Does Race Matter for Police Use of Force? Evidence from 911 Calls

Mark Hoekstra and CarlyWill Sloan*

August 10, 2020

Abstract

While there is much concern about the role of race in police use of force, identifying causal effects is difficult. This is in part because of selection, and in part because researchers typically observe only interactions that end in use of force, necessitating nontrivial benchmarking assumptions. This paper addresses these problems by using data on officers dispatched to over two million 911 calls in two cities. Importantly, neither city allows for discretion in the dispatch process, which generates random variation in the race of the officer dispatched. Results indicate that white officers use force 60 percent more on average than black officers, and use force with a gun twice as often. To examine how civilian race affects use of force, we compare how white officers increase use of force as they are dispatched to more minority neighborhoods, compared to minority officers. Perhaps most strikingly, we show that while white and black officers use force with a gun at similar rates in white and racially mixed neighborhoods, white officers are five times as likely to use gun force in predominantly black neighborhoods. Similarly, white officers increase use of any force much more than minority officers when dispatched to more minority neighborhoods. Consequently, difference-in-differences estimates indicate black (Hispanic) civilians are 30 – 60 (75 – 120) percent more likely to experience any use of force, and five times as likely to experience use of force with a gun, compared to if white officers scaled up force similarly to minority officers. Estimates are robust to various neighborhood, time, and neighborhood-by-time fixed effects, individual officer fixed effects, and randomization inference. These findings highlight the importance of race as a determinant of police use of force, including and especially lethal force.

*Sloan: Claremont Graduate University, carlywill.sloan@cgu.edu. Hoekstra: Texas A&M University, NBER, and IZA, markhoekstra@tamu.edu
Acknowledgements: We would like to thank the police departments in two anonymous cities for providing the data. We would also like to thank multiple officials in those cities for helpfully answering our numerous questions. We also thank Scott Carrell, Jan Feld, Felipe Goncalves, Benjamin Hansen, Samuel Myers, Jr., Isabelle Sin, James West, seminar participants at the University of Florida, Georgia Tech, University of Mississippi, Mississippi State University, and conference participants at the 2019 Stata Texas Empirical Microeconomics Conference, the Victoria University of Wellington Applied Econometrics Workshop, the 2019 SEA Annual Conference, the 2019 ASSA Annual Conference, and the 2020 NBER Summer Institute for helpful comments and suggestions. Any errors are our own.

1 Introduction

There are widespread concerns regarding police officer treatment of minorities. These concerns are rooted in a long history of police mistreatment of black Americans, and are reflected by the fact more black civilians report having "no confidence" in local police (24 percent) than have "a lot of confidence" (14 percent). This distrust of police is perhaps strongest with respect to police use of force, as only 33 percent of black civilians believe officers use the right amount of force for the situation, and only 35 percent believe police treat racial and ethnic groups equally (Pew Research Center, 2016). Concerns over the role of race in police use of force have been voiced most forcefully in the protests over police shootings of unarmed black civilians and by the Black Lives Matter movement. Most recently, this movement has grown exponentially in the aftermath of the George Floyd murder, with more than 1,700 demonstrations across all 50 states (Haseman et al., 2020). Importantly, this distrust has important efficiency implications in that reduced civilian cooperation likely leads to less effective policing and higher social costs of crime.

However, documenting whether race matters for police use of force is difficult. This is in part because researchers often do not observe interactions in which force was not used. As a result, researchers must make assumptions regarding the appropriate "benchmark", such as violent crime rates or arrests. In addition, it is difficult for researchers to observe whether the underlying risk of situations involving white and minority civilians, or white and minority officers, is similar in terms of whether force was merited. It is at best unclear whether controlling for observed contextual factors is sufficient to overcome bias due to selection. This is in part because officers almost always observe factors not recorded in the data, in part because much of what is in the data is recorded after force was used, and in part because recorded characteristics of the encounter could themselves be impacted by race.

In this paper, we use a fundamentally different approach we believe most closely mirrors the ideal thought experiment. Specifically, we observe settings in which white and minority officers are as-good-as-randomly sent to otherwise similar situations, and where the same black and white officers are observed responding to situations in white and minority neighborhoods. We do this using administrative data on over two million 911 calls, which enables us to observe police use of force for a defined set of interactions, independent of whether the interaction involved use of force. Importantly, the data come from two cities in which the dispatch protocols allow for no discretion on the part of the officer or the operator with respect to which officer is dispatched. Rather, in the first city, the dispatcher observes on the computer screen whether the officer on duty for that beat is available and if so, dispatches that officer. If the dispatcher's monitor indicates the beat officer is unavailable—which happens if, for example, the beat officer is currently engaged in a traffic stop—then she dispatches the available officer observed to be closest to the call's location. The protocol for the second city requires that the operator dispatch the available officer who is closest geographically to the location. Both protocols imply that conditional on police beat and time fixed effects, the variation in the race of the officer dispatched is as good as random. Our interviews with dispatchers indicate they follow the protocol. We also show empirically that officer race in both cities is uncorrelated with exogenous call characteristics and with predicted use of force based on those covariates.

We use this exogenous variation in police officer race to answer two questions. The first is whether white officers use force at higher rates than minority officers when responding to otherwise similar calls. The second is whether officers are more likely to use force on opposite-race civilians. We answer this second question by asking whether white officers increase their use of force more then minority officers as they are dispatched to more-minority neighborhoods. We do so in a difference-in-differences style framework similar to the Price and Wolfers (2010) study on racial bias by NBA referees, and show robustness of our estimates to varying combinations of neighborhood, time, and individual officer fixed effects. The advantage of this approach is we avoid imposing selection-on-observables assumptions in comparing interactions across civilian race. Rather, we assume that in the absence of an opposite-race effect, white and minority officers should increase use of force by a similar amount as they are dispatched from white to minority neighborhoods. The limitation is that as in all police use of force studies, we do not observe the proper amount of force. As a result, our measure of whether civilian race matters is necessarily a relative one: how would black civilians have fared if the white officers had scaled up their use of force the same as black officers?

