Consequences of Body-worn Cameras on the Court System: Evidence from State District and Circuit Court Data

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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 data for criminal courts through footage of criminal defendants. These data can provide evidence pertinent to criminal cases, but do so at a cost of attorney time. I use rich data on criminal charges in Virginia state courts from 2006-2020 and a new, self-collected body-worn camera data set to investigate whether body-worn cameras affect both policing and court outcomes. Leveraging the staggered adoption of body-worn cameras by local law enforcement through a difference-in-differences estimation strategy, I find evidence that body-worn cameras lead to an approximately ten percent reduction in new case filings for offenses initiated during a police interaction, like resisting arrest. These cases may be particularly likely to show behavioral effects of body-worn cameras, but the cameras generate evidence toward a broader base of cases. However, using a set of case disposition, sentencing, and timing outcomes, I do not find evidence that body-worn cameras changed court processes or case resolutions on average. If attorneys substitute time across their caseloads, this result is not explained by offsetting evidentiary and attorney time use effects.

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 in the United States, outfitting law enforcement with body-worn cameras (BWCs) has broad public support.[1] Between 2020 and mid-2021, six states mandated body-worn cameras for law enforcement (NCSL, 2021).

Although increasingly commonplace, body-worn cameras are a recent technological advancement for law enforcement in the United States. In 2010, less than 5 percent of law enforcement agencies used body-worn cameras (LEMAS-BWCS, 2016). This changed rapidly after a police officer killed eighteen-year-old Michael Brown in Ferguson, Missouri in August 2014 (Buchanan et al., 2015), leading Ferguson police to begin using body-worn cameras to increase transparency, accountability, and public trust (BBC News 2014). Across the nation, between 2013 and late 2016 the share of general purpose law enforcement agencies using body-worn cameras rose from approximately 12 percent to nearly half (LEMAS-BWCS, 2016).[2]

Body-worn camera advocates intend the technology to increase transparency and improve safety in police interactions, reducing police misconduct and some visible criminal activities. However, criminal justice practitioners warn of broader effects on policing and the courts. In addition to inducing behavioral changes amongst police and members of the public, body-worn cameras generate recorded data that may be relevant to criminal cases. While these data may allow for a more accurate resolution of criminal charges, accessing the evidentiary value of the data comes at a cost of already scarce attorney time. Across Virginia, attorneys report that this tension between attorney time constraints and the additional labor demands of cases with body-worn camera footage can be detrimental for vulnerable criminal defendants. As the Executive Director of the Virginia Indigent Defense Commission wrote, "... we have significant concerns that our attorneys will not be able to continue to meet their ethical and professional responsibilities" (Compensation Board, 2018).

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

[2] 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).

In this paper, I extend the base of research on body-worn cameras as a *policing tool* to incorporate their 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 filings, resolutions and court processes after law enforcement begin using body-worn cameras. In doing so, I discuss three channels through which body-worn cameras can affect the courts. First, body-worn cameras can induce civilization (behavioral) effects amongst law enforcement and the public, which can change the set of cases that are filed in the courts. Then, once cases are filed, body-worn camera footage can introduce evidentiary and attorney time use effects that change the outcomes of the case or the process by which these outcomes are realized.

My contributions to researchers' understanding of body-worn camera effects in policing build upon work by criminologists (Ariel et al., 2015; Katz et al., 2014; Yokum et al., 2017) and contribute to an emerging literature in economics (Kim, 2020; Çubukçu et al., 2021). 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 (Chalfin and McCrary, 2017). While previous body-worn camera studies often focus on changes in police use-of-force – an important but uncommon outcome – I test for civilization effects in more common interactions by measuring changes in the frequency and composition of charges that are filed in criminal courts.

Less is known about if and how body-worn cameras affect court processes and case resolutions once charges are filed. Two local impact evaluations provide contradicting evidence: Yokum et al. (2017) do not find effects of body-worn cameras on case outcomes in Washington D.C., whereas Katz et al. (2014) note some prosecutorial changes coincided with body-worn camera adoption in Phoenix. Outside of criminal courts, Çubukçu et al. (2021) find that body-worn camera footage evidence affects the resolution of citizen complaints against police. I provide some of the first empirical evidence around these court-based effects and the first evidence using data from multiple court jurisdictions.

To advance the research on the effects of body-worn cameras on the courts, I collected a new data set detailing the timing of body-worn camera adoption across Virginia court

jurisdictions. Existing data collections on body-worn camera adoption either suffered from poor data quality in key fields or employed sampling structures that encouraged agency-level analyses. Because multiple law enforcement agencies can operate within a single court jurisdiction, court-level analyses with a sampling of agency-level data would be limited to those courts for which key agencies in the court jurisdiction were sampled. These data constraints led to a high representation of urban areas in body-worn camera evaluations and less evidence from small and mid-sized localities. In contrast, my adoption data reflects the broadest coverage of Virginia law enforcement agencies to date, covering the major law enforcement agencies in 90 percent of Virginia court jurisdictions. I combine these new body-worn camera data with a second data set containing the near-universe of criminal court charges in Virginia from 2008-2020[3] to create court-level panels covering 102 Virginia circuit courts and 107 Virginia district courts.

I analyze these data in a difference-in-differences framework. Because law enforcement began using body-worn cameras at different points in time, I implement both the traditional two-way fixed effects estimator and an alternative imputation estimator proposed by Borusyak, Jaravel, and Spiess (2021) to test for effects of body-worn cameras on case filings, processes, and resolutions. Although a new econometric literature (Goodman-Bacon, 2021; Sun and Abraham, 2020) shows that the traditional two-way fixed effects estimator can produce biased estimates under staggered treatment timing and heterogeneous treatment effects, both estimators provide similar results in the case of body-worn cameras in Virginia.

Across two court levels and several outcomes measuring changes in case filings, processes, and resolutions, I find a strong pattern of results. Body-worn cameras reduced the prevalence of a subset of charges arising out of interactions with police including resisting arrest, assaulting an officer, and similar offenses. However, beyond this subset, they do not appear to have altered police behaviors as a whole: the number of cases filed, the share of those cases involving multiple charges, and charge severity did not change after police began using body-worn cameras.

The cases that are filed within criminal courts also do not systematically proceed through

[3]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.

the courts or resolve differently after body-worn camera adoption – despite practitioner reports of an indigent defense system buckling under the weight of new body-worn camera data. One potential explanation for these null results could be that the evidentiary effects of the videos offset attorney time use effects for a net zero effect. However, I show that there are no differential effects of body-worn cameras on cases more likely to have associated video. Because a common pool of attorneys litigate video and non-video cases, if attorneys substitute time across the cases they represent then the lack of a differential effect suggests that the offsetting-effects hypothesis does not hold. Finally, because the increase in body-worn camera programs across the U.S. was motivated in part by racial disparities in policing, I test for evidence of differential effects for Black and non-Black defendants. I do not find compelling evidence that body-worn cameras differentially helped or harmed Black defendants.

The paper proceeds as follows. Section 2 provides an institutional context for Virginia courts and body-worn camera adoption, and an economic context for understanding the consequences of court outcomes. Section 3 presents 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. Section 6 presents results using both traditional two-way fixed effects and the new imputation method. Section 7 dissects the null result presented further and tests for heterogeneity in treatment effects for cases more and less likely to have body-worn camera footage as well as heterogeneity in effects by race. Section 8 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 Law Enforcement Management and Administrative Statistics- Body-worn Camera Supplement (LEMAS-BWCS, 2016), 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 percent 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.[4] 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. Section 4.1 describes these data in detail, 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.

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

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

ability (19 percent), and a reduction in/faster resolution of citizen complaints (15 percent)[5] 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.

The LEMAS-BWCS shows that adopting agencies almost always establish a formal policy that outlines expectations for when body-worn cameras must be turned on. These policies cover a broad range of police interactions that can end with court proceedings. Of those agencies that set requirements, 93 percent required that the cameras record traffic stops and nearly 85 percent required officers to turn on their cameras when executing arrest or search warrants, deploying firearms, and initiating contact with members of the public. Typically policies require that these recordings be preserved for between one month and one 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

Police often interact with members of the public; the Bureau of Justice Statistics estimates that in 2018, about 24 percent of the U.S. population (60 million people) had contact with the police (Harrell and Davis, 2020). These contacts are often resident-initiated and typically do not result in criminal charges, yet a nontrivial share of the U.S. population will find themselves in court in their lifetime. In Virginia in 2013, there were nearly 700,000 felony or misdemeanor filings in State General District Courts (Office of the Executive Secretary, 2014a), and 190,000 in State Circuit Courts (Office of the Executive Secretary, 2014b). These two sets of courts are the primary venues for criminal litigation in Virginia. They largely share geographic jurisdictions, with approximately one circuit court and one district court

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

in each county or independent city across the state.[6] However, they differ in the scope of the cases they hear: district courts hold jurisdiction over misdemeanor cases whereas the circuit courts hear felonies.[7] 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.[8] I provide a basic case road map outlining the entities involved in various states of criminal litigation in Table 1. Broadly, the court actors can influence outcomes for the defendant ranging from the final set of charges to be ruled on in court to the outcomes of those charges, their sentencing, and even the pace at which the case is resolved. More specifically, prosecutors can alter, drop, or add charges to the case against the defendant; judges dismiss or rule on charges and determine sentences[9]; and both prosecuting and defense attorneys

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

[7] 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.

[8] 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.

[9] This is a simplification of the full role of judges: judges also rule on a variety of motions presented to

lobby for preferred dispositions and sentencing.[10]

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.[11] 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.

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. Even 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). Appendix D shows 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. Assigned counsel are paid a fixed hourly rate for a maximum of approximately 1.3 hours of paid work on misdemeanor charges at the district court level, and less than 5 hours of paid work on a typical circuit court felony charge.[12, 13] Even before law enforcement began using body-worn cameras, assigned counsel attorneys rarely reported working fewer than the maximum compensated hours (Bollman, 2021).

the court and in some cases oversee jury trials wherein a jury rules on a case.

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

[11]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).

[12]Virginia Code §19.2-163

[13]There are some opportunities for fee waivers that would increase the maximum compensation. However in a companion paper (Bollman, 2021) I calculate fee waivers to be rarely granted, given for only about 3.3% of charges.

