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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 overall arrest rate in likely pretextual stops increases 33-34%, and that the racial disparity in arrest rate becomes much smaller and statistically insignificant.

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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, 2019, 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 itself.[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] Recently, as part of a broader focus on racial disparities in the US criminal justice system, pretex-

[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).

tual stops have drawn considerable public scrutiny.[3] Yet to date, evidence 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 Louisville traffic stops spanning 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 violation or relatively small group. In conventional traffic stops, citation rates of different violations partly reflect the rates at which: motorists commit different violations, different committed violations are detected, and different detected violations are cited.[4] 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) that motorists commit 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 however, by definition, 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.

Combining 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

[3]News reports and editorials from June and July of 2020 scrutinize, allege, or report allegations of pretextual traffic stops in: Austin, TX; Bennington, VT; Berkeley, CA; Columbia, MO; Davis, CA; Dayton, OH; Delaware County, NY; the District of Columbia; Iowa; Lincoln County, MO; Los Angeles, CA; Minnesota; Oregon; Orlando, FL; San Diego, CA; South Carolina; Sparks, NV; Texas; Urbana, IL; and Youngstown, OH.

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

common violations. Yet, correlation between propensity to commit the violation and criminality could also explain such a phenomenon. Given the motive that defines pretextual stops, conditioning on whether or not multiple moving violations were cited during stop can resolve this ambiguity. If a high arrest rate given a particular violation stems from correlation of criminality with propensity to commit it; then the arrest rate should be high whether or not other moving violations are cited (because the underlying correlation is unaffected). However, a significant decrease in arrest rate conditional on citation of other moving violations would suggest the practice of pretextual stops, which—aimed at investigating suspicion of unrelated crimes—will be made as soon as *any* violation is committed (because pretext requires only one violation).[5]

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.[6] Following stops where failure-to-signal is the only cited traffic violation, the arrest rate is 0.522. Controlling for several stop-specific factors, the presence of other traffic violations reduces that arrest rate by 0.287, a 54.98 percent relative decrease. This pattern is evident within each of the eight geographically-defined LMPD patrol divisions. Per week, likely pretextual stops occurred 11.5 times and yielded 6.7 arrests on average.

Using likely pretextual stops, I then add to a growing economic literature examining racial disparities in the US criminal-justice system.[7] In testing for racial bias, pretextual stops offer appealing features. First, because they are made prior to a face-to-face inter-

[5]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 truly pretextual stop of the driver would occur before any other violation were committed.

[6]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.

[7]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.

action 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.[8] 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.[9] In addition to utilizing these appealing features, I exploit the LMPD's adoption and gradual deployment of body-worn cameras (body cams) to assess the plausibility of bias versus infra-marginality in explaining in pretextual-stop arrest-rate disparities.

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 bias. Police are permitted to conduct pretextual stops, but using race as a deciding factor in conducting them is typically prohibited. Body cams and—more importantly—LMPD policies for video storage and review, likely increase officers' expected costs of using race-dependent thresholds in initiating pretextual stops. 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 will likely persist afterward because body cams don't affect the cost of such practices. However, if disparities initially arise from race-dependent pretextual-stop thresholds, they may decrease with body cams in use.

Prior to body-cam adoption, 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

[8] 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*.

[9] In the context of police-search decisions, 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.

arrest rate for black and Hispanic motorists prior to body-cam adoption is 17.72 percent lower relative 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 frequency only slightly lower than before. 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. In light of these findings, pre-adoption disparities in pretextual-stop arrest rate are more plausibly explained by racial bias than by infra-marginality.

2 Data

Data are from the Uniform Citation dataset provided on the Louisville Open Data portal.[10] 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 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

[10]See Louisville Metro Government (2019).

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 that specializes in policing illegal weapons. It should be noted that the ninth division conducted the controversial stop referenced in the opening paragraph. Stops conducted by the ninth division are not reported in the data however.[11]

Among traffic citations, I group similar charges to identify 19 common violations.[12] Of the 469,705 traffic citations issued by the geographically-defined patrol divisions, 465,080 belong to one of these common traffic violations. The remaining 4,625 citations involve traffic-related charges that are relatively obscure.