Our data include the universe of 911 calls made in two cities linked to police officer race and use of force. As a condition of acquiring the data, we cannot disclose the names of the cities. The first city has a population and police force composed primarily of white and black officers. It has a population of more than 240,000 and a homicide rate that ranks in the top 20 among the nation‘s largest 100 cities. In this city, we have administrative records on over 1.2 million 911 calls over a three to five year period starting after 2010.[1] These calls resulted in 1,300 police uses of force, 94 of which involved the discharge of an officer's gun. The data include the time and date of the call, the priority score assigned to the call by the 911 operator, a short description of the call, the first officer(s) dispatched to the scene by the operator, and whether or not force was used (and by which officer) at the scene. Importantly, we observe the first officer(s) dispatched to the scene, even if other officers also arrive after the dispatched officer arrives. In addition, we observe the address from which the call originated, which we geocode into a Census Block Group to assign civilian race. In the second city, we have data on just under one million 911 calls. This city is composed primarily of white and Hispanic civilians and police officers. This city has a population of more than 150,000, and the call records include similar information. There are just under 3,000 incidents of police use of force linked to these calls. We do not observe the type of force used in this city.

Results indicate that white officers use force 60 percent more often than black officers on average, and use force with a gun more than twice as often. In both cases estimates are highly significant, and demonstrate the difference in propensity to use force between black and white officers. We also estimate (shrunken) random effects for individual officers, which show that the overall differences by officer race are driven by a shift in the distribution, rather than differences in the tails. This suggests that the type of white person attracted to the police force is systematically different than the typical black person when it comes to likelihood of using force. In contrast, we find no evidence of an overall difference in the second city, where white and Hispanic officers use force at the same rate, on average.

Importantly, police use of force in both cities varies systematically by civilian race. Perhaps most strikingly, we show that while white and black officers use gun force at approximately the same rate in white and racially mixed neighborhoods, white officers use force with a gun five times as often in neighborhoods that are 80+ percent black. That is, while black officers use force with a gun at most modestly more when they are dispatched to calls

[1] We do not report the exact years to protect the anonymity of the city.

in more black neighborhoods, white officers use gun force much more often when they are dispatched to predominantly black neighborhoods. As a result, ordinary least squares (OLS) estimates indicate dispatching a white officer to a call in a black neighborhood increases the use of gun force by three to five times; logit estimates are even larger. In addition, we find a similar pattern for all use of force. Results indicate that dispatching an opposite-race officer increases use of force by 30 to 60 percent, though only OLS estimates are significant at conventional levels. These estimates are driven by the fact that white officers increase their use of force significantly as they are dispatched to calls in more black neighborhoods. In contrast, black officers use force at most modestly more as they are dispatched to calls in more black neighborhoods.

Results from the second city indicate even though white and Hispanic officers use force at the same overall rate, use of force is disproportionately concentrated in different-race neighborhoods. Specifically, we show that white officers increase their use of force more when dispatched to more Hispanic neighborhoods, compared to Hispanic officers. Estimates indicate that dispatching an officer of a different race roughly doubles the likelihood that force will be used.

For both cities, we show that these results are evident in the raw data and corresponding unconditional estimates, and are also robust to including beat, time, and beat-by-time fixed effects as well as call characteristics. In addition, we show that the estimates and statistical significance of the difference-in-differences estimates are robust to the inclusion of individual officer fixed effects, though we note these estimates for the second (white/Hispanic) city are significant at the 10 percent level. We also show that the officer race effects (e.g., white vs. black) are robust to controlling for whether this is the officer‘s home beat, as proxied by the beat to which he is most frequently dispatched. Similarly, difference-in-differences estimates are shown to be robust to the inclusion of home beat and home beat interacted with officer race, in addition to individual officer fixed effects. This suggests effects are not driven by a familiarity effect. We also examine the extent to which our opposite-race results can be explained by the inclusion of all observed call characteristics interacted with officer race, and find such effects could explain only a small fraction of our findings. Finally, we show estimates in the first city are robust to whether we define use of force at the call level (i.e., assign use of force to the dispatched officer even if another officer was the one to use force) or at the level of the officer who used the force.

We also perform randomization inference and show that estimates of this magnitude are unlikely to occur due to chance in data sets similar to the actual data, where (by construction) incidents of use of force occur with identical frequency. In addition, we show the estimates on use of force with a gun are robust to dropping any one of those incidents from the data.

The main contribution of this paper is to overcome the benchmarking problem by using 911 calls, and to overcome the selection problem by studying contexts where white and minority officers are exogenously assigned to calls in white and minority neighborhoods. In doing so, this paper joins a larger literature examining the impact of race in the criminal justice system. It is related to work on racial bias in police vehicle searches (e.g., Anwar and Fang, 2006; Persico and Todd, 2006; Antonovics and Knight, 2009). To address the issue of officer selection into interactions with civilians, this literature tests for racial bias by modeling police behavior and implementing tests based on vehicle search "hit rates" relative to a benchmark encounter rate. A related literature addresses the difficulty of assessing the benchmark encounter rate by exploiting changes in ambient light to test for racial profiling in traffic stops (Grogger and Ridgeway, 2006; Horrace and Rohlin, 2016). This paper complements these literatures by taking a substantively different approach to solving problems created by endogenous police-civilian interactions. In doing so, this paper is more closely related to work by Weisburst (2017), who uses 911 calls to estimate the value-added of individual police officers, and West (2018), who tests for racial bias in traffic citations using conditionally-random variation in the race of officers called to traffic accidents. The advantage of this paper relative to West (2018) is we examine the impact of race in arguably a more important context with more important outcomes—911 calls and police use of force. The advantage of West (2018) is he has objective information on whether certain citations were merited, which is not possible for use of force. In addition, this paper also complements research on racial bias in the criminal justice system more generally, including racial bias by prosecutors (Sloan, 2019; Tuttle, 2019), juries (Anwar, Bayer, and Hjalmarsson, 2012; Flanagan, 2018) and judges (Arnold, Dobbie, and Yang, 2018; Bielen, Marneffe, and Mocan, 2018).[2]

[2]More generally, it also complements a broader literature on own-group bias in education, labor, housing, and product markets (e.g., Feld, Slamanca, and Hamermesh, 2015; Ayres and Siegelman, 1995; Dahl and Moretti, 2008; Goldin and Rouse, 2000; Lavy, 2008; Neumark, Bank, and van Nort, 1996; Moss-Racusin, Corinne, Dovidio, Brescoll, Graham, and Handelsman, 2012; Price and Wolfers, 2010; Parsons, Sulaeman, Yates, and Hamermesh, 2011.)

