The Power of Observation:
An Empirical Analysis of the Effects of Body Worn Cameras on Police Use of Force and Productivity

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Abstract

Using the staggered adoption of body worn cameras (BWCs) by police departments in the 2010s, this paper examines the effects of introducing BWCs on police use of force and performance. Previous studies on BWCs in single-agency settings have been hampered by empirical challenges of spillover effects and common agency-wide effects, which likely explain why some studies found null effects. As the first cross-agency study on BWCs, this paper is able to overcome these empirical challenges. I find that BWCs are associated with a drop of 43% in use of force, a reduction of 81% in subject injury, yet not with officer injury, or other productivity measures such as crime and clearance rates. These findings imply that BWCs can be a powerful tool in the recent efforts to reduce use of force and improve public trust in police.

1 Introduction

Recent high-profile and controversial police use of force incidents have spurred protests across the nation, calls for justice, and a renewed push for police accountability and transparency. In the ensuing debate, officer body worn cameras (BWCs) have received extensive attention as a technological advancement that provides video documentation of police encounters with community members.

In the US, President Obama's Task Force on 21st Century Policing recogized BWCs as a potential solution to restore public trust in police. In addition, widespread public support and buy-in from law enforcement executives and officers have materialized into policy changes across the nation. The Obama Administration proposed in 2014 a subsidy of $263 million for the purchase of 50,000 cameras by local law enforcement agencies. Backed by an infusion of these federal funds, and reinforced by grants from state and local governments, BWCs are now widely used in the US. Specifically, they have been fully deployed by 60% of local police departments and 49% of sheriff's

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offices in the US as of 2016 (Hyland, 2018). Currently there is active national debate on whether to equip all US law enforcement officers with BWCs.

The question of whether BWCs are in fact a viable solution, however, is far from settled. Opponents cite their high expenses, possibility of de-policing and weakening in police authority, privacy concerns, and inconsistency of research evidence for their benefits.

Indeed research on BWCs so far has found mixed evidence. For example, in the earliest and one of frequently cited works among the supporters, Ariel, Farrar, and Sutherland (2015) randomized shifts in a small department in Rialto, CA, to find that use of force dropped by 58% for officers in treatment shifts. However, another randomized controlled trial (RCT) in Washington, DC, that randomized officers into wearing BWCs did not find a reduction in use of force or policing complaints (Yokum, Ravishankar, and Coppock, 2019).

The biggest challenge that hampered previous studies has been being limited to a single department or a handful of departments. Treatment and control groups are then assigned at the shift or officer level. As officers often work together in a team setting or they interact with officers in different shifts, researchers were constrained in their ability to control the treatment and control status of their subjects. Controls group officers could have direct effects of BWCs by working with treatment officers or they could modify their behavior through peer effects from treatment officers. In some RCT studies, the same officers would hold both control and treatment status as they rotate to different shifts. This contamination likely contributed to some studies finding null effects of BWCs on use of force.

Also single-agency studies are not able to capture agency-wide effects of introducing BWCs. This is because crime and policing variables are influenced by interactions, potentially over time, of subjects, officers and other civilians, and eventually affect both the treatment and control groups within the agency. For example, if BWCs induce officers to pull back on crime control activities (Peterson, Yu, La Vigne, and Lawrence, 2018), this would affect the agency-wide crime rates and, subsequently, other related policing variables such as officer injury, use of force, and clearance rates for both the treatment and control offcers. Also, to the extent that subjects cannot perfectly predict whether the back-up officers would come with BWCs, their compliance decisions would be affected by their expectations that officers with BWCs could arrive to the scene. Hence subjects, and potentially the officers, would adjust their behavior even if the officers facing the subjects do not actually have BWCs. Single-agency studies that randomize treatment status within an agency are likely to miss these important agency-wide effects of BWCs and could understate the effects of BWCs. Moreover, single-agency studies are not able to directly measure crime rates. This means that even if they find reductions in use of force, they cannot examine if those improvements came at the expense of crime control capabilities.

I overcome the above empirical challenges by conducting the first cross-agency study on the effects of BWCs. The aim is to approximate the ideal experiment in which agencies that are randomly assigned to the treatment group are compared with those assigned to the control group. This paper uses a quasi-experimental event study approach using a newly gathered incident-level use of force database that includes all local agencies in New Jersey and another novel database that contains the exact dates of BWC adoption for a large number of US agencies. By using the staggered adoption of BWCs across different agencies and high-frequency monthly panel data, I am able to credibly measure the BWC effects while avoiding the within-agency contaminations and taking into account the agency-wide effects. In addition, the large and representative sample used in this study greatly improves upon previous studies in terms of generalizability.

Using my sample of 126 New Jersey police departments, I find that BWCs had substantial effects in reducing police use of force. BWCs led to a 43% drop in overall use of force, with the effects present across all force types. The reduced use of force is also accompanied by an 81% decline in subject injuries. These estimates are comparable to the upper range of estimates found in single-agency RCT studies. Moreover, these declines did not come at the expense of other margins of possible adjustments, as I do not find evidence for changes in officer injury and crime and clearance rates. I further check the latter results on police performance using a national sample of 2,781 agencies, and confirm that BWCs did not hamper police performance.

The results from this study indicate the importance of properly taking into account the spillover and agency-wide effects. There is a wide variation of findings on the estimates for the BWC effects from single-agency RCT studies. Single-agency studies with greater contamination from spillovers and agency-wide effects are likely to find null or underestimated results.

In addition to contributing to the existing literature on BWCs which I delineate in Section 2.3, I add to a few literatures in Economics. Most closely, this paper contributes to the literature on the effects of police inputs on crime outcomes. Most of this literature has focused on measuring the impact of hiring more police officers (Levitt, 1997; McCrary, 2007; Miller and Segal, 2019; Mello, 2019). Recent papers evaluate innovations in policing such as the use of computerization (Garicano and Heaton, 2010) and DNA databases (Doleac, 2017). By studying a technology that has been introduced mainly to improve use of force and accountability, this paper differentiates from previous research on inputs that sought to improve general crime rates and clearance rates.

The focus on use of force follows a recent literature that examines use of force, civilian complaints and their determinants. Fryer Jr. (2018) explores racial differences in police use of force while Ba (2017) and Rivera and Ba (2019) study the interactions between civilian oversight and use of force and complaints. Annan-Phan and Ba (2019) also examines the effects of patrol environment on deadly force. Rozema and Schanzenbach (2019) studies intervention methods to predict officers with the most civilan allegations for misconduct.

More broadly, this paper adds to the large literature of technology adoption, work organization, and performance (e.g. Bresnahan, Brynjolfsson, and Hitt, 2002; Hubbard, 2003; Athey and Stern, 2002; Acemoglu, Aghion, Lelarge, Van Reenen, and Zilibotti, 2007; Aral, Brynjolfsson, and Van Alstyne, 2007). This paper focuses on employee monitoring technology that is growing in the workplace and yet has not been studied in this literature. On monitoring and supervison, I relate to the literature that has theoretically designed and empirically tested ways to curb undesirable behavior in organizations (e.g. Becker and Stigler, 1974; Rauch and Evans, 2000; Di Teila and Schargrodsky, 2003; Duflo, Greenstone, Pande, and Ryan, 2013).

I start with providing instititutional details of BWCs and describing previous studies in Section 2. Section 3 describes the data on use of force, BWC adoption dates, and other performance measures. Section 4 compares the characteristics of agencies that adopt and those that do not, and lays out the empirical strategy to overcome the hurdle of comparing these two groups of agencies. Section 5 presents my main results, both visually and in summary estimates for the effects of

BWCs. The incident-level data on use of force in New Jersey allows me to examine the effects of BWCs, while I can expand to my US sample to study other policing measures. I conclude with a cost-benefit analysis in Section 6 that shows that BWCs can also be beneficial by delivering savings from reduced payouts for civil lawsuits and settlements.

2 Background and Expected Effects of Body-worn Cameras

2.1 Adoption and Policies of BWCs

As a recording device that is more mobile than previous technologies such as car dashboard cameras, BWCs have the potential to reshape police-citizen interactions. BWCs can be worn at the front of the uniform or can be clipped on the head gear. Different departments have varying policies on what events to record. In my data sample, the majority of police departments that adopted BWCs require officers to turn on BWCs during routine calls for service, traffic stops, officer-initiated citizen contacts, firearms deployments, and execution of arrest or search warrants. A common "fail-safe" feature of BWCs allows them to be always on and save the 30 seconds of footage prior to the officer activating the record button.

Once the footages are recorded, they are usually stored with security protection to deal with concerns of privacy and evidence integrity. In the data, most agencies keep a log of or track internal access to video files. The video files are kept in storage for varying periods of time, with the modal response in my data being one month to a year. The videos may be kept for longer periods of time if, for example, they are associated with use of force, a citizen complaint, or a legal preceeding.

BWCs are not a new technology, although they keep evolving by incorporating recent smart technological advances. BWCs were tested in the United Kingdom as far back as 2005 (Harris, 2010). However, they did not gain widespread adoption in the US until a string of high-profile and controversial officer-involved killings in New York, Missouri, Illinois, and Ohio spurred public demand in the second half of 2014(Maskaly, Donner, Jennings, Ariel, and Sutherland, 2017). Figure 1 depicts the evolution of BWCs in the US and New Jersey. After a modest rise in the US, BWC adoption takes off in 2014. New Jersey lags behind a bit with the first adoption in the data happening in 2013.

The decision to adopt BWCs is not necessarily binary. Departments can choose to deploy them in select assignments such as traffic enforcement, criminal investigation, and drug enforcement. They may also choose to adopt at different scales. In the data, the mean cameras-to-officers ratio is 65% in the US and 54% in New Jersey. Most of the total acquision happen in the first month of adoption; for the departments that obtained BWCs in the month of the survey, the mean adoption ratio is 42% in the US.

From the perspective of police departments that are looking to obtain BWCs, BWCs were more than just a device. BWCs required very large data storage capabilities and extra personnel to manage the devices. Also they engendered public interest in viewing police videos and records, which required the departments to hire extra administrative personnel to review and redact sensitive contents. These factors posed a significant financial barrer.

The survey of US police executives, which also serves as one of my main data sources, shows that costs were a real concern (Table 1). A sheer 86% of executives that did not adopt BWCs said that costs were a primary reason; this is followed by public request burden, which was listed by 70% of the executives. Among those that adopted BWCs, the biggest obstacle facing them in implementation was that costs were greater than anticipated as reported in Table 2.

Because of the high expense, agencies looking to purchase BWCs resorted to funding opportunities given by local, state or federal governments. In addition to the high expense and the burden of grant processes, other obstacles such as privacy and liability concerns and support from the constituents further hindered their ability to implement BWCs. This cumbersome and lengthy process of obtaining BWCs aids my identification strategy as it helps rule out alternative explanations that involve other reforms that may happen at the same time as adoption of body cameras. In order to confound the estimates in my empirical framework, the reforms needs to happen in the same month of acquiring BWCs with the same long pre-reform waiting periods. However, there is hardly any technology or reform that reasonably matches BWCs in terms of their financial burden and expected effects on policing.

