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Abstract

A moving-violation traffic stop is *pretextual* when it is motivated by suspicion of an unrelated crime. Despite concerns that they infringe on civil liberties and enable discrimination against minority motorists; evidence on the use, frequency, and nature of pretextual stops is mostly anecdotal. Using nearly a decade's worth of traffic citation data from Louisville, KY, I find evidence suggesting that pretextual stops predicated on a particular moving violation—failure to signal—were relatively frequent. Compared to stops involving other similarly common moving violations, where arrest rates range from 0.01 to 0.09, stops involving failure-to-signal yield an arrest rate of 0.42. More importantly, pretext to stop a vehicle requires *only one* traffic violation. In stops involving failure-to-signal, the arrest rate is 0.52 when no other traffic violations are cited, and the presence of other traffic violations yields a 55% relative decrease in the probability of arrest. Relative to conventional traffic stops, black and Hispanic motorists account for a disproportionate share of likely pretextual stops. Yet, within likely pretextual stops, they are arrested at a significantly lower rate than other motorists. Following departmental adoption of body-worn cameras (body cams) I find that the arrest rate in likely pretextual stops increases 33-34%, and the racial disparity in the arrest rate becomes much smaller and statistically insignificant.

Preliminary draft: comments welcome.

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1 Introduction

In early February 2019, body-cam video from a Louisville Metro Police Department (LMPD) traffic stop was posted to YouTube. By early April, the video had been viewed more than one-million times and sparked discussion in the city over perceptions of racial bias in policing. The motorist in the video—an 18-year-old black male—is stopped for making a wide turn. He is directed to exit the car and is frisked. When asked, he declines consent to a vehicle search. A police dog then inspects the outside of the vehicle which the officers contend alerts them to the presence of contraband. The motorist is placed in handcuffs, the vehicle is searched, and no contraband is ultimately found. On April 11, the editorial board of the Louisville Courier-Journal offered the incident as an example of a *pretextual* traffic stop: a moving-violation traffic stop motivated by suspicion of a crime unrelated to the violation.[1] That is, the traffic violation is used as pretext to conduct what would otherwise be considered an investigatory traffic stop.

Absent a traffic violation, police are permitted to conduct investigatory traffic stops under reasonable suspicion of a crime. For traffic stops, the standard of reasonable suspicion—stemming from *Terry v. Ohio*, 392 U.S. 1 (1968)—requires specific and articulable facts to suggest that criminal activity is afoot. In *Whren v. United States*, 517 U.S. 806 (1996), the US Supreme Court upheld pretextual traffic stops as constitutional in a unanimous decision. In the opinion, Justice Scalia wrote that the "temporary detention of a motorist upon probable cause to believe that he has violated the traffic laws does not violate the Fourth Amendment's prohibition against unreasonable seizures, even if a reasonable officer would not have stopped the motorist absent some additional law enforcement objective."

Following the *Whren* decision, pretextual stops elicited wide discussion among legal scholars, with potential racial bias in their use being a common concern.[2] Yet to date, evidence

[1] The video is found here: https://www.youtube.com/watch?v=9CCQv-i6UBI; the article, here: https://www.courier-journal.com/story/opinion/2019/04/11/louisville-police-traffic-stops-harassing-west-end-editorial/3429651002/.

[2] Among others, see: Davis (1996), Harris (1996), Hecker (1996), O'Day (1997), or Sklansky (1997).

regarding the frequency and nature of pretextual stops is mostly anecdotal. This dearth of empirical evidence is understandable. The reasoning behind any particular stop is known only to the officer making it; and police departments have no incentive to share whether or how they use pretextual stops. Using citation data from traffic stops in Louisville, KY, that span January 2010 to August 2019; I show that despite the difficulty of identifying pretextual stops on a case-by-case basis, their use in the aggregate can potentially be detected by understanding the distinct motive underlying them.

Conventional and pretextual traffic stops stem from distinctly different motives. The former are motivated by enforcement of traffic laws; the latter, by investigating suspicion of an unrelated crime. This defining motive will generate two distinct features of pretextual stops. First, given sufficient variation in the rates at which different moving violations are committed, pretextual stops will concentrate around a particular moving violation or relatively small group. In conventional traffic stops, citation rates of different moving violations partly reflect the rates at which: motorists commit different violations, different committed violations are detected, and different detected violations are cited.[3] In pretextual stops however, because suspected motorists will be stopped as soon as they commit any traffic violation, concentration will occur around the violation(s) committed most often. Second, in conventional stops, arrest occurs following a successful search, but searches only occur if the officer develops suspicion during the post-stop interaction with the driver. In pretextual stops, suspicion exists before a traffic violation is committed. If those suspicions are even slightly well-founded, pretextual stops should yield a higher arrest rate than conventional traffic stops.

Given these two features, if pretextual stops are sufficiently frequent, a moving violation on which they are often predicated should carry a higher arrest rate than other moving violations. Yet, under a null hypothesis of no pretextual stops, an unusually high arrest

[3]Stops are costly and violations vary in the danger they pose and fines they carry. With an objective of promoting public safety, generating revenue (see *e.g.*, Garrett and Wagner, 2009), or some combination; optimal policing likely involves stopping and citing different violations at different rates.

rate given a particular moving violation might stem from correlation between propensity to commit it, and criminality. Exploiting the unique motive underlying pretextual stops, I condition arrest rate on the presence of other traffic violations as a test of this hypothesis. If a high arrest rate given a particular violation is due to correlation between criminality and propensity to commit the violation; then the arrest rate should remain high when additional traffic violations are cited. Alternatively, a significant decrease in arrest rate conditional on the presence of other traffic violations would suggest the practice of pretextual stops, which—aimed at investigating suspicion of a crime—will be made as soon as any traffic violation is committed. This is because pretext for a stop requires *only one* traffic violation.[4]

Within the sample of Louisville traffic citations, I find evidence suggesting that a particular moving violation—failure to signal—was frequently used as a basis for pretextual stops. Among the sample's four most commonly cited objective moving violations, stopped drivers were arrested at rates of: 0.009 when cited for speeding, 0.067 when cited for disregarding a traffic light, 0.086 when cited for disregarding a stop sign, and 0.416 when cited for failure to signal.[5] Following stops where failure-to-signal is the only cited traffic violation, arrest occurs at a rate of 0.522. Controlling for several stop-specific factors, the presence of other traffic violations reduces the arrest rate by 0.287, a 54.98 percent relative decrease. This pattern also holds across each of the eight geographically-defined LMPD patrol districts. Per week, likely pretextual stops occurred 11.5 times and yielded 6.7 arrests on average.

Using these likely pretextual stops, I then add to a growing economic literature examining racial disparities in the US criminal-justice system.[6] In testing for racial bias, pretextual stops offer appealing features. First, because they are made prior to a face-to-face interaction

[4]For example, a pretextual stop can be ruled out if a driver is cited for both a moving violation and a faulty tail light, because a faulty tail light alone provides pretext to stop the motorist. A pretextual stop of the driver would have occurred before any moving violation were committed.

[5]Arrest rates are out of 213,693, 19,283, 15,341, and 8,641 traffic stops respectively, and exclude traffic stops that resulted in charges for driving under the influence of drugs or alcohol.

[6]See for instance: Arnold et al. (2018) on bail decisions; Fryer (2019) or Hoekstra and Sloan (2020) on police use of force; West (2018) on police investigations; Grogger and Ridgeway (2006) or Horrace and Rohlin (2016) on traffic stop decisions; Goncalves and Mello (2017) on traffic-violation reporting; and Knowles et al. (2001), Anwar and Fang (2006), Antonovics and Knight (2009), or Ritter (2017) on search decisions following traffic stops.

with the driver, pretextual-stop decisions are based on a relatively limited amount of information. Compared to search decisions following conventional traffic stops, race is typically one of a much smaller set of factors which are observable at the time of pretextual-stop decisions.[7] Second, because they are essentially marginal investigatory stops, the Becker (1957) outcome test, comparing success (arrest) rates among marginal searches of different groups—which should be equal, absent discrimination—will partially overcome the *infra-marginality problem* when applied to pretextual-stop decisions.[8] Beyond these appealing features, I exploit the LMPD's adoption and gradual deployment of body-worn cameras (body cams) from June 2015 to March 2016, to further circumvent the infra-marginality problem.

Among marginal pretextual stops, if the arrest rate is lower for minority motorists, it suggests a lower threshold was exercised against them when initiating these stops, revealing racial bias. Police are permitted to conduct pretextual stops, but they are not supposed to use race as a factor in deciding whether to conduct them. Body cams heighten accountability and increase the cost to officers of using race-dependent thresholds in such a manner. Prior to body-cam adoption, if—despite race-neutral thresholds—racial disparities in pretextual-stop arrest rates arise due to infra-marginality, then these disparities should persist afterward because body cams don't affect the cost of such practices. However, if initial disparities arise from race-dependent pretextual-stop thresholds, these disparities may decrease with body cams in use.

Prior to the adoption of body cams, while representing only 29.20 percent of conventional traffic stops, black and Hispanic motorists account for 49.33 percent of likely pretextual stops. Yet, compared to other motorists, their arrest rate following likely pretextual stops is significantly lower. Controlling for numerous stop-specific factors, the pretextual-stop arrest rate for black and Hispanic motorists prior to body-cam adoption is 17.72 percent lower rel-

[7] As such, pretextual stops may be especially prone to bias stemming from race-specific errors in judging the probability of a crime; what Bohren et al. (2019) term *inaccurate statistical discrimination*.

[8] Typically, the infra-marginality problem arises because researchers only observe average search-success rates and lack sufficient information to identify the marginal searches. If the probability of carrying contraband is distributed differently across races, average success rates by group may differ even if a race-neutral search threshold is applied. See Ayres (2002) for a thorough discussion.

ative to all other motorists, and 19.06 percent lower relative to non-Hispanic whites. When restricting analysis to periods of daylight—when driver race is more easily observed—these disparities are even larger.

Following body-cam deployment, likely pretextual stops were conducted at a slightly lower frequency, but body cams appear to have little effect on this. However, consistent with use of a higher threshold in general, I find that body-cam adoption explains a 33 to 34 percent relative increase in pretextual-stop arrest rate overall: with a 41.2 to 43.9 percent relative increase among black and Hispanic motorists, and a 25.7 to 27.6 percent relative increase among all other motorists. After accounting for the effect of body cams, the disparity in pretextual-stop arrest rate between black-or-Hispanic and other motorists, becomes small and statistically insignificant. These results challenge the notion that pre-adoption disparities in arrest rate resulted from infra-marginality. Rather, they are more consistent with racially-biased use of pretextual stops before body-cam deployment, and more careful and race-neutral use thereafter.

2 Data

Data come from the Uniform Citation dataset provided on the Louisville Open Data portal.[9] The dataset covers all uniform citations issued by the LMPD, and my sample spans January 1, 2010 to August 19, 2019. The data record an issued citation's date, time, and location (typically intersection or street block). Also recorded are the police division and beat in which citations were issued, the age, race, ethnicity, and gender of the cited individual, as well as all charges that were pressed against the individual as part of the stop.