In assessing the effect of officer and civilian race on use of force, this paper is most similar to work by Fryer (forthcoming) and Johnson et al. (2019). Fryer (forthcoming) uses an impressive range of data sets including detailed data on all interactions from Stop and Frisk in New York City. In addition, he uses data on officer-involved shootings in several other cities and counties, which he benchmarks using arrests. Using these data, he implements a selection-on-observables design to control for contextual factors. He concludes that blacks and Hispanics are more likely to experience non-lethal force all else equal, but not more likely to experience an officer-involved shooting. Weisburst (2019) extends this work using data on use of force and arrests from Dallas, and similarly reports that conditional on arrest there is no racial difference in use of force. Johnson et al. (2019) use data on fatal officer-involved shootings across counties and conclude there is little evidence of bias, though others have criticized the underlying assumptions in the analysis (Knox and Mummolo, 2019; Knox, Lowe, and Mummolo, 2019). The main advantage of this paper relative to this prior work is we are able to estimate the effect of race in a context where black and white officers are as-good-as-randomly dispatched to similar situations, and where each officer is as-good-as-randomly dispatched to calls in more and less black neighborhoods. In this way, we avoid making potentially problematic assumptions about the appropriate benchmark. In addition, by assessing the impact of civilian race by comparing how white and black officers scale up use of force as they are dispatched from white to black neighborhoods, we avoid concerns about whether we control for enough contextual factors that may differ across civilian race. Similarly, we avoid potentially over-controlling for factors described by the officer after force was used (e.g., in an arrest report), which would lead to understating effects. The limitations of our approach are that our difference-in-differences estimate of whether race matters is necessarily a relative one, and we address this question in only two cities.

Our results have important implications for policing in the United States. Perhaps most importantly, they provide rigorous evidence in support of the common civilian perception of that race is an important determinant of police use of force. The results of this paper suggest that at least in the contexts studied here this belief seems warranted, especially with respect to the use of lethal force. In addition, this study demonstrates that race matters even in a time and context during which police departments generally, and white officers in particular, know they are under close scrutiny by the media and the public.

2 Background and Data

2.1 Background and Dispatch Procedures

As noted above, the protocol for dispatching officers to the scene of 911 calls is critical for our research design. For this reason, we contacted police departments in more than a dozen cities inquiring about their system for dispatching officers to calls, as well as the availability of data. In particular, we needed to be able to observe and link the race of the police officer to 911 calls and use of force. We were able to obtain data in two cities that met both criteria.[3] As part of the agreement to obtain the necessary data, which includes officer identifiers, we were required not to disclose the names of the cities. However, we can state that the first city we study has large black and white populations, a total population of over 240,000 and has a homicide rate that ranks in the top 20 among the nation's 100 largest cities. We note this set of cities does not overlap with those studied by Fryer (2019) and Weisburst (2019), none of which have a homicide rate that ranks in the top 20 among the nations's largest cities.

In this city, a civilian's 911 call is given to the first dispatcher available. The dispatcher then records important aspects of the call and assigns the call to a primary unit. Specifically, the computer system used by dispatchers records the time, exact location, and police beat of the call. The dispatcher will then ask the caller about details surrounding the call, categorize the seriousness and urgency of the call and rate it from highest priority (1) to low priority (higher values). The dispatcher also records a short description of the call. For example, a dispatcher may record a call as a "domestic disturbance" and then assign it priority of 2. Calls are then dispatched based on the priority of the call. This means more urgent calls, like assaults or crimes in progress, will be dispatched first, while less serious calls, like stolen cars, will be given lower priority and dispatched later.

After recording the above aspects of the call, the dispatcher assigns a primary unit to the call. The majority of calls (98 percent) in the first city are assigned only one primary unit. In 1.2 percent of calls, two primary units are dispatched, while in the remaining calls there are three to five primary units. To dispatch a primary unit, the police dispatcher will

[3] We also obtained data from a third city that we were told used a protocol in which officers and operators had no discretion. However, upon receiving the data we discovered that did not appear to be the case, and thus do not use that city in this analysis.

refer to her computer screen, which displays the location of all available police officers. An officer will not be available if they are responding to another call for service or a self-initiated event. For example, if an officer makes a traffic stop, they will use their in-car computer to communicate that they are not available, in which case they will not appear on the police dispatcher's screen for that period of time. If the beat officer for the beat of the call is available, then the call will be dispatched to that unit.[4] If the beat officer is not available, the closest officer (geographically) will be dispatched. Importantly, in this setting, officers do not select the calls to which they respond.[5] After the primary unit is dispatched, other officers may observe the call on their police car computer and respond to the call. We do not observe these officers in our data, and assign only the primary dispatched officer(s) to each call. In this way, we perform an intent-to-treat analysis. Given the dispatch procedure, we need to at most condition on police beat-by-time fixed effects to isolate as good as random variation in police officer race. In Section 4, we show empirical evidence consistent with this identifying assumption.

Once a primary unit is dispatched to a call they may encounter a situation that leads to use of force. If an officer uses any type of force, police department administrative procedure dictates they must immediately file a report describing the details of the incident and the use of force type. This report number will be recorded in the officer in-car computer and linked to the call for service where the use of force occurred. Even if the use of force report is made later—for example, after the officer has been dispatched to another call—we are still able to link this use of force to the call for service using the police report number. A use of force report must be recorded even in events where non-deadly force (punches/kicks, etc.) is used. In the event an officer discharges his gun, he must allow the ranking officer on the scene to inspect the weapon and issue replacement ammunition. If an officer has shot someone, a detective or internal affairs investigator will take possession of the weapon. All use of force reports are reviewed quarterly by a community use of force committee, which makes recommendations about the use of force policy to the Chief of Police.

We also study use of force in a second city, where the population is more than 95 percent Hispanic or white, and less than 10 percent black. This city has a population of more than

[4] For each shift a beat is assigned a beat officer. If a beat officer is not on a call for service, they are expected to patrol their beat. This city has over 50 beats.

[5] These data do not include officer-initiated incidents where officers observe an incident, call it in, and have the dispatcher assign them to that incident.