3 Literature and Conceptual Framework

The determinants of crime are multifaceted. Empirically they have been shown to range from inequality and other socioeconomic factors (Kelly, 2000; İmrohoroğlu et al. 2000; Fajnzylber et al., 2002(a); Fajnzylber et al., 2002(b); Grogger, 1998; Buonanno and Montolio, 2008), to alcohol access (Heaton, 2012; Groönqvist and Niknami, 2014), social networks (Damm and Dustman, 2014; Billings et al. 2019), family background or adverse childhood experiences (Doyle, 2008; Currie and Tekin, 2012; Eriksson et al., 2016) and more. Even schooling and entertainment can affect crime by incapacitating would-be offenders (Dahl and DellaVigna, 2009; Jacob and Lefgren, 2003). The effects of crime are similarly pervasive, with studies showing detrimental effects on outcomes spanning housing prices and wealth accumulation, mental well-being, and youth academic performances (Linden and Rockoff, 2008; Cornaglia et al., 2014; Schwartz et al., 2016).

Policing is the most direct lever through which governments seek to reduce crime. Police serve as monitors who can disrupt criminal activities and also increase the costs of criminality by bringing defendants into the courts. In addition to the immediate costs of liberty or finances, incarceration and criminal records diminish later economic self-sufficiency by increasing barriers to formal employment (Agan and Starr, 2017; Dobbie et al., 2018), which grow with longer incarcerations (Mueller-Smith, 2015). Numerous studies affirm that police deter and reduce criminal activity (Evans and Owens, 2007; Draca et al., 2011; Vollaard and Hamed, 2012 ; Chalfin and McCrary, 2018; Weisburd, 2021), and that incarceration can incapacitate would-be offenders (Barbarino and Mastrobuoni, 2014; Mastrobuoni, 2019).

However, policing and the criminal justice system also carry a variety of costs. Fiscally, in 2018 states spent $119 billion on police (Urban Institute, 2021) and an average of over $30,000 per prison inmate each year (Mai and Subramanian, 2017).[14] Incarceration can spur post-release criminal activity and increased use of public assistance programs (Bayer et al., 2009; Mueller-Smith, 2015). And, the manner in which police pursue crime reduction goals affects the public more generally. Instances of police misconduct are costly to budgets, trust in criminal justice institutions, and social well-being. In 2019 the City of Chicago paid

[14]Calculated using data available from 45 states.

nearly $47 million in settlements and court awards stemming from police misconduct (City of Chicago, 2020).[15] And, even indirect exposure to police use-of-force can reduce academic attainment for high school students, particularly Black and Hispanic students (Ang, 2021).

New police technologies affect both costs and benefits in the criminal justice system. Body-worn cameras may enhance officer's ability to deter crimes and may also deter police from engaging in socially costly actions. Simultaneously, body-worn cameras generate evidence that can increase accuracy in convictions and improve targeting of sentences. However, the technology has fiscal costs to implementation and alters the workloads of other public service employees. In the following subsections I detail these three prospective channels through which body-worn cameras can affect policing and the courts, present the existing evidence of their roles, and describe their sometimes conflicting predicted effects 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 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, 2018 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 adhere more closely to legal

[15]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).

and social standards when recorded.[16] Under these "civilization effects", both police and the public should be less likely to engage in criminal behaviors in the presence of body-worn cameras. Some advocates and practitioners posit that these effects are more comprehensive and can foster milder police-public interactions overall.

While body-worn cameras are a new tool for police, research on other technologies that increase the probability that a defendant is caught or convicted of a crime can help predict their effects on the public. For police, existing evaluations of body-worn camera programs as well as research on police responses to oversight can inform the accountability effects of body-worn cameras.

Empirically, numerous studies find that criminals respond to innovations that increase their expected costs to criminal behavior. These innovations include DNA databases for convicted felons (Doleac, 2017), electronic monitoring (Di Tella and Schargrodsky, 2013), and Lojack stolen car recovery devices (Ayres and Levitt, 1998; Gonzalez-Navarro, 2013). Even so, the mechanisms through which these technologies reduce crime vary. Lojack has limited ability to link an individual to an offense, but it reduces the expected value of the stolen goods by increasing the likelihood that a stolen vehicle is recovered. In contrast, DNA databases and electronic monitoring are highly targeted for prior offenders and can directly link a specific offender with a specific offense.

Closed-circuit television (CCTV) may reside in a middle ground; these surveillance cameras target locations rather than individuals, but also provide video evidence that can help link specific individuals to specific crimes. In this way, CCTV is similar to body-worn cameras, and both increase the likelihood that a criminal is caught and convicted if their offense is committed in view of a camera. In a meta-analysis of 76 criminology studies of CCTV, Piza et al. (2019) found an estimated 13 percent reduction in crime in CCTV areas compared to controls.

However, body-worn cameras also differ from these deterrence technologies in key ways that may alter their effectiveness in deterring unwanted behaviors. CCTV surveillance cameras constantly transmit from a fixed, pre-determined vantage point, while body-worn cam-

[16]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.

eras must be activated in order to preserve video and record an officer's regularly changing viewpoint. Body-worn cameras also differ from Lojack, electronic monitoring, and CCTV in that they provide no monitoring benefits. Monitoring deters crime by enabling a real-time response to criminal activities (Gonzalez and Komisarow, 2020). Electronic monitoring technologies alert law enforcement if an individual violates boundaries established by the court. Lojack, once activated, notifies law enforcement of the real-time location of a stolen vehicle. And, in heterogeneity analyses, Piza et al. (2019) found that effect magnitudes across evaluations were larger when CCTV was actively monitored. Because officers inherently serve as monitors, body-worn camera adoption is unlikely to induce deterrent effects through this channel. Even so, CCTV crime reductions are not wholly attributable to increased monitoring, suggesting that recorded video from body-worn cameras may nonetheless have a deterrent effect; Gómez et al. (2021) show that CCTV expansions in Medellín, Colombia that were not accompanied by expansions to monitoring capacities decreased reported crimes and arrests.

For police, body-worn cameras enable additional oversight, which may improve conduct amongst officers and sort out low-quality officers from the ranks. A less desired potential outcome of heightened oversight is *de-policing*, or reduced interactions between police and members of the public.

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

While use of force is a salient and influential outcome to study, these events are relatively rare in policing. Complaints of excessive use of force are even more so: using data from Chicago police, Chalfin and Kaplan (2021) found that 84 percent of officers generated no use-of-force complaints over a 5-year period. If we consider that the "better behavior" caused by civilization effects more broadly reduces the likelihood that an interaction escalates either physically or verbally, then we can expect to find broader changes in the charges 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. This may be particularly pronounced if officers engage in de-policing.[18]

[17]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.

[18]Outside of the body-worn camera context there is a theoretical basis for de-policing (Prat, 2004), and some empirical evidence showing de-policing under heightened oversight (Ba and Rivera, 2019). 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 as would be more similar to routine internal review of body-worn camera videos.

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.[19] However, further study is needed to validate these findings outside of the Phoenix context.

3.2 Evidentiary Effects

Secondly, body-worn camera recordings can provide evidentiary value in court proceedings, affecting how judges and juries perceive the events that unfolded during a police interaction. Influentially, court actors resolve many cases outside of the courts through plea negotiations in which prosecutors and defendants (through their legal representation) agree upon a set of terms under which a defendant will admit culpability to the court– sometimes trading more lenient sentences or dropping charges in exchange for resolving the case without a time consuming trial. Guilty pleas are common and accounted for 88 percent of case resolutions in U.S. district courts in 2009 (Sourcebook, 2009).[20] 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 evi-

[19]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.

[20]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

dentiary 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. [21] Ultimately, the balance of these elements determines whether additional evidence benefits or harms defendants on average– which is itself an empirical question.[22] 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 that officers *believe* body-worn cameras provide evidentiary value and make cases more prosecutable. These beliefs are qualitatively consistent with the LEMAS-BWCS. However, a concurrent staffing intervention within the department contaminated tests of the accuracy of this perception. Nonetheless, multiple sources show that a nontrivial share of cases in body-worn camera-using localities have related body-worn camera footage. The Katz et al. (2014) study comments on low compliance amongst officers, but also reports that footage was available for as high as 42 percent of calls in a month.

3.3 Time Use

"It's a razor thin wire, because you're looking to be sure your client's due process rights are preserved. On the other hand, I have 120 other clients. I have to preserve their due process rights too." -Newport News Public Defender Robert Moody (Albiges, 2019)

[21] 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

[22] 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.

Ascertaining whether body-worn camera video provides evidentiary value to a case requires that someone review available footage. Between 2016 and 2018 Virginia's Henrico County Commonwealth's Attorney's office reportedly annually viewed one hour of video for each of over 2,000 cases on average (Compensation Board, 2018). This is approximately equivalent to the workload of a full-time employee.[23] Similarly, 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).[24] However, staffing levels for publicly provided attorneys are sticky, and assigned counsel compensation caps depend only on case types – not on the workload or available evidence. For cases with video, assigned counsel would use over 20 percent of their compensated time for a typical felony charge just reviewing body-worn camera video if they spent the same amount of time on this task as the Henrico County CA's office reported spending on average.

Some of the time, body-worn camera video review may substitute for other case tasks. Other times, attorney time constraints may cause this review to crowd out non-body-worn camera activities. The extent to which each takes place remains ambiguous due to limited data. However the dominant narrative supported by practitioners is one of crowd-out. The Executive Director of the Virginia Indigent Defense Commission raised an alarm about attorneys workloads under body-worn camera expansion, writing, "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

[23] A broader October 2018 Commonwealth Attorney (CA) survey showed that 51 prosecutor offices reported receiving an estimated 180,000 hours of body-worn camera footage over a 12 month period (Compensation Board, 2018). This amounts to an average of about 300 hours per month per office.

[24] The offices also employ non-attorney personnel.

cases. Breaching the legal and ethical standards is obviously not an option" (Compensation Board, 2018). These practitioner concerns are leading to policy changes in Virginia aimed at ameliorating some attorney time use effects (VACO, 2019).

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.[25] I fill this data gap by collecting a more comprehensive set of law enforcement body-worn camera adoption data, which I aggregate to a quarterly court-level adoption indicator. Observing body-worn camera adoption at the court-level rather than the case-level allows me to take a broad view of direct and spillover effects of body-worn cameras on cases with and without footage. I combine these body-worn camera data with charge-level data from Virginia courts to form quarterly court-level panels that I use to explore changes in charging, case processes, and case resolutions.