The citations included in the raw data are not exclusively from traffic stops. However, I am able to identify citations stemming from traffic stops using text descriptions of charges, violated statutes, and Uniform Crime Reporting (UCR) codes. I identify 495,933 citations for traffic-related charges stemming from 448,922 traffic stops. The LMPD has

[9]See Louisville Metro Government (2019).

eight geographically-defined patrol divisions that accounted for 469,705 of the traffic-related citations and 425,169 of the traffic stops. Figure 1 provides a map of the coverage areas for these patrol divisions. The division codes reported for the remaining 5,967 stops suggest that they were made across nine different specialty units. Empirical assessments will focus on stops made by the eight major patrol divisions. The LMPD has a "mobile" (not geographically defined) ninth division which focuses on illegal weapons. It should be noted that it was the ninth division that conducted the controversial stop referenced in the opening paragraph. Stops conducted by the ninth division are not reported in the data however.

Among traffic citations, I group similar charges to identify 19 common violations.[10] Of the 469,705 traffic citations issued, 465,080 belong to one of these common traffic violations. The remaining 4,625 citations involve traffic-related charges that are issued relatively infrequently.

Among these 19 common traffic violations, I distinguish between three types: objective driving violations, subjective driving violations, and non-driving violations. Whereas driving violations are based on how a vehicle is operated (*e.g.* speeding or disregarding a stop sign), non-driving violations are charges unrelated to the manner in which the motorist drives (*e.g.*, driving with an expired license plate or with a defective brake light). That is, non-driving violations are break statutes that a motorist and their vehicle must comply with before taking to the road.

I define objective driving violations as those whose commission can be established without the exercise of judgment. For example, failure-to-signal is committed any time a driver changes lanes without first signaling his intent. The signal is either given, or it isn't. Conversely, subjective driving violations are those whose commission depends on the judgment of the officer. For instance, the Kentucky statute against following-too-close reads that the "operator of a motor vehicle shall not follow another vehicle more closely than is reasonable

[10] Some of these 19 traffic violations encompass multiple different charges. For instance, I characterize speeding in general here as one traffic violation, although in Louisville, distinct charges are issued for each mile-per-hour over the speed limit a motorist drove.

and prudent, having regard for the speed of the vehicle and the traffic upon and condition of the highway." This violation can't be established without judging what is reasonable and prudent.

Table 1 reports the frequency with which each of the 19 traffic violations were cited, as well as the percentage of traffic stops in which each violation was cited. Among objective driving violations, notice that speeding, disregarding a traffic light, disregarding a stop sign, and failure to signal are cited far more frequently than any others. In light of this clear cutoff, testing for the presence of pretextual stops will focus on these four most common objective driving violations.

3 LMPD Adoption of Body-worn Cameras

I gather information on the process by which body-worn cameras were adopted and deployed among Louisville police from Schaefer et al. (2017), a report prepared for the LMPD one year after the body-cam deployment. The LMPD began researching the possibility of body cams in 2012 based on a sense that major police departments in the US were trending toward their adoption. The perceived benefits were that they would increase transparency and thereby improve community relations, but also support officers by providing records of civilian contacts. It should be noted that the initial interest in body cams, the eventual decision to adopt them, and the timing of their deployment, do not appear to be particularly related to the practice pretextual stops.

Following the initial exploratory research period, body cams were not adopted due to budgetary constraints. At issue was not the fixed costs of the equipment—which the report approximates to be $800 per camera—but rather the data-storage costs involved. In addition to these costs, community interest in body cams at that time was reportedly "mild". This changed sharply however in the summer of 2014 when national attention was drawn to the officer-involved shooting of Michael Brown in Ferguson, Missouri. According to Schaefer et al. (2017), budgetary provisions for LMPD body cams and video storage were made soon after that incident. It is especially notable that the impetus for body-cam adoption was a highly publicized incident in another city and state. Thus, the timing of body-cam deployment across the LMPD appears plausibly exogenous to concurrent police practices within Louisville.

The LMPD then established policies on the use of body cams. Officers wear body cams on their heads or uniform collars, and are required to record "all law enforcement related activities"; which encompasses all calls for service and any time an enforcement action (such as a traffic stop) is taken. Officers must upload all video to a cloud-based storage system either at the end of a shift or the beginning of their next shift. Except in cases of crimes where the State's evidence requirements mandate longer, all videos are stored for 30 days. Officers' supervisors are required review video of all critical incidents, use-of-force incidents, and civilian complaints.

The deployment of body cams within the LMPD was staggered at the partol-division level. The department deemed it prudent to initially have a single pilot division use the body cams as a means of trouble-shooting potential unanticipated complications. According to Schaefer et al. (2017), the "Fifth Division was chosen for the pilot study because it is a moderately active division and command staff felt that if there was a failure in the camera deployment it would not result in the program being killed across the department." The concern seemed to be that if it were introduced in a highly active patrol division, those officers might view the changes as more of an inconvenience, and word-of-mouth might lead to the program losing officer support department-wide.

After the pilot study, the department began deploying body cams in a staggered manner one division at a time. Anticipating an initial learning process in working with the body cams, the department chose to stagger deployment so as to avoid requests for assistance and unexpected challenges from coinciding department-wide. Following the Fifth Division, the ordering of deployment was partly focused on patrol areas known for lower income and relatively higher crime. Table 2 presents the body-cam deployment dates for each of the LMPD patrol divisions.

Note that body cams were not deployed in the sixth division until March 11, 2016. All other patrol divisions had body cams deployed from June 1 through October 2 of 2015. This was due to connectivity issues. During the deployment phase, it was determined that the sixth division would require the installation of new fiber-optic cables, hence the delay of more than five months. This delay proves empirically advantageous as, from October 2, 2015 to March 10, 2016, the sixth patrol division provides a control group for the other divisions to be compared against.

4 Detecting the Use of Pretextual Stops

My empirical work has two general components. Initially, among traffic stops involving the most common objective driving violations, I use post-stop arrest rates to test for the use of pretextual stops, as well as identify and characterize stops that were likely pretextual. Then, I examine racial disparities in the use of likely pretextual stops, and assess how these disparities were affected by the adoption and deployment of body cams. Here, I discuss the methodology used to test for the use of pretextual stops, present these initial results, and then characterize the frequency of pretextual stops, and heterogeneity in their use across LMPD patrol division. Empirical considerations and results related to racial disparities in pretextual stops are presented in Section 5.

4.1 Methodology

The unique motive underlying pretextual stops suggests that they should concentrate among a particular traffic violation (or small group) and yield a higher post-stop arrest rate than conventional (non-pretextual traffic stops). I exclude traffic stops resulting in any charges for driving under the influence of alcohol or drugs (DUI) because suspicion of this particular crime does relate to common driving violations. Intoxicated drivers presumably are more likely to speed, disregard traffic signals, and fail to signal turns or lane changes. The notion of a pretextual stop is that it is motivated by suspicion of a crime that is unrelated to the driving violation being cited.

Among the common traffic violations that were present in at least 2 percent of all stops, Table A1 of the Appendix reports arrest rates following stops including citations for these violations. The arrest rate of 0.4158 following failure-to-signal seems remarkably high just given the nature of the violation. It seems especially high when compared to the arrests rates following other common objective driving violations which range from 0.0087 to 0.0859. It is even more than double the arrest rate conditional on reckless driving, a violation that more plausibly relates to suspicion of other crimes.

Frequent use as the basis for pretextual stops would explain the high arrest rate conditional on failure-to-signal. However, if criminality strongly correlates with propensity to commit failure-to-signal (but not other objective driving violations), then this unusually high arrest rate could occur under a null hypothesis of no pretextual traffic stops. To test this hypothesis, I estimate the following equation with the sample restricted to traffic stops in which failure-to-signal was cited:

$$Arrest_i = \alpha_1 Multiple_i + \mathbf{X}_i{}' \boldsymbol{\theta} + \epsilon_i. \tag{1}$$

$Arrest_i$ is a binary indicator equal to 1 if traffic stop i resulted in an arrest, and equal to zero otherwise. $Multiple_i$ is a binary indicator equal to 1 if more than one traffic violation was cited in stop i, and equal to zero otherwise. If the high arrest given failure to signal is due to correlation between criminality and propensity to commit the violation, then on average, the arrest rate should be high any time failure-to-signal is cited. The presence of other traffic violations should have no effect on arrest rate, or if anything, increase it. Thus, under the null hypothesis, $\alpha_1 \geq 0$.

Alternatively, if the presence of other traffic violations causes a significant decrease in arrest rate, it suggests that failure-to-signal was frequently used as the basis for pretextual stops. Because they are motivated by investigating suspicion of an unrelated crime, pretextual stops will be made as soon as a violation is committed. That is, pretextual stops won't include multiple traffic violations, because just one violation provides sufficient pretext to stop the vehicle. Among the 8,641 stops in the estimation sample involving failure-to-signal, 2,885 involved more than one traffic violation. Table A2 in the Appendix reports the frequency and percent frequency at which other traffic violations were cited within those stops.

Table A1 reports simple differences in means between stops in which multiple traffic violations were cited, and stops in which only one traffic violation was cited. Column (1) reports this estimate from stops including failure to signal. For comparison, similar estimates from stops including speeding, disregarding a traffic light, and disregarding a stop sign are reported in columns (2), (3), and (4), respectively. Absent any other controls, relative to when failure-to-signal is the only traffic violation cited, the presence of other traffic violations results in a statistically significant 60.8 percent decrease in arrest rate. Conversely, in columns (2) through (4), the presence of other traffic violations increases arrest rate, as would be expected under the null hypothesis. Because these simple estimates may omit factors that influence both the presence of multiple violations and probability of arrest, the vector $\mathbf{X}_i$ contains fixed effects for the LMPD division conducting the stop, as well as the hour of the day, day of the week, month of the year, and year in which the traffic stop occurred.

4.2 Results

Table 4 presents ordinary least squares (OLS) estimates of equation (1) under the full specification.[11] The estimate in column (1) comes from stops including failure to signal, and estimates in columns (2), (3), and (4) are from stops including other common driving violations for the purposes of comparison. In column (1), the coefficient on *Multiple* strongly

[11] Robust standard errors are clustered at the division-by-year level.

rejects the null hypothesis of no pretextual stops. Relative to a predicted arrest rate of 0.5217 when failure-to-signal is the only traffic violation cited, the presence of multiple violations generates a 55.01 percent decrease in the arrest rate. In columns (2), (3), and (4) however, the presence of multiple traffic violations increases the arrest rate (if anything), as would be expected from conventional traffic stops.

Restricting attention to stops involving failure-to-signal, Table A3 in the Appendix reports estimates of equation (1) under an alternative specification where division fixed effects are replace with division-by-beat fixed effects. These estimates are similar in sign, significance, and magnitude to those under the specification from Table 4.

The editorial referenced in the opening paragraph suggested that pretextual stops were not especially common on Louisville's more affluent east end. To assess this claim, I augment equation (1) by interacting the patrol-division indicators with $Multiple_i$. Note from Figure 1 that divisions 1 through 4 lie mostly to the west of divisions 5 through 8. Estimates of this specification are reported in Table 5. The second column reports the regression's predicted arrest rate within a division when $Multiple = 0$. The third column reports the coefficient on the interaction term for the division's indicator and *Multiple*.

While the arrest rate when $Multiple = 0$ varies a bit, the presence of multiple traffic violations has a large negative effect on arrest rate across all eight patrol divisions of the LMPD. This suggests that across all eight police divisions, pretextual stops were practiced, and commonly predicated on failure-to-signal. It also suggests that the practice is not exclusive to Louisville's west-end neighborhoods. However, as section 4.3 will demonstrate, pretextual stops are significantly more frequent on the west end.