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.*, operating a vehicle with an expired license plate or a defective brake light).

I define objective driving violations as those whose commission can be established without the exercise of individual 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 can't be established without some judgment from an 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 and prudent, having regard for the speed of the vehicle and the traffic upon and condition of the highway." Commission of this violation can't be established without an individual judgment as to what is reason-

[11]This will likely cause my analyses to understate the true prevalence of pretextual stops within Louisville.

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

able or 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, my tests for evidence 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 body cams had been fully deployed. Reportedly, 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 seem 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 noteworthy that the impetus for body-cam adoption was a highly publicized incident in another city and state. Thus, the timing of body-cam adoption and 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 particularly inconvenient, and negative word-of-mouth might soften officer support for the program 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.

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 to 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. Presumably, intoxicated drivers 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; hence, the exclusion.

Among the common traffic violations that were present in at least 2 percent of all stops, Appendix Table A1 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 (more than double the arrest rate conditional on reckless driving, which perhaps more plausibly relates to suspicion of other crimes); and seems especially high when compared to the arrests rates following other common objective driving violations which range from 0.0087 to 0.0859.

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 variable indicating that traffic stop i resulted in an arrest. $Multiple_i$ is a binary variable indicating that more than one traffic violation was cited in stop i. I test the null hypothesis, $\alpha_1 = 0$. 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.

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 one violation alone provides sufficient pretext for a stop. Among the 8,641 stops in the estimating sample that cite failure-to-signal, 2,885 involved more than one traffic violation. Appendix Table A2 reports the frequency and percent frequency at which other traffic violations were cited within those stops.

4.2 Results

Table 3 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 might be expected in conventional stops. Yet, because these simple comparisons 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.

Table 4 presents ordinary least squares (OLS) estimates of equation (1) under the full specification.[13] The estimate in column (1) comes from stops involving failure to signal, and estimates in columns (2), (3), and (4) are from stops involving other common driving violations for the purposes of comparison. In column (1), the coefficient on *Multiple* again contradicts correlation between criminality and propensity to commit failure-to-signal. Relative to a predicted arrest rate of 0.5217 when failure-to-signal is the only traffic violation cited, the coefficient in column reflects a 55.01 percent decrease in the arrest rate conditional on $Multiple_i = 1$. In columns (2), (3), and (4) however, the presence of multiple traffic violations is associated with (if anything) higher arrest rates.

Restricting attention to stops involving failure-to-signal, Appendix Table A3 reports estimates of equation (1) under an alternative specification where division fixed effects are replaced 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.

[13] Standard errors are clustered at the division-by-year level, and reported in parentheses.

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 each division's interaction term.

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 of 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 used 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 that 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 about 20.45% of the stops where failure-to-signal was the only cited violation and arrest occurred were conventional.[14] 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 patrol division, 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 pretextual stops and conventional moving-violation traffic stops are conducted within each of the LMPD patrol divisions, and reveals significantly disproportional use of pretextual stops in particular areas of the city.[15] Notably, Division 2 accounts for more likely pretextual stops *and* fewer conventional traffic 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, Body-Worn Cameras, and the Use of Pretextual Stops

Economic studies of racial disparities in traffic policing have typically focused on either stop decisions in general, or post-stop search decisions in general. In studying post-stop search decisions, 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 things that researchers cannot. Moreover, if race correlates with

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

[15]Likely pretextual stops are those in which failure-to-signal is the only cited traffic violation. In these frequencies, all traffic stops—excluding those that are likely pretextual—which include a citation for at least one moving violation are treated as conventional.

propensity to carry contraband, optimal search decisions may produce disparate search rates—what Arrow (1973) termed *statistical discrimination*.

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*). In practice however, 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.

By nature, pretextual stops are advantageous in overcoming these challenges. Following conventional traffic stops, a face-to-face interaction with the motorist informs the officer's search decision, potentially introducing problematic omitted variables. The pretextual stop however, is one in which desire to stop and search 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 such factors are not part of the search decision.