150,000, which ranks in the largest 300 cities in the country. The protocol for dispatching officers to calls is similar to the city described above in that calls are dispatched according to a protocol that does not allow for discretion on the part of the operator or officer. However, in this city, the operator first dispatches the geographically closest available officer to the call. In addition, in this city it is more common to have more than one officer initially dispatched. Specifically, 44% of calls have one unit assigned, 31% have two units assigned, and 25% have three or more units assigned initially. As with the first city, after any use of force, department procedure dictates that the officer record the incident report electronically, and that their supervisor review the report.

2.2 Data

The police administrative dataset for the first city includes all calls for service from a three to five year period after 2010 where at least one officer was dispatched. For each call in our dataset, we observe the primary unit, beat, priority, time between call and dispatch, latitude, longitude, time of the call, time of dispatch, and date of the call. There are over 50 beats in our city.[6] We also observe the race, gender, and years of experience of each police officer. Because Hispanics make up less than five percent of police officers in our city, we exclude those officers from the sample. Additionally, we observe if the call resulted in use of force and the type of use of force. Type of force is recorded by the police officer, including whether the force involved the firing of a gun by the officer. We classify use of force in two different ways, and show results for each. The first is to classify all officers in the assigned primary unit(s) as having used force if any officer assigned to the call used force. For example, if two officers are dispatched to the same event, but only one uses force, in this approach we assign both officers as having used force. We use this event-level assignment procedure in order to account for the joint decision-making process of responding officers. For example, one officer could fail to deescalate a situation, causing another responding officer to use force. The second way we classify use of force is to assign it at the officer level, in which an officer is only assigned use of force if that officer used force. Results from these two analyses are nearly identical. We report results where force is defined at the call level in our main analysis, and report results where force is defined at the officer level in Appendix Tables A1 and A2.

[6] In order to protect the anonymity of the cities, we are not revealing the exact time period or number of beats.

In the first city, we define civilian race as the proportion of the population that is black from the Census Block Group from which the call originated. We do so using the 2010 Census for each of the several hundred different Census Block Groups from which calls in our city originated.[7] In the second city, we assign civilian race as the proportion of the population that is minority (e.g., black or Hispanic), noting that less than five percent of the population in that city is black. Importantly, we note that any measurement error generated by proxying civilian race with neighborhood race should be uncorrelated with the race of the police officer, given how the dispatch process works.[8]

As with many cities in the U.S., there is significant sorting by race across neighborhoods. For example, in the first city about 35 percent of our Census Block Groups are more than 75 percent black and about 25 percent of our census block groups are more than 75 percent white. This is also evident in the distribution of 911 calls. This distribution is shown in Figure 1, where proportion black for the Census Block Group of the originating call is shown on the x-axis and varies from 0 to 1. It is clear that while there are calls originating from all types of neighborhoods, we have a significant number of calls originating from nearly-all-white or (especially) nearly-all-black neighborhoods. Panels b and c of Figure 1 shows that black and white officers are dispatched to both types of neighborhoods, as well as neighborhoods of mixed race.

Summary statistics for the first city are shown in Table 1. The sample includes 1.2 million calls for service. There were over 1,300 incidents of police use of force representing 0.109 percent of all calls, of which 94 (0.0076 percent) involved a gun. Overall, 7 percent of use of force involved a gun; 38 percent involved a taser, and the remaining 55 percent was grouped into a category that included use of hands, feet, mace, baton, etc. to subdue the civilian. Thirty-eight percent of responding officers were black, and 16 percent were female. Average officer experience was 10 years. On average 58.6 percent of callers were black. It takes 6.5 minutes for a primary unit to be dispatched to a call.

In columns 2 and 3 we show summary statistics separately for black and white responding officers, respectively. We note that this comparison does not reflect our research design since

[7]We classify civilians as black if they are only black, and white if they are only white. This results in the classification of over 90 percent of this city's citizens as only black or only white. Hispanics make up less than 5 percent of the population in this city.

[8]In addition, the type of measurement error we believe to be most likely would lead us to understate effects. For example, if it were true that some civilians with whom police interacted even in all white neighborhoods were black, then our estimates would need to be scaled up to capture the actual change in civilian race across neighborhoods.

it does not account for potential officer selection by race into different police beats. However, given these data only include use of force resulting from 911 calls, and since officers of both races respond to calls in all neighborhoods at roughly the same rates (as shown in Figure 1) and have no discretion in how they are dispatched, we find it instructive nonetheless. Columns 2 and 3 show that black officers are somewhat more likely to be female (18.9 versus 14.2 percent), have slightly more experience (10.5 versus 9.9 years), and respond to calls in slightly more black neighborhoods (0.603 versus 0.575). Black and white officers respond to calls of similar priority and are dispatched similarly quickly (6.51 versus 6.46 minutes) and to locations with similar x and y coordinates. However, use of force is quite different across officer race. While black officers use force 7.8 out of every 10,000 calls, white officers use force 12.8 times per 10,000 calls. White officers use gun force approximately 1 out of every 10,000 calls, which is more than twice as often as black officers.

Summary statistics for the second city are shown in Appendix Table A4. Eighty-five percent of civilians are minority, and 81% are Hispanic. The police force reflects these demographics, as 86 and 83 percent are minority and Hispanic, respectively. We observe just under 3,000 incidents of force linked to 911 calls. Unfortunately, we were not able to acquire information on the type of force used. White and minority officers use force at a similar rate of around 3 in 1,000.

3 Research Design and Methodology

Our identification strategy in both cities relies on the as-good-as-random variation in officer assignment to 911 calls. In order to avoid confusion between the findings in the two cities, we first discuss our estimation approach and findings for the first city, and then do so for the second city.

To estimate the difference between white and minority officers in overall use of force, we estimate the following equation:

$$UseofForce_c = \beta_0 + \beta_1 I(WhiteOfficer)_c + Beat * Year * Week * Shift_c + X_c + \epsilon_c \quad (1)$$

Use of force is a binary variable equal to one when a call c ends in a use of force and zero for calls that do not involve a use of force. $White\ Officer$ takes on a value of one when the police officer is white and zero otherwise. β_1 captures the difference in the probability of force across officer race. X_c includes control variables at the call-level. Specifically, X_c includes controls for officer gender, priority of call, latitude, longitude, and time between call and dispatch, as well as fixed effects for day of the week, call description, call taker, and home beat, as proxied by the beat in which the officer responds to the most calls. Our preferred specification includes $Beat$*$Year$*$Week$*$Shift$ fixed effects since this should be sufficient to isolate the as-good-as-random variation in the race of the officer assigned. However, we also show unconditional estimates and estimates that include only beat fixed effects, or only beat and month fixed effects.