4.3 Characteristics of Pretextual Stops

Section 4.2 provides evidence suggesting that pretextual stops in Louisville were commonly predicated on failure-to-signal citations and sufficiently frequent to be detected in the data. Here, I explore several characteristics these stops. Before attempting to characterize pretextual stops, it's important to recognize that stops in which failure-to-signal is the only cited traffic violation are an imperfect identifier. Some of these stops may not have been pretextual, and some pretextual stops may not have been based on failure-to-signal. While we can't determine whether or to what extent other stops in the data may have been pretextual, we can use stops in which failure-to-signal was the only traffic violation cited to conservatively assess the frequency of pretextual stops.

Within my sample, excluding stops that led to DUI, the eight geographically-defined LMPD patrol divisions conducted 5,756 stops in which failure-to-signal was the only cited traffic violation. If we assume all such stops were pretextual, then pretextual stops were conducted at least 1.64 times per day and 11.45 times per week, on average. These stops resulted in 3,003 arrests. Alternatively, if we treat only the stops that resulted in arrest as pretextual—equivalent to assuming that pretextual stops are based on perfect suspicion of an arrestable offense—then pretextual stops occurred at least 0.95 times per day, or at least 6.66 times per week. Finally, the estimates in column (1) of Table 4 suggest an arrest rate of 0.2045 when failure-to-signal is one of multiple cited traffic violations. If we further assume that, in addition to the 2,753 stops that did not lead to arrest, 690 of the stops resulting in arrest were also conventional traffic stops, it implies among these 5,756 stops: 3,443 were conventional traffic stops and 2,313 were pretextual stops. This composition arises if we assume that pretextual stops always lead to arrest, and that conventional traffic stops involving failure-to-signal result in arrest at a rate of about 0.2045.[12] Even under this most conservative set of assumptions, the data suggest that pretextual stops occurred at least 5.13 times per week.

Recall from Table 5 that within each of the LMPD patrol divisions, the presence of multiple traffic-violation citations produced substantial decreases in arrest rate. However, arrest rates following stops in which failure-to-signal was the only cited traffic violation ranged from 0.34 to 0.61 across divisions. Table 6 compares the frequencies with which likely pre-

[12]Hence the 690 arrests, which make for approximately 20.04 percent of 3,443 stops.

textual stops and conventional moving-violation traffic stops are conducted within each of the LMPD patrol divisions, and reveals significant disproportionality in the use of pretextual stops in particular areas of the city.[13] Notably, Division 2 accounts for more likely pretextual stops *and* fewer conventional moving-violation stops, than any other division. Additionally, Division 1 conducted more likely pretextual stops than five of the eight patrol divisions, despite accounting for the second fewest conventional moving-violation traffic stops.

5 Driver Race and the Use of Pretextual Stops

Economic studies of racial disparities in traffic policing have typically focused on stop decisions in general, or post-stop search decisions in general. Attempts to demonstrate a higher search rate among minority motorists relative to observably similar white motorists, are often undermined by concerns over omitted variables. Search decisions are informed by post-stop interactions through which officers observe many factors that researchers cannot. Moreover, if race correlates with propensity to carry contraband, optimal search decisions may produce disparate search rates, what Arrow (1973) termed *statistical discrimination.* With data on officer race, Antonovics and Knight (2009) partially circumvent these concerns. Inconsistent with statistical discrimination, they find that officers in Boston were more likely to search motorists of a different race.

Alternatively, Becker (1957) proposed a simple outcome test. Absent discrimination, searches of marginal motorists—the least suspicious motorists that officers wish to search—should turn up contraband at equal rates among minority and white motorists. If marginal searches of minority motorists find contraband at lower rates, it suggests a lower threshold was used against them, revealing racial bias due either to animus (so-called *taste-based discrimination*) or race-specific errors in judging successful-search probabilities (*inaccurate statistical discrimination*).

[13]Likely pretextual stops are those in which failure-to-signal is the only cited traffic violation. Conventional moving-violation traffic stops are all traffic stops—excluding those that are likely pretextual—which include a citation for at least one driving violation.

In practice, the outcome (or "hit-rate") test is complicated by an infra-marginality problem: researchers only observe average success rates and lack sufficient information to identify the marginal searches. If the probability of carrying contraband is distributed differently across races, average success rates by group may differ even if a race-neutral threshold is applied. Knowles et al. (2001) develop a model suggesting that, in equilibrium, infra-marginality may not arise if motorists strategically respond their race-specific search probability. Alternatively, Anwar and Fang (2006) partially circumvent the issue by noting that absent racial bias, the ranking of search rates and average success rates for a given race of motorists should be the independent of an officer's race. With data from Florida Highway Patrol they fail to reject a hypothesis of no relative racial prejudice.

By nature, pretextual stops are advantageous in overcoming these common challenges. Following conventional traffic stops, there is a face-to-face interaction with the motorist through which the officer observes many factors that are unobservable to researchers. The pretextual stop however, is one in which desire to stop and search a vehicle are established before a traffic violation is even committed. Thus, the omission in estimation of factors observed post stop is no longer a concern, because these factors are also omitted from the pretextual-stop decision itself.

Restricting attention to likely pretextual stops also helps to partially overcome infra-marginality concerns. Recall that investigatory stops may be conducted when the officer has articulable facts to support reasonable suspicion of a crime. When these articulable facts are not present, a traffic violation may be used as pretext to conduct what is essentially an investigatory stop. Thus, pretextual stops can be viewed as the marginal subgroup of investigatory traffic stops. This means that many infra-marginal investigatory stops—those where the standard of reasonable suspicion is met, and a stop can be initiated absent a traffic violation—are effectively excluded when we focus solely on pretextual stops.

In addition to these advantageous features, I exploit the adoption and gradual deployment of body cams across LMPD patrol divisions to further circumvent the problem of infra-marginality. Before assessing how the deployment of body cams affected characteristics of likely pretextual stops, I use the sample period prior to body-cam adoption to establish baselines.

5.1 Racial Disparities Before Body-Cam Adoption

The economic literature mentioned earlier documents disparities faced by both blacks and Hispanics in the US criminal justice system. As such, much of my analysis looks at these two groups in tandem, and results are typically similar within each subgroup. It should be noted however, that Hispanics account for a relatively small share (2.9 percent) of the observed traffic citations in Louisville.

Before the adoption of body cams, black motorists accounted for 46.04 percent of likely pretextual stops, and 26.30 percent of conventional traffic stops.[14] Given the disproportionate representation (relative to conventional traffic stops) of black motorists in likely pretextual stops, a natural question is how productive these stops are on average. Using likely pretextual stops from January 1, 2010 through May 31, 2015 (the day before body cams were initially deployed in the fifth patrol division), I estimate

$$Arrest_i = \beta_1 Black_i + \beta_2 Hispanic_i + \mathbf{X}_i{}' \boldsymbol{\theta} + \epsilon_i, \tag{2}$$

where $Black_i$ and $Hispanic_i$ are indicators equal to 1 if a stopped motorist is black and Hispanic, respectively. As before, the vector $\mathbf{X}_i$ contains fixed effects for the LMPD division conducting the stop, as well as the hour of the day, day of the week, month of the year, and year in which the stop occurred. I also augment equation (2) as

$$Arrest_i = \gamma_1 \left(Black_i + Hispanic_i\right) + \mathbf{X}_i{}' \boldsymbol{\theta} + \epsilon_i, \tag{3}$$

[14]Hispanic motorists accounted for 3.29 percent of likely pretextual stops, and 2.89 percent of conventional traffic stops.

and jointly estimate the difference in pretextual-stop arrest rate among black and Hispanic motorists.

Table 7 reports estimates of equation (2) in columns (1) and (3), and estimates of equation (3) in columns (2) and (4). In columns (1) and (2), the estimating sample includes stopped motorists of any race. In columns (3) and (4), the sample is restricted to stopped motorists who are black, Hispanic, or white. Among motorists who are neither black nor Hispanic, estimates in column (1) predict an arrest rate of 0.4646 in likely pretextual stops. Relative those other motorists, the pretextual-stop arrest rate is 15.84 percent lower for black motorists, 36.07 percent lower for Hispanic motorists, and 17.71 percent lower among black and Hispanic motorists jointly. Among white motorists, estimates in column (3) predict a pretextual-stop arrest rate of 0.4744. Relative to white motorists, the pretextual-stop arrest rate is 17.18 percent lower for black motorists, 37.42 percent lower for Hispanic motorists, and 19.06 percent lower for black and Hispanic motorists jointly.

Table 7 reveals aggregate disparities in arrest rate conditional on a likely pretextual stop. This is notable because, even if significant racial disparities exist at the individual-officer level, disparities might not appear in the aggregate if officers are sufficiently heterogeneous. The data do not report identifiers for individual officers. However, I augment equation (3) by interacting $(Black_i + Hispanic_i)$ with the division fixed effects, and examine heterogeneity across patrol divisions. Estimates of this augmented specification are reported in Table 8. The estimating sample for column group (1) includes likely pretextual stops of all motorists. For column group (2), the estimating sample is restricted to stopped motorists who are black, Hispanic, or white.

The estimates in Table 8 show that racial disparities in pretextual-stop arrest rate were somewhat specific to particular patrol divisions. While the pretextual-stop arrest rate among black and Hispanic motorists is lower within all divisions, among the fourth and fifth divisions, the difference is very small and statistically insignificant. In the second, seventh, and eighth divisions however, the disparity is statistically significant and very large.

Prior to body-cam deployment, while black and Hispanic motorists account for a disproportionately large share of likely pretextual stops, these stops were significantly less productive on average. Pretextual stops impose costs on the motorists involved. So whatever the source of this disparity, that result itself is quite meaningful. Yet, the lower pretextual-stop arrest rate among this group may not be due to racial bias. If race-neutral thresholds for pretextual stops were implemented, such disparities could arise from infra-marginality. Perhaps all motorists are subjected to the same pretextual-stop threshold, but conditional on meeting that threshold, white motorists are more likely to have committed arrestable offenses.

The adoption and deployment of body cams can shed light on which potential source of the disparity is more likely. Patrol officers are permitted to use failure-to-signal as a basis for pretextual traffic stops; but they are not supposed to use driver race as a factor in stop decisions. If the initial disparity results from infra-marginality, we wouldn't expect the deployment of body cams to significantly affect the disparity. However, if the disparity is more the result of different race-dependent thresholds for pretextual stops, the heightened accountability that body cams bring about may lead to a reduction in the disparity.

5.2 The Effect of Body-Worn Cameras on Pretextual Stop Frequency

Before turning to arrest rates, I assess whether body-worn cameras affected the frequency of likely pretextual stops. My full sample is composed of 502 full weeks, indexed by w.[15] Let $Y_{j,w}$ denote the number of likely pretextual stops that patrol division j made in week w of the sample. So long as the timing of body-cam deployment is uncorrelated with unobserved factors that influence Y, a treatment effect of body cams is identified by the "two-way fixed

[15] January 1, 2010 was a Friday. In order to have weekly periods beginning on Sundays, the first period includes 9 days.

effects" (TWFE) specification:

$$\mathrm{Y}_{j,w} = \gamma_1 BodyCam_{j,w} + \phi_j + \psi_w + \epsilon_{j,w}. \tag{4}$$

$BodyCam_{j,w}$ equals 1 if body cams were deployed for division j in, or before, week w.

Goodman-Bacon (2018) demonstrates that the OLS coefficient, $\hat{\gamma}_1$, will be a variance-weighted average of all possible "two-by-two difference-in-differences" (2×2 DD) estimates. A possible 2×2 DD estimate exists between two divisions, over a period where the status of *BodyCam* changes for one division, and does not change for the other. In later periods, this will include comparisons of divisions where body cams are deployed, with divisions where body cams were already deployed. In light of this property, attributing $\hat{\gamma}_1$ to the effect of body cams assumes parallel trends in all of these different counterfactual outcomes. That is, over a where period body-cam status switches for one division but does not for another, Y would have exhibited a common trend for both divisions following the week of the status switch, had body-cam status not changed in that week.