2.2 The Expected Effects of Body Worn Cameras

Theory has ambiguous implications for the effects of BWCs on use of force and police performance. BWCs provide a more objective documentation of police acitivities. BWCs might deter potential excessive use of force by increasing the cost of detection and punishment for the officers. This would put downward pressure on use of force. Use of force incidents might decrease without affecting other police performance. However, officers may decrease policing efforts with the presence of BWCs as their performance come under greater scrutiny. If excessive use of force occurs as accidents, as naturally occurring portions of overall policing, and officers do not have much control over the amount of excessive use of force given a fixed level of policing efforts, they might choose to decrease their policing efforts. In this case, use of force incidents would be accompanied by an increase in crime, a decrease in clearance rates, or both.

On the other hand, we might see an increase in use of force. BWCs have gained popularity among some police officers as they believe that the technology provides protection against false civilian complaints and lawsuits. In fact, recent studies on police misbehavior (Goncalves and Mello (2018) and Rozema and Schanzenbach (2019)) find that a small portion of officers cause disproportionate amount of problems. BWCs may act as an insurance for non-problematic officers and encourage them to increase policing efforts and use greater amount of force.

Reinforcing the effects of BWCs on either direction is the equilibrium consideration of other actors in police-civilian interactions. As modelled in Fryer Jr., 2018, subjects might decide how much to comply with the directions of the police officers depending on their beliefs about officers' use of force decisions. BWCs can signal to subjects on the extent to which officers modify their behavior. Also BWCs may have civilizing effects on subjects whose inappropriate behavior is now better captured as evidence. Subjects' decisions on compliance behavior feed into officers' current or future use of force decision-making.

Apart from the main channels through use of force discussed above, BWCs might independently affect police performance in other ways. BWCs might improve perception of fairness of police in the public mind. As civilians trust police more, they might help more with police investigations. Also, BWCs can be used as training tools or additional resource to gather prosecutory evidence.

This would help increase police productivity. On the other hand, insofar as civilians view BWCs as privacy invasion, they may hesitate starting potentially useful interactions with police. In this case, police performance may decline. These theoretical ambiguities call for well-idenfied empirical studies of the effect on BWCs.

2.3 Previous Studies on Body-worn Cameras

Lagging behind the wide-scale interest and adoption of BWCs has been a gradually increasing body of academic literature that has sought to examine whether BWCs have delivered on their promises of greater accountability and efficacy by the police and improved relations with the public (Lum, Stoltz, Koper, and Scherer, 2019). These studies have coalesced into multiple areas of inquiry. A significant number of those studies have used a deterrence framework to examine the impact of BWCs on officer behavior, including use of force (Ariel, 2016; Braga, Sousa, Coldren, and Rodriguez, 2018; Jennings, Lynch, and Fridell, 2015; Yokum et al., 2019), citizen complaints about officer behavior or conduct (Hedberg, Katz, and Choate, 2017; Peterson et al., 2018), and arrests (Ariel, 2016; Ready and Young, 2015). The results are mixed with a number of experimental studies that found that BWC-wearing officers used less force than non-BWC-wearing officers and other studies that demonstrated no significant difference in use of force by the presence or absence of BWCs.

Studies of BWCs have also looked at the impact of the technology on citizen behavior, generally looking at physical responses to police actions, for example manifesting in assaults on officers (Ariel, 2016; Ariel et al., 2018) or how the presence of the cameras may deter criminal or antisocial behavior (Ellis, Jenkins, and Smith, 2015; Police and Crime Standards Directorate, 2007; ODS Consulting, 2011). Here, again, the results have been mixed. With regards to assaults on officers, a number of studies have actually found the presence of BWCs increases those incidents while others have found a null effect (Headley, Guerette, and Shariati, 2017; White, Todak, and Gaub, 2017). Taken together, then, this group of studies finds very little evidence that BWCs have a civilizing effect on citizen behavior.

This growing body of research have added considerably to the overall knowledge of how BWCs affect various aspects of policing. Nonetheless, the previous studies have been limited by contamination issues and the inability to take into account agency-wide effects.

Single-agency studies are constrained by questions of spillover effects, as they randomize BWCs to officers or shifts. Officers with BWCs may work, or even partner, with officers without BWCs, and in studies where randomization occurs by shifts, the same officers may work with or without BWCs depedending on the day. The researchers significantly lose their ability to assign treatment status in a clean way, and hence the ability to learn about the treatment effect of BWCs. Furthermore, officers in the control group can learn from officers with BWCs through peer effects and adjust their behavior. By having agencies as the unit of observations in this study, I am able to overcome this contamination issue.

Relatedly, the agency-level framework in this paper is able to capture agency-wide effects from BWCs. Officers, potential victims, potential offenders, and other civilans are likely to adjust their behaviors over time as they get accustomed to BWCs. The actions and presence of BWCs-equipped officers have implications beyond their own activities and eventually affect all the officers in the agency. Previous studies are likely to miss the agency-wide effects that affect both the control and treatment groups and to have biased estimates. Also, directly assessing BWC effects on crime rates was not possible for previous single-agency studies. Measuring the effects on crime is important as it allows us to understand whether there was a trade-off from reduction in use of force.

3 Data

This study uses the differential adoption timing of law enforcement agencies to quantify the effects of BWCs. I gather agencies' adoption decisions from the Law Enforcement Management and Administrative Statistics (LEMAS) survey which has been administered by the Bureau of Justice Statistics (BJS) every three years since 1987. Garicano and Heaton (2010) uses the LEMAS survey to construct a panel dataset of police departments and examines the effects of information technology on law enforcement productivity. I use the Body-Worn Camera Supplement (LEMAS-BWCS) which was administered for the first time in 2016. As it was released recently in 2019, this data has not been used in academic research. Drawn from a nationally representative sample of general-purpose law enforcement agencies in the US, this data includes about a quarter of the total existing agencies in 2016. The LEMAS-BWCS contains responses by heads of agencies on topics that range from the current status of BWC use, reasons for adoption, to obstacles they faced. Most importantly for this research, it contains their answers on when (year and month) they adopted BWCs.

Data on incidents and crime control activities come from the Uniform Crime Reporting (UCR) database maintained by the Department of Justice. It contains agency-wide information on monthly index crimes and clearance[1].

In my analysis on use of force in New Jersey, I rely on the Force Report collected and maintained by NJ Advance Media. NJ Advance Media, the leading media company in New Jersey, collected and digitized all use of force forms covering 2012 through 2016 from all local law enforcement agencies in New Jersey. The database contains incident-level information on levels of force, date, agency and officer names, subject demographics, and officer and subject injury. As the administrative record of use of force incidents, this data is likely to reliably capture the true patterns of use of force. When the data was released at the end of 2018, the New Jersey Attorney General praised the work as "nothing short of incredible" and "something they should be doing."[2]

To the monthly data of UCR, I merge in BWC surveys so that my final sample only includes local police departments that were recruited and partcipated in the LEMAS-BWCS survey. In total, they are 2,781 departments, with about half having adopted BWCs. For my study on use of force in New Jersey, I also merge in monthly aggregates of use of force in New Jersey departments. My final sample of study runs from the beginning of 2012, when the Force Report starts, to June of 2016, which is as far as LEMAS-BWCS covers. Table 6 presents summary statistics on this main dataset. All the variables are adjusted for 10,000 population of the city. Focusing on New Jersey, on average about 0.78 use of force per 10,000 capita occurs in a month. The majority of that is physical force – the lowest type available for officers. About 9% of the total force used is the

[1]Previous literature using this data noted the need to clean this data as agencies have wide discretion on their methods of reporting. I follow the regression-based approach used in the literature to clean the data. I first fit quartics and month fixed effects for each agency, then for each population group of agencies defined in the UCR data, I drop months that have too extreme (97.5%) differences between the predicted and actual measures.

[2]https://www.nj.com/news/2018/11/njcom-probe-of-police-force-nothing-short-of-incredible-njs-top-cop-says-now-hes-promising-major-reform.html

intermediary type – mechanical force. Deadly force is very rare. Subject and officer injuries occur more than mechanical and deadly force on average, indicating that some physical force incidents lead to injuries. Officer assault, crime, and clearance rates come from the UCR data, so are available for both in New Jersey and US sample. Crime and clearance are quite comparable in New Jersey and the US, while officer assault occurs more frequently in New Jersey, possibly reflecting the fact that New Jersey is more urban than an average state.

4 Empirical Strategy

My main empirical approach exploits staggered adoption of BWCs by law enforcement agencies using both time and cross-sectional variations. The availability of data on the exact adoption dates of BWCs and high-frequency monthly data on police activities further helps me finely tease out the effects of treatment effects of BWCs from alternative explanations.

The main goal of this paper is to convincingly attribute changes in policing variables of interest to the adoption of BWCs by different agencies. BWC adoption is not random and is related to variables potentially linked to the outcomes of interest. Table 3 compares the characteristics of departments by adoption status, before and after the study period. The data for the characteristics come from the 2010 and 2017 American Community Surveys and their estimates on places and county subdivisions. The adopting agencies were located in cities with higher population, higher minority population, and lower income. Mirroring the population size difference, the adopting agencies were also much larger. These initial differences persisted after the study period. These patterns are also relfected in New Jersey, the state where I study the impact of BWCs on use of force, although the differences are more muted (Table 4). Table 5 examines the differences in a regression framework and tests whether the timing of adoption can be predicted by pre-determined covariates of the agencies. Focusing first on the US sample, a number of key demographics and economic characteristiscs predict either the decision to adopt or the timing. In the New Jersey sample, which has a smaller sample size, none of the observable characteristics predict the extensive or intensive margin. Nonetheless, the coefficients are of similar magnitudes, and many larger, than those in the US sample. To account for these potential threats to identification, I include county-by-time fixed effects and agency-specific linear trends in addition to agency fixed effects in the regression framework.

In the event study and difference-in-differences specifications, I estimate the following regressions for agency j and month t:

$$Outcome_{jt} = \sum_{\tau=-24}^{24} \beta_\tau BC_{jt}^{\tau} D_j + \phi_j + \delta_{c(j)t} + \gamma_j(t) + \epsilon_{jt},$$

$$Outcome_{jt} = \beta Post_{jt} + \phi_j + \delta_{c(j)t} + \gamma_j(t) + \epsilon_{jt},$$

where BC_{jt}^{τ} is a dummy variable that indicates whether the agency adopts BWCs in τ months. ϕ_j and $\delta_{c(j)t}$ denote agency and county-by-time fixed effects. The binay treatment indicator D_j is 1 if the agency ever adopted BWCs. $Post_{jt}$ is a binary indicator equal to 1 only after adoption and 0 otherwise. The main coefficients of interest β_τ and β estimate the divergence in outcome variables net of changes in untreated departments after adjusting for covariates and secular trends.

The main outcome variables are overall use of force, injury, as well as other crime outcomes such as clearance and index crime rate. In addition to the overall use of force, I further examine use of force separately by three different types of force based on New Jersey Attorney General's Use of Force Policy: physical, mechanical, and deadly force. Physical force includes various methods of hand-to-hand confrontation such as wrestling a resisting subject to the ground, wrist locks or arm locks, and striking with hands or feet. Mechanical force involves the use of some device or substance, other than a firearm, such as a baton, canine physical contact, chemical spray, and a more enhanced method of conducted energy devices. Deadly force denotes the use of firearms intended to cause death or serious bodily harm.