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 (were there greater suspicion, the pretext of a traffic violation would be unnecessary). 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 helpful features, I use the adoption and staggered deployment of body cams across LMPD patrol divisions to further circumvent 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 referenced earlier documents disparities in outcomes among 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.[16] Given the disproportionate representation (relative to conventional traffic stops) of black motorists in likely pretextual stops, a natural question is how productive these stops were 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

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

[16] 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 (non-BH), estimates in column (1) predict an arrest rate of 0.4646 in likely pretextual stops. Relative to 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 racial disparities in arrest rate conditional on a likely pretextual stop. 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. Whatever the source of the disparity, that result itself is meaningful because pretextual traffic stops impose significant costs on the motorists involved. Still, 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, non-BH motorists are more likely to have committed arrestable offenses.

The adoption and deployment of body cams can shed light on the plausibility of these competing explanations. Patrol officers are permitted to use failure-to-signal as a basis for pretextual traffic stops; but they are not supposed to base these stop decisions on driver race. If the initial disparity results from infra-marginality, we wouldn't expect the deployment of body cams (and the storage and review policies for body-cam footage) to significantly affect it. However, if the disparity results from different race-dependent thresholds for pretextual stops, the heightened accountability that body cams bring about may lead to its reduction.

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

Before turning to arrest rates, I assess whether body cams affected the frequency of likely pretextual stops. My full sample is composed of 502 full weeks, indexed by w.[17] Let $\mathrm{Y}_{j,w}$ 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 effects" (TWFE) specification:

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

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

$BodyCam_{j,w}$ 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.[18] Attributing $\hat{\gamma}_1$ to the effect of body cams assumes parallel trends in all of these different counterfactual outcomes.

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. Full-sample residuals from this regression are then used as the dependent variable to re-estimate equation (4). The estimate from this approach, is reported in column (2) of Table 9. Results of the regression producing these residuals are reported in Appendix Table A4, 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 TWFE 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

[18]In later periods, this will include comparisons of divisions where body cams are deployed, with divisions where body cams were already deployed.

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 year f about body-cam deployment, minus the conditional expectation of Y in each division's final year before body-cam deployment.

In addition to estimating equation (5) as specified, I also include controls for four separate points events which might have affected police practices. 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 they solicited prices from body-cam vendors in the fall of 2013. Because both actions might signal greater scrutiny of patrol decisions, I include an indicator equal to 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 relating to the officer-involved shooting of Michael Brown in Ferguson, Missouri. Schaefer et al. (2017) suggest that media attention to the incident shifted public interest in body cams within Louisville, and led to the city funding their adoption. Because these changes may have immediately influenced police practices, 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. The grand-jury decision not to indict the officer involved was announced on November 24, and followed by several days of protest.

Estimates of equation (5) as specified, are reported in Table 10. Column (2) reports estimates with controls for the four events just discussed. 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 of common trends in the time periods leading to body-cam deployment. Both suggest an initial statistically-significant, but short-lived, decrease in pretextual-stop frequency following body-cam deployment. Appendix Figure A1 presents the coefficient and 95-percent confidence intervals from column (3) of this table. Ultimately, body cams appear to have, at most, a modest and fleeting effect 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. However, the effect of body cams on pretextual-stop arrest rates can be assessed at traffic-stop level. I begin by estimating a TWFE specification,

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

$Arrest_{i,j,t}$ is a binary variable indicating that stop i, made in division j on date t, resulted in arrest. 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 that required installation of new fiber optic cables. Appendix Table A5 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 while limiting the post-deployment period to dates where stops, with and without body cams in use, could be contemporaneously observed. These estimates are very similar in sign, significance, and magnitude to full-sample estimates.