4.1 Body-worn Camera Data

I measure body-worn camera adoption at the geographic court jurisdiction level and define a court as "treated" when the first major law enforcement agency operating in its jurisdiction implements a body-worn camera program. Multiple law enforcement agencies of varying sizes may operate within a single jurisdiction, so I use the "major" designation to focus on those agencies likely to contribute influentially to court caseloads. Using the 2016 Law Enforcement Agency Roster (United States Department of Justice, 2017), I identified a set of agencies that each employed at least 25 percent of the total officers or served at least 25 percent of the population in their court jurisdiction.[26] I excluded agencies without policing duties and omitted some sheriff's offices that primarily handled jail and court security. From

[25]The LEMAS-BWCS data included a sample of 85 local law enforcement agencies in Virginia. A survey of Virginia Commonwealth's Attorneys often generated missing or incomplete responses on questions pertaining to the timing of body-worn camera adoption. None of these data are linked to specific cases.

[26]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.

the remaining agencies I sought information about body-worn camera adoption through Freedom of Information Act (FOIA) requests. I extended FOIA requests to 157 qualifying agencies and obtained information from an additional 32 agencies through direct contact, departmental websites, and local media.[27,28,29]

Ultimately I obtained complete body-worn camera adoption 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. A map of adopting jurisdictions is available in Appendix A.1.[30]

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). 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

[27]Data from these 32 agencies were collected in the exploratory stages of the project.

[28]Within the FOIA requests I asked for separate information for pilot programs, if applicable. Departments commonly 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 pilot and preliminary programs as adoptions.

[29]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.

[30]The missing localities either failed to respond to the FOIA request or had incomplete records.

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 – that is, 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 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 dropped by the prosecutor 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 "dropped" and "guilty" disposition. Overall for analyses, I focus on these two dispositions of "guilty", and "dropped". I also show a simple binary measure of whether an individual was sentenced to serve a nonzero amount of time in a jail or prison.[31]

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. To capture the effects of body-worn cameras at these distinct stages, I use different sample selection criteria for

[31]I discuss the sentencing data in more detail and show results for additional sentencing outcomes in Appendix B.4. 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 main definition of this variable, but include supplementary results showing the use of suspended sentences after body-worn camera introduction in Appendix B.4

analyses of policing-based and court-based effects at the district and circuit court levels. I detail these criteria in the following section and present baseline descriptive statistics for each sample in Table 2.

4.3.1 Case Filing Samples

To test for civilization effects in routine police interactions, I create a quarterly court-level panel encompassing all district court filings for cases involving infractions, misdemeanors, and felony offenses. By including all of these case types, I capture police interactions that involve the courts without drawing distinctions across the severity of the cases. I measure changes in these filings across both extensive and intensive margin outcomes.

On the extensive margin, I calculate the total number of cases filed within the courts as well as the prevalence of a subset of cases that include charges for which body-worn cameras are particularly salient. This subset of "civilization effect charges" includes the charges that I expect to be most responsive to civilization effects. These charges all originate or escalate in the presence of a police officer and include disorderly conduct, eluding police, resisting arrest, and assault or other offenses specifically directed toward law enforcement.[32] Civilization effects should reduce the total number of cases entering the courts by either of these measures, although the total case count will only show reductions if civilization effects are widespread.

I measure intensive margin changes using two share variables: the share of cases consisting of multiple charges and the share of misdemeanor cases. When measuring the share of misdemeanor cases I specifically look at the share of misdemeanors relative to the total number of felony and misdemeanor cases. Here civilization effects should reduce the share

[32]I identify these charges using 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 specifically charged with assault and battery against a law enforcement officer. 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.

of multi-charge cases and shift felonies toward misdemeanors on average.[33]

Because court actors typically only influence district court charges after they are filed, changes in these outcomes reflect changes in the policing stage of the criminal justice system. In contrast, since circuit court filings typically follow district court proceedings, changes in this set of filings will reflect all three channels of body-worn camera influence. Although the interpretations of analyses of these two panels will differ, I create a parallel panel for circuit court case filings to test for changes in the quantity and composition of circuit court cases. At the circuit court level, I include variables for the quantity of cases filed, the quantity of civilization effect cases, and the share of multi-charge cases. I omit the misdemeanor share variable because criminal circuit court filings are institutionally set up to be dominated by felony cases.

I restrict the sample frame for main analyses to only those cases filed by Q1, 2019 to mirror the time frame of the court process and resolutions samples.[34] Law enforcement began using body-worn cameras recently[35] so in order to preserve one year of post-adoption data for included localities I drop mid-late 2018 adopters from all samples. At the other end, the earliest adopters began using body-worn cameras in 2007. My district court data begins in 2009, so I omit localities that adopted before 2011 from all district court analyses to allow for two years of pre-adoption data for all adopters. Such early adoptions are rare, comprising less than 4 percent of the localities that adopted body-worn cameras by 2019.

[33]These predictions rely on distributional assumptions about the effects of body worn cameras across case severities and as such observed intensive margin results should be interpreted in the context of observed extensive margin results. For example, if body-worn cameras "civilize" all cases, then intensive margin outcomes will show fewer multi-charge cases and a higher share of misdemeanors relative to felonies. If, however, body-worn cameras simply truncate the distribution by removing less severe cases from the courts then we could find the opposite effects. My prediction assumes that more severe case types, such as felonies, are civilized. This is consistent with existing literature that shows body-worn cameras affect police behavior in severe situations (such as use of force).

[34]The outcome variables of interest in the court process and resolutions sample will not be realized or observable immediately upon filing, and so I shorten the sample to allow adequate time to observe case resolutions. I discuss my selection of the March, 2019 end date in the next section and also show results using a longer sample window for case filing effects in Appendix 3.1.

[35]15 percent of adoptions before 2019 took place in 2018

4.3.2 Court Process and Resolution Samples

After cases enter the courts, I am interested in how evidentiary and time use effects may affect cases. To test for these court-based effects, I construct quarterly district and circuit court case process and resolution panels. I partition the district court sample by case type, creating separate panels for misdemeanor and felony cases to account for differences in the potential outcomes and case processes across these case types. The circuit court panel only includes cases with at least one felony charge.[36] 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 cases for which all charges were filed by March 12, 2019 to allow at least one year for cases to resolve before the onset of the coronavirus pandemic.[37] Within these samples I construct a series of outcome variables that measure meaningful changes in case resolutions and the process by which these resolutions come to pass.

The resolution outcomes of interest include the share of cases for which the defendant is declared guilty and the share that are sentenced to incarceration. The alternative to a guilty case under this definition is that a defendant is acquitted of all charges that are not already dropped by the prosecutor's office or dismissed by a judge. Charges can be dropped even in cases where a defendant is found guilty of at least one remaining charge through plea negotiations, a lack of evidence, or demonstrated error. I use the share of cases for which any charges are dropped as the first case processes outcome.

For district court misdemeanors and circuit court felonies, cases conclude when all charges reach one of these final resolutions.[38] However, while district court felony cases can be resolved in the district court, more commonly they go through a certification process to advance the case to the circuit court for further litigation. I use the share of cases that

[36]Stand-alone circuit court misdemeanors consist of appeals from the district court and are excluded from this sample. I omit infraction cases from circuit and district samples since infractions rarely entail litigation, will not qualify for an indigent defender, and often are resolved by pre-paying a set fine without ever interacting with the court or court actors.

[37]The Governor of Virginia declared a state of emergency on March 12, 2020. Courts and attorneys suspended and/or substantially modified their operations due to Covid, so cases after this time did not have a "normal" year to be resolved.

[38]A small number are transferred to alternate jurisdictions, but these are exceptions. Cases can also be appealed after reaching a (guilty) resolution.

advance to the circuit court as my second court process outcome. Finally, ongoing criminal litigation is disruptive for defendants, and so I use the amount of time a case is active within the courts as my third process outcome of interest. Specifically, I use the share of cases that were resolved within 1 year of filing at the circuit court level. This precise resolution variable is not available at the district court level so I substitute the most recent hearing date for district court cases instead.[39]

In addition to measuring changes in case duration, this disposition timing variable fulfills a second key role in the circuit court analyses. I selected the sample window to allow cases a full year to resolve, but some cases will take longer. Circuit court cases encompass more severe charges and often more intense litigation, so longer and more complex circuit court cases would systematically drop from the sample in later periods without correction. To mitigate the effects of this censoring, I condition circuit court outcome variables on having been observed within 1 year of the filing date. [40] The case length variable will alert me to compositional changes in my sample stemming from this timing criteria.

The predicted effect of body-worn cameras on each of these outcomes is conceptually ambiguous: attorney time use effects are expected to worsen outcomes for defendants by forcing a reallocation of attorney time from higher marginal value activities to lower (on average), which would correspond to an increase in both guilty and incarceration shares. However, it is unclear who benefits from evidentiary effects on average. If body-worn camera footage typically supports defendant narratives, then the evidentiary channel may partially or fully offset the time use effects. This tension across effects is also present in the dropped charges outcome; prosecutors may be eager to speed cases along in plea negotiations by offering to drop charges, or may find their negotiation position strengthened by the new video evidence and do so less often. According to attorney advocates, dominant time use effects should lengthen the amount of time between case filings and resolutions.

[39] 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.

[40] For example, rather than examining the share of guilty cases, I use the share of cases for with a guilty outcome for at least one charge that is observed *within 1 year of filing*. In base year 2006, 83 percent of circuit court cases in my sample were resolved within 1 year of filing. District court cases tend to be simpler and faster-moving than circuit court cases and do not suffer the same issue. I discuss this in more depth in Appendix B.1.

5 Methods

5.1 Methods: Two-way Fixed Effects

I test for the effects of body-worn cameras on policing and the courts using a difference-in-differences strategy based on the rollout of body-worn cameras across law enforcement agencies in Virginia. In my first specification, I use OLS with two-way fixed effects (TWFE) to estimate the ATT of body-worn cameras under the following model:

$$Y_{lt} = \alpha + \tau D_{lt} + \delta X_{lt} + \gamma_t + \lambda_l + \epsilon_{lt}$$

Here, $D_{lt} = 1[t \geq T_l]$ is an indicator that takes the value of one for adopting localities during or after the quarter of adoption. The vectors γ_t and λ_l account for quarter and locality-specific fixed effects, while X_{lt} contains covariates that vary across locality (l) and quarter (t). The key parameter of interest is τ, the effect of body-worn camera adoption on the outcome of interest.