The main assumption for my empirical approach is that after accounting for the covariates and the agency specific trends, the agencies that adopt BWCs are comparable to those that do not. As a test of the identifying assumption, I will be careful to observe if there are any pre-trends before the adoption, or $\tau < 0$.

5 Results

5.1 The effects on use of force in New Jersey

Figure 2 plots the evolution on all use of force around the adotion of BWCs using the coefficients from the event study. Before the adoption, the estimates are flat providing no evidence of differential trend in use of force in treated departments. The econometric model performs well in adjusting for potential differences among the departments. Following the adoption of BWCs, however, use of force falls precipitously.

I next examine the effects on use of force by different types. The lowest type – physical force – exhibits a similar behavior in Figure 3 as the overall use of force. This is expected as physical force accounts for the majority of the total use of force. The intermediate type – mechanical force – also follows a qualitatively similar pattern of being flat before the adoption, then sharply falling right after (Figure 4). Turning to deadly force in Figure 5, the estimates before adoption are more noisy, reflecting the rarity of its occurence. Although I take caution in interpreting the results for deadly force, the trend line for deadly force seems to quickly break from the pre-tend after adoption to become a more subdued one leading into a decline.

A decline in use of force, especially in more serious types, implies that there would be a decrease in subject injury incidents. Figure 6 upholds this prediction. After a relatively flat trend before the adoption, subject injury sharply falls immediately, sustaining the decline for the rest of the sample window.

In Figure 7, I show estimates in the figures above without department-specific time trends. Most of the figures show upward sloping trends both before and after adoption, highlighting the need to control for the trends. A systematic relationship between the trends and adoption of BWCs would bias the coefficients. The upward trends are consistent with an interpretation that factors associated with a rising use of force increased the pressure for introducing BWCs. It is worth noting that even in the unadjusted figures, we see a break in trends after adoption, most prominently in for mechanical force and subject injury.

I summarize the findings so far using difference-in-differences estimates in Table 7. Introduction of BWCs leads to a statistically significant drop of 0.47 per 10,000 capita in overall use of force. Compared to the mean just before adoption, this represents a large 43% decline. This drop is echoed by substantial drops in all of physical, mechanical, deadly forces as well as subject injury. These estimates confirm the patterns presented in the event study figures above.

5.2 Heterogeneity in use of force by agency

How do BWCs' effects on use of force vary by the types of departments? The effects need not be uniform. Departments that face higher pressure for change, either because of public demand or greater margins of improvement, would see a greater reduction in use of force after adoption. Departments may not see much change, or even increase in use of force, if officers previously restrained themselves from using force and BWCs embolden them to use force by acting as an insurance against potential lawsuits.

Table 8 presents the heterogeneity by department types. I define high minority and high-white departments as those with 50% or more in minority (Blacks and Hispanics) and White populations, respectively. Other departments are denoted as multi-racial. For both total use of force and force used on minority, departments in cities with high minority do not have much effect from BWCs. The strongest effects come from departments with high White or multi-racial populations, indicating higher pressure for change in these settings. Use of force also experience greater change in urban departments, defined as those with 90% or more urban populations. However, the dampening effects of BWCs on use of force seem to be present across all department sizes. In the last column, we see that small (less than 15 officers), mid-sized (between 15 and 100), and large (more than 100) all experience reductions in use of force.

5.3 Did agencies sacrifice policing capabilities?

The evidence presented thus far strongly indicates that BWCs put a downard pressure on use of force. Insofar as BWCs reduced unnecessary or excessive use of force, or encouraged subjects to comply more, this suggests that BWCs can be successfully used in agencies' efforts to avoid controversial use of force, reduce liability, and restore public faith. However, did this improvement come at the expense of policing capabilities? Reduction in use of force may not be desirable if officers are forced to use less force by sacrificing their own safety. Figure 8 shows that this was not the case. After being flat before adoption, officer injury incidents do not show either a rise or fall. This pattern can also be seen using the data on officer assaults from the UCR data in Figure 9.

It is also important to check if the reduction in use of force came at the expense of crime control capabilities. Officers may have reduced policing efforts in order not to come under the burden of being monitored. Figure 10 shows that BWCs did not bring about a marked difference in crime. If anything we see a modest decline after adoption. Relatedly, I also examine the effects on clearance rates. Figure 11 shows that body cameras also did not introduce a noticeable change in clearance rates. Table 9 summarizes the null results using the DID specification.

For the above three measures, I am able to supplement my analysis using the national sample of 2,781 agencies. Using the larger sample, I also do not find evidence that body cameras brought about clear trend breaks in clearance rate and index crime as seen in Figures 12, 13 and 14. Taken together, the results suggest that BWCs did not force departments to sacrifice policing capabilities.

6 Conclusion

Since the early 2010s, BWCs have taken a prominent role in the efforts to improve police accountability and transparency. Currently there are policy debates as to whether to continue to expand BWC programs. However, previous research on BWCs have not been able to give conclusive answers because of limitations in research settings.

This cross-agency study provides the first opportunity to examine the effects of BWCs in a setting that can credibly deal with within-agency spillovers and agency-wide changes from BWCs. Previous studies likely underestimated the BWC effects by omitting those key factors. An important implication of this study is that investments in BWCs can deliver large returns in terms of improved police-civilian interactions. BWCs lead to large reductions in use of force and subject injury while preserving policing capabilities.

Although improvement in use of force by itself can serve as a valid reason to adopt BWCs, I also perform a cost-benefit analysis of implementing BWCs from the perspective of law enforcement agencies. I compare the costs of BWC programs with the potential gains from reduced lawsuits and settlements related to excessive policing. Data on lawsuits and, especially, settlements for police departments are not readily available, but I can use large US cities as a guidance. From 2010 to 2014, the 10 largest US cities have each spent $204 million annually[3]. I assume that lawsuits and settlements would decrease by the same percentage (43%) decrease of use of force calculated in this study. This may be an underestimate if BWCs decrease proportionally more excessive use of force than justified use of force. I also use the more conservative percentage of use of force rather than subject injury (81%). A 43% reduction of the yearly payouts is equivalent to a savings of $88 million. On the other side of the equation, based on a cost calculation implementing the BWC program in the Las Vegas Metropolitan Police Department, a reasonable estimate can be $1,100 a camera per year (Braga, Coldren Jr., Sousa, Rodriguez, and Alper, 2017). This calculation considers amortized equipments, the IT infrastructure, training, as well as labor costs involved in responding to freedom-of-information requests. For an average force of 8,415 for the largest departments, a full deployment would cost $8 million at most. Given that in the study sample the reduction happened with a much lower deployment rate 56%, BWCs can serve as a powerful cost saving tool for police departments.

Left out of this calculation are aspects no less important, but harder to quantify: a reduction in civilian injuries and its implications for the well-being, improvement in trust in police, and better prosecutory evidence. The results from this study show that BWCs can help police departments meet the societal demands that arose from the recent high-profile excessive policing cases and the ensuing protests. The conservative cost-benefit analsis further shows that adopting BWCs also makes sense from the cities' financial perspectives.

[3]https://www.wsj.com/articles/cost-of-police-misconduct-cases-soars-in-big-u-s-cities-1437013834

Figure 1: Adoption of body cameras

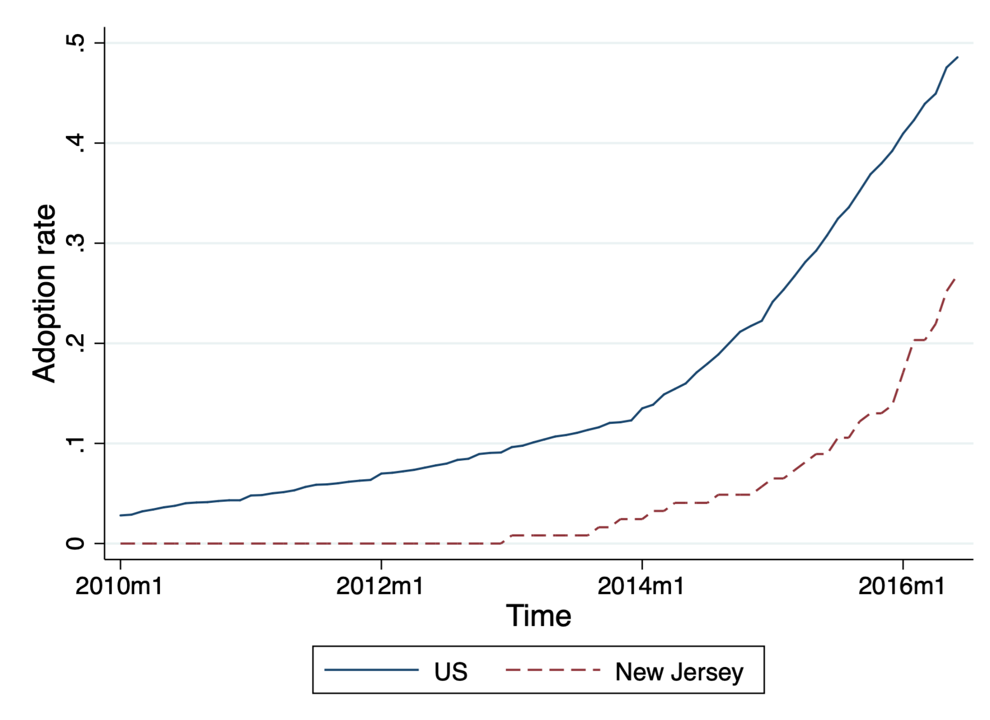


Table 1: Survey: obstacles to adopting BWCs

Reasons for not acquiring	%
Costs	86.3%
Public request burden	69.7%
Privacy	40.7%
Storage Procedures	30.8%
Liability	24.4%
Technical obstacles	17.3%
No benefits	12.4%
Officer support	4.5%
Leadership support	3.1%
Public support	2.8%
Total	1,895

Notes: This table shows survey answers from all local agencies that did not adopt BWCs as to why they did not acquire BWCs. The source is the LEMAS-BWCS.

Table 2: Survey: obstacles in using BWCs

Greatest obstacles encountered	%
Costs greater than anticipated	13.5%
Storage Procedures	11.1%
Privacy	7.7%
Technical obstacles	6.6%
Public request burden	5.5%
Security	2.9%
Liability	2.3%
Officer support	1.2%
No benefits	0.4%
Public support	0%
Others	2.7%
Missing	46.1%
Total	1,788

Notes: This table shows survey answers from all local agencies that did adopted BWCs as to what was the single greatest obstacle they faced. The source is the LEMAS-BWCS.