Appendix Table A6 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, 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, $f = 0$.[19] 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 in division j on date t, 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 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 division fixed effects, and controls for the four events discussed in section 5.2. In column (2), day-of-week

[19] I use 364-day periods for balance. These "years" are composed of exactly 52 weeks.

fixed effects are also included. In column (3), division-specific linear annual trends are added to account for any division-level differences in trend that might otherwise confound estimation. Estimates across all three specifications are quite similar. They suggest a 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. Disaggregating this increase by driver race may 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:

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

Conditional on division and week-of-sample fixed effects: ρ_3 is the difference in expected arrest rate between black or Hispanic (BH) 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 (non-BH); and $(\rho_1 + \rho_2)$ is the difference in expected arrest rate from pre-deployment to post-deployment for a BH motorist. Finally, ρ_1 is the difference in body cams' effects on expected pretextual-stop arrest rate between BH and non-BH motorists.

Estimates of equation (8) are reported in Table 13. Across all four specifications we see a significant increase in pretextual-stop arrest rate among non-BH motorists following the deployment of body cams. In column (1), notice that the pretextual-stop arrest rate is lower among BH motorists by 0.0843 prior to division 5's deployment date.[20] Beyond the rise of 0.1194 exhibited among non-BH motorists, the arrest rate among BH 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 weekday of a stop, results remain very similar. In the other two specifications, the additional increase among BH motorists is slightly smaller, yet overcomes 61.8 to 64.18 percent of the initial disparity. While this additional increase—beyond the increase exhibited by non-BH motorists—is not significantly different from 0 at conventional levels, the absolute increase in pretextual-stop arrest rate among BH motorists is. Across columns (1) through (4), the effect of body cams on pretextual-stop arrest rate among BH motorists 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 top three coefficients and their standard errors, Table 13 reports the sum, $(\hat{\rho}_1 + \hat{\rho}_3)$, and its standard error. This estimates the arrest-rate disparity between BH and non-BH motorists conditional on included controls. In columns (3) and (4), controls for the four aforementioned events and their interaction with the BH indicator are added. This prevents any change in the disparity observed between the first of these events (October 1, 2012) and body-cam deployment from being reflected in the post-deployment disparity. 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 component of that disparity remaining 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), the remaining disparity is larger. When restricting the estimating sample to prior to division 6's deployment date, Table A8 shows estimates that are similar in magnitude.

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

A potential concern regarding these estimates is that, prior to body-cam deployment, the pretextual-stop arrest rate among BH motorists may have been on an upward trend relative to non-BH motorists. If so, persistence of this pre-deployment difference in trends may be mistakenly attributed to 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 Appendix Table A9. All fail to reject a common pre-trend between the two groups of motorists at conventional significance levels.

5.5 Racial Disparities and Visibility of Race

Grogger and Ridgeway (2006) developed the "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.[21] Here, I borrow on the underlying intuition of Grogger and Ridgeway (2006), and 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 estimating sample to stops made after the beginning (in the morning), and before the end (in the evening), of civil twilight on each date. These estimates are reported in Table 14.

After excluding stops made during darkness, body cams have a much smaller effect on pretextual-stop arrest rate among non-BH motorists. In stops made during daylight, all four specifications reveal even larger initial racial disparities. Moreover, across all four specifications, the use of body cams has an additional effect on pretextual-stop arrest

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

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.

6 Concluding Remarks

In 1996, the Supreme Court of the United States held by unanimous decision that pretextual traffic stops do not violate the Fourth Amendment's protection against unreasonable search and seizure. Even if a reasonable officer would not make the stop absent some additional law enforcement objective, probable cause to believe that a motorist has violated a traffic law is sufficient for his temporary detention. More recently, amid growing attention to racial disparities in various realms of the US criminal justice system, the practice of pretextual traffic stops has drawn elevated scrutiny. Yet, because pretextual stops are by defined officers' motives, detecting empirical evidence of their practice is understandably challenging. Despite that difficulty, I demonstrate that if pretextual stops are sufficiently frequent, evidence of their practice may be detectable in traffic stop data.

An officer who wishes to stop and investigate a motorist on suspicion of a crime unrelated to driving, will do so as soon as that motorist commits any traffic violation providing pretext. As such, pretextual traffic stops should concentrate around whatever violation motorists are most likely to commit; should carry relatively high arrest rate (if underlying suspicions are somewhat well-founded); and should typically involve *only one* traffic citation—pretextual stops will be made before the motorist has a chance to commit a second traffic violation.