5.2 Methods: Imputation Estimator

With staggered treatment timing, this TWFE estimator implicitly requires the assumption that τ is time and treatment cohort invariant. However, oftentimes treatment effects will exhibit heterogeneity in these dimensions, introducing bias in the estimates. Goodman-Bacon (2021) and Sun and Abraham (2020) show how estimates of τ or even more flexible estimates of τ_{lt} calculated with conventional event studies are not always reliable and reflect weighted averages of many comparisons across groups. These weighted comparisons may not reflect the intentions of the researcher.[41] Multiple new and modified estimators emerged in recent years to address these shortcomings (Borusyak, Jaravel, and Spiess, 2021; Callaway and Sant'Anna, 2020; Sant'Anna and Zhao, 2020; Chaisemartin and D'Haultfoeuille 2020 ; Wooldridge, 2021).

[41] For example, the aggregated treatment effect does not exclude the "forbidden comparison" of newly treated to previously-treated groups, and treatment effects for units treated in the middle of the sample will receive greater weights than earlier or later treated units. With heterogeneous treatment effects over time, including forbidden comparisons can even cause estimates to be mis-signed.

In my context, law enforcement adopt body-worn cameras at different points in time, and heterogeneity in effects is theoretically plausible due to changes in salience at the policing stage and attorney adaptation within the courts. In light of this, I also implement the modified event study framework developed by Borusyak, Jaravel, and Spiess (2021). This imputation estimator (BJS) uses untreated observations to estimate locality and quarter fixed effects, which are then used to impute counterfactual untreated outcomes for treated observations. The difference between the observed outcomes and their imputed counterfactuals gives a locality and quarter specific treatment effect which can then be aggregated into the desired estimand. That is, using only control observations, I estimate

$$Y(0)_{lt} = \lambda_l + X'_{lt}\delta + \epsilon_{lt}$$

I then use the estimates of $\hat{\lambda}_l$ and $\hat{\delta}$ to calculate $\hat{\tau}_{lt} = Y_{lt} - \hat{Y(0)}_{lt}$ for each locality in each quarter.[42,43] I aggregate these in two ways: one showing an overall ATT across all treated locality-quarters, which will be interpreted like the ATT from a constant effect TWFE model, and one showing estimated average treatment effects for each of the four quarters following implementation. In doing so, I can discuss short term treatment dynamics.

5.3 Identifying Assumption

The key underlying assumption for the difference-in-differences strategy employed by both of the above methods is that of parallel trends.[44] I provide empirical support for this assumption using the test proposed by BJS (2020), wherein I estimate an expanded version of the previous model using OLS on only untreated observations.

$$Y(0)_{lt} = \lambda_l + X'_{lt}\delta + W'_{lt}\gamma + \tilde{\epsilon}_{lt}$$

[42] $X'_{lt}\delta$ nests the time fixed effects but also includes time-varying controls, such as defendant race and sex shares and the prevalence of different case types. A similar generalization can be made for the locality fixed effects term to nest both these fixed effects and unit -specific trends in $A'_{lt}\lambda_l$ however I use only the fixed effects in my specification.

[43] Wooldridge (2021) describes the BJS estimator with unit-specific and time dummies as identical to his extended TWFE estimator.

[44] A distinct but related assumption is that of no anticipatory effects, however the same methods used to test for parallel trends can reveal violations of the no anticipatory effects assumption.

In contrast to the model used to estimate body-worn camera treatment effects, this includes a vector W_{lt} of indicator variables for quarters leading up to adoption. I then conduct a joint significance test of the coefficients on these leads.[45] 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. Across the 22 pre-trends tests for my primary results, none show statistically significant evidence of pre-trend violations at the ten percent significance level when using a four-period lead. When using the eight-period lead, I found two such violations. There does not appear to be evidence of systematic violations of the parallel trends assumption.

One possibility that this test cannot fully account for is that the mechanism for treatment assignment introduces an undetected violation of the parallel trends assumption. Body-worn camera adoption is not random by design or in practice; police departments must choose to implement a body-worn camera program. The resulting set of treated courts, shown in Table 2, are more likely to be in populous areas with more cases, a higher share of Black defendants, and are more likely to be served by a public defender's office. This nonrandom assignment alone does not necessarily violate the parallel trends assumption, but Kahn-Lang and Lang (2018) argue that it does require further justification that the assumption is valid.[46]

To this point, I argue that body-worn camera adoption decisions are plausibly exogeneous to court processes and resolutions. The LEMAS-BWCS survey shows that law enforcement adopted the technology in pursuit of improvements to police interactions rather than desired changes in the courts. Fifty of the 53 adopting agencies surveyed in Virginia cited non-court motivations as the primary reasons for using the cameras. Within my samples, the baseline differences between treated and untreated courts shown in Table 2 are largely related to community characteristics rather than court processes or resolutions. The shares of cases with a guilty disposition, dropped, or amended charges are all similar between treated and untreated courts.

[45]This method differs from other commonly used pre-treatment trend tests primarily in that it uses only untreated observations. This restriction on usable observations comes from the same concerns about treatment effect heterogeneity established in the new DiD literature. This test also sidesteps the issues raised in Roth (2021) regarding survivor bias for estimates that pass common pre-trends tests. See Wooldridge (2021) for equivalence between this method and a common pre-trends test in a fully saturated model.

[46]Commonly this justification invokes plausible exogeneity in treatment assignment or timing (Rambachan and Roth, 2020).

Body-worn camera adoption may not be exogeneous to the police behaviors I study with the case filing panel. As a result, I provide supplementary analyses that directly mitigate the selection concern by using only not-yet treated localities as controls. To do so, I end the sample period at the fourth quarter of 2017 and use the 2018 adopters as "never treated" controls.[47] By using only adopters I weakened the criteria for exogeneity of treatment: now adoption itself can be endogeneous but the timing of adoption for this group should not be. Deshpandi and Li (2019) use this strategy in their study of the effects of social security office closings on disability program participation, as does Kim (2020) in his national study of the effects of body-worn cameras on policing. Intuitively, the earliest adopters could break this assumption. For example, they may be more innovative or motivated than the typical department. To mitigate this concern, I use only courts treated during the surge in adoptions from 2014-2017 for this supplementary analysis. For this group, factors such as administrative hurdles, as Kim (2020) demonstrated and exploited, can stagger the timing of body-worn camera adoption amongst an otherwise similarly selected group.

6 Results

6.1 District Court

6.1.1 Case Filing Effects

I test for effects of body-worn cameras on policing by evaluating changes in the set of cases that enter district courts. I begin by looking narrowly at the subset of cases most likely to show body-worn camera effects, and then broaden the scope of the analyses to evaluate changes in the average filing. I test for changes in the prevalence of "civilization effect cases" using a log-transformed count variable, $Y_{it} = ln(civ\_case\_counts_{it} + 1)$.[48] Civilization effect cases are relatively uncommon: there are 33.1 such filings per court-quarter in the sample. In some court-quarters there are no such cases, so to include these zero-counts I add one to all counts before applying the transformation.[49] I apply a similar transformation

[47] This step enables me to calculate treatment effects for the 2017 adopters with BJS.

[48] This definition allows me to interpret estimates in approximate percent change terms.

[49] At times, particularly when there are many zeros, results can be sensitive to the selection of the added constant when shifting the outcome variable before applying a natural log transformation. I conduct sensi-

to the second outcome of interest, the overall number of cases filed in each quarter after adoption. However I do not observe zero-counts in this outcome and thus do not shift the variable to accommodate the log transformation. I define this overall case count variable as $Y_{it} = ln(case\_counts_{it})$. I then transition to testing for evidence of changes in the average characteristics of the cases that enter the courts by evaluating changes in the share of cases with multiple charges and the share of misdemeanors.

I provide estimates of the overall ATT for each outcome in Table 3a using the traditional TWFE estimator, the new BJS imputation estimator, and the BJS estimator on the restricted sample of only 2014-2017 adopters. Results are stable across all three estimation samples and methods, showing similar point estimates and significance levels.

The results in Table 3a provide evidence that body-worn cameras induce civilization effects in the narrower set of "civilization effect cases". All three specifications show economically and statistically significant reductions in these cases, ranging from an 8.1 percent to an 11.5 percent decline. The average of these estimates, 10.2 percent, would be equivalent to a reduction of 3.3 such cases per quarter from the mean across courts.

In Figure 4 I show this result decomposed into the BJS quarterly ATT estimates in event time. The plot shows the estimated treatment effect in each of the first four quarters following body-worn camera adoption as well as the estimates on the lead variables under the pre-trends test described in section 5.3. In quarter 0, the quarter in which courts become treated, there is an immediate decline in civilization effect cases compared to the pre-adoption periods. This decline reflects a level shift in the outcome at the time of adoption that persists throughout the following year.[50]

However, while these results show that body-worn cameras affected police interactions, it does not appear that the effects are widespread: the typical case filing did not change after police began using body-worn cameras. I do not find evidence that body-worn cameras

tivity tests by varying the constant and using an alternative inverse hyperbolic sine transformation to verify that this selection does not drive my results. The results are similar across sensitivity tests: for example, using 0.1 as the added constant yields a point estimate of -0.110, statistically significant at the 5% level using the BJS method while the inverse hyperbolic sine transformation yields an estimate of -0.109, statistically significant at the 1% level.

[50]This event study does not show heterogeneity in treatment effects over time, which could drive bias under a traditional TWFE model. This, coupled with the relatively short window in which most adoptions took place, could explain why the choice of BJS or TWFE appears to be inconsequential for my context.

reduced the overall number of case filings, nor that case characteristics changed. These results are not only statistically indistinguishable from zero, but are economically insignificant as well. The TWFE estimate for the transformed overall case count variable amounts to a 0.6 percent reduction in case filings, and the BJS estimate shows a statistically insignificant increase of only 1.5 percent. The intensive margin outcomes are precisely estimated null results which show minimal variability in the estimates across specifications. The estimated reduction in the share of cases with multiple charges ranged from 0.1 to 0.4 percentage points while the estimated reduction in the misdemeanor share ranged from 0.2 to 0.3 percentage points.

In total, while body-worn cameras induce policing effects, the "politeness" that cameras may engender does not measurably affect the typical case filing.

