criminal cases which I cannot pick up in my aggregated analyses. Additionally, even the richest criminal case data provide merely a snapshot of the broader costs and benefits borne by actors in the criminal justice system. In a companion paper (Bollman, 2021), I dissect the attorney time use channel from an alternative angle, attorney labor market responses. However, additional outcomes such as public perceptions of fairness in the justice system would also provide valuable insight into the holistic effects of body-worn cameras. Such outcomes can be influential; the attorney concerns described in this paper eventually culminated in legislative efforts to increase funding for prosecutors offices in Virginia. Where implemented, this will exacerbate funding differentials between indigent defenders and prosecutors in the years to come. My counter-intuitive 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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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. Because 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.

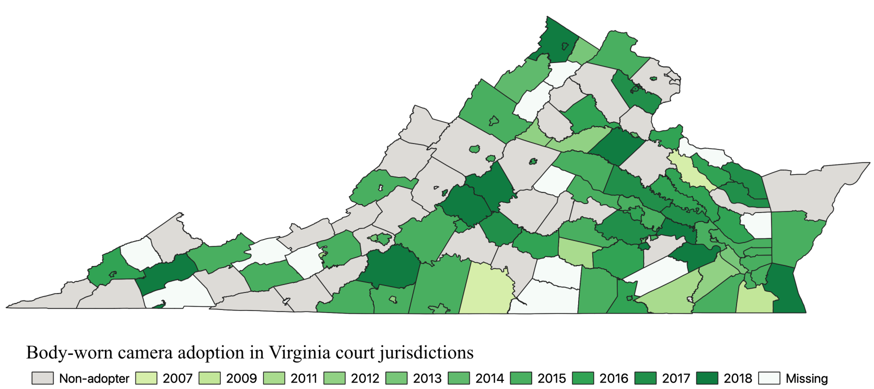
Law Enforcement Force Size and Characteristics: I use policing role indicators and force size measures from the 2016 Law Enforcement Agency Roster (LEAR). The LEAR itself includes variables pulled from other sources. Thus the LEAR 2016 officer counts I use are counts from the 2008 Census of State and 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.

Case Number: CR08[redacted]	**Filed:** [redacted]	**Commenced by:** Reinstatement	**Locality:** COMMONWEALTH OF VA
Defendant: G[redacted]	**Sex:** Male	**Race:** Black (Non-Hispanic)	**DOB:** 10/01/****
Address: ARLINGTON, VA 22207			
Charge: VIOL PROBATION ON FEL OFF	**Code Section:** 19.2-306	**Charge Type:** Felony	**Class:** U
Offense Date: [redacted]	**Arrest Date:** [redacted]		

Final Disposition

Disposition Code: Sentence/Probation Revoked	**Disposition Date:** [redacted]	**Concluded By:** Revoked
Amended Charge:	**Amended Code Section:**	**Amended Charge Type:**

Jail/Penitentiary: Penitentiary	**Concurrent/Consecutive:** Sentence Is Run Consecutively With Another	**Life/Death:**
Sentence Time: 3 Year(s)	**Sentence Suspended:**	**Operator License Suspension Time:**
Fine Amount:	**Costs:** $658.00	**Fines/Cost Paid:**
Program Type:	**Probation Type:**	**Probation Time:**
Probation Starts:	**Court/DMV Surrender:**	**Driver Improvement Clinic:**
Driving Restrictions:	**Restriction Effective Date:**	
VA Alcohol Safety Action:	**Restitution Paid:**	**Restitution Amount:**
Military:	**Traffic Fatality:**	

Appealed Date:

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, because 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 ((case type)+year+(sequential number)+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: because 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.

casenumber	fileddate	person_id	case_id_ba~s
[redacted]4310-00	21138	100000000000005	11
[redacted]4311-00	21138	100000000000005	11
[redacted]4309-00	21138	100000000000005	11
[redacted]8756-00	21319	100000000000005	13
[redacted]2897-00	21404	100000000000005	14
[redacted]3327-00	21418	100000000000005	15

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 B.1: BJS estimates, parallel trends tests

	District (A)		District (B)		Circuit	
Number of pre-periods	4	8	4	8	4	8
Cases	0.291	0.352			0.455	0.510
Civilization Effect Cases	0.318	0.162			0.164	0.039
Multi-charge Cases	0.433	0.782			0.287	0.304
Share Misdemeanor	0.337	0.519			--	--
Prosecutor Dropped Charge	0.586	0.328	0.201	0.067	0.500	0.668
Case Certified	--	--	0.907	0.437	--	--
Guilty	0.363	0.487	0.399	0.158	0.512	0.829
Sentenced to Time	0.556	0.917	0.502	0.500	0.270	0.581
Disposition: 1 year	0.497	0.572	0.951	0.986	0.711	0.897

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.