To estimate whether white and black officers scale up the use of force similarly as they go from white to black neighborhoods, we use a difference-in-differences style approach. Formally, we estimate the following for the first city in our analysis:

$$UseofForce_{ic} = \beta_0 + \beta_1(ProportionBlackCivilians)_c + Officer_i + \beta_2 I(WhiteOfficer * ProportionBlackCivilians)_{ic} + Beat * Year * Week * Shift_c + X_c + \epsilon_{ic} \tag{2}$$

where $Officer_i$ is an individual officer fixed effect and captures time-invariant officer characteristics including officer race. $Proportion\ Black\ Civilians_c$ is the proportion of black civilians in the Census Block Group of the call, and controls for differences in probability of use of force across neighborhoods of different racial compositions. The variable of interest is the interaction between $Proportion\ Black\ Civilians_c$ and $White\ Officer_i$. We interpret the coefficient, β_2, as the effect of dispatching an opposite-race officer on use of force. As discussed earlier, this measure is necessarily a relative one. It provides an estimate of how much additional force is used by white officers on black versus white civilians, compared to if white officers increased their use of force the same as black officers. We also note that because we do not observe—and view as unknowable—whether force *should have* been used on a given call. As a result, without imposing additional assumptions we cannot know whether a nonzero effect means white officers used too much force on black civilians, or too little on white civilians, or whether black officers used too much force on white civilians, or too little on black civilians. The coefficient β_2 captures the sum of those effects.

We note that in equations 1) and 2), we identify effects using only the race of the dispatched officer(s), even though others may choose to respond to the call as well. In this way, we implement an intent-to-treat analysis. Relatedly, for the analysis of the first city we observe whether force was used by the dispatched officer, or by another officer who also responded to the same call.[9] Our main analysis assigns use of force to a dispatched officer if force was used by any officer on that call. However, in Appendix Tables A.1 and A.2 we show estimates are almost identical when use of force is defined as force used by that dispatching officer. We also note that for some calls, multiple officers are dispatched to the call. This is relatively rare in the first city (two percent of calls), though is more common in the second city (56 percent). As a result, we include each officer-call as an observation, and weight each observation by the inverse of the number of officers dispatched to the call.

In addition to estimating each equation with ordinary least squares, we also estimate using a logit model and report odds ratios. However, we note that doing so has two disadvantages. First, in order to attain convergence of the logit model, we are only able to control for police area and month fixed effects, or police area by year fixed effects, rather than beat-by-year-by-week-by-shift fixed effects. Police areas are roughly five times as big as a beat in the first city, and more than ten times as large in the second city. We do this in order to attain convergence of the logit model. Perhaps more problematically, given a fixed number of observations in each group, the logit estimator is not consistent as the number of groups increases (Chamberlain, 1980; Heckman, 1987). As a result, while we show logit estimates for the sake of completeness, we strongly prefer OLS estimates, particularly in models that include individual officer fixed effects.

For both approaches, we report standard errors that are clustered at the officer level to allow observations to be correlated across cases for a particular officer. In addition, for ordinary least squares (OLS) specifications we also report [in square brackets] standard errors that are two-way clustered at the officer and beat levels. In addition, for specifications without controls we also report empirical p-values from randomization inference. Specifically, we randomly assign race to each officer in our sample. We do so using the actual proportion of officer race in our sample, such that the resulting distribution of officer race is identical to that observed in the data. We then estimate and record the coefficient of interest. We repeat

[9] For the second city, we only observe whether the dispatched officer used force.

this exercise for 10,000 iterations and report the empirical p-value, which is the fraction of randomized estimates more extreme (i.e., larger in absolute value) than our estimate from the real data. We report these empirical p-values in the text and in the note below each table.

Intuitively, this difference-in-differences approach compares differences in the probability of use of force between black civilians and white civilians for black officers and white officers. Importantly, this model allows for encounters with black civilians to merit more or less force than interactions with white civilians. In this way, our approach differs significantly from approaches that rely on controlling for observable contextual factors to account for selection in the type of interactions with white versus black civilians. Rather, we identify opposite-race effects by comparing how a given white officer's use of force differs when he is dispatched to a more black versus less black neighborhood, compared to what happens when a given black officer is dispatched to more and less black neighborhoods. In short, we include beat and time or beat-by-time effects to ensure white and black officers are dispatched to similar calls, and individual officer fixed effects to ensure differences in use of force across civilian race are not driven by nonrandom sorting of officers across neighborhoods.

There are several mechanisms through which opposite-race police officers can affect use of force, as measured by our coefficient of interest β_2. The first is racial bias by police officers. For example, if officers receive utility from using force on opposite-race civilians, the threshold for using force will be lower. This bias will then be reflected in β_2. In addition, it is also possible that officers are less skilled at interacting with members of different races. If, for example, black officers are better at de-escalating situations involving black civilians compared to white officers, this will generate a nonzero estimate of β_2. Similarly, if white officers misperceive behavior by black civilians as being threatening, while black officers correctly perceive that same behavior, that misperception could drive any effects we estimate. Finally, we note our estimate of the effect of opposite-race police officers will also capture the net impact of differential civilian response to opposite-race police officers. Importantly, for this response to drive a nonzero estimate of β_2, it must be the case that civilians behave differently for opposite-race officers than for same-race officers. In contrast, any overall racial difference in civilian behavior is accounted for in the beat fixed effect. In addition, we note that failing to control for any differential civilian response to opposite-race officers has an *a priori* ambiguous impact on the estimates we report. On the one hand, if (say) black civilians were to respond more aggressively to white officers than black officers—even if those officers act in exactly the same way—then our estimates will overstate the impact of opposite-race behavior by the police. On the other hand, if black civilians were to respond more respectfully to white officers than otherwise similar black officers—perhaps out of fear of racial animus—then additionally controlling for that differential behavior would increase the magnitude of our opposite-race estimates. We view both potential responses as plausible, though we remain agnostic regarding the impact of either on the estimates we report.[10]

Both the cross-sectional approach and the difference-in-differences approach rely on the assumption that conditional on a beat or beat-by-time fixed effect, the variation in police officer race is as good as random. Given our understanding of dispatch protocol in both cities, we believe there are *ex ante* reasons to believe this assumption is valid. In addition, we also empirically assess the validity of our research design. First, when we estimate effects, we will examine the extent to which adding controls affects our estimates of interest. Specifically, we control for call characteristics including call priority, latitude, longitude, time between call and dispatch, as well as fixed effects for day of the week, call description, call taker, and home beat. We also add controls for officer gender and years of experience. If our identifying assumption is valid, we expect adding these controls should not affect our coefficient of interest.