This underlying assumption can be partially supported by accounting for any divisional differences in trends prior to division 5's deployment week. I implement this by regressing $\mathrm{Y}_{j,w}$ from that period on division fixed effects interacted with a linear monthly trend variable. I then use full-sample residuals from this regression as the dependent variable and re-estimate equation (4). The estimate from this approach, is reported in column (2) of Table 9. Results for the regression producing these residuals are reported in Table A4 of the Appendix, and show no significant differences in divisional trends. Estimates of equation (4) as specified are reported in column (1). Both approaches suggest an effect of body-cams on pretextual-stop frequency that is negative, yet small and not significantly different from zero at conventional significance levels.

Goodman-Bacon (2018) shows that if the treatment effect of interest grows (away from zero) over time, the two-way fixed effects estimator will be biased toward understating the true effect. An alternative approach that circumvents this issue is to compare outcomes for divisions at similar stages relative to their deployment week. I construct the following specification:

$$\mathrm{Y}_{j,w} = \sum_{f=0}^{3} (\theta_f \times \mathrm{I}_{j,w,f}) + \sum_{f=-6}^{-2} (\theta_f \times \mathrm{I}_{j,w,f}) + \phi_j + \epsilon_{j,w}, \tag{5}$$

where f represents one-year periods defined about divisions' body-cam deployment weeks. For the 52-week period beginning with a division's deployment week, $f = 0$. For the 52-week period ending just before a division's deployment week, $f = -1$. In a division's second-to-last untreated (by body cams) year, $f = -2$, and so on. $\mathrm{I}_{j,w,f}$ is an indicator of division j being in period f about body-cam deployment, during week w. By construction, $f = 3$ when divisions are in at least their fourth treated year, and $f = -6$ when divisions are not yet in their fifth-to-last untreated year. This leaves the last year before body-cam deployment as the only stage not controlled for. As such, θ_f is the conditional expectation of Y when divisions are in half-year f about body-cam deployment, minus the conditional expectation of Y in divisions' last years before body-cam deployment.

In addition to estimating the equation as specified above, I also include controls for four separate points in time which might have affected policing decisions (regarding pretextual stops and in general). Though they don't give exact dates, Schaefer et al. (2017) report that the LMPD's initial exploratory research of body cams began in the fall of 2012, and that in the fall of 2013 the department sought price quotes from body-cam vendors. Because both actions might be perceived as signaling greater scrutiny of officer behavior from leadership, I include an indicator equaling 1 in the weeks including and after October 1, 2012, and an indicator equal to 1 in the weeks including and after October 1, 2013. I also control for two events stemming from the officer-involved shooting on Michael Brown in Ferguson, Missouri. Schaefer et al. (2017) suggest that the national attention the incident received shifted public interest in body cams within Louisville, and ultimately led to the city funding their adoption. Due to this, and because the attention the incident received may have caused changes in police behavior, I include two additional indicators. The first equals 1 in, and after, the week beginning August 10, 2014 (two days after the shooting). The second equals 1 in, and after, the week beginning November 23, 2014. November 23 was the day before the announcement that the grand jury hearing the case had decided not to indict the officer involved, which was followed by several days of protest.

Estimates of equation (5) as specified, are reported in Table 10. Column (2) reports estimates with the controls for accounting possible changes following: initial exploration of body cam adoption, solicitation of vendor quotes regarding body cams, the officer-involved shooting of Michael Brown (in a different city and state), and the decision not to indict the involved officer. In addition to these controls, estimates in column (3) include division-specific linear annual time trends (by year of the sample). In particular, the specifications used in columns (2) and (3) appear fairly supportive common trends in the time periods leading to body-cam deployment. Both suggest an initial statistically-significant decrease in pretextual stop frequency following body-cam deployment, however, the effect appears short lived. Figure A1 of the Appendix presents the coefficient and 95-percent confidence intervals from column (3) of this table. Ultimately, there is little evidence to suggest an effect of body cams on pretextual-stop frequency.

5.3 The Effect of Body-Worn Cameras on Pretextual-Stop Arrest Rates

The outcome variable in section 5.2 is a weekly count of likely pretextual stops within a division. In 32.4 percent of observations, that count is zero. To assess the effect of body cams on pretextual-stop arrest rates, estimation must occur at the traffic-stop level. With a similar specification to equation (4), I begin by estimating

$$Arrest_{i,j,t} = \lambda_1 BodyCam_{i,j,t} + \phi_j + \psi_{w(t)} + \epsilon_{i,j,t}. \tag{6}$$

$Arrest_{i,j,t}$ equals 1 if stop i, made on date t and by division j, results in arrest, and equals 0 otherwise. Division and week-of-sample fixed effects are also included.

Using likely pretextual stops, estimates of equation (6) as specified are reported in column (1) of Table 11. In column (2), day-of-week fixed effects are also added. In columns (3) and (4), controls for the four events mentioned in section 5.2—which might have affected pretextual-stop decisions and subsequent arrest rates—are added to the specifications from columns (1) and (2), respectively. Across all four specifications, estimates suggest a significant and very large positive effect of body cams on the arrest rate in likely pretextual stops. Prior to division 5's deployment date, likely pretextual stops yielded an arrest rate of 0.4500. Relative to that rate, these estimates suggest a 33 to 34 percent increase in pretextual-stop arrest rate with body cams in use.

On March 11, 2016, division 6 became the last division to have body cams deployed. This last deployment occurred much later than other divisions due to a connectivity issue with division 6 that required the installation of new fiber optic cables. Table A5 of the Appendix reports estimates of equation (6) under the same four specifications, but with the sample restricted to observations before division 6's deployment date. This estimates the effect of body cams using only the period where pretextual-stop arrest rates, with and without body cams in use, could be contemporaneously observed. These estimates are very similar to those using the full sample in sign, significance, and magnitude.

Finally, Table A6 of the Appendix reports results from regressing $Arrest_{i,j,t}$ on division fixed effects, interacted with a linear monthly trend, using likely pretextual stops made prior to division 5's deployment date. Using full-sample residuals from that regression as a dependent variable, Table A7 reports estimates mirroring those in Table 11. Even after removing all pre-deployment differences in trends across divisions, the presence of body cams suggests a significant and fairly large increase in pretextual-stop arrest rate.

The OLS estimate $\hat{\lambda}_1$ from equation (6) relies partially on comparisons of arrest rates for divisions in periods where $BodyCam_{j,w}$ switches from 0 to 1, with arrest rates for divisions where $BodyCam_{j,w}$ equals 1 throughout that same period. This could be problematic if the effect of body cams on pretextual-stop arrest rates evolves over time. Thus, similar to the approach employed in equation (5), I implement an "event study" specification

$$Arrest_{i,j,t} = \sum_{f=0}^{3} (\theta_f \times \mathrm{I}_{i,j,t,f}) + \sum_{f=-6}^{-2} (\theta_f \times \mathrm{I}_{i,j,t,f}) + \phi_j + \epsilon_{i,j,t}, \tag{7}$$

for pretextual-stop arrest rate as well.

In equation (7), f represents one-year periods defined about divisions' body-cam deployment dates. For the 364-day period beginning on a department's deployment date, $f = 0$. For the 364-day period ending the day before a division's deployment date, $f = -1$. In a division's second-to-last untreated year, $f = -2$, and so on. $\mathrm{I}_{i,j,t,f}$ indicates that stop i, made on date t in division j, occurred in year f about deployment. By construction, $f = 3$ when divisions are in at least their fourth treated year, and $f = -6$ when divisions are not yet in their fifth-to-last untreated year. This leaves the last untreated year as the only such period not captured by an indicator. As such, θ_f is the conditional expectation of $Arrest_{i,j,t}$ when divisions are in year f about body-cam deployment, minus the conditional expectation of $Arrest_{i,j,t}$ when divisions are in their last untreated year.

Column (1) of Table 12 reports estimates of equation (7) with LMPD Division fixed effects, and controls for any changes that may have been caused by: the LMPD's initial research of body-cam adoption, the LMPD's solicitation of bids from body-cam vendors, the officer-involved shooting of Michael Brown in Ferguson, MO, or the grand-jury decision not to indict the officer involved months later. In column (2), day-of-week fixed effects are also included. In column (3), division-specific linear annual trends are also included to account for any division-level differences in trend that might otherwise confound estimation of body cams' effect. Estimates across all three specifications are quite similar. They suggest significant and substantial increase in pretextual-stop arrest rate with body cams in use. This affect is immediately apparent following body-cam deployment, and mostly stable and persistent in the years that follow. Figure 2 plots the coefficients from column (3) and their 95-percent confidence intervals in these annual periods about body-cam deployment. Importantly, the significant increase in pretextual-stop arrest rate following body-cam deployment does not appear to stem from a pre-existing trend.

5.4 The Effect of Body-Worn Cameras on Racial Disparities

Prior to division 5's deployment date, black and Hispanic motorists were arrested at a significantly lower rate in likely pretextual stops. Section 5.3 shows a significant increase in pretextual-stop arrest rate following body-cam deployment, suggesting that the threshold (across all motorists) for initiating pretextual stops increased. Examining the composition of this increase among black and Hispanic motorists relative to others can shed light on whether the initial disparity was more likely the result of bias or infra-marginality.

Let BH_i indicate that stop i was of a black or Hispanic motorist. Including BH_i as well its interaction with $BodyCam$, I augment equation (6) as follows:

$$Arrest_{i,j,t} = \rho_1 \left(BodyCam_{i,j,t} \times BH_i \right) + \rho_2 BodyCam_{i,j,t} + \rho_3 BH_i + \phi_j + \psi_{w(t)} + \epsilon_{i,j,t}. \tag{8}$$

Conditional on division and week-of-sample fixed effects: ρ_3 is the difference in expected arrest rate, conditional on division and week-of-sample fixed effects, between black or Hispanic motorists and all others, prior to body-cam deployment; ρ_2 is the difference in expected arrest rate from pre-deployment to post-deployment for a motorist that is neither black nor Hispanic; and $(\rho_1 + \rho_2)$ is the difference in expected arrest rate from pre-deployment to post-deployment for a black or Hispanic motorist. Finally, ρ_1 is the difference in body cams' effects on expected pretextual-stop arrest rate between black or Hispanic and other motorists.

Estimates of equation (8) are reported in Table 13. Column (1) reports estimates of the equation as specified above. In column (2) day-of-week fixed effects are also included. In columns (3) and (4) the indicators switching to 1 after: September 2012, September 2013, the officer-involved shooting of Michael Brown, and the subsequent failure to indict the officer; as well as the interactions of these indicators with BH_i, are added to the specifications used in columns (1) and (2) respectively. If any of those four events caused a change in the racial arrest-rate disparity, the inclusion of these controls will protect against mistakenly attributing that change to the effect of body cams.