6.1.2 Case Processes and Resolutions

Although I found evidence of civilization effects at the policing stage, these changes were restricted to a small subset of charges representing only about 0.1 percent of case filings in the sample. Outside of this subset, the case filings did not systematically change and so results at the court-based outcomes stage are unlikely to reflect selection arising from policing effects.[51] Because of this, effects of body-worn cameras on court-based outcomes reflect only the evidentiary and attorney time use channels.

I show three case process results of interest (the share of cases in which a prosecutor drops a charge, the share of felony cases that are certified to the circuit court from the district court, and the share of cases disposed within 1 year) as well as two case resolution outcomes of interest (whether the defendant is found guilty and whether they are sentenced to time) in Table 3b.[52] Given the parity across BJS and TWFE estimates, I present only the BJS estimates in this and subsequent tables, but include TWFE results in Appendix C.3.

[51] If the cases entering the courts appreciably change after body-worn camera adoption due to policing changes, then estimates of body-worn camera effects on case outcomes may reflect not only the court-based evidentiary and time use effects but also these policing-based case changes. For example, if police make fewer marginal arrests after body-worn camera adoption 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. However, in practice, the cases are not observably changing.

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

Across both the felony and misdemeanor samples and five outcomes, I do not find any statistically significant effects of body-worn cameras on case processes and resolutions. The magnitudes of the point estimates on treatment effects are also small. In felony cases, prosecutors are estimated to have dropped charges in 0.4 percentage points fewer cases, while the share declined by 0.2 percentage points for misdemeanors. Felonies advanced to the circuit court at practically the same rate after body-worn cameras as before; this share increased by only 0.7 percentage points, again not statistically distinguishable from zero. And, cases were no less likely to move through the courts within one year after body worn cameras were introduced: the shares of cases resolving in a year were estimated to decline by 1 percent and 0.1 percent for felony and misdemeanor cases, respectively. The resolutions of these cases were also unchanged: the share of district court misdemeanor cases that concluded with a guilty disposition was estimated to reduce by 0.1 percentage points after body-worn camera adoption and 0.5 percentage points more often amongst felonies. For felony cases, the point estimates of the share that resulted in incarceration was estimated to decline by 0.6 percentage points while this share reduced by 1.1 percentage points at the misdemeanor level.

6.2 Circuit Court

By the time a case advances to the circuit courts both police and court actors will have interacted with the case. Although the circuit court has jurisdiction over felonies, preliminary hearings for these cases take place at the district court level and so evidence, negotiations, and pleas routinely result in charges being dropped, dismissed, or otherwise disposed before reaching the circuit courts. Because of this, circuit court filings are a selected set of cases that persisted past district court-level off-ramps and changes to these filings can reflect a combination of all three prospective channels for body-worn camera effects. Yet while the mechanisms for any changes are difficult to disentangle at this level, the aggregate changes themselves are valuable to characterize. Felony convictions carry sentences of at least a year of imprisonment, and the average sentence length in my sample was six years.[53] Felony convictions can restrict labor market opportunities post-release, and until recently caused

[53]See Appendix B.3. for a more thorough discussion of this measure of sentence length.

permanent disenfranchisement of convicted offenders in Virginia (Brennan Center, 2018).

I use a similar set of case filing outcomes at the circuit court level as at the district, including measures for the overall number of cases and number of civilization effect cases entering the courts as well as the share of these cases that carry multiple charges.[54]

Estimates of the effect of body-worn cameras on these filings are more volatile than the district court estimates and also more sensitive to the selection of the control group. Under my primary BJS specification I find some evidence for short-term reductions in both civilization effect cases and the overall number of cases entering the circuit courts after police begin using body-worn cameras. In the adopters-only sample, however, which covers a slightly truncated time period compared to the full sample, the point estimate for the reduction in civilization effect cases diminishes from -9.6 percent to a statistically insignificant -3.9 percent.[55] Similarly, the estimated 6.5 percent 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 and disappears when restricting analyses to the adopters samples. Given the district court results, the precise mechanisms for these potential reductions are not entirely clear: across specifications I did not find evidence of civilization, time use, or evidentiary effects at the district court level that would cause cases to attrit from the courts before reaching the circuit court sample.

Despite the incongruity in case filing results across specifications at the circuit court level, the case process and resolutions results are stable and confirm the narrative of no effects evident in the district court. As in the district court analysis, I use the share of cases in which a prosecutor drops a charge and the share of cases disposed within one year as measures of court processes and the share of cases with a guilty disposition and positive

[54]As before, I use a natural log transformation for case counts. Because there are fewer cases in circuit courts, I define both overall cases and civilization case outcomes as $Y_{it} = ln(count_{it} + 1)$. The estimates for the overall case count outcome variable are not sensitive to my choice of shift constant or to selection of transformation: the point estimates differ from my primary specification by only 0.001 and 0.0002 when using a shift constant of 0.1 and the inverse hyperbolic sine transformation, respectively. For civilization effect charges, which present with more court-quarters with no qualifying cases, this choice becomes more influential but the overall result is still the same; I find a statistically significant reduction of -0.091 (significant at $\alpha = 0.05$) under my primary specification, -0.114 ($\alpha = 0.10$) with the alternative shift constant, or -0.101 ($\alpha = 0.05$) with the inverse hyperbolic sine transformation.

[55]If I include all adopters, not just the 2014-2017 cohorts, the effect disappears entirely, with a point estimate of 0.2 percent.

sentence time as measures of case outcomes.

Table 4 shows that cases do not show evidence of slowing down to an economically or statistically significant degree; the share of cases which resolved fully within one year declined by only 0.6 percentage points after body-worn camera adoption, which is statistically not distinguished from zero. Because at the circuit court level I add a one-year timing condition to my case process and resolution outcomes, this result also affirms that the timing condition does not cause influential compositional changes in the sample after body-worn camera adoption.

I do find a statistically and economically significant reduction in the the other case process outcome, the share of cases in which the prosecutor dropped a charge. However, I am cautious in interpreting this result too strongly; this is the only statistically significant effect across 13 regressions across two court levels intended to capture changes in court processes and resolutions and does not correspond to changes in related outcomes. For example, the estimated three percentage point reduction in cases with dropped charges does not coincide with any increase in guilty dispositions or the share of defendants sentenced to time. On the contrary, these point estimates were both negative. This could be explained if prosecutors dropped individual charges less frequently after body-worn camera adoption but did so in cases with alternative charges for which defendants were still found guilty, maintaining stable case-level conviction rates. Taken in total, the results do not provide convincing evidence that body-worn cameras altered case processes or dispositions.

7 Heterogeneity Analyses

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

disconnect raises additional questions about how these null results came to be.

It could be that body-worn cameras are simply a smaller shock to the system than practitioners and advocates perceive them to be: case processes and resolutions could be sticky and unresponsive to changes in attorney time use or the noise reduction produced by body-worn cameras. Relatedly, body-worn cameras could be strongly influential only in a small subset of cases that do not change aggregate processes and resolutions. 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 the subset of Black defendants experienced differential changes to case filings, processes, or resolutions due to body-worn camera adoption.

7.1 Revisiting Evidentiary and Time Use Channels

In the primary results, case filing outcomes suggest that body-worn camera civilization effects do not induce widespread changes in caseloads or characteristics. The subsequent case process and resolution outcomes also show no effect, this time from the combined influence of evidentiary and attorney time use channels. One driver of the null result could be that these two channels offset one another: evidentiary benefits cancel attorney time costs in the aggregate. I test this counteracting effects hypothesis using variation in the likelihood that footage is available for a case. While I cannot observe whether body-worn camera footage was available for or used in a specific case, I can identify a subset of charges more likely to take place in front of an officer. These cases should be more likely to have body-worn camera video, and thus experience both evidentiary and time use effects. However, this does not mean that these are the only cases affected by body-worn cameras. Attorneys often work multiple cases at a time. If attorneys substitute their hours across cases to the

case activities with the highest marginal benefit[56] 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 and without body-worn camera footage should diverge. This divergence would be dominated by the evidentiary effect.

I classify a subset of charges as "more likely treated" (MLT) if they are likely to take place in view of an officer. 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 officers 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. Such charges differ from civilization effect charges because the alleged offense can also be initiated prior to interaction with an officer.[57] Under this partitioning, localities have an average of 40.6 MLT and 50.8 non-MLT cases per quarter in the circuit court; 49.9 MLT and 64.4 non-MLT felony cases in the district court; and 157.6 MLT and 824.2 non-MLT misdemeanor cases in the district courts.

I calculate for each locality-quarter the differences in the shares of each outcome across MLT and non-MLT cases. For example, if in court A in quarter 1 MLT cases received guilty dispositions 50 percent of the time and non-MLT received guilty dispositions 45 percent of the time, the differenced guilty outcome would be equal to 5 percentage points, expressed 0.05. I then estimate the model

$$Y\_Diff_{lt} = \alpha + \tau D_{lt} + \delta X_{ltg} + \gamma_t + \lambda_l + \epsilon_{lt}$$

[56]It's important to keep in mind the attorney will be optimizing her time use in a way 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.

[57]For example, one can refrain from pushing a police officer, forgoing a civilization effect offense. However, one cannot choose to not be in possession of an illicit substance after an interaction with an officer has begun.

When including control variables for the share of black and female defendants, case types, and multi-charge cases, I include separate variables for the MLT and non-MLT cases. The interpretation of the treatment effect estimates under the differenced-outcome are intuitive in that a statistically and economically significant point estimate would indicate a divergence in the outcomes across case types that is attributable to body-worn camera implementation. [58] 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, overall 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 resolutions as a whole.

7.2 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. Public advocacy for body-worn cameras grew against the backdrop of a police shooting in Ferguson, Missouri that ignited large-scale protests centered on racial disparities in policing (BBC News 2014). 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 (AP-NORC, 2015; Jones, 2020).

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 discrim-

[58] The underlying model when using this outcome is 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, $Y_{ltg} = \alpha + \tau D_{lt} + \delta_1 X_{ltg} + \delta_2 MLT * X_{ltg} + \gamma_{gt} + \lambda_{gl} + \epsilon_{glt}$ 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. Intuitively, because a fully partitioned specification is equivalent to differencing the treatment effects from separate regressions for each group, covariates are only used within-group for estimation. In contrast, in the differenced model, the full set of information is used simultaneously. Results are similar across specifications when using fully interacted and differenced outcomes: I show fully interacted TWFE results in Appendix C3. Also in Appendix C3 I show the robustness of my results to the exclusion of small localities, since the differenced outcome cannot be calculated if either no MLT or MLT cases are observed in a given court-quarter.

ination" (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 shows that some policies intended to ameliorate these disparities and their effects can unintentionally exacerbate them (Doleac and Hansen, 2020).