The second way in which we assess the validity of our research design is to examine directly the correlation between call characteristics and officer race. Specifically, we test whether officer race is correlated with the race of the caller, the call priority, time between call and dispatch, the geographic location of the call (i.e., X and Y coordinates), whether the call came from an officer's home beat, and other Census Block Group characteristics (i.e. per capita income, proportion unemployed, and proportion with less than a high school degree). We formally test this in Table 2, where we regress each of these characteristics on an indicator for whether the dispatched officer was white. In Panel A, we report estimates from

[10] We are skeptical regarding the extent to which researchers are capable of distinguishing the impact of police behavior from civilian behavior since speech, tone, and body language are typically not observed in the data. To our knowledge the best evidence to date comes from Voigt et al. (2017), who analyze officer speech from officers' body cam footage in Oakland during traffic stops. They report that officers speak more disrespectfully to black civilians than white ones, controlling for observed contextual factors. The authors note it is possible for at least some of this effect to be caused by civilian behavior, though they argue it is unlikely all due to civilian behavior given the difference is present in the first five percent of words spoken by the officers, and is judged to be present even when evaluated in the context of what was said by the civilian.

specifications that include no other controls. In Panel B we additionally control for beat fixed effects, and in Panel C we control for beat-by-year-by-week-by-shift fixed effects. Panel A shows that there is relatively little sorting by police officer race in this city. In particular, of the nine coefficients, only one is significant at the 10 percent level (per capita income, coefficient = $789) and none are significant at the five percent level. Similarly, Panels B and C show little evidence of correlation between officer race and call characteristics once conditioning on beat and beat-by-time fixed effects, respectively. In both cases only one of nine coefficients are significant at the 10 percent level (X coordinate and proportion black civilians), and none are significant at the five percent level. In addition, the economic magnitude of the coefficients is small. For example, the marginally significant estimate in Panel C indicates that white officers are dispatched to calls in areas that are 0.1 percentage points less black than the calls to which black officers are dispatched. Collectively, the lack of statistical and economic significance of the coefficients reported in Table 2 is consistent with the identifying assumption of our study.

In addition, the third way in which we assess our research design is to use all call characteristics to predict officer use of force. Specifically, we first regress police use of force on beat-year-week-shift fixed effects. We then regress these residuals—which capture the deviation from the average use of force for that beat and time—on every covariate we observe for each call. These include proportion black civilians in the block group, call priority, latitude, longitude, time between call and dispatch, home beat, per capita income, proportion of civilians with less than high school degree and proportion unemployed, as well as fixed effects for call description and call taker. We use the resulting regression equation to predict the likelihood force would be used for each officer on each call. Intuitively, this produces a linear combination of exogenous call characteristics, where the weights are chosen as to best predict the likelihood of force being used. We then ask whether white and black officers are dispatched to calls of similar underlying danger when assigned to a neighborhood of a given racial composition. If the identifying assumption of our approach is valid, predicted use of force should be the same for white officers as black officers.

We show results of this test graphically for the first (black/white) city in Figure 2. Panel a shows results for all use of force, while panel b shows results for only use of force with a gun. In both cases, results demonstrate that conditional on the police beat-year-month-week-shift of the call, white and black officers are dispatched to calls that are of similar underlying risk. This is consistent with the identifying assumption, and with our understanding of how officers are dispatched.

Finally, for the difference-in-differences approach, we also add controls for interactions between officer race and all call characteristics. We do so in order to shed light on the mechanism underlying the opposite-race effects. In particular, we test whether the effect can be explained by officers having an increased propensity to use force for the type of calls that occur in opposite-race neighborhoods. For example, if white officers were more likely to use force when dispatched to domestic disturbance incidents, and if domestic disturbance incidents were more likely to occur in black neighborhoods, that could generate a nonzero difference-in-differences estimate. It is important to note, however, that because this behavior has a disparate impact on opposite-race civilians, the effect is the same as explicit bias.

4 Results

We begin by showing results for the first city graphically. Results for all use of force are shown in Figure 3. Each graph shows local averages of use of force by race of officer, as represented by the blue circles (white officers) and red squares (black officers). Each circle/square includes the same number of calls. We also fit lines to the underlying data, by officer race.

Panel a of Figure 3 shows actual use of force by officer race. It reveals two main takeaways. The first is that regardless of the racial composition of the neighborhood, white officers are more likely to use force than black officers. This suggests that with respect to the likelihood of using force, white officers seem to be drawn from a different distribution than black officers. The second takeaway from Figure 3 is that while the propensity of black officers to use force increases only modestly as they are dispatched to neighborhoods with higher proportions of black civilians, white officers use significantly more force as they are dispatched to more black neighborhoods. This suggests that having an opposite-race officer dispatched to a scene seems to have a large effect on the likelihood of force. For example, if officer race (white versus black) mattered for use of force, but having an officer of opposite-race did not, we would expect parallel slopes for white and black officers in both panels of Figure 3. The difference in slopes suggests that dispatching an opposite-race officer to a call results in a higher likelihood of police use of force. Panel a of Figure 3 shows that while white officers use force approximately 25 percent more in all-white neighborhoods, they use force around 85 percent more in all-black neighborhoods.