Across all four specifications we see a significant increase in pretextual-stop arrest rate among non-black and non-Hispanic motorists following the deployment of body cams. In column (1), notice that the pretextual-stop arrest rate is lower among black and Hispanic motorist 0.0843 prior to division 5's deployment date.[16] Beyond the rise of 0.1194 exhibited among other motorists, the arrest rate among black and Hispanic motorists in likely pretextual stops exhibits an additional increase of 0.0706 following body-cam deployment. This additional increase is statistically significant and overcomes 83.75 percent of the initial disparity. After controlling for the day of the week in which a stop occurs, results remain very similar. In the other two specifications, the additional increase among black and Hispanic is slightly smaller, yet overcomes 61.8 to 64.18 percent of the initial disparity. While this additional increase—beyond the increase exhibited by other motorists—is not significantly different from 0 at conventional levels, the overall increase in pretextual-stop arrest rate among black and Hispanic motorists is. Across columns (1) through (4), the effect of body cams on pretextual-stop arrest rate is estimated to be: 0.1900, 0.1908, 0.1791, and 0.1825. All are different from zero at the 99-percent significance level.

Beneath the three coefficients and their standard errors, Table 13 reports the sum, $(\hat{\rho}_1 + \hat{\rho}_3)$, and its standard error. In column (1) and (2), this is simply the estimated disparity when body cams are in use, conditional on division, week-of-sample, and (in the latter column) day-of-week fixed effects. In columns (3) and (4) the controls for the four aforementioned events and their interaction with black-or-Hispanic indicator. The first of these events

[16] As expected, this is very similar to what was found in section 5.1.

begins October 1, 2012, and reductions in the arrest-rate disparity between that date and a division's body-cam deployment date is not attributed to body.[17] Thus, in those columns, $\hat{\rho}_3$ estimates the racial disparity prior to October 1, 2012, and $(\hat{\rho}_1 + \hat{\rho}_3)$ estimates the disparity that remains after accounting for the effect attributed to body cams. In columns (1) and (2), notice that with body cams in use, the estimated racial disparity in pretextual-stop arrest rate is very small and statistically insignificant. In columns (3) and (4), as expected, the remaining disparity is larger, but still much smaller than the estimated disparity prior to the LMPD's initial research on body-cam implementation. When restricting the estimating sample to prior to division 6's deployment date, Table A8 shows estimates that are similar in magnitude.

A potential concern regarding these estimates is that, prior to body-cam deployment, the pretextual-stop arrest rate among black and Hispanic motorists may have been on an upward trend relative to other motorists. If so, this pre-existing difference in trends may be mistakenly attributed to the effect of body cams. Inclusion of the four event-specific controls, along with their interactions with BH_i helps to alleviate some of this concern. However, to further address this, I estimate the following equation:

$$Arrest_{i,j,t} = \chi_1 \left(Trend_t \times BH_i \right) + \chi_2 \, Trend_t + \chi_3 BH_i + \phi_j + \psi_{w(t)} + \epsilon_{i,j,t}, \tag{9}$$

using pretextual stops prior to division 5's deployment date. The linear trend variable is monthly. In addition to the four specifications used for equation (8), I also estimate a simple specification where the trend, BH_i, and their interaction are the only variables included. These estimates, reported in Table A9 of the Appendix, all fail to reject a common pre-trend between the two groups of motorists. Notice also, that when the event-specific controls and interactions are included, $\hat{\chi}_1$ becomes negative. These results further alleviate concerns over pre-deployment trends among the two subgroups.

[17]The estimates indicate that the disparity did decrease on average during that period.

5.5 Racial Disparities and Visibility of Race

Grogger and Ridgeway (2006) developed a "veil-of-darkness" test for racial bias in routine traffic stops which exploits the fact that driver race is more difficult to observe in darkness.[18] Borrowing on that intuition, I re-estimate equation (8) with the sample restricted to stops made during periods of daylight only. Using data for Louisville collected from https://www.timeanddate.com, I restrict the estimation sample to stops made after the beginning (in the morning), and before the end (in the evening), of civil twilight on a given date. This provides a more refined assessment of racial disparities as it limits analysis to observations where race is more easily observed prior to a stop.

Estimates of equation (8), using likely pretextual stops made during daylight, are reported in Table 14. These results reveal that, excluding stops made during darkness—when driver race is more difficult to observe—body cams have a much smaller effect on pretextual-stop arrest rate among non-black and non-Hispanic motorists. Columns (1) and (2) show that the initial racial disparity is even larger when restricting analysis to stops made during daylight. Prior to division 5's deployment date, the pretextual-stop arrest rate was 0.5289 for white motorists during daylight. Relative to whites over that same period, the estimates in columns (1) and (2) suggest a pretextual-stop arrest rate among black and Hispanic motorists that was 21.35 to 21.54 percent lower during daylight. Moreover, across all four specifications, the use of body cams has an additional effect on pretextual-stop arrest rate that is very large, and statistically significant at conventional levels. This additional increase among black and Hispanic motorists during daylight, is enough to overcome anywhere from 71.50 to 97.05 percent of the initial disparity.

[18] Extensions of the test are found in Horrace and Rohlin (2016), Kalinowski et al. (2017), and Ritter (2017).

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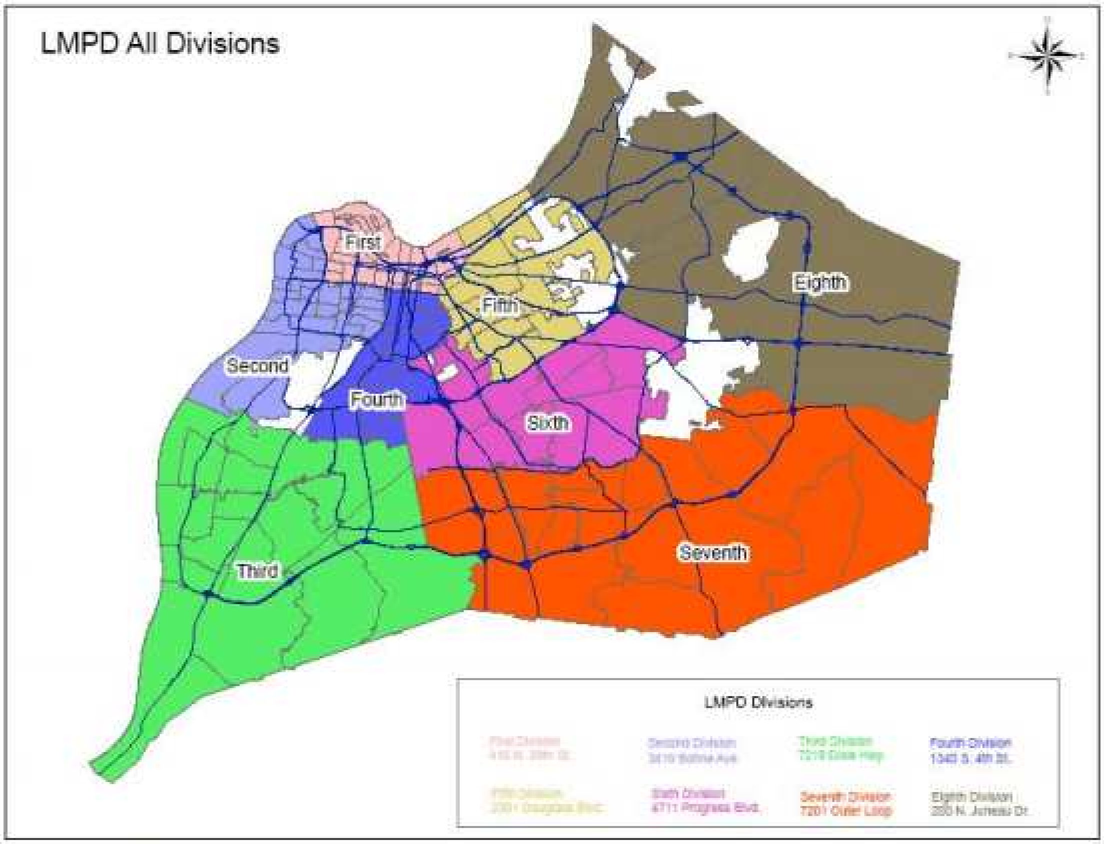

Figure 1: LMPD Patrol Divisions

Map of the eight geographically-defined patrol divsions of the LMPD. Image is from the department's 2014 annual report, retrieved online at: https://louisvilleky.gov/sites/default/files/police/sop_searchable_and_reports/lmpd_2014_annual_report-final.pdf.

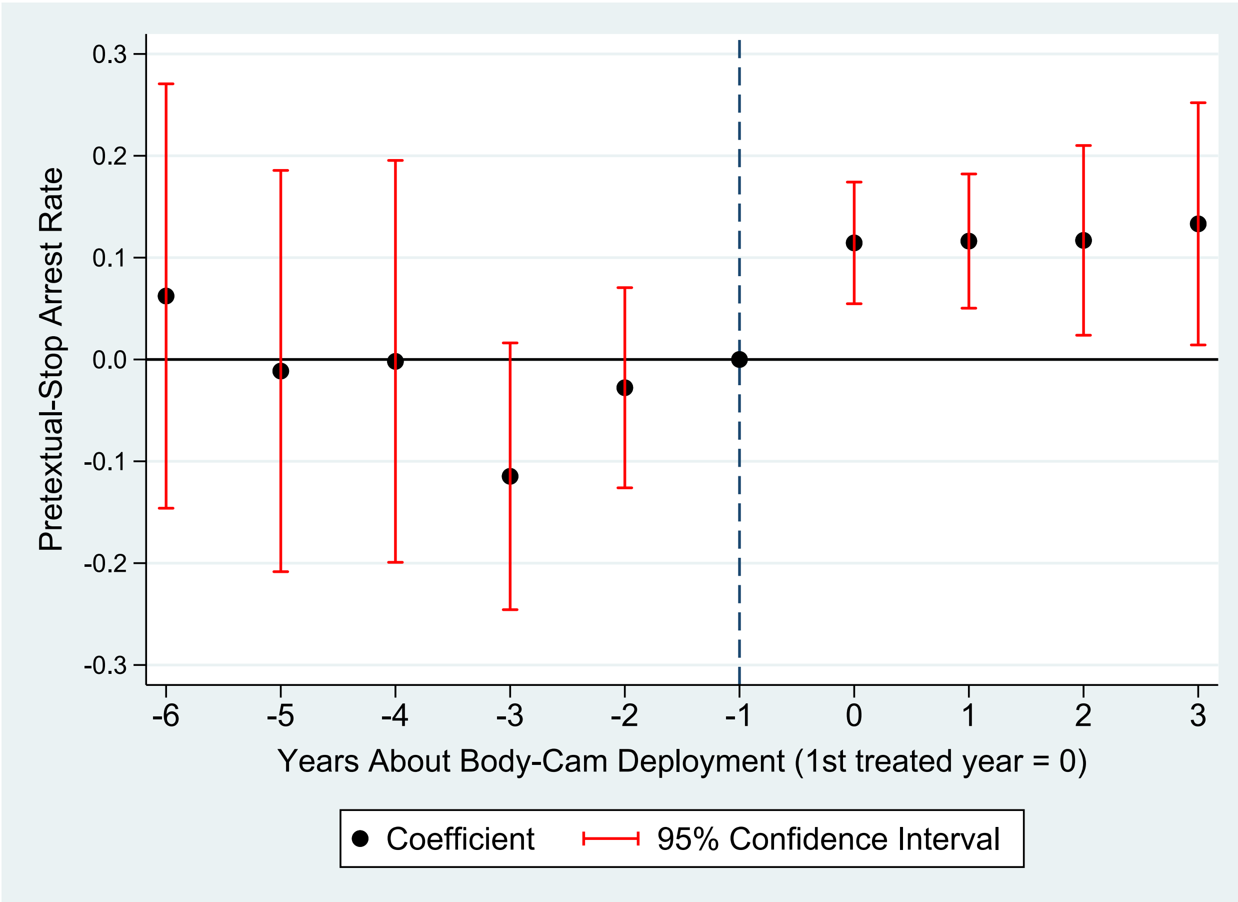

Figure 2: Event Study: Pretextual-Stop Arrest Rate and Body-Cam Deployment

This figure plots $\hat{\theta}_f$ from estimating equation (7) under the specifcation reported in column (3) of Table 12. The horizontal axis tracks one-year periods defined about each division's body-cam deployment date. The value 0 marks the first treated year. The vertical axis measures the pretextual-stop arrest rate about the average frequency in divisions' last year before body-cam deployment.