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

Table B.2.a: BJS estimates, case effects adopter sample				
VARIABLES	Case Count	Civilization Case Count	Share Multicharge Cases	Share Misdemeanor
Treatment Effect	0.022	0.002	-0.034**	--
Circuit Court	0.050	0.063	0.010	
Mean	175.8	8.2	0.477	
Observations	3,952	3,952	3,950	
Treatment Effect	0.055†	-0.061	-0.004	-0.001
District Court	(0.028)	(0.042)	(0.004)	(0.003)
Mean	3,578.0	38.2	0.192	0.924
Observations	2,736	2,736	2,736	2,736

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.

Table B.3.b: BJS estimates, case processes and resolutions; treated sample					
VARIABLES	Prosecutor Dropped Charge	Case Certified	Guilty	Sentenced to Time	Disposition: 1 year°
Treatment Effect	-0.020	--	-0.005	-0.012	-0.002
Circuit Court	(0.014)		(0.019)	(0.021)	(0.015)
Mean	0.261		0.719	0.706	0.833
Observations	3,952		3,952	3,952	3,952
Treatment Effect	0.008	0.002	0.003	-.003	-0.005
District Court (Fel.)	(0.016)	(0.016)	(0.15)	(.015)	(0.007)
Mean	0.405	0.587	0.295	0.252	0.969
Observations	2,667	2,667	2,667	2,667	2,667
Treatment Effect	-0.007	--	0.003	-0.006	-0.004
District Court (Misd.)	(0.006)		(0.009)	(0.007)	(0.004)
Mean	0.115		0.746	0.212	0.959
Observations	2,700		2,700	2,700	2,700

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

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%

Table B.2.c: BJS estimates, parallel trends tests; adopters sample						
	District (A)		District (B)		Circuit	
Number of pre-periods	4	8	4	8	4	8
Cases	0.080	0.167			0.664	0.580
Civilization Effect Cases	0.380	0.440			0.881	0.478
Multi-charge Cases	0.452	0.744			0.176	0.227
Share Misdemeanor	0.351	0.585			--	--
Prosecutor Dropped Charge	0.947	0.574	0.150	0.019	0.836	0.865
Case Certified	--	--	0.902	0.828	--	--
Guilty	0.630	0.768	0.469	0.067	0.755	0.850
Sentenced to Time	0.324	0.704	0.471	0.236	0.555	0.846
Disposition: 1 year	0.833	0.767	0.732	0.926	0.975	0.429
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						

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

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.

B.2 Selected Event Study Plots

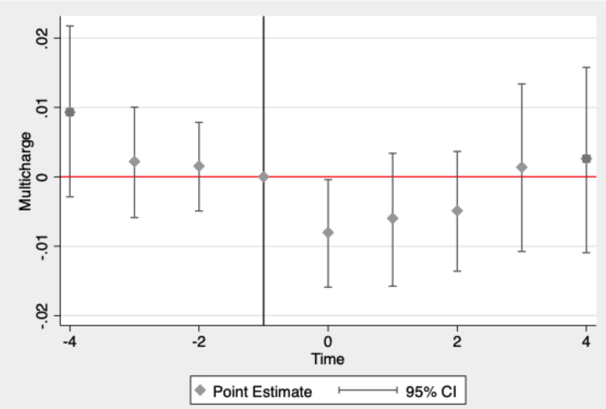


Figure x: District Court Event Study, Multi-charge cases

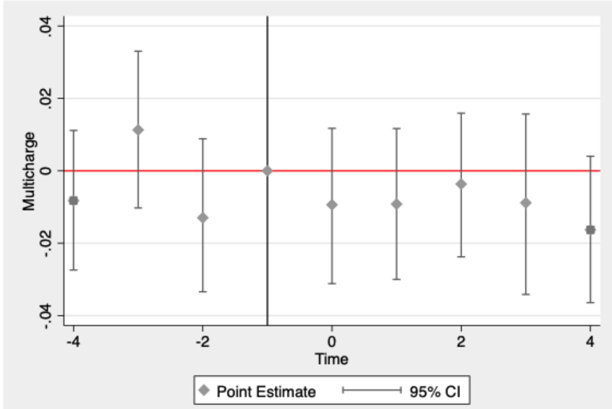


Figure Xb: Circuit Court Event Study, Multi-charge cases

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 ($\approx 60\%$ 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 B.3: Pooled difference-in-differences estimates, supplementary sentence length