We note that one potential downside of the unconditional means in panel a of Figure 3 is that there is the potential for non-random sorting within neighborhoods of similar race to generate bias. For example, if white officers were systematically assigned to the neighborhoods within a given racial composition that generate the most dangerous calls, that could bias estimates of how black and white officers differ in their propensity to use force. We note, however, that there is little evidence of this type of sorting, as shown in Panel a of Table 2. In addition, we note that the type of sorting necessary to bias difference-in-difference estimates is more nuanced. In particular, if white officers were consistently assigned to the most dangerous neighborhoods of a given racial composition, this would be differenced out in our analysis. Instead, what would be problematic is if white officers were only assigned to more dangerous shifts or neighborhoods for, say, predominantly black neighborhoods, but not for predominantly white neighborhoods. We test for this directly in Appendix Figure A1, which shows call priority, whether the call was between 10 pm and 5 am, and the number of calls per beat, which proxies for how dangerous the neighborhood is. Results indicate there is little within-neighborhood difference in the priority level of calls served or the average level of danger for a given neighborhood. We do observe that black officers are five or six percentage points more likely to be dispatched to a call at night (Figure 1b). However, this is unlikely to bias difference-in-differences estimates, given this difference is roughly constant across neighborhoods of different racial composition. Collectively, results in Panel A of Table 2 and Appendix Figure A1 indicate there is little non-random sorting of officers of the type that would invalidate even an unconditional analysis.

Nevertheless, in panel b of Figure 3 we show residualized use of force, after first regressing use of force on beat-by-year-by-week-by-shift fixed effects. We do so because this specification effectively addresses all of the possible sources of non-random assignment of officers across beats of a given racial composition, or across shifts within a given beat. It shows a similar pattern: white officers are more likely to use force in all neighborhoods, and white officers scale up force much more than black officers as they go from white to black neighborhoods.

Figure 4 shows results for use of force with a gun. It shows that while black and white officers have roughly similar propensities to fire their guns when assigned to majority-white neighborhoods, they differ significantly when dispatched to neighborhoods with 80+ percent black residents. In those neighborhoods, white officers are roughly five times more likely to use gun force compared to black officers. Again, this suggests that white officers are more likely to use force overall. In addition, it suggests that they are especially likely to use more force in mostly-black neighborhoods, suggesting large opposite-race effects. This pattern is clear in both the raw data shown in Figure 4a, and in the residuals shown in Figure 4b after removing beat-by-time fixed effects.

We note that the opposite-race effects apparent in Figures 3 and 4 include the impact of any potentially non-random sorting of different types of white and black officers across neighborhoods of different race. While the impact of this type of sorting may be of interest in its own right—for example, the allocation of the most aggressive white officers to predominantly black neighborhoods could itself drive disparities—in the results below we also estimate opposite-race effects controlling for individual officer fixed effects.

4.1 The Effect of Dispatching White versus Black Officers on Use of Force

Estimates of the average effect of exposure to a white officer (compared to black) are shown in Table 3, where each column represents a different regression. Panel A shows coefficients from OLS estimation, while Panel B shows odds ratios from a logit regression. Column 1 reports results from an unconditional regression of use of force on white officer. Column 2 additionally controls for beat fixed effects in Panel A, and police area fixed effects (five times the size of a beat) in Panel B. Column 3 controls for beat-by-year-by-week-by-shift fixed effects in Panel A, and police area by month fixed effects in Panel B. Finally, Column 5 adds controls for neighborhood and call characteristics including time between call and dispatch, per capita income, unemployment, and proportion with less than a high school degree, as well as fixed effects for day of the week, priority of call, call description, call taker, officer gender, officer years of experience, and officer home beat, as proxied by the beat to which he responded to the most calls.

Results in column 1 of Panel A indicate that white officers are 0.0480 percentage points more likely to use force than black officers. Given the average use of force of 0.106 percent, this suggests that white officers are 45 percent more likely to use force relative to the mean, and 62 percent more likely relative to the mean for black officers of 0.078 percent, as shown in Table 1. The estimate in column 2 changes only slightly to 0.0485 percentage points, which again suggests there is little sorting by officer race across beats with the same neighborhood racial composition. Adding month fixed effects in column 3 leaves the estimate unchanged at 0.0485 percentage points, while adding beat-by-time fixed effects increases the estimate slightly to 0.0507 percentage points. Finally, the estimate in Column 5 is 0.0428 percentage points. Notably, estimates in all five columns of Panel A are statistically significant at the one percent level. In addition, results from the randomization inference exercise also suggest that estimates of this magnitude are unlikely to occur due to chance. Specifically, two-sided empirical p-values for estimates in columns 1 through 4 are 0.001, 0.001, 0.001, and 0.000.

Table 4 shows results for use of force with a gun, and follows the same form as Table 3. Column 1 in Panel A indicates that white officers are 0.00506 percentage points more likely to fire their gun on a call compared to black officers. This represents a 119 percent increase in use of force with a gun by white officers compared to black officers. Estimates in columns 2 through 5 range from 0.0000460 to 0.0000540, and represent increases of 108 to 127 percent. All five estimates in Panel A are significant at the five percent level. In addition, two-sided p-values for the estimates in columns 1 through 4 are 0.0698, 0.0655, 0.0648, and 0.0629, respectively.

In short, results from Table 3 indicate that white officers use force 55 to 65 percent more often than black officers when attending otherwise similar calls, and use gun force approximately twice as often as black officers. In addition, results in Figure 2 suggest that the difference in the likelihood of using lethal force is largely due to differences in predominantly black neighborhoods.

4.2 The Effect of Opposite-Race Officers on Use of Force

We now turn to estimating the effect of opposite-race police officers on use of force. Specifically, we test whether white officers scale up the use of force similarly to black officers, when going from white to black neighborhoods. Results are shown in Table 5, which is similar to Tables 3 and 4 in that column 1 reports estimates that include only an indicator for whether the officer was white, a variable measuring the proportion of the Census Block Group that is black, and the interaction of the two. Column 2 includes beat fixed effects, and column 3 includes beat and month fixed effects. Consistent with Figure 3, estimates in columns 1 through 3 of Panel A indicate strong evidence that white officers scale up use of force more than black officers as they are dispatched from white to black neighborhoods. Estimates in columns 1 through 3 range from 0.000426 to 0.000443 and are significant at the five percent level. The similarity in the estimates also suggests there is relatively little sorting of police officers by race across shifts and neighborhoods, or across neighborhoods with similar racial composition, consistent with results in Appendix Figure A1 discussed earlier. These estimates represent 40 percent increases relative to the mean use of force rate in the data, and more than 100 percent relative to the mean for black officers. Adding beat-by-time fixed effects in column 4 increases the estimate to 0.000566, which is significant at the one percent level and represents a 54 percent increase relative to the mean.