Table 3: US: Comparison of Adopters and Non-adopters

	Mean (SD)			
	Non adopters (2010)	Adopters (2010)	Non adopters (2017)	Adopters (2017)
Population	27,641	67,642	28,384	67,925
	(55,147)	(313,938)	(58,761)	(319,207)
Male	48.6	48.7	38.4	45.2
	(2.5)	(3)	(19.9)	(13.1)
White	82.4	76.6	82.8	77.4
	(18.2)	(19.9)	(17.8)	(20.2)
Black	7.73	11.9	7.86	12.4
	(14.4)	(17.5)	(14.3)	(18)
Hispan	9.67	12.9	10.4	13.2
	(15.1)	(17.5)	(15.6)	(17.9)
Median age	39.3	37.5	40.2	38.2
	(5.68)	(5.86)	(6.38)	(6.37)
Labor force	64.6	63.1	63.2	60.9
	(8.08)	(8.69)	(7.94)	(8.76)
Unemployed	7.54	8.26	6.33	6.9
	(3.85)	(3.91)	(3.53)	(3.72)
Median income	36,371	32,627	40,313	35,852
	(11,940)	(10,314)	(13,612)	(11,507)
Poverty	9.44	12.2	10.1	13.1
	(7.45)	(7.86)	(7.74)	(8.31)
Agency size			51.6	161
			(122)	(1,196)
N	1,404	1,377	1,404	1,377

Notes: Table compares non-adopting and adopting agencies in the US sample, before the study window in 2010 and after in 2017. The characteristiscs of the agencies come from the 5 year estimates of ACS. The relevant geographic levels I use in the ACS are county subdivisions and places, and I matchthem with FIPS codes and ORI agency codes (most are matched).

Table 4: NJ: Comparison of Adopters and Non-adopters

	Mean (SD)			
	Non-adopters(2010)	Adopters(2010)	Non-adopters(2017)	Adopters(2017)
Population	29,044	35,156	29,813	36,082
	(39,129)	(46,829)	(40,067)	(49,485)
Male	48.6	48.6	28.2	29.8
	(1.84)	(1.44)	(24.1)	(23.8)
White	75.2	72.2	75.9	73.5
	(21.4)	(25.2)	(21)	(24.5)
Black	9.71	15	9.65	14.2
	(16.5)	(19.2)	(16.4)	(18.2)
Hispan	15.6	14.2	16.4	14.8
	(18.5)	(14.3)	(19.1)	(15.7)
Median age	40.7	39.4	41.8	40.9
	(6.05)	(5.47)	(6.85)	(6.15)
Labor force	66.4	68.1	65.3	65.6
	(6.36)	(4.91)	(6.17)	(4.54)
Unemployed	7.07	8.27	6.42	7.38
	(2.5)	(3.64)	(2.98)	(3.22)
Median income	49,137	44,395	54,042	47,802
	(14,263)	(11,121)	(17,165)	(13,157)
Poverty	5.83	7.32	6.73	8.78
	(5.74)	(6.95)	(7.25)	(7.61)
Agency size			68.6	92.9
			(115)	(158)
N	90	36	90	36

Notes: Table compares non-adopting and adopting agencies in the New Jersey sample, before the study window in 2010 and after in 2017. The characteristiscs of the agencies come from the 5 year estimates of ACS. The relevant geographic levels I use in the ACS are county subdivisions and places, and I matchthem with FIPS codes and ORI agency codes.

Table 5: Predicting adoption of BWCs

	US sample		NJ sample	
	Adopt	Months to adopt	Adopt	Months to adopt
Population (10K)	0.006***	-0.003	0.011	0.226
	(0.002)	(0.095)	(0.034)	(1.672)
Male	-0.001	-0.368	0.003	-2.266
	(0.004)	(0.270)	(0.032)	(1.863)
White	-0.000	0.289***	0.004	0.123
	(0.001)	(0.101)	(0.005)	(0.381)
Black	0.002	0.396***	0.003	0.319
	(0.002)	(0.108)	(0.007)	(0.463)
Hispanic	0.001	0.183***	-0.006	0.177
	(0.001)	(0.062)	(0.005)	(0.300)
Median age	-0.010***	-0.063	0.004	0.886
	(0.002)	(0.171)	(0.012)	(0.854)
Labor force	-0.004**	0.065	0.012	-0.225
	(0.002)	(0.117)	(0.009)	(0.596)
Unemployed	-0.004	-0.264	0.023	-1.583
	(0.003)	(0.234)	(0.023)	(1.688)
Median income (100K)	-0.389***	12.253	-0.589	-11.557
	(0.118)	(10.006)	(0.504)	(40.143)
Poverty	0.001	0.055	0.009	0.598
	(0.002)	(0.160)	(0.016)	(0.961)
Urban	-0.061**	0.832	0.084	-14.545
	(0.031)	(2.318)	(0.276)	(14.120)
Agency size	-0.000**	0.001	-0.000	0.018
	(0.000)	(0.002)	(0.001)	(0.052)
Crime (pre)	0.001	0.203**	-0.006	-0.251
	(0.001)	(0.081)	(0.008)	(0.550)
Force (pre)			0.013	3.821
			(0.083)	(5.490)
Dep. var. mean	.486	28.1	.288	42.2
Observations	2,344	1,121	125	33

Notes: Table reports results from running cross sectional regression on the decision to adopt BWCs in the US and New Jersey samples. The dependent variable for the decision to adopt is a binary variable equal to one if the agency ever adopted BWCs. The dependent variable for the timing is the number of months away from January 2012. The sample sizes differ from the main sample of analysis because of data availability for some variables.

Table 6: Summary statistics of main outcome variables

	New Jersey	US
Force	.78	
	(1.34)	
Physical force	.729	
	(1.28)	
Mechanical force	.0665	
	(.254)	
Deadly force	.00329	
	(.0375)	
Subject injury	.173	
	(.612)	
Officer injury	.081	
	(.345)	
Officer assault	.156	0.047
	(5.14)	(1.27)
Crime	11.1	14.37
	(25.7)	(14.39)
Clearance	.234	.261
	(.285)	(.263)
N	6,642	115,536

Notes: All force incidents, injury, assault and crime variables are adjusted per 10,000 capita. Clearance rates are defined as total index crime arrests divided by total index crimes.

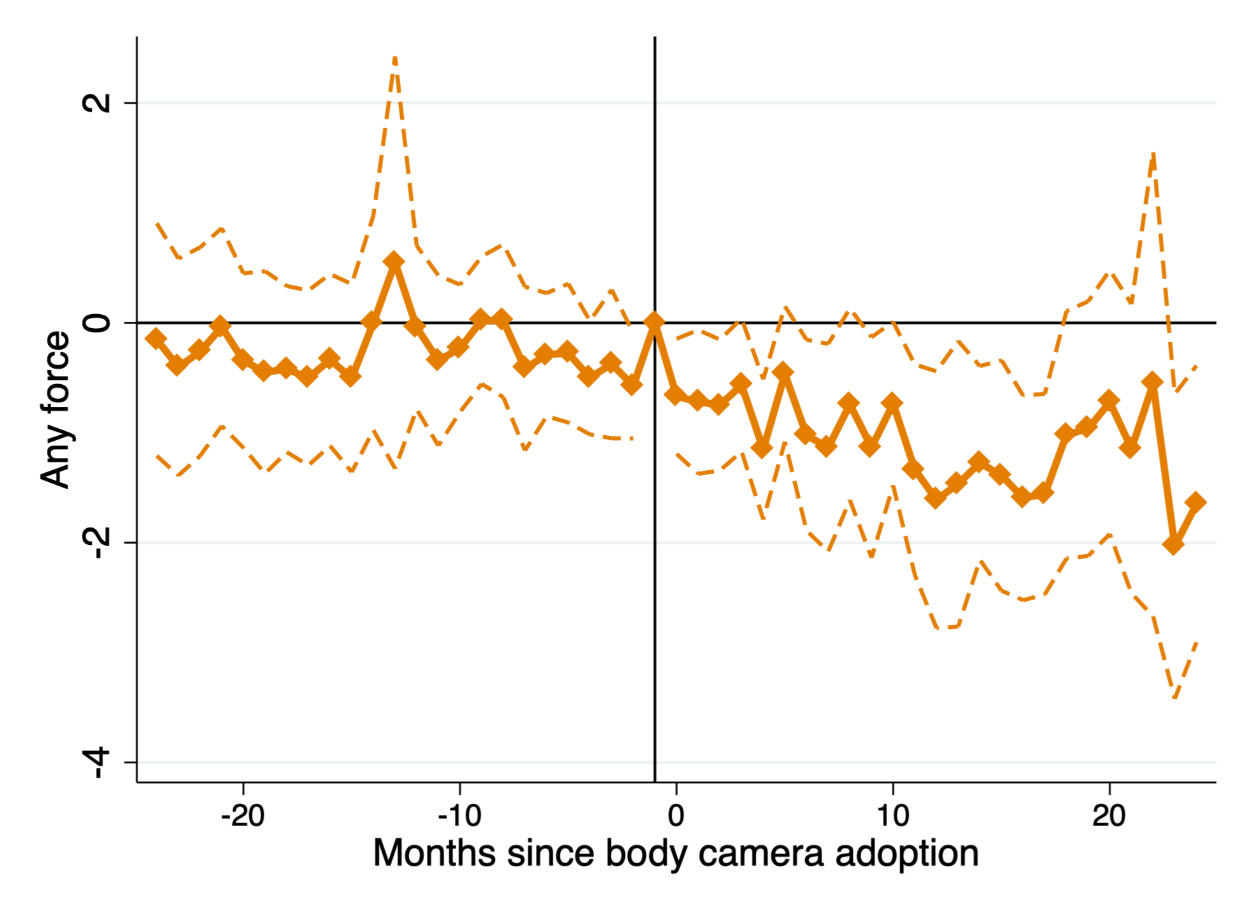


Figure plots event study estimates with 95% confidence bands for total use of force. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 2: Effects of body cameras on all use of force