Because body-worn cameras are intended by many to especially improve police interactions for Black members of the public, I test for differential effects of body-worn cameras on Black defendants. At the policing stage, civilization effects may be more pronounced for Black defendants. At the courts stage, if court actors are biased against Black defendants, body-worn camera evidence may differentially reinforce the testimonies of Black defendants.

I employ the same techniques used in the previous section of this paper to test for differential effects, but apply them to a modified panel where cases are aggregated to the quarterly court level within two racial groups: Black and non-Black defendants.[59] 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-

[59] 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.

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.

8 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 its 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 that 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 researchers and a broad base of community stakeholders in criminal justice policy decisions may mitigate the unintended consequences of seemingly simple policy changes.

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10 Tables and Figures

10.1 Tables

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*

Table 2: Comparison of treated and untreated localities at early-sample baseline

Panel A: Case Filings	**District**		**Circuit**	
	Untreated	*Treated*	*Untreated*	*Treated*
Cases	4,424.8	3,986.1	80.2	184.4
Civilization Effect Cases	27.6	42.1	3.6	9.0
Multi-charge Cases	17.2%	19.2%	49.1%	46.4%
Count Localities	33	70	28	68

Panel B: Case Processes & Outcomes	***District Misdemeanors***		***District Felonies***		***Circuit Felonies***	
	Untreated	*Treated*	*Untreated*	*Treated*	*Untreated*	*Treated*
Female	28	29	22	24	21	22
Black	21	34	26	43	27	45
Multi-charge	17	18	57	54	47	43
% Public Defender	33	44	33	43	25	46
% Cases with amended charge	9	9	4	4	2	2
Avg Sentence Time (days)	21.7	28.0	67.1	81.8	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	136.1	53.7	124.3
Count Localities	33	70	33	70	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. Case filing panels are not case-type specific.

Table 3a: BWC adoption effect estimates, district court case filings

VARIABLES	TWFE	BJS	BJS 2014-2017 Adopters	Mean Value
Ln(Cases)	-0.006	0.015	0.007	3,578.8
	(0.029)	(0.030)	(0.035)	
Ln(Civilization Cases + 1)	-0.106**	-0.115**	-0.081†	33.1
	(0.035)	(0.037)	(0.049)	
Share Multi-charge	-0.004	-0.004	-0.001	0.188
	(0.004)	(0.005)	(0.005)	
Share Misdemeanor Cases	-0.003	-0.002	-0.003	0.924
	(0.003)	(0.003)	(0.004)	
Observations	4,141	4,141	2,340	

Cluster-robust standard errors in parentheses
**p<0.01, * p<0.05, † p<0.1

Note: Means of rows 1 and 2 reflect the average counts for the underlying variable across the full sample, but the outcomes used to calculate estimates are ln(count) and ln(count+1), respectively.

Table 3b: BJS estimates, case processes and resolutions

VARIABLES	Prosecutor Dropped Charge	Case Certified	Guilty	Sentenced to Time	Disposition: 1 year°
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

Table 4: BJS estimates, circuit court outcomes

	Case filings		
	Ln(Cases + 1)	Ln(Civilization Cases + 1)	Share Multicharge Cases
Treatment Effect	-0.065†	-0.091*	-0.007
	(0.033)	(0.037)	0.009
Mean	145.3	6.9	0.486
Observations	5,472	5,472	5,463

	Case processes and resolutions				
	Prosecutor Dropped Charge	Case Certified	Guilty	Sentenced to Time	Disposition: 1 year°
Treatment Effect	-0.030*	--	-0.006	-0.018	-0.006
	(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

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*
Means of count variables reflect the average counts of the underlying variable, while outcome variable used in regression is ln(count + 1).
Controls included in case process/resolutions regressions include share female, black, and of case classes.

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

VARIABLES	Prosecutor Dropped Charge	Case Certified	Guilty	Sentenced to Time	Disposition: 1 year°
Treatment Effect	0.006	--	0.013	0.014	0.011
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	0.019		0.334	0.300	0.842
Treatment Effect	-0.001	-0.001	0.002	-0.004	0.002
District Court (Fel.)	(0.014)	(0.015)	(0.016)	(0.015)	(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	0.957	0.752	0.903	0.800	0.764
Treatment Effect	0.006	--	0.007	0.011	-0.004
District Court (Misd.)	(0.007)		(0.012)	(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	0.435		0.556	0.379	0.704

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 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.*

Table 7: BJS estimates, racial heterogeneity

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

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.

10.2 Figures

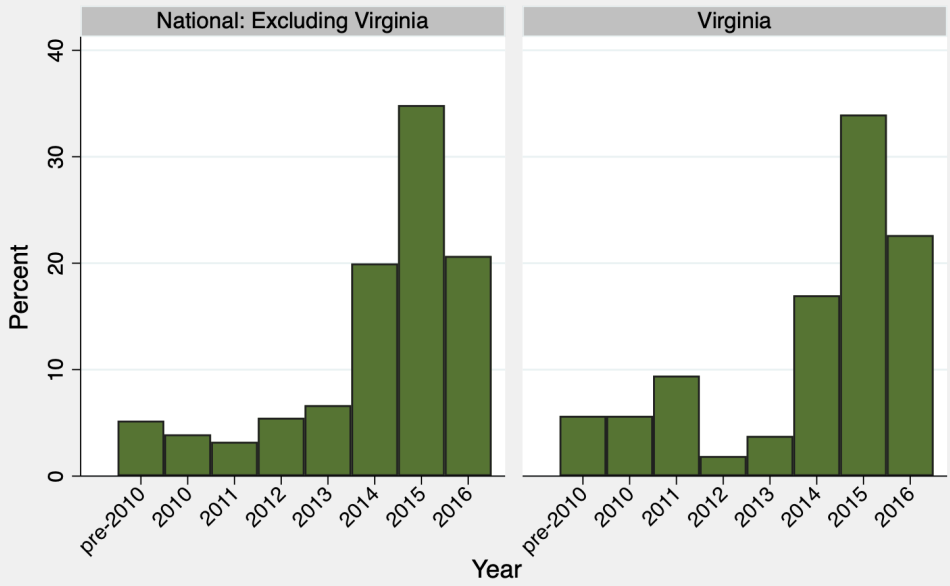


Figure 1: Year of BWC adoption for law enforcement agencies in LEMAS-BWCS (adopters only)
National adopters: n = 1862 (out of 3843 respondents)
Virginia adopters: n = 53 (out of 85 respondents)

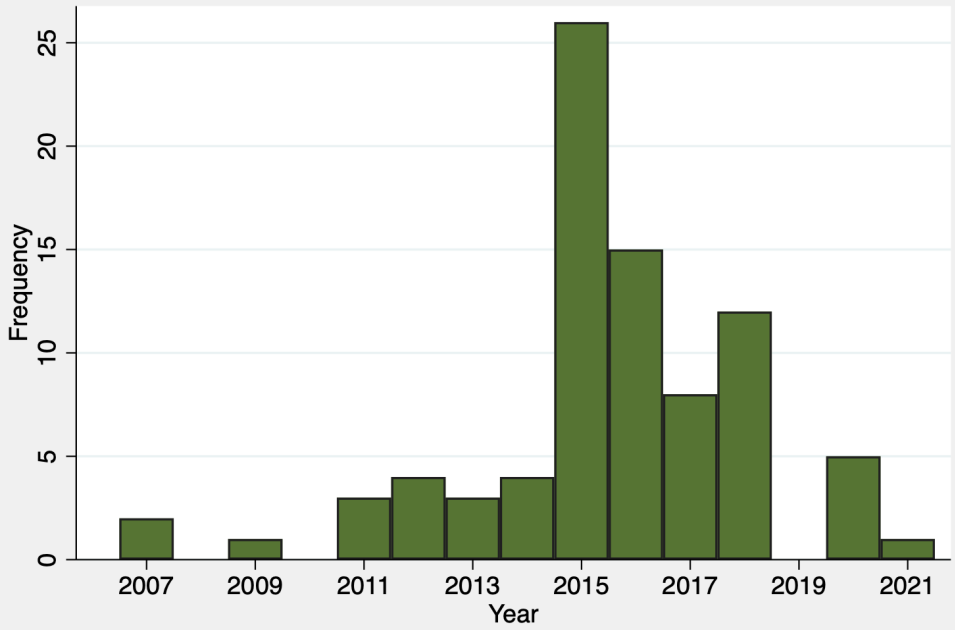


Figure 2: Year of BWC adoption by district court jurisdiction, Virginia
Source: author's data collection

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

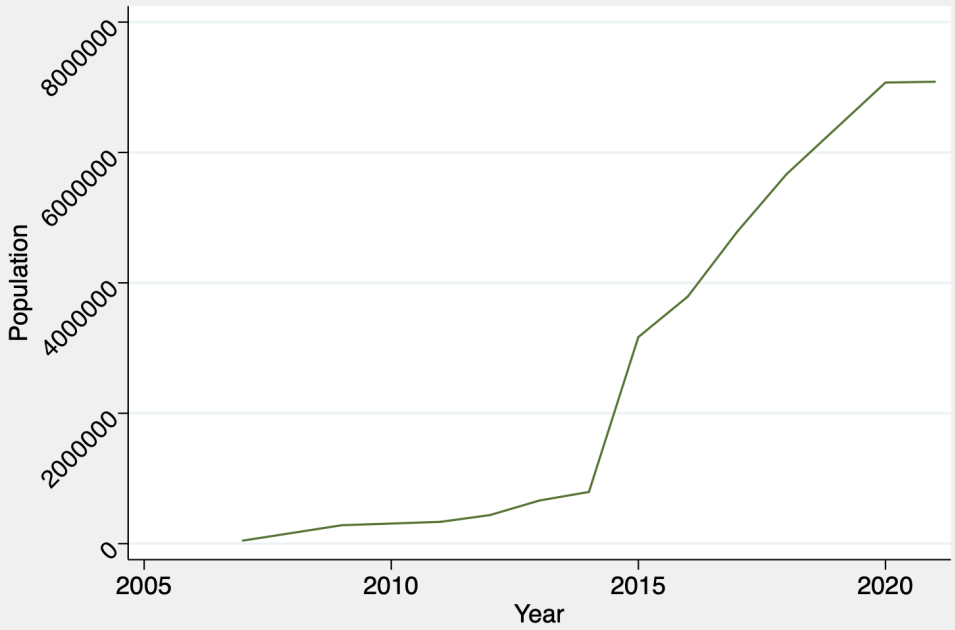


Figure 3: Population living in a district court jurisdiction where BWCs are used
source: author's data collection, intercensal population estimates (described in data appendix)

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.