Analyzing Louisville Metro Police Department (LMPD) traffic citations issued from January 2010 to August 2019, I find that failure-to-signal—one of the four most commonly cited driving violations—was frequently used as a basis for pretextual traffic stops. I also find evidence suggesting that pretextual stops were fairly common and used in a manner yielding racial disparities. Prior to the LMPD's initial deployment of body cams in June 2015, though representing only 29.20 percent of conventional traffic stops, black and Hispanic motorists account for 49.33 percent of likely pretextual stops. Yet, even after controlling for numerous stop-specific factors, likely pretextual stops led to arrests of black and Hispanic motorists at a significantly lower rate than other motorists.

Exploiting the LMPD's staggered division-level deployment of body cams, I assess the plausibility of bias versus infra-marginality in explaining that initial disparity in arrest rate. Following body-cam deployment, the overall arrest rate in likely pretextual stops increased significantly, and the racial disparity in arrest rate observed prior to body-cam deployment was reduced anywhere from 62 to 84 percent. Rather than infra-marginality, these findings are more consistent with the initial disparities stemming from racial bias in the use of pretextual stops.

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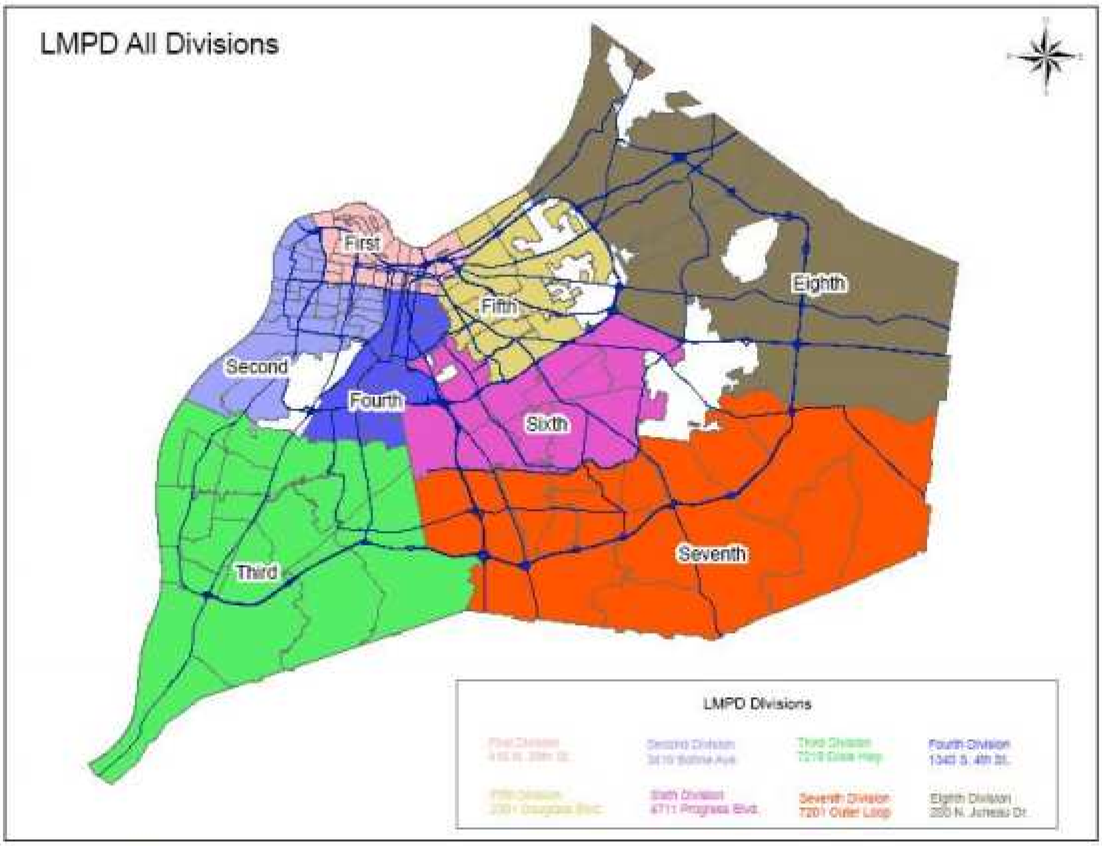

Figure 1: LMPD Patrol Divisions

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.