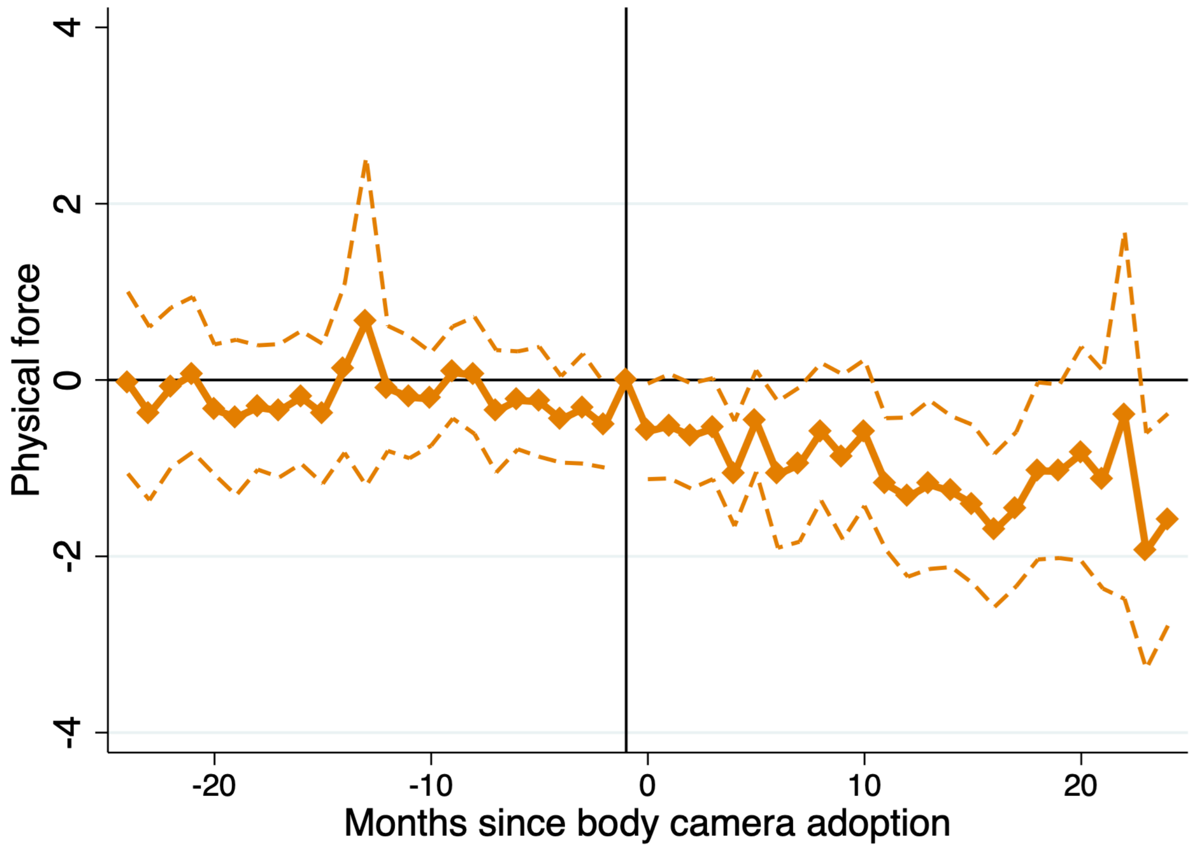


Figure plots event study estimates with 95% confidence bands for the lowest type of force – physical force. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 3: Effects of body cameras on physical force

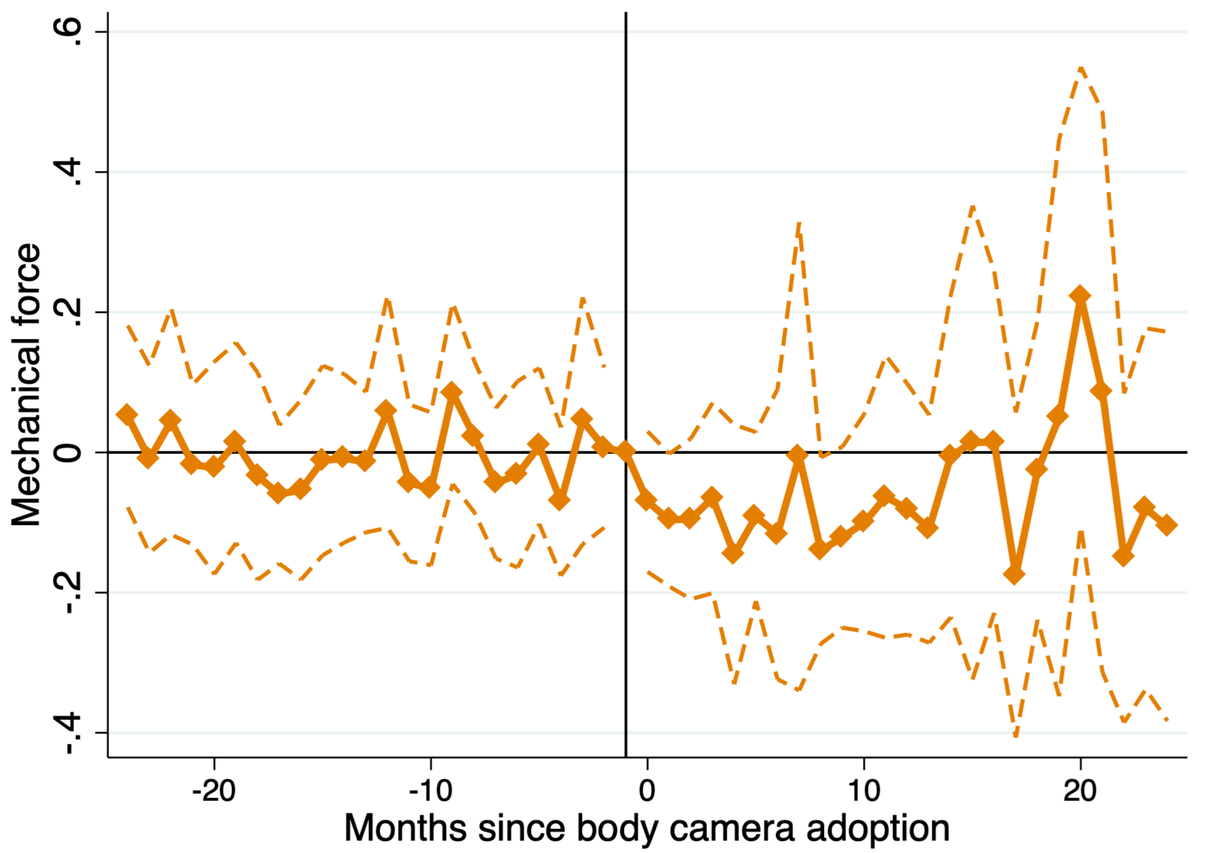


Figure plots event study estimates with 95% confidence bands for the intermediate type of force – mechanical force. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 4: Effects of body cameras on mechanical force

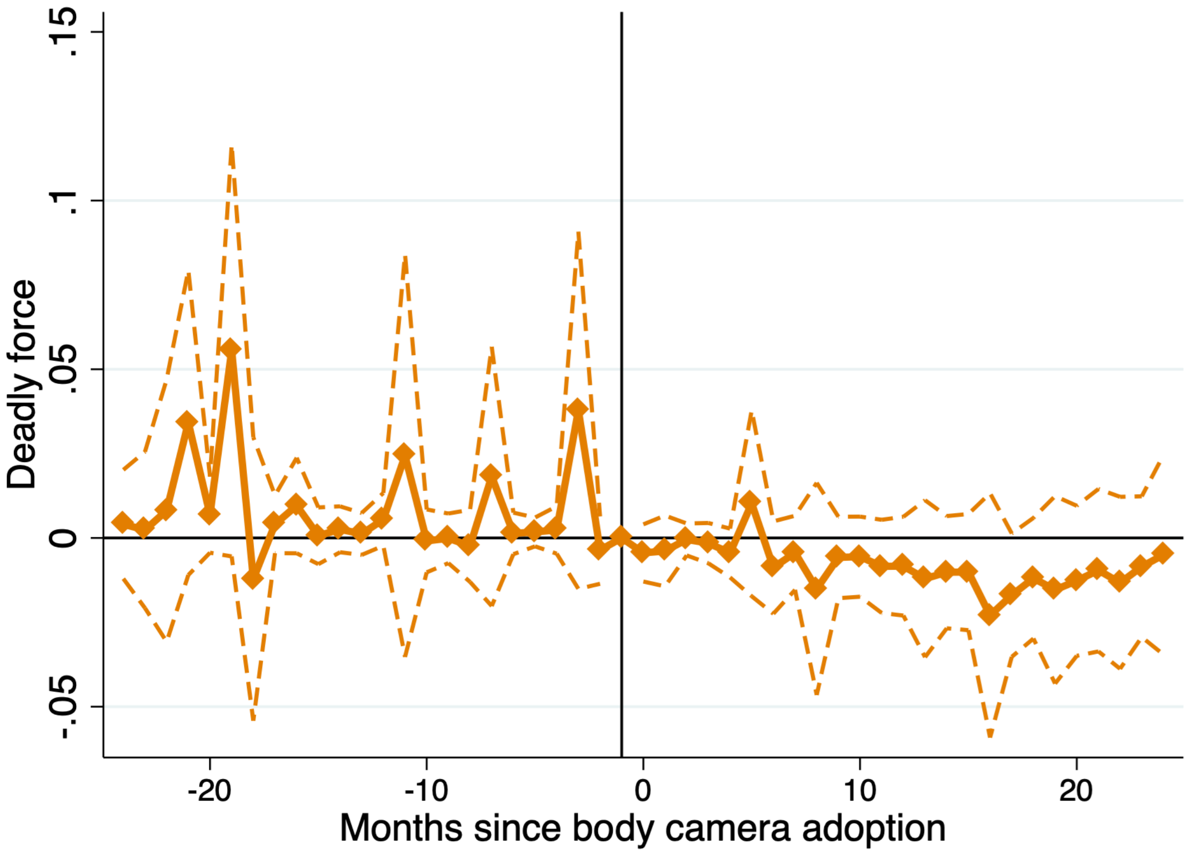


Figure plots event study estimates with 95% confidence bands for the highest type of force – deadly force. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 5: Effects of body cameras on deadly force

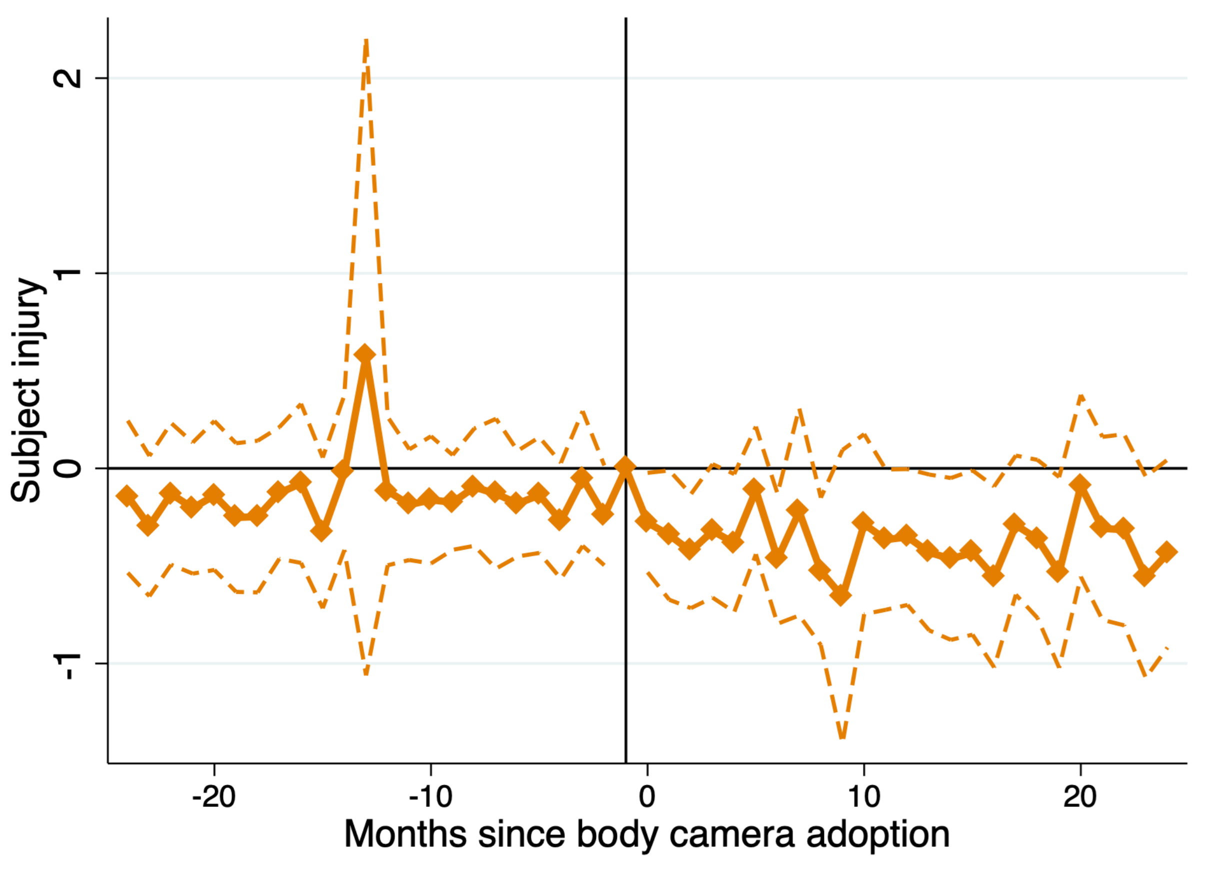


Figure plots event study estimates with 95% confidence bands for incidents including subject injury. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 6: New Jersey: Effects on subject injury