VARIABLES	Sentenced to 30 days	Sentenced to 6 months	Sentenced to 1 year	Sentenced to 3 years	Sentenced to 5 years	Sentence Time	Sent. Suspended
Treatment Effect	--	--	-0.020	-0.016	-0.001	137.4	-0.025†
Circuit Court			(0.012)	(0.012)	(0.012)	(88.9)	(0.013)
Mean			0.664	0.491	0.392	2177.2	0.677
Observations			5,439	5,439	5,439	5,439	5,439
Treatment Effect	-0.005	-0.009	-0.012†	--	--	-5.5	-0.007
District Court (Fel.)	(0.012)	(0.008)	(0.007)			(4.3)	(0.011)
Mean	0.241	0.136	0.119			73.8	.228
Observations	4,129	4,129	4,129			4,129	4,129
Treatment Effect	-0.011*	-0.003	--	--	--	-1.9†	-0.011†
District Court (Misd.)	(0.006)	(0.002)				(1.1)	(0.006)
Mean	.183	0.036				27.4	0.190
Observations	4,182	4,182				4,182	4,182
Race, sex covariates	x	x	x	x	x	x	x
Offense year, quarter FE	x	x	x	x	x	x	x
Locality FE	x	x	x	x	x	x	x

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

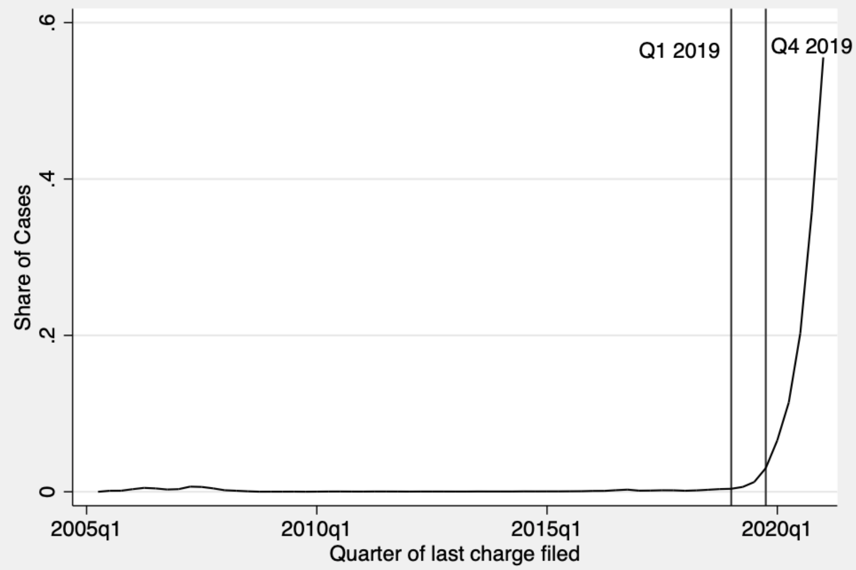


Figure B.1.a: Share of cases not completely disposed, district court misd

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.