Table 1: Traffic Violation Summary Statistics

Violation	Obs.	Pct. Traffic Stops with Violation
Objective Driving Violations		
Speeding	215,574	50.70
Disregarding Traffic Light	20,159	4.74
Disregarding Stop Sign	15,702	3.69
Failure to Signal	9,374	2.20
Use Communication Device	1,674	0.39
Improper Turn	1,615	0.38
Fail to Yield Right-of-Way	1,050	0.25
Subjective Driving Violations		
Reckless Driving	12,250	2.88
Careless Driving	12,094	2.84
Following too Close	3,591	0.84
Improper Passing	1,145	0.27
Too Fast for Conditions	544	0.13
Non-driving Violations		
Invalid Plate/Registration	145,317	33.17
One Head Light	13,062	3.07
No Tail Lights	3,569	0.84
Fail to Illuminate Head Lights	2,898	0.68
No Brake Lights	2,711	0.64
Obstructed Vision/Windshield	1,870	0.44
Fail to Dim Head Lights	881	0.21

Reported statistics are from citations issued by officers in the eight geographically-defined partol divisions of the LMPD. Sample period: January 1, 2010 to August 19, 2019.

Table 2: LMPD Deployment of Body-Worn Cameras

LMPD Patrol Division	Date of Body-Cam Deployment
Fifth Division	June 1, 2015
Second Division	July 21, 2015
First Division	August 4, 2015
Fourth Division	August 12, 2015
Third Division	August 18, 2015
Ninth Mobile Division	August 20, 2015
Seventh Division	September 24, 2015
Eighth Division	October 2, 2015
Sixth Division	March 11, 2016

Table 3: Arrest Rates and the Presence of Multiple Traffic Violations

Variable	(1) $Arrest_i$	(2) $Arrest_i$	(3) $Arrest_i$	(4) $Arrest_i$
Multiple	-0.3172***	0.0280***	0.0424***	0.0525***
	(0.0109)	(0.0026)	(0.0064)	(0.0083)
Intercept	0.5217***	0.0061***	0.0597***	0.0773**
	(0.0171)	(0.0007)	(0.0061)	(0.0112)
Sample: stops including	*Failure to Signal*	*Speeding*	*Disregarding Traffic Light*	*Disregarding Stop Sign*
R-squared	0.0921	0.0076	0.0039	0.0048
N	8,641	213,693	19,283	15,341

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates with no additional controls. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. "Sample: stops including" indicates that the column's estimates are from stops including the violation listed (excluding any resulting in DUI).

Table 4: Arrest Rates and the Presence of Multiple Traffic Violations: Full Specification

Variable	(1) $Arrest_i$	(2) $Arrest_i$	(3) $Arrest_i$	(4) $Arrest_i$
Multiple	-0.2870***	0.0268***	0.0260***	0.0067
	(0.0114)	(0.0025)	(0.0059)	(0.0077)
LMPD Division FE	Y	Y	Y	Y
Hour-of-Day FE	Y	Y	Y	Y
Day-of-Week FE	Y	Y	Y	Y
Month-of-Year FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Sample: stops including	*Failure to Signal*	*Speeding*	*Disregarding Traffic Light*	*Disregarding Stop Sign*
R-squared	0.1523	0.0150	0.0541	0.1202
N	8,641	213,693	19,283	15,341

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. "Sample: stops including" indicates that the column's estimates are from stops including the violation listed (excluding any resulting in DUI).

Table 5: Arrest Rate and the Presence of Multiple Traffic Violations: Failure-to-Signal by Patrol Division

LMPD Division	$\widehat{Arrest}_0$	*Multiple*
Division 1	0.5839	-0.2709***
		(0.0191)
Division 2	0.6101	-0.3085***
		(0.0200)
Division 3	0.4891	-0.3205***
		(0.0231)
Division 4	0.5502	-0.3057***
		(0.0384)
Division 5	0.3473	-0.2312***
		(0.0290)
Division 6	0.5024	-0.3063***
		(0.0240)
Division 7	0.4158	-0.2663***
		(0.0225)
Division 8	0.3413	-0.2323***
		(0.0330)
R-squared	0.1530	
N	8,641	

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops involving failure-to-signal. Within each division, $\widehat{Arrest}_0$ is the predicted arrest rate when $Multiple = 0$. The third column reports the interaction term between a division's indicator and $Multiple_i$. Robust standard errors for those coefficients, clustered at the division-by-year level, are reported in parentheses. Addition controls: LMPD Division FE, hour-of-day FE, day-of-week FE, month-of-year FE, year FE.

Table 6: Conventional and Likely Pretextual Traffic Stops by Patrol Division

	Likely Pretextual Stops†			Conventional Moving-Violation Stops		
LMPD Division	N	Arrests	Arrest Rate	N	Arrests	Arrest Rate
Division 1	995	581	0.5839	11,384	1,263	0.1109
Division 2	1,226	748	0.6101	8,905	1,229	0.1380
Division 3	597	292	0.4891	24,450	910	0.0372
Division 4	1,165	641	0.5502	25,895	1,317	0.0509
Division 5	311	108	0.3473	57,686	502	0.0087
Division 6	615	309	0.5024	49,757	960	0.0193
Division 7	469	195	0.4158	37,554	598	0.0159
Division 8	378	129	0.3413	50,128	575	0.0115

†Likely pretextual stops are those in which failure-to-signal was the only cited traffic violation. Conventional moving-violation stops are those that: (1) result in the citation of at least one moving violation, and (2) don't belong to the likely pretextual category. Both categories exclude stops resulting in DUI.

Table 7: Racial Disparities in Pretextual-Stop Arrest Rate Before Body-Cam Adoption

Variable	(1) $Arrest_i$	(2) $Arrest_i$	(3) $Arrest_i$	(4) $Arrest_i$
Black	-0.0736***	—	-0.0815***	—
	(0.0196)	—	(0.0188)	—
Hispanic	-0.1676***	—	-0.1775***	—
	(0.0423)	—	(0.0427)	—
Black or Hispanic	—	-0.0823***	—	-0.0904***
	—	(0.0189)	—	(0.0182)
LMPD Division FE	Y	Y	Y	Y
Hour-of-Day FE	Y	Y	Y	Y
Day-of-Week FE	Y	Y	Y	Y
Month-of-Year FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Sampled Motorists	All	All	Black, Hispanic & White	Black, Hispanic & White
R-squared	0.0681	0.0670	0.0678	0.0667
N	3,369	3,369	3,281	3,281

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops where failure to signal was the only cited traffic violation. All stops are prior to June 1, 2015. The estimating sample for columns (1) and (2) includes motorists of any race. Estimates in columns (3) and (4) are from stops of black, Hispanic, and white motorists only. Robust standard errors, clustered at the division-by-year level, are reported in parentheses.

Table 8: Racial Disparities in Pretextual-Stop Arrest Rate Before Body-Cam Adoption

	(1)		(2)	
LMPD Division	$\widehat{Arrest}_0$	(*Black or Hispanic*)	$\widehat{Arrest}_0$	(*Black or Hispanic*)
Division 1	0.5097	-0.0579**	0.5153	-0.0637**
		(0.0243)		(0.0216)
Division 2	0.6968	-0.1768***	0.7034	-0.1811***
		(0.0408)		(0.0361)
Division 3	0.4905	-0.0727	0.5033	-0.0844
		(0.0713)		(0.0696)
Division 4	0.4500	-0.0154	0.4602	-0.0273
		(0.0308)		(0.0301)
Division 5	0.3082	-0.0066	0.3182	-0.0174
		(0.1034)		(0.1023)
Division 6	0.5298	-0.0738	0.5506	-0.0839
		(0.0559)		(0.0605)
Division 7	0.4225	-0.1641***	0.4375	-0.1795***
		(0.0446)		(0.0497)
Division 8	0.3380	-0.1022*	0.3383	-0.1032*
		(0.0512)		(0.0478)
Sampled Motorists	*All*		*Black, Hispanic & White*	
R-squared	0.0700		0.0695	
N	3,369		3,281	

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops where failure to signal was the only cited traffic violation. All stops are prior to June 1, 2015. The estimating sample for column group (1) includes motorists of any race, for column group (2) it is stops of black, Hispanic, and white motorists only. $\widehat{Arrest}_0$ is the predicted arrest rate in a division when the motorist is neither black nor Hispanic. Coefficients on a division's indicator and the indicator for a stopped motorist being black or Hispanic are reported in the second column of each group. Robust standard errors for these coefficients, clustered at the division-by-year level, are reported in parentheses. For both samples, the additional controls are: LMPD Division FE, hour-of-day FE, day-of-week FE, month-of-year FE, and year FE.

Table 9: Body-Worn Cameras and the Frequency of Likely Pretextual Stops

Variable	(1) $Y_{j,w}$	(2) Residual $Y_{j,w}$
BodyCam	-0.2511	-0.2488
	(0.1950)	(0.1902)
LMPD Division FE	Y	Y
Week-of-Sample FE	Y	Y
Division-Specific Pre-Trends Removed	N	Y
R-squared	0.3293	0.1904
N	4,016	4,016

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

OLS estimates with division-week unit of observation. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. The dependent variable in column (1), $Y_{j,w}$, is the number of likely pretextual stops made by division j during week-of-the-sample w. The dependent variable in column (2) is the residual from regressing $Y_{j,w}$, in the period prior to Division 5's deployment week (the first deployment week in the sample), on division fixed effects interacted with a weekly trend. The estimates that produced these residuals are reported in Table A4 of the Appendix.

Table 10: Body-Worn Cameras and the Frequency of Likely Pretextual Stops: Event Study

Variable	(1) $Y_{j,w}$	(2) $Y_{j,w}$	(3) $Y_{j,w}$
After third treated year	-0.0272	-0.0034	-0.3130
	(0.1565)	(0.1531)	(0.2822)
Third treated year	0.4183**	0.4412**	0.2369
	(0.1942)	(0.1913)	(0.2315)
Second treated year	-0.1082	-0.0852	-0.2179
	(0.1255)	(0.1220)	(0.1574)
First treated year	-0.2548***	-0.2319***	-0.2916***
	(0.0776)	(0.0727)	(0.0885)
Second-to-last untreated year	0.0986	0.2541	0.2717*
	(0.1125)	(0.1632)	(0.1569)
Third-to-last untreated year	0.1779**	0.1823	0.1875
	(0.0769)	(0.2666)	(0.2486)
Fourth-to-last untreated year	0.1562	0.0019	-0.0312
	(0.1230)	(0.3701)	(0.3448)
Fifth-to-last untreated year	0.2524***	0.0881	0.1210
	(0.0878)	(0.3720)	(0.3660)
More than five years before treatment	0.0833	-0.0823	-0.0450
	(0.1264)	(0.3711)	(0.3717)
LMPD Division FE	Y	Y	Y
Post September 2012 FE	N	Y	Y
Post September 2013 FE	N	Y	Y
Post Michael Brown Shooting FE	N	Y	Y
Post Grand Jury Decision FE	N	Y	Y
Division-Specific Annual Trends	N	N	Y
R-squared	0.2108	0.2118	0.2278
N	4,016	4,016	4,016

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates. Unit of observation is a division-week. Robust standard errors, clustered at the division-by-year level, are reported in parentheses.