Table 7: DID Effects of body cameras on use of force

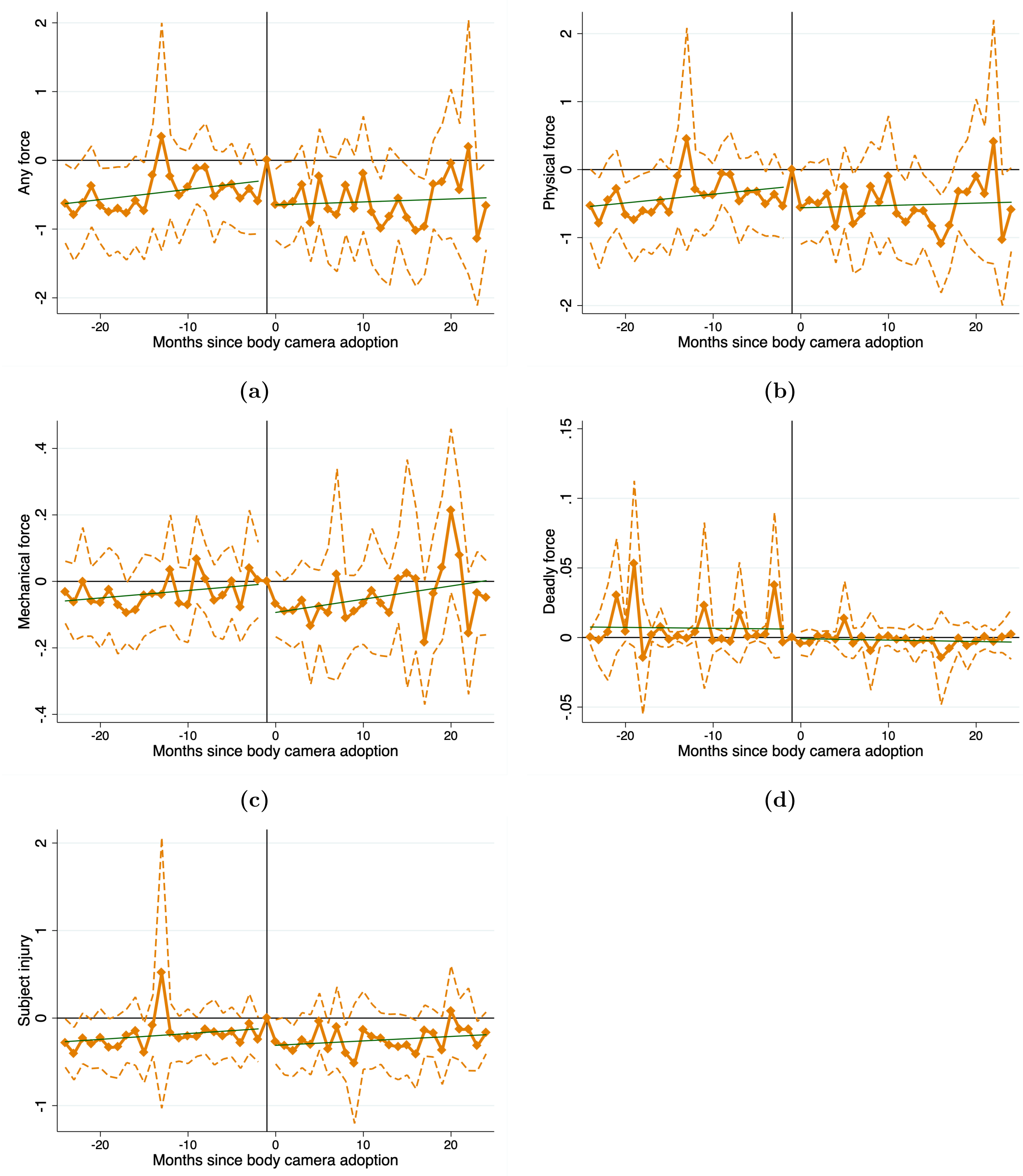
	All	Physical	Mechanical	Deadly	Subject injury
Camera	-0.471***	-0.399**	-0.062**	-0.012**	-0.227**
	(0.163)	(0.158)	(0.031)	(0.006)	(0.087)
Mean at $-3 \leq t \leq -1$	1.09	0.98	0.12	0.012	0.28
Observations	6,642	6,642	6,642	6,642	6,642

Notes: Table 7 shows difference-in-differences estimation of body camera adoption using data between 2012 and June of 2016. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level. For comparison of the magnitudes I include the mean of the dependent variables during the short window right before adoption.

Table 8: Heterogeneous effects by department types

	Force	Force on minority	Force on white	Force	Force
High minority	-0.077	-0.033	-0.015		
	(0.158)	(0.121)	(0.083)		
High white	-0.399**	-0.213**	-0.174		
	(0.161)	(0.106)	(0.109)		
Multi-racial	-1.202*	-1.018*	-0.150		
	(0.673)	(0.555)	(0.168)		
More urban				-0.548***	
				(0.177)	
Less urban				0.059	
				(0.227)	
Small					-0.677*
					(0.354)
Medium					-0.337*
					(0.183)
Large					-0.541
					(0.336)
Observations	6,642	6,642	6,642	6,642	6,642

Notes: Table shows difference-in-differences estimation of body camera adoption for different department types. High-minority and high-White departments are those that have more than 50% of the respective ethnicity in the cities. All the others are defined as multi-racial cities. A department is more urban is 90% of the residents are in urban areas as defined in the ACS 2010. Small departments are those with less than 15 officers, medium-sized departments have more than 15 and less than 100 officers, and the rest are large departments. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.



(a) **(b)** **(c)** **(d)** **(e)**

Figure plots event study estimates with 95% confidence bands for use of force and subject injury. They come from the same event study plots as the full specification except for the agency-specific trends. Standard errors are clustered at the agency level.

Figure 7: Event study estimates without department specific time trends

Figure 8: New Jersey: Effects on officer injury

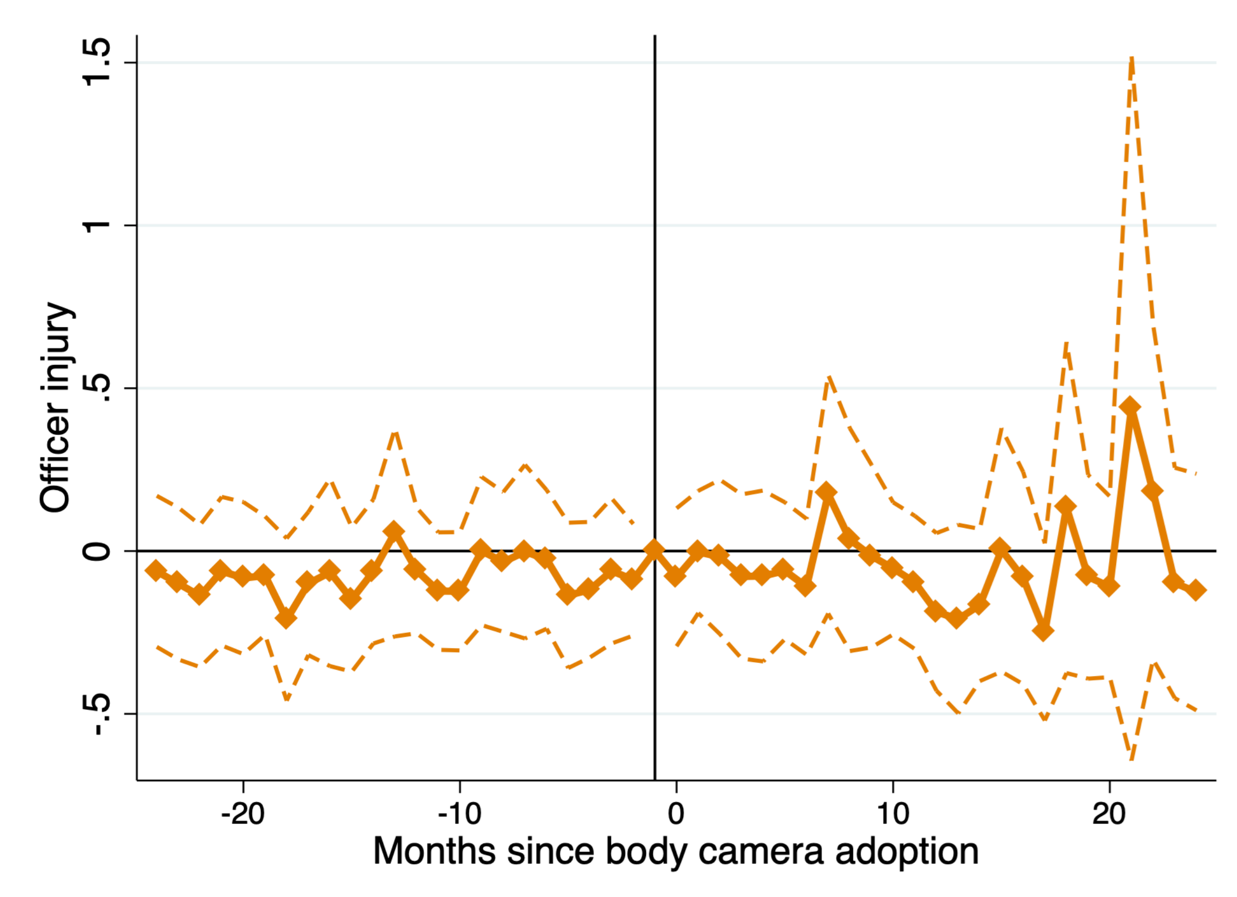


Figure plots event study estimates with 95% confidence bands for incidents including officer injury. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 9: New Jersey: Effects on officer assault