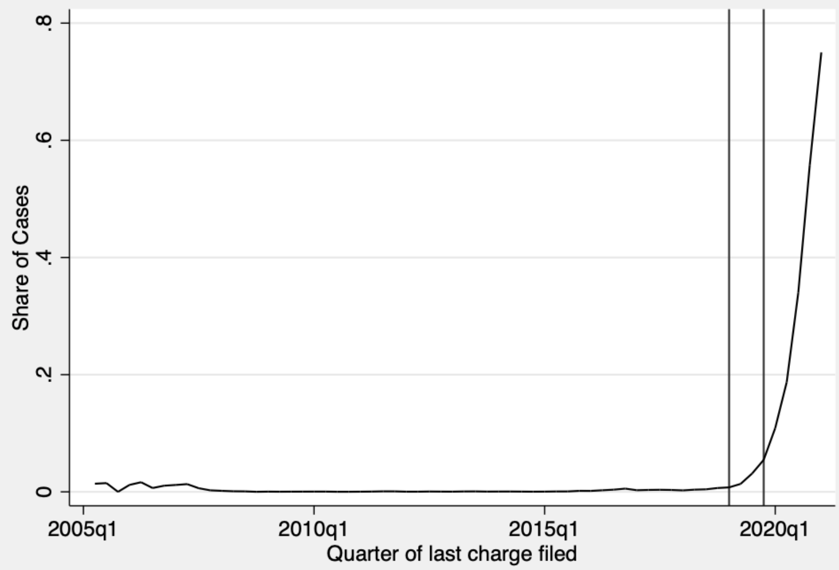


Figure B.1.b: Share of cases not completely disposed, district court felonies

		Circuit Court		
		50% threshold		
		Untreated	Treated	Unclassified
25% threshold	Untreated	30	0	0
	Treated	4	68	0
	Unclassified	3	1	14

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

		District Court		
		50% threshold		
		Untreated	Treated	Unclassified
25% threshold	Untreated	33	0	0
	Treated	4	70	0
	Unclassified	2	1	15

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

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

VARIABLES	Case Count	Civilization Case Count	Share Multicharge Cases
Treatment Effect	-0.064†	-0.110**	-0.007
Circuit Court	(0.033)	(0.039)	0.009
Mean	147.4	7.1	0.487
Observations	6,420	6,420	6,410
Treatment Effect	-0.017	-0.097**	0.003
District Court	(0.030)	(0.036)	(0.004)
Mean	3,513.6	33.7	0.187
Observations	4,972	4,972	4,972

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.

Appendix Table C.3.a: DiD estimates, case effects

VARIABLES	Case Count	Civilization Case Count	Share Multicharge Cases
Treatment Effect	-0.050	-0.091**	-0.009
Circuit Court	(0.030)	(0.033)	(0.008)
Mean	148.8	7.2	0.486
Observations	6,240	6,240	6,230
Treatment Effect	-0.010	-0.103**	-0.002
District Court	(0.027)	(0.034)	(0.004)
Mean	3548.6	34.3	0.188
Observations	4,796	4,796	4,796

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 so indicate a binding time constraint under the ABA guidelines.

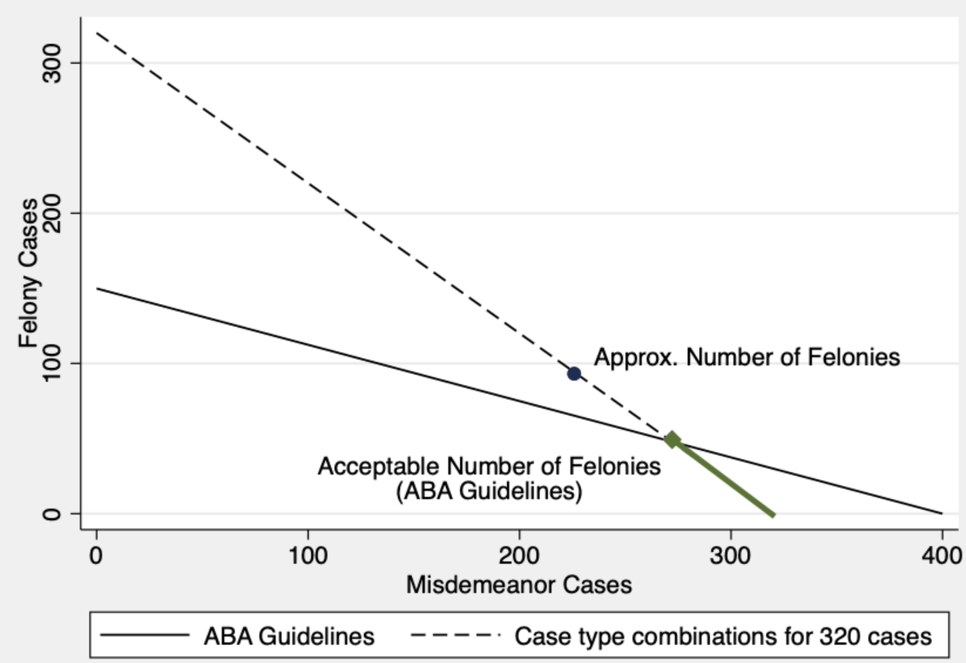


Figure D.1: PPF for defense attorneys under ABA guidelines and caseloads for VA public defenders