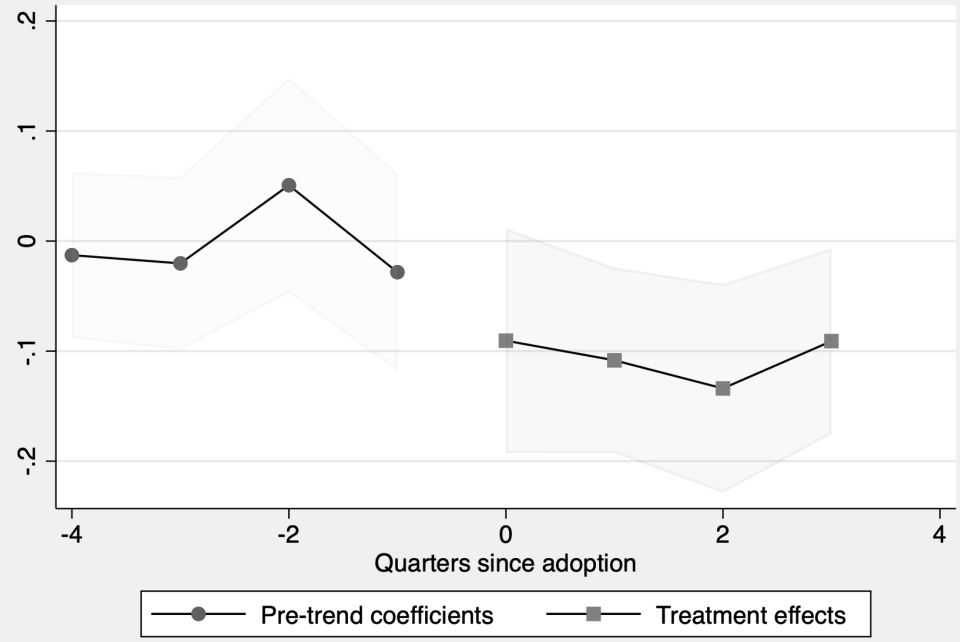


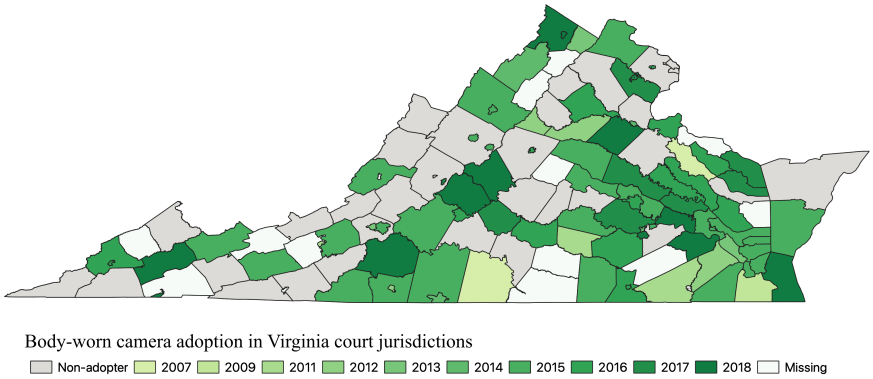
Figure 4: District court civilization effect charge quarterly estimates

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 cal-

culated 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]		

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

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:

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

As discussed in section 5.3, 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. The descriptive statistics presented in Table 2 show 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. Here I show court-stage results using an adopter-only sample to demonstrate the robustness of my results to using an exclusively not-yet-treated control group.

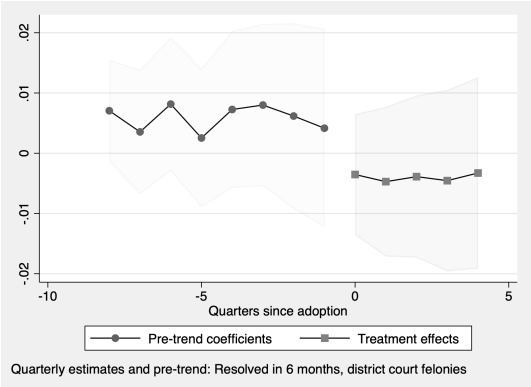
Table B.2: BJS estimates, case processes and resolutions; treated sample

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

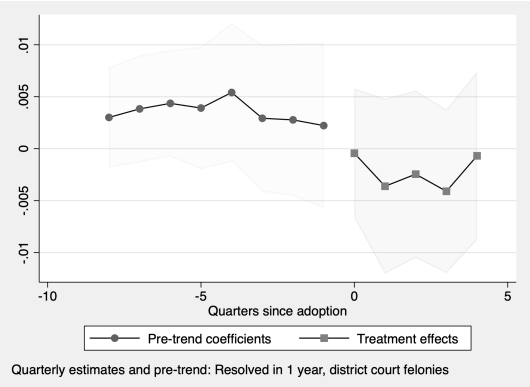
Robust standard errors in parentheses
**p<0.01, * p<0.05, † p<0.1

°*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

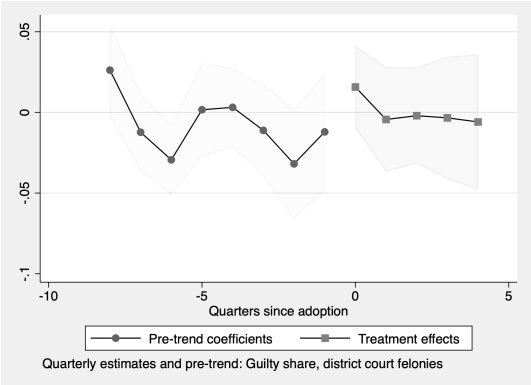
B.2 Selected Event Study and Quarterly Plots



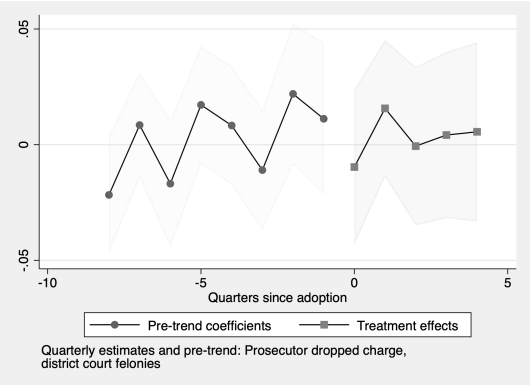
(a)



(b)



(a)



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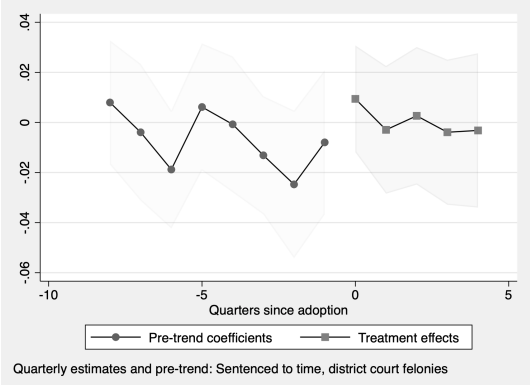
.04
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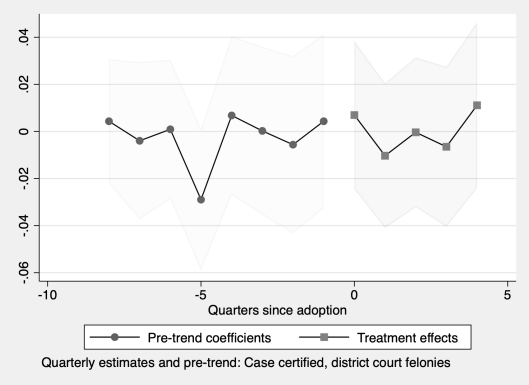
Quarters since adoption
Pre-trend coefficients
Treatment effects
Quarterly estimates and pre-trend: Sentenced to time, district court felonies

(a)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Quarters since adoption
Pre-trend coefficients
Treatment effects
Quarterly estimates and pre-trend: Case certified, district court felonies

(b)