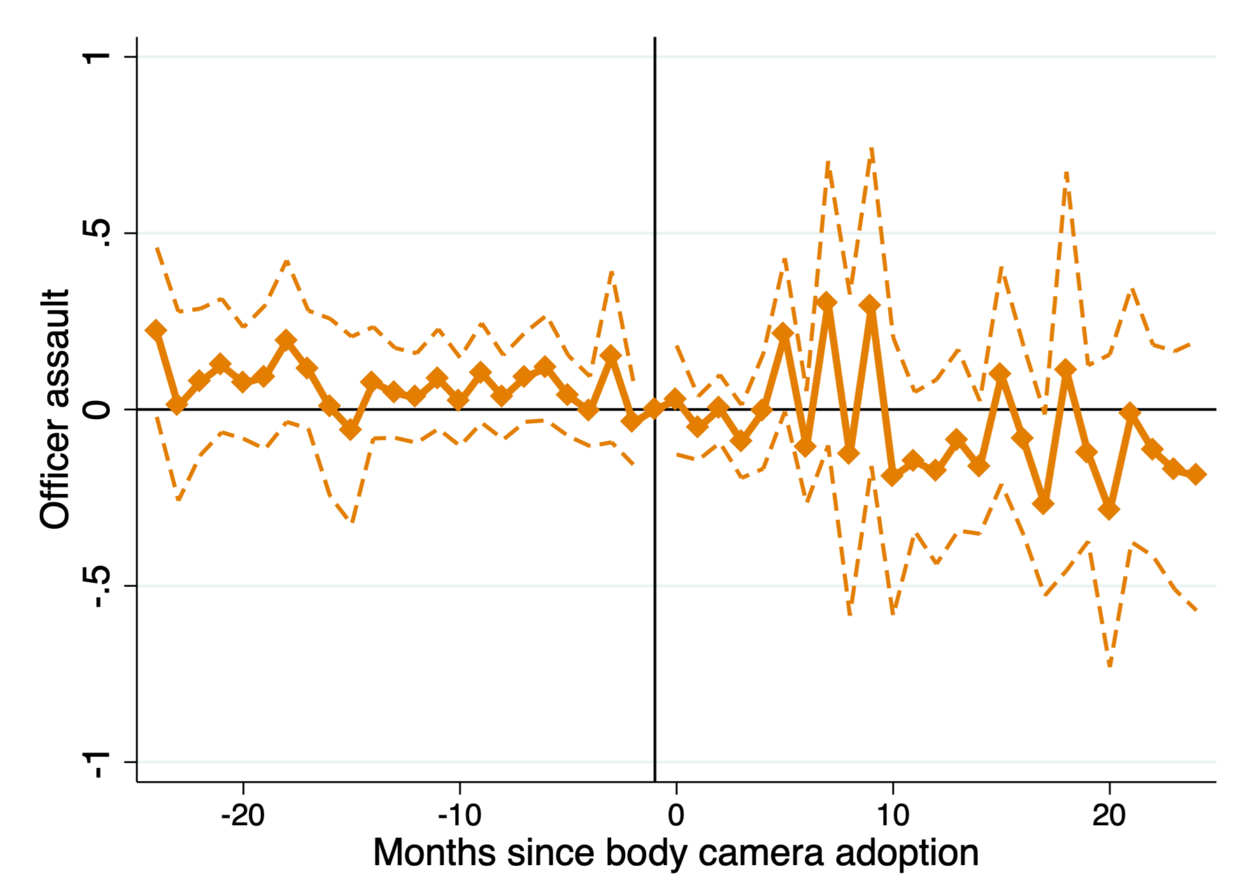


Figure plots event study estimates with 95% confidence bands for incidents including officer assaults. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 10: New Jersey: Effects on index crime

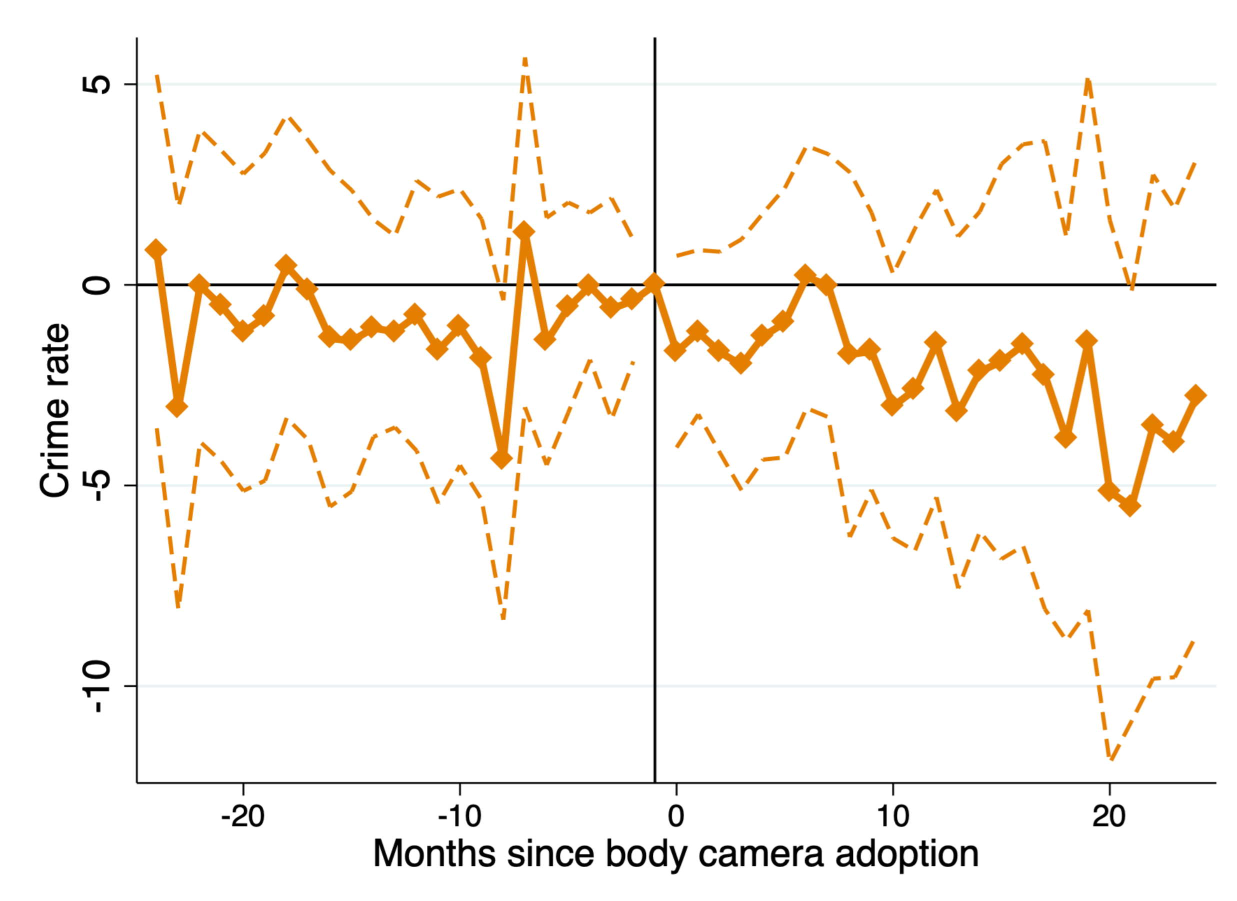


Figure plots event study estimates with 95% confidence bands for index crime. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 11: New Jersey: Effects on clearance rate

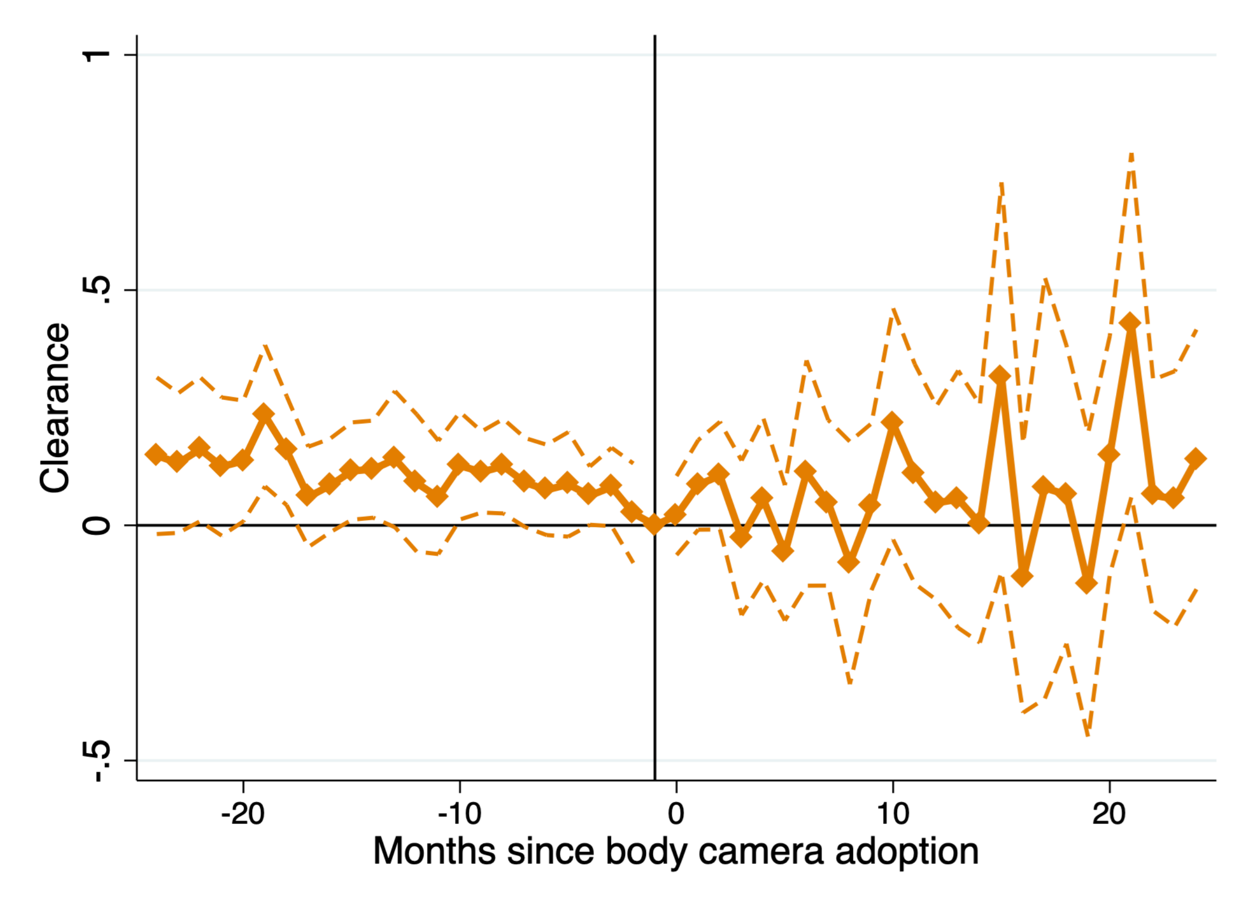


Figure plots event study estimates with 95% confidence bands for index crime clearance rates. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Table 9: DID Effects of body cameras on other police performance measures