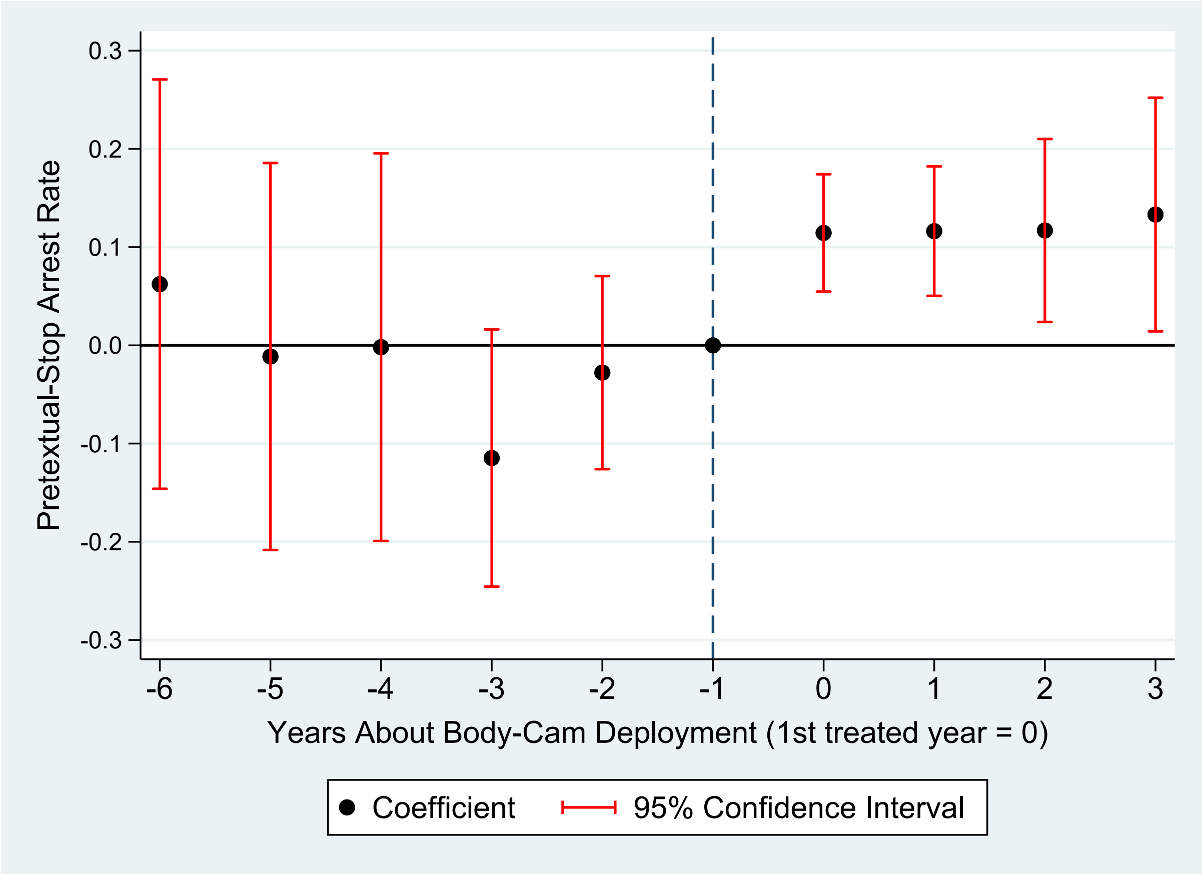

Figure 2: Event Study: Pretextual-Stop Arrest Rate and Body-Cam Deployment

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. 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. 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 $Multiple_i$. 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 Traffic 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 traffic stops are those that: (1) result in the citation of at least one traffic 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. 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. 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. 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 Appendix Table A4.