Table 11: The Effect of Body-Worn Cameras on Pretextual-Stop Arrest Rate

Variable	(1) $Arrest_{i,j,t}$	(2) $Arrest_{i,j,t}$	(3) $Arrest_{i,j,t}$	(4) $Arrest_{i,j,t}$
BodyCam	0.1504**	0.1526***	0.1507**	0.1529***
	(0.0576)	(0.0562)	(0.0576)	(0.0562)
LMPD Division FE	Y	Y	Y	Y
Week-of-Sample FE	Y	Y	Y	Y
Day-of-Week FE	N	Y	N	Y
Post Sept. 2012 FE	N	N	Y	Y
Post Sept. 2013 FE	N	N	Y	Y
Post Michael Brown Shooting FE	N	N	Y	Y
Post Grand Jury Decision FE	N	N	Y	Y
R-squared	0.1493	0.1528	0.1497	0.1652
N	5,756	5,756	5,756	5,756

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops where failure-to-signal was the only cited traffic violation. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. $Arrest_{i,j,t}$ indicates an arrest occuring in stop i, made on date t, by division j.

Table 12: Body-Worn Cameras and Pretextual-Stop Arrest Rate: Event Study

Variable	(1) $Arrest_{i,j,t}$	(2) $Arrest_{i,j,t}$	(3) $Arrest_{i,j,t}$
After third treated year	0.0833***	0.0864***	0.1332**
	(0.0284)	(0.0282)	(0.0607)
Third treated year	0.0729**	0.0744**	0.1169**
	(0.0306)	(0.0304)	(0.0475)
Second treated year	0.0891***	0.0889***	0.1163***
	(0.0303)	(0.0305)	(0.0336)
First treated year	0.1061***	0.1038***	0.1145***
	(0.0271)	(0.0272)	(0.0305)
Second-to-last untreated year	-0.0325	-0.0242	-0.0278
	(0.0519)	(0.0523)	(0.0502)
Third-to-last untreated year	-0.1142*	-0.1035	-0.1147*
	(0.0672)	(0.0665)	(0.0668)
Fourth-to-last untreated year	0.0116	0.0225	-0.0019
	(0.1014)	(0.1010)	(0.1007)
Fifth-to-last untreated year	0.0234	0.0316	-0.0114
	(0.0969)	(0.0959)	(0.1005)
More than five years before treatment	0.1137	0.1211	0.0623
	(0.1041)	(0.1030)	(0.1063)
LMPD Division FE	Y	Y	Y
Post September 2012 FE	Y	Y	Y
Post September 2013 FE	Y	Y	Y
Post Michael Brown Shooting FE	Y	Y	Y
Post Grand Jury Decision FE	Y	Y	Y
Day-of-Week FE	N	Y	Y
Division-Specific Annual Trends	N	N	Y
R-squared	0.0618	0.0654	0.0708
N	5,756	5,756	5,756

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops where failure-to-signal was the only cited traffic violation. Robust standard errors, clustered at the division-by-year level, are reported in parentheses.

Table 13: Body-Worn Cameras and Racial Disparities in Pretextual-Stop Arrest Rate

Variable	(1) $Arrest_{i,j,t}$	(2) $Arrest_{i,j,t}$	(3) $Arrest_{i,j,t}$	(4) $Arrest_{i,j,t}$
$BodyCam \times BH_i$	0.0706***	0.0683***	0.0521	0.0541
	(0.0258)	(0.0258)	(0.0404)	(0.0397)
$BodyCam$	0.1194**	0.1226**	0.1270**	0.1284**
	(0.0585)	(0.0573)	(0.0617)	(0.0611)
BH_i	-0.0843***	-0.0852***	-0.0993***	-0.1002***
	(0.0196)	(0.0195)	(0.0276)	(0.0276)
$(BodyCam \times BH_i) + BH_i$	-0.0136	-0.0169	-0.0472	-0.0460
	(0.0185)	(0.0184)	(0.0484)	(0.0473)
LMPD Division FE	Y	Y	Y	Y
Week-of-Sample FE	Y	Y	Y	Y
Day-of-Week FE	N	Y	N	Y
Post Sept. 2012 FE $\times BH_i$	N	N	Y	Y
Post Sept. 2013 FE $\times BH_i$	N	N	Y	Y
Post M.B. Shooting FE $\times BH_i$	N	N	Y	Y
Post Grd. Jury FE $\times BH_i$	N	N	Y	Y
Post Sept. 2012 FE	N	N	Y	Y
Post Sept. 2013 FE	N	N	Y	Y
Post M.B. Shooting FE	N	N	Y	Y
Post Grd. Jury FE	N	N	Y	Y
R-squared	0.1528	0.1565	0.1537	0.1574
N	5,756	5,756	5,756	5,756

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops where failure-to-signal was the only cited traffic violation. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. BH_i equals 1 if the stopped motorist is black or Hispanic, and 0 otherwise. Each column reports three regression coefficients followed by their standard errors. Then, the sum of the column's coefficients on $(BodyCam \times BH_i)$ and BH_i are reported followed by the standard error for the combination. This estimates the racial disparity in arrest rate following the effect of body cams.

Table 14: Body-Worn Cameras and Racial Disparities During Daylight

Variable	(1) $Arrest_{i,j,t}$	(2) $Arrest_{i,j,t}$	(3) $Arrest_{i,j,t}$	(4) $Arrest_{i,j,t}$
$BodyCam \times BH_i$	0.0860**	0.0807**	0.1265**	0.1283***
	(0.0376)	(0.0370)	(0.0496)	(0.0483)
$BodyCam$	0.0410	0.0405	0.0255	0.0224
	(0.0933)	(0.0945)	(0.0961)	(0.0989)
BH_i	-0.1139***	-0.1129***	-0.1332***	-0.1322***
	(0.0229)	(0.0225)	(0.0350)	(0.0340)
$(BodyCam \times BH_i) + BH_i$	-0.0279	-0.0322	-0.0067	-0.0039
	(0.0304)	(0.0302)	(0.0594)	(0.0575)
LMPD Division FE	Y	Y	Y	Y
Week-of-Sample FE	Y	Y	Y	Y
Day-of-Week FE	N	Y	N	Y
Post Sept. 2012 FE $\times BH_i$	N	N	Y	Y
Post Sept. 2013 FE $\times BH_i$	N	N	Y	Y
Post M.B. Shooting FE $\times BH_i$	N	N	Y	Y
Post Grd. Jury FE $\times BH_i$	N	N	Y	Y
Post Sept. 2012 FE	N	N	Y	Y
Post Sept. 2013 FE	N	N	Y	Y
Post M.B. Shooting FE	N	N	Y	Y
Post Grd. Jury FE	N	N	Y	Y
R-squared	0.2234	0.2266	0.2253	0.2286
N	2,898	2,898	2,898	2,898

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

OLS estimates from stops where failure-to-signal was the only cited traffic violation. All stops were made after the beginning, and before the end of, civil twilight. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. BH_i equals 1 if the stopped motorist is black or Hispanic, and 0 otherwise. Each column reports three regression coefficients followed by their standard errors. Then, the sum of the column's coefficients on $(BodyCam \times BH_i)$ and BH_i are reported followed by the standard error for the combination. This estimates the racial disparity in arrest rate following the effect of body cams.

A Appendix

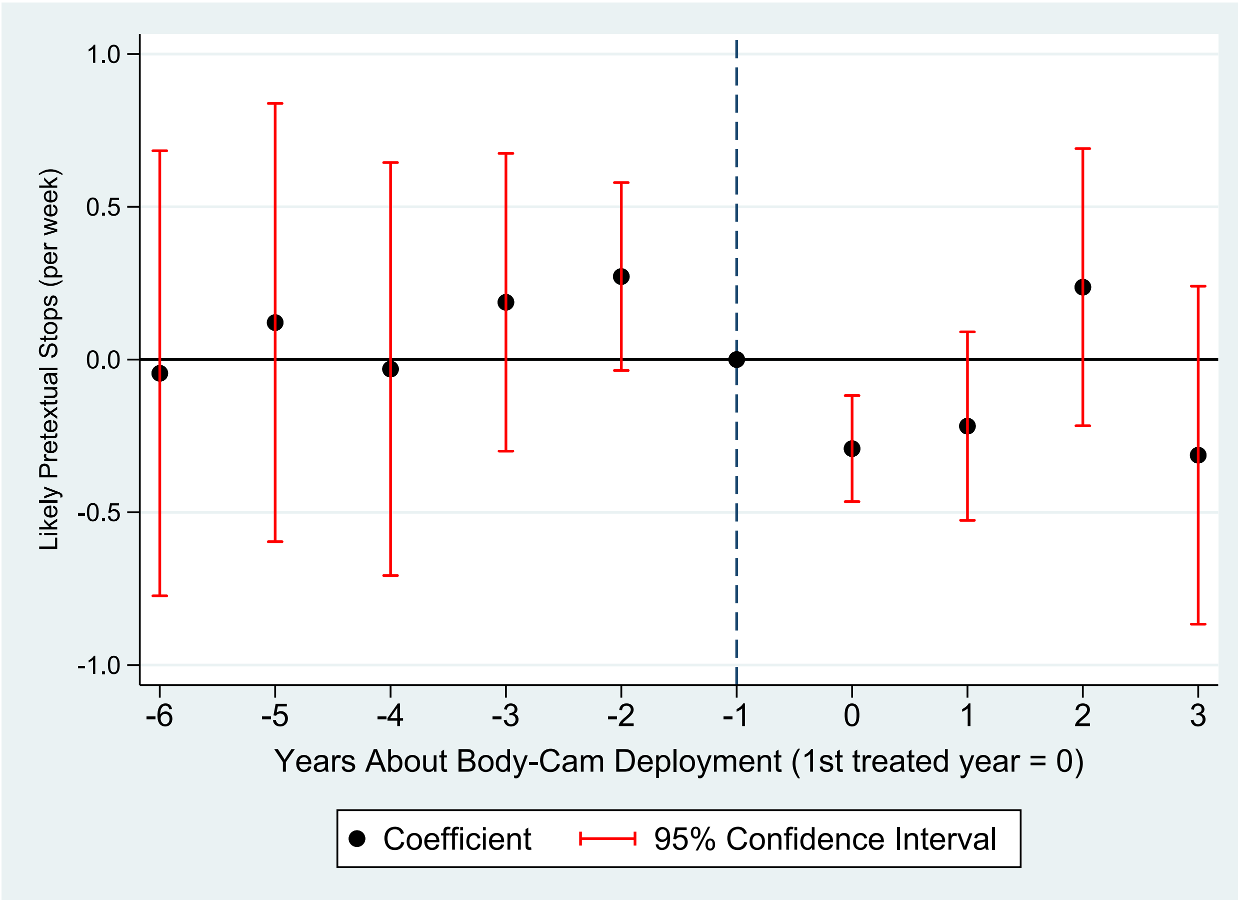

Figure A1: Event Study: Pretextual Stop Frequency and Body-Cam Deployment

This figure plots $\hat{\theta}_f$ from estimating equation (5) under the specifcation reported in column (3) of Table 10. The horizontal axis tracks one-year periods defined about each division's body-cam deployment week. The value 0 marks the first treated year. The vertical axis measures the frequency likely pretextual stops at the division-week level about the average frequency in divisions' last year before body-cam deployment.