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

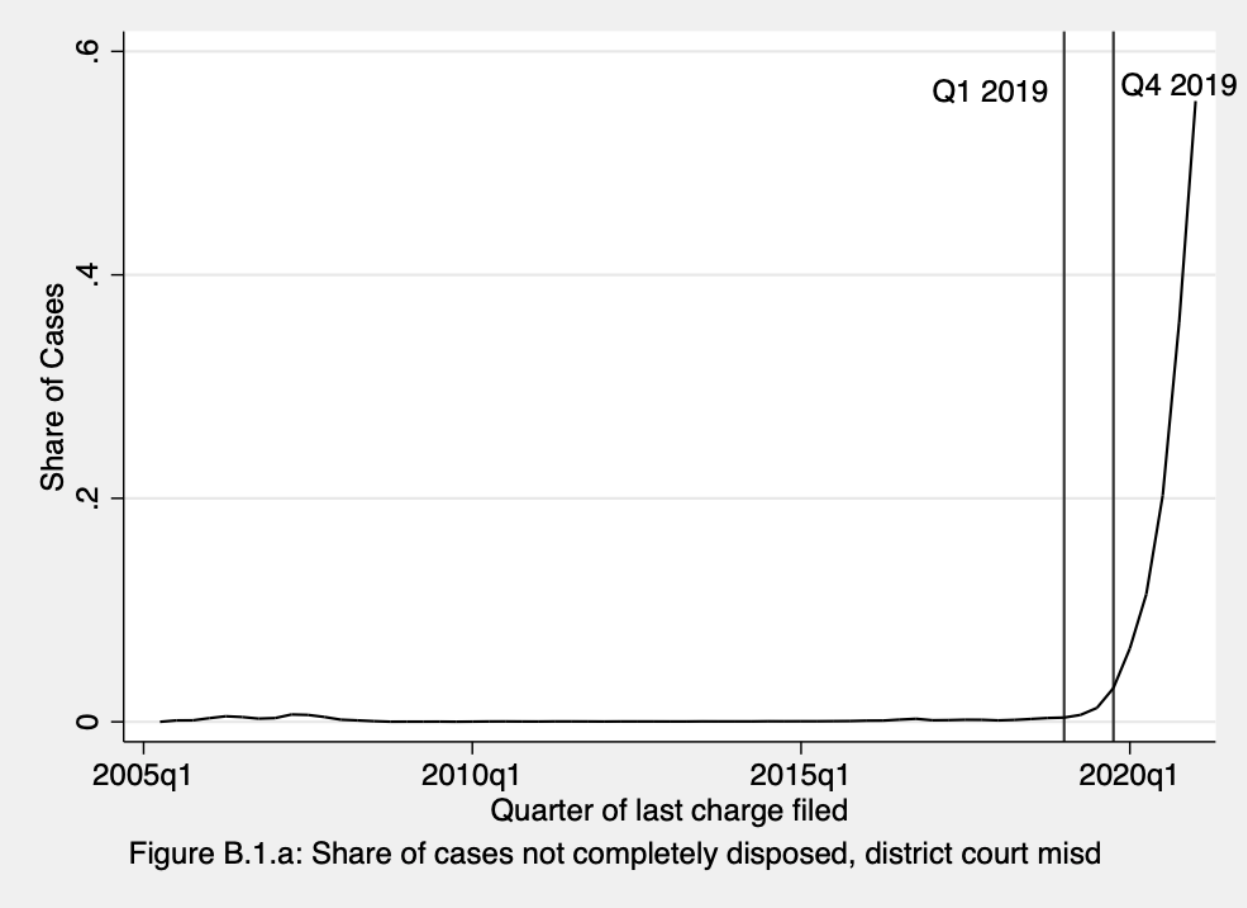

Figure B.1.a: Share of cases not completely disposed, district court misd

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

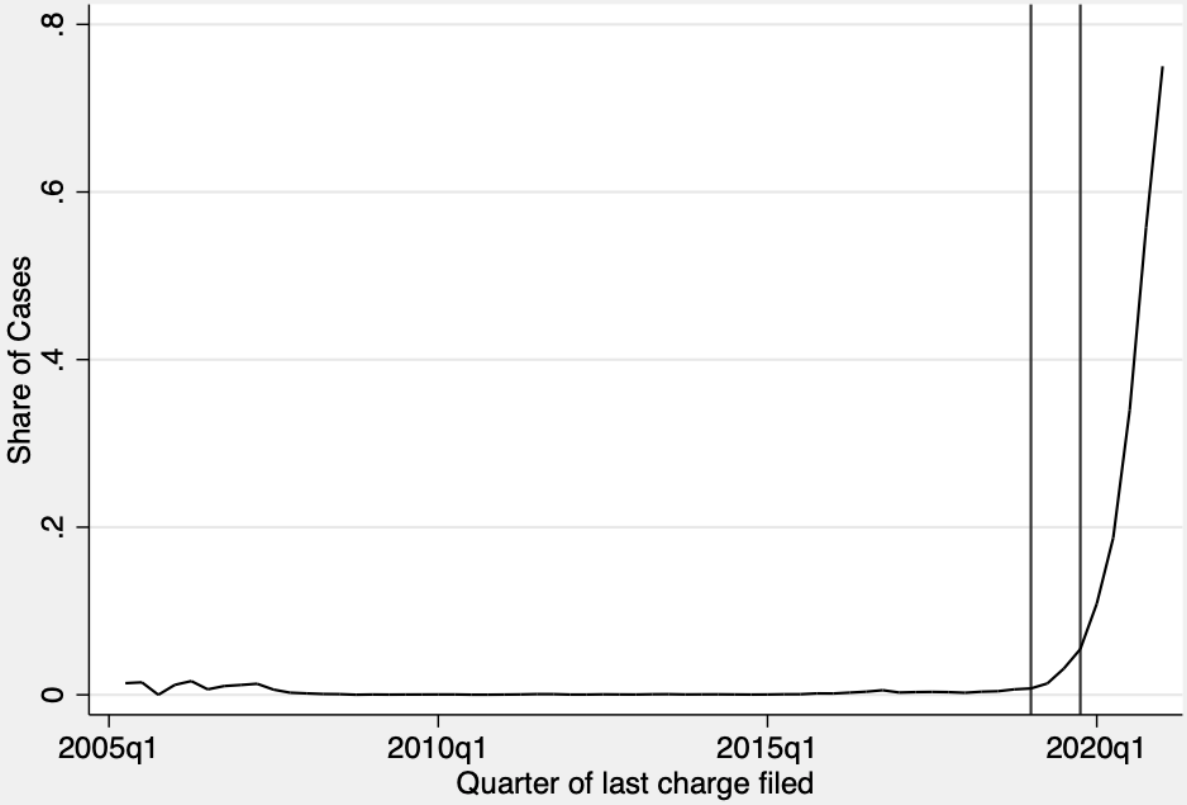

Figure B.1.b: Share of cases not completely disposed, district court felonies

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.

		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 TWFE.

Table C3: Pooled difference-in-differences estimates, court-based effects

VARIABLES	Charge Dropped	Case Certified	Guilty	Sentenced to Time	Disposition: 1 year°
Treatment Effect	-0.016	--	-0.009	-0.016	-0.009
Circuit Court	(0.013)		(0.011)	(0.013)	(0.009)
Mean	0.258		0.721	0.709	0.825
Observations	5,439		5,609	5,609	5,439
Treatment Effect	-0.004	0.005	0.003	-0.001	-0.008
District Court (Fel.)	(0.012)	(0.013)	(0.013)	(0.011)	(0.006)
Mean	0.402	0.592	0.294	0.245	0.964
Observations	4,129	4,129	4,211	4,211	4,129
Treatment Effect	0.002	--	-0.004	-0.008	-0.002
District Court (Misd.)	(0.004)		(0.006)	(0.006)	(0.004)
Mean	0.115		0.735	0.196	0.956
Observations	4,182		4,264	4,264	4,182
Race, sex covariates	x		x	x	x
Offense year, quarter FE	x		x	x	x
Locality FE	x		x	x	x

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*

Table C4: MLT heterogeneity: Robustness

		Fully interacted TWFE	Differenced TWFE	Differenced BJS	BJS Dropping Small Localities
Circuit	Prosecutor Dropped Charge	-0.001	0.004	0.006	--
		(0.011)	0.010	(0.011)	
	Guilty	-0.001	-0.002	0.013	--
		(0.012)	0.012	(0.013)	
	Sentenced to Time	-0.001	-0.001	0.014	--
		(0.012)	0.012	(0.013)	
	Disposition: 1 year	0.003	-0.001	0.011	--
		(0.010)	0.009	(0.010)	
District Felony	Prosecutor Dropped Charge	0.006	0.005	-0.001	0.007
		(0.013)	(0.013)	(0.014)	(0.014)
	Guilty	0.015	0.008	0.002	-0.010
		(0.016)	(0.015)	(0.016)	(0.013)
	Certified	-0.001	-0.004	-.001	0.005
		(0.014)	(0.014)	(.015)	(0.014)
	Sentenced to Time	0.005	0.001	-0.004	-0.010
		(0.014)	(0.014)	(0.015)	(0.012)
	Disposition: 1 year	0.003	0.003	0.002	-0.003
		(0.006)	(0.006)	(0.006)	(0.005)
District Misd.	Prosecutor Dropped Charge	0.005	0.007	0.006	--
		(0.007)	(0.007)	(0.007)	
	Guilty	0.003	0.001	0.007	--
		(0.010)	(0.010)	(0 .012)	
	Sentenced to Time	0.009	0.004	0.011	--
		(0.011)	(0.011)	(0.012)	
	Disposition: 1 year	-0.003	-0.004	-0.004	--
		(0.008)	(0.009)	(0.009)	

This table shows the robustness of likelihood of footage heterogeneity estimates to alternative specifications. The localities included in analysis for the "dropping small localities" sample omits all localities that recorded zero MLT or non-MLT cases in a given quarter year. This substantially changes the circuit court sample and introduces pre-trends violations in all outcomes and so I do not report these results here.

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.

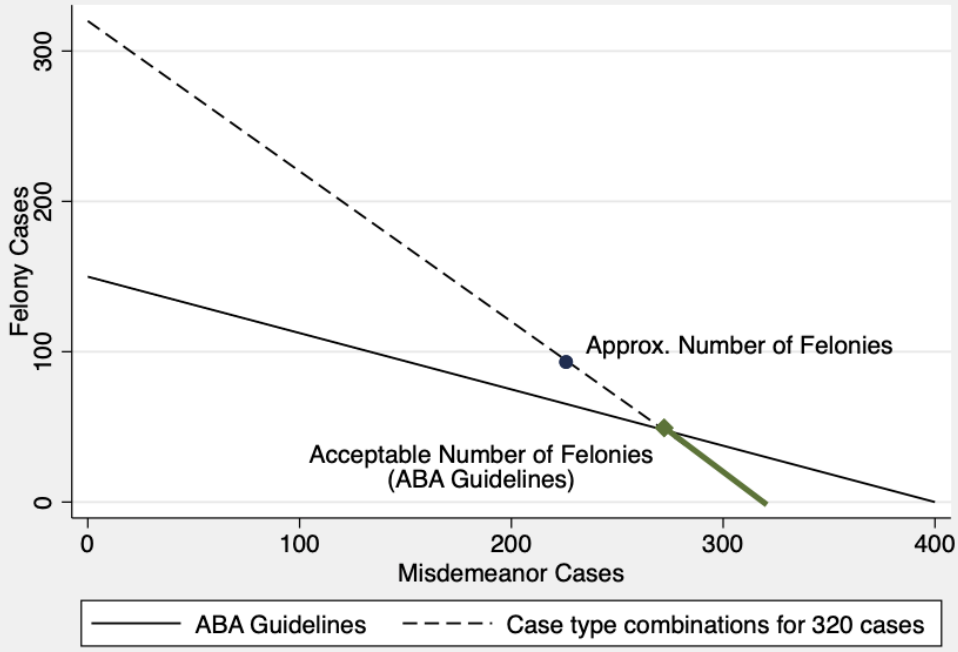

Figure D.1: PPF for defense attorneys under ABA guidelines and caseloads for VA public defenders