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. 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. 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. 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 × 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	0.1528	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. 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. 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

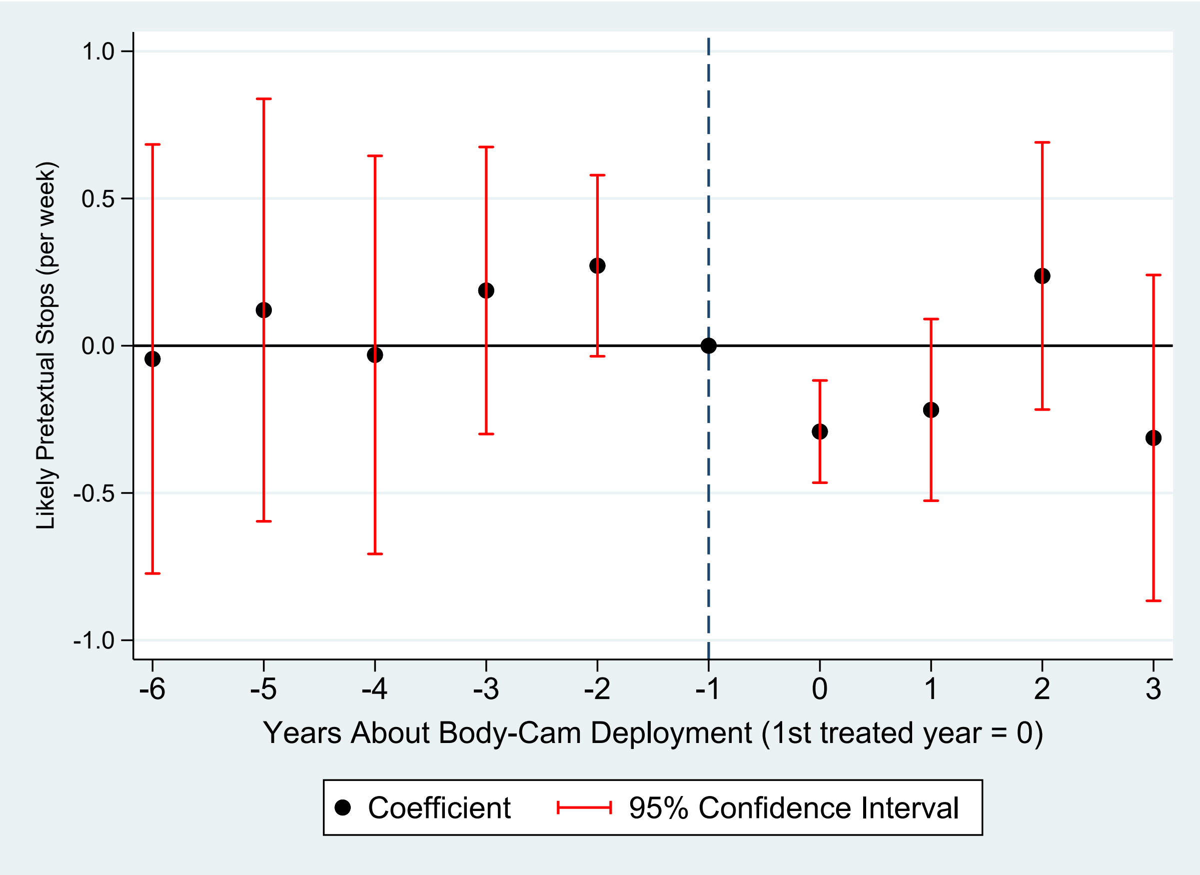

Figure A1: Event Study: Pretextual Stop Frequency and Body-Cam Deployment

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. 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. 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). 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. 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. 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 $\times$ 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	0.1413	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. 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 $\times$ BH_i	N	N	N	Y	Y
Post Sept. 2013 FE $\times$ BH_i	N	N	N	Y	Y
Post M.B. Shooting FE $\times$ BH_i	N	N	N	Y	Y
Post Grd. Jury FE $\times$ 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. 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.