Table A1: Post-Stop Arrest Rates Conditional on Common Traffic Violations

Violation	Arrest Rate	(S.E.)	Traffic Stops Including Violation
Objective Driving Violations			
Speeding	0.0087	(0.0002)	213,693
Disregarding Traffic Light	0.0665	(0.0018)	19,283
Disregarding Stop Sign	0.0859	(0.0023)	15,341
Failure to Signal	0.4158	(0.0053)	8,641
Subjective Driving Violations			
Careless Driving	0.0939	(0.0028)	10,751
Reckless Driving	0.1976	(0.0041)	9,514
Non-driving Violations			
Invalid Plate/Registration	0.0870	(0.0008)	138,209
One Head Light	0.0615	(0.0021)	12,765

Reported statistics are from stops conducted by the eight geographically-defined partol divisions of the LMPD, and exclude stops resulting in charges of DUI.

Table A2: Failure-to-Signal Stops: Summary of Other Traffic Violations

Violation	Citations	Pct. of other Traffic Violation Citations
Careless Driving	357	9.59
Disregarding Stop Sign	299	8.03
Disregarding Traffic Light	175	4.70
Fail to Dim Head Lights	4	0.11
Fail to Illuminate Head Lights	23	0.62
Failure to Yield Right-of-Way	4	0.11
Following too Close	106	2.85
Improper Passing	59	1.58
Improper Turn	54	1.45
Invalid Plate/Registration	1,320	35.45
No Brake Lights	68	1.83
No Tail Lights	36	0.97
Obstructed Vision/Windshield	54	1.45
One Head Light	94	2.52
Other	97	2.60
Reckless	299	8.03
Speeding	642	17.24
Too Fast for Conditions	13	0.35
Use Communication Device	20	0.54

Reported statistics are from stops in which failure-to-signal was one of multiple cited traffic violations. Stops resulting in DUI charges are excluded.

Table A3: Arrest Rate and the Presence of Multiple Traffic Violations: Alternative Specifications

Variable	(1) $Arrest_i$	(2) $Arrest_i$
Multiple	-0.2870***	-0.2798***
	(0.0114)	(0.0120)
LMPD Division FE	Y	N
(LMPD Division $\times$ Beat) FE	N	Y
Hour-of-Day FE	Y	Y
Day-of-Week FE	Y	Y
Month-of-Year FE	Y	Y
Year FE	Y	Y
R-squared	0.1523	0.1620
N	8,641	8,641

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops involving failure-to-signal. Robust standard errors, clustered at the division-by-year level, are reported in parentheses.

Table A4: Frequency of Likely Pretextual Stops: Pre-Deployment Trends

Variable	$Y_{j,w}$
(Trend) × (Division 5)	0.0030
	(0.0067)
Division 5	-0.7649***
	(0.2363)
(Trend) × (Division 2)	0.0035
	(0.0052)
Division 2	0.7670***
	(0.2053)
(Trend) × (Division 1)	-0.0068
	(0.0061)
Division 1	0.7090***
	(0.2102)
(Trend) × (Division 4)	-0.0078
	(0.0056)
Division 4	0.9086***
	(0.2429)
(Trend) × (Division 3)	0.0038
	(0.0054)
Division 3	-0.1786
	(0.229)
(Trend) × (Division 7)	0.0061
	(0.0057)
Division 7	-0.6150**
	(0.2471)
(Trend) × (Division 8)	0.0030
	(0.0062)
Division 8	-0.4706**
	(0.2188)
Trend	-0.0024
	(0.0042)
Intercept	1.5086***
	(0.1677)
R-squared	0.1348
N	2,256

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from the period prior to Division 5's deployment week (first in the sample). $Y_{j,w}$, is the number of likely pretextual stops made by division j during week-of-the-sample w. The linear trend variable is the month of the sample. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. Division 6 is absorbed by the intercept.

Table A5: Body-Worn Cameras and Pretextual-Stop Arrest Rate: Before March 11, 2016

Variable	(1) $Arrest_{i,j,t}$	(2) $Arrest_{i,j,t}$	(3) $Arrest_{i,j,t}$	(4) $Arrest_{i,j,t}$
BodyCam	0.1567**	0.1600***	0.1571**	0.1604***
	(0.0631)	(0.0596)	(0.0632)	(0.0597)
LMPD Division FE	Y	Y	Y	Y
Week-of-Sample FE	Y	Y	Y	Y
Day-of-Week FE	N	Y	N	Y
Post Sept. 2012 FE	N	N	Y	Y
Post Sept. 2013 FE	N	N	Y	Y
Post Michael Brown Shooting FE	N	N	Y	Y
Post Grand Jury Decision FE	N	N	Y	Y
R-squared	0.1365	0.1446	0.1372	0.1453
N	3,753	3,753	3,753	3,753

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops where failure-to-signal was the only cited traffic violation, and prior to Division 6's deployment date (the latest in the sample). Robust standard errors, clustered at the division-by-year level, are reported in parentheses.

Table A6: Pretextual-Stop Arrest Rate: Pre-Deployment Trends

Variable	$Arrest_{i,j,t}$
(Trend) × (Division 5)	0.0001
	(0.0006)
Division 5	-0.1961***
	(0.0647)
(Trend) × (Division 2)	0.0010**
	(0.0004)
Division 2	-0.0609
	(0.0603)
(Trend) × (Division 1)	0.0011***
	(0.0004)
Division 1	-0.1617***
	(0.0589)
(Trend) × (Division 4)	0.0010***
	(0.0003)
Division 4	-0.1792***
	(0.0395)
(Trend) × (Division 3)	0.0007*
	(0.0004)
Division 3	-0.0919
	(0.0640)
(Trend) × (Division 7)	0.0011***
	(0.0003)
Division 7	-0.2777***
	(0.0305)
(Trend) × (Division 8)	-0.0001
	(0.0005)
Division 8	-0.1582**
	(0.0633)
Trend	-0.0033***
	(0.0010)
Intercept	0.5897***
	(0.0241)
R-squared	0.0309
N	3,369

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from the period prior to Division 5's deployment date (the first deployment date in the sample). The linear trend variable is the month of the sample. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. Division 6 is absorbed by the intercept.

Table A7: The Effect of Body-Worn Cameras on Pretextual-Stop Arrest Rate

Variable	(1) Residual $Arrest_{i,j,t}$	(2) Residual $Arrest_{i,j,t}$	(3) Residual $Arrest_{i,j,t}$	(4) Residual $Arrest_{i,j,t}$
BodyCam	0.1001*	0.1022*	0.1004*	0.1025*
	(0.0592)	(0.0584)	(0.0593)	(0.0584)
LMPD Division FE	Y	Y	Y	Y
Week-of-Sample FE	Y	Y	Y	Y
Day-of-Week FE	N	Y	N	Y
Post Sept. 2012 FE	N	N	Y	Y
Post Sept. 2013 FE	N	N	Y	Y
Post Michael Brown Shooting FE	N	N	Y	Y
Post Grand Jury Decision FE	N	N	Y	Y
R-squared	0.1218	0.1253	0.1223	0.1258
N	5,756	5,756	5,756	5,756

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops where failure-to-signal was the only cited traffic violation. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. The dependent variable is the full-sample residual of $Arrest_{i,j,t}$ from the regression reported in Table A6.

Table A8: Body-Worn Cameras and Racial Disparities in Pretextual-Stop Arrest Rate: Before March 11, 2016

Variable	(1) $Arrest_{i,j,t}$	(2) $Arrest_{i,j,t}$	(3) $Arrest_{i,j,t}$	(4) $Arrest_{i,j,t}$
$BodyCam \times BH_i$	0.0628	0.0735	0.0433	0.0598
	(0.0707)	(0.0704)	(0.0691)	(0.0665)
$BodyCam$	0.1288**	0.1272**	0.1367**	0.1326**
	(0.0602)	(0.0578)	(0.0604)	(0.0579)
BH_i	-0.0808***	-0.0820***	-0.0950***	-0.0971***
	(0.0203)	(0.0204)	(0.0295)	(0.0297)
$(BodyCam \times BH_i) + BH_i$	-0.0179	-0.0085	-0.0517	-0.0373
	(0.0697)	(0.0700)	(0.0749)	(0.0726)
LMPD Division FE	Y	Y	Y	Y
Week-of-Sample FE	Y	Y	Y	Y
Day-of-Week FE	N	Y	N	Y
Post Sept. 2012 FE × BH_i	N	N	Y	Y
Post Sept. 2013 FE × BH_i	N	N	Y	Y
Post M.B. Shooting FE × BH_i	N	N	Y	Y
Post Grd. Jury FE × BH_i	N	N	Y	Y
Post Sept. 2012 FE	N	N	Y	Y
Post Sept. 2013 FE	N	N	Y	Y
Post M.B. Shooting FE	N	N	Y	Y
Post Grd. Jury FE	N	N	Y	Y
R-squared	0.1413	0.1495	0.1426	0.1509
N	3,753	3,753	3,753	3,753

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

OLS estimates from stops where failure-to-signal was the only cited traffic violation, prior to division 6's deployment date. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. BH_i equals 1 if the stopped motorist is black or Hispanic, and 0 otherwise. Each column reports three regression coefficient followed by their standard errors. Then, the sum of the column's coefficients on $BodyCam \times BH_i$ and BH_i are reported followed by the standard error for the combination. This estimates the racial disparity in arrest rate following the effect of body cams.

Table A9: Pretextual-Stop Arrest Rate and Group Trends: Before June 1, 2015

Variable	(1) $Arrest_{i,j,t}$	(2) $Arrest_{i,j,t}$	(3) $Arrest_{i,j,t}$	(4) $Arrest_{i,j,t}$	(5) $Arrest_{i,j,t}$
$Trend \times BH_i$	0.0007	0.0006	0.0006	-0.0005	-0.0004
	(0.0009)	(0.0009)	(0.0009)	(0.0030)	(0.0031)
$Trend$	-0.0007	-0.0009	-0.0009	0.0081	-0.0046
	(0.0010)	(0.0471)	(0.0471)	(0.0457)	(0.0459)
LMPD Division FE	N	Y	Y	Y	Y
Week-of-Sample FE	N	Y	Y	Y	Y
Day-of-Week FE	N	N	Y	N	Y
Post Sept. 2012 FE × BH_i	N	N	N	Y	Y
Post Sept. 2013 FE × BH_i	N	N	N	Y	Y
Post M.B. Shooting FE × BH_i	N	N	N	Y	Y
Post Grd. Jury FE × BH_i	N	N	N	Y	Y
Post Sept. 2012 FE	N	N	N	Y	Y
Post Sept. 2013 FE	N	N	N	Y	Y
Post M.B. Shooting FE	N	N	N	Y	Y
Post Grd. Jury FE	N	N	N	Y	Y
R-squared	0.0012	0.1320	0.1320	0.1333	0.1414
N	3,369	3,369	3,369	3,369	3,369

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

OLS estimates from stops where failure-to-signal was the only cited traffic violation, prior to division 5's deployment date. Robust standard errors, clustered at the division-by-year level, are reported in parentheses. BH_i equals 1 if the stopped motorist is black or Hispanic, and 0 otherwise. Each column reports three regression coefficients followed by their standard errors. Then, the sum of the column's coefficients on $BodyCam \times BH_i$ and BH_i are reported followed by the standard error for the combination. This estimates the racial disparity in arrest rate following the effect of body cams.