	Crime	Clearance	Officer injury	Officer assault
Camera	0.024	-0.030	0.027	0.016
	(0.506)	(0.032)	(0.040)	(0.038)
Dep. var. mean	11.7	.231	.0766	.163
Observations	6,236	6,236	6,534	6,236

Notes: Table shows difference-in-differences estimation of the effects on police performance measures from body camera adoption using data between 2012 and June of 2016. All regressions include agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 12: US: Effects on officer assault

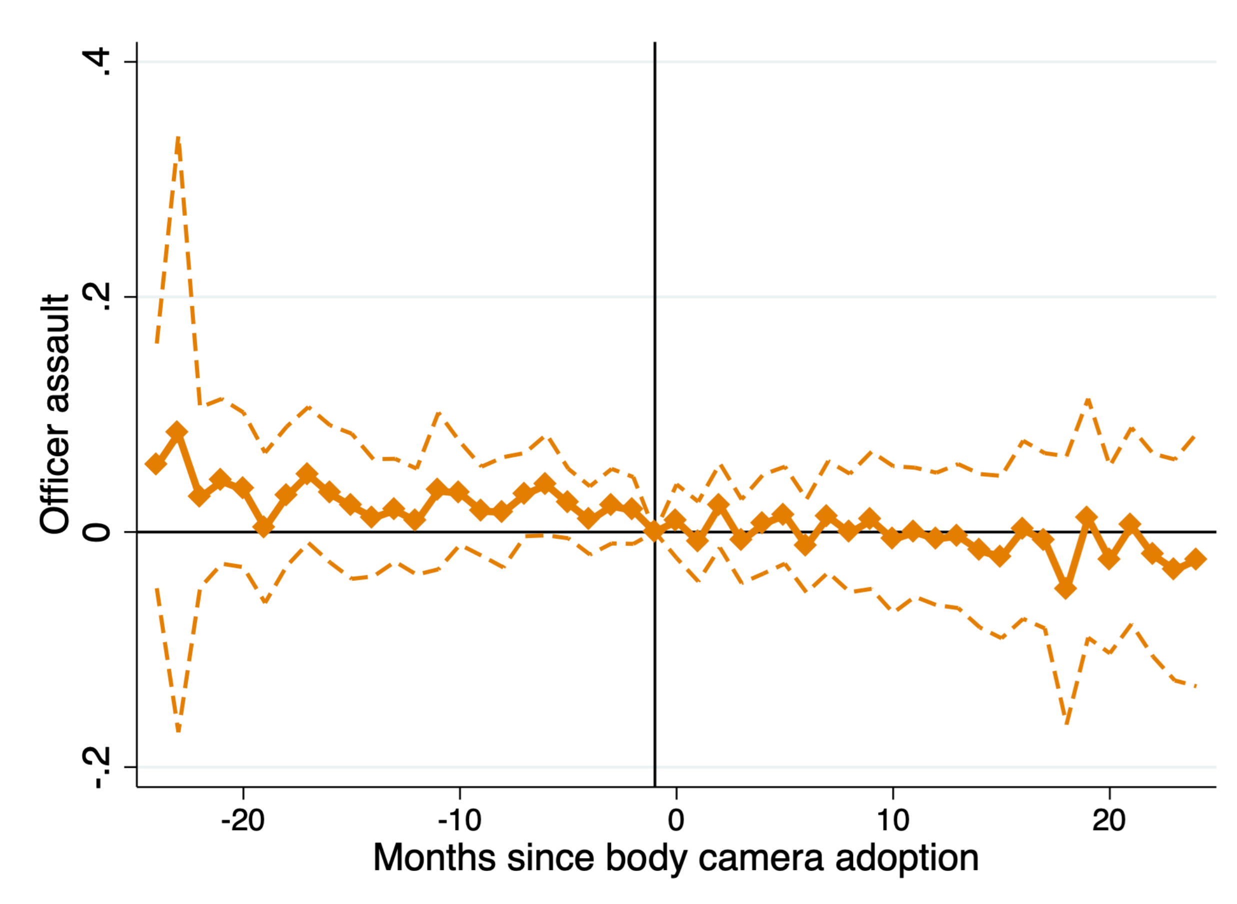


Figure plots event study estimates with 95% confidence bands for officer assaults in the US. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 13: US: Effects on index crime

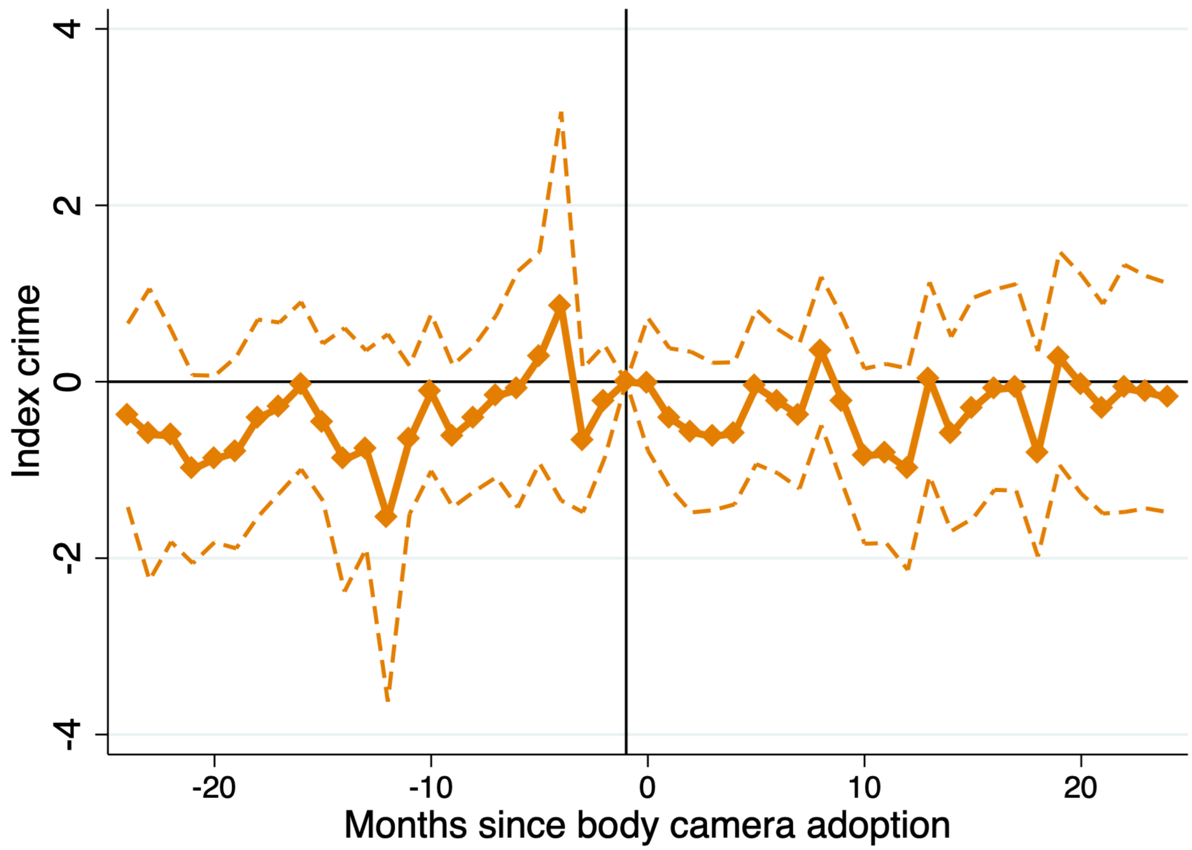


Figure plots event study estimates with 95% confidence bands for crime rates in the US. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

Figure 14: US: Effects on clearance rate

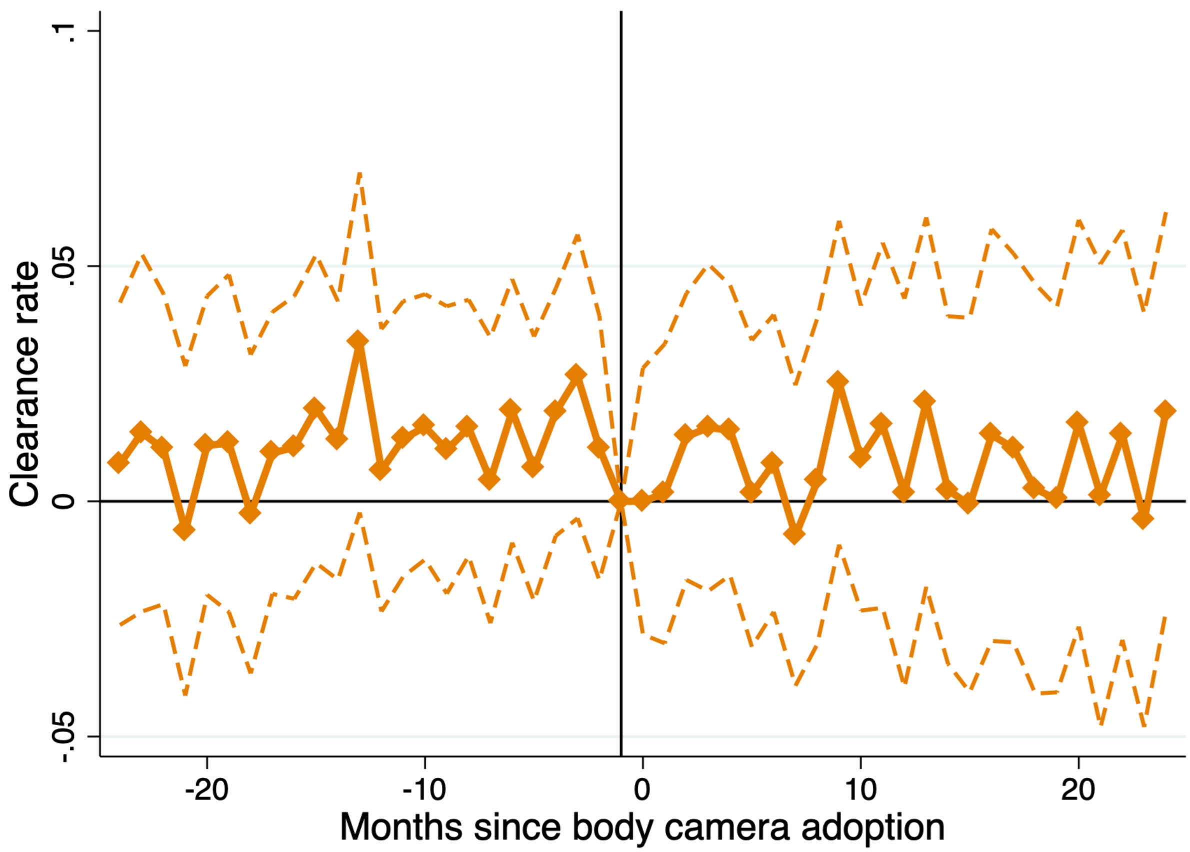


Figure plots event study estimates with 95% confidence bands for clearance rates in the US. The regression includes agency FE, county-by-time FE, and agency-specific trends. Standard errors are clustered at the agency level.

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