In column 5, we additionally control for individual officer fixed effects. Doing so increases the estimate slightly to 0.000613, which is significant at the five percent level and represents a 58 percent increase relative to the mean. This demonstrates that the estimates in columns 1 through 4 are not driven by the nonrandom allocation of, say, more aggressive white officers to black neighborhoods, and less aggressive white officers to white neighborhoods. Adding call controls does little to change the estimate (0.000629), which remains significant at the five percent level.

Finally, column 7 adds call controls interacted with officer race. We do so in order to assess the extent to which the estimated effect in columns 1 - 4 is picking up the response of officers to a call characteristic that is correlated with civilian race of the opposite race. The estimate in Panel A is 0.000400, which suggests that this differential response can explain roughly one-third of the overall effect. We note, however, that even if part of the overall effect is due to officers responding more harshly to calls that tend to come from opposite-race neighborhoods, that still represents the type of policy-relevant disparate impact about which interested groups worry.

In addition, to address any potential concerns about statistical inference, we also performed randomization inference for specifications in columns 1 through 5 of Panel A. The empirical two-sided p-values for estimates reported in those columns are 0.1878, 0.1782, 0.1766, 0.1011, and 0.0000, respectively.

Panel B reports odds ratios from logit regressions. Odds ratios range from 1.3 to 1.6, indicating that white officers scale up use of force 30 to 60 percent more than black officers as they are dispatched from white to black neighborhoods. However, only one of the estimates is significant at the 10 percent level, and none are significant at the five percent level.

In summary, results in Table 5 indicate that there is evidence of opposite-race effects on overall use of force. OLS estimates from officer fixed effect models range from 40 - 60 percent, relative to the overall mean. Estimates are significant at the 5 percent level using clustered standard errors at the officer or officer and beat level, and empirical p-values from specifications that include beat-by-time and officer fixed effects are 0.1011 and 0.0000, respectively. By comparison, logit estimates are of similar magnitude (30 - 60 percent), but are not generally significant at conventional levels.

4.3 The Effect of Opposite-Race Officers on Use of Force with a Gun

Next, we formally estimate whether white and black officers scale up their use of force with a gun in a similar way, as they are dispatched from white to black neighborhoods. Results are shown in Table 6, which takes the same form as Table 5. Column 1 includes controls only for officer race, the proportion of the neighborhood that is black, and the interaction of those two variables. The estimate in column 1 indicates white officers scale up their force by 0.0156 percentage points more than black officers, which is significant at the one percent level. This represents an increase of 205 percent relative to the mean rate of use of force with a gun of 0.00762 percent, and 366 percent relative to the mean for black officers of 0.00426 percent. The inclusion of beat fixed effects and beat and month fixed effects leaves coefficients unchanged at 0.0157 and 0.0158 percentage points, respectively, both of which are also significant at the one percent level. Adding beat-by-year fixed effects in column 4 increases the estimate to 0.0171 percentage points, which is significant at the one percent level and represents increases of 224 and 401 percent relative to the overall mean and mean for black officers, respectively.

Column 5 shows that adding individual officer fixed effects increases estimates to 0.0379 percentage points. This estimate is significant at the one percent level, and indicates that white officers use force in an additional 4 out of 10,000 calls when dispatched from white to black neighborhoods, compared to black officers. This increase is five times the overall use of force of 0.76/10,000 observed in that data, and nine times the average use of force by black officers. Adding call controls and a control for home beat and the interaction between home beat and officer race in column 6 changes the estimate only slightly to 0.0368 percentage points. This suggests that the effect of opposite-race officers on use of force with a gun is not driven by lack of familiarity with the police beat. This effect is also robust to including interactions between call characteristics and officer race, as shown in column 7 (estimate = 0.0299 percentage points, significant at the one percent level). This suggests that the effect of opposite-race officers on use of force with a gun is not driven by differential response to certain types of calls that are more prevalent in opposite-race neighborhoods.

As in Table 5, we also performed randomization inference for specifications reported in columns 1 through 5 of Panel A. The two-sided empirical p-values from those exercises are 0.0994, 0.0971, 0.0955, 0.0715, and 0.0000, respectively. These p-values represent the odds that two randomly chosen groups of officers would scale up their use of force as differently as we observe black and white officers doing, as they go from calls in white to black neighborhoods. That is, these p-values indicate that estimates of the magnitude estimated in Table 6 are unlikely to occur due to chance in data sets consisting of the same number of 911 calls, officers, and uses of force, and the same racial composition of both officers and the neighborhoods from which the calls originate, and that this is particularly true for models that include beat-by-time fixed effects or individual officer fixed effects.[11]

The corresponding odds ratios for columns 1 through 4 in Panel B range from 5.2 to 5.7, and are significant at the 10 percent level. Logit estimates are even larger when officer fixed effects are included, with odds ratios of 14.9 (column 5) and 10.2 (column 6). These are significant at the five percent level, though we interpret these estimates cautiously in light of the incidental parameters problem with the logit estimator in cases like this (Chamberlain, 1980; Heckman, 1987).

One potential concern with the analysis of use of force with a gun is that the use is

[11] One might be concerned that two groups with differing propensities to use force may tend to scale up use of force differently as they go from white to black neighborhoods. We can test this explicitly for officers with below- or above-median experience (less experienced officers use more force) or male versus female officers. Results are shown in Figures A1 and A2 for the first city, and Figures A3 and A4 for the second city. A comparison of experienced officers (who use less force on average) versus inexperienced officers (who use more force on average) in the first city suggest the scaling up may be different, the residuals shown in panel b of those figures show there is no such pattern after accounting for beat-by-time effects. This contrasts with the comparison of residuals for black and white officers shown in Figure 3.

relatively rare. While we believe the randomization inference is a sensible way of addressing the possibility that standard inference fails to account for the possibility estimates of this magnitude could be observed by chance, we can also document the extent to which the estimates are sensitive to any one shooting incident. In Appendix Figure A6, we report the coefficients that result from dropping any one of the 94 incidents of force with a gun observed in the data. We do so for specifications shown in columns 1 through 5 of Table 6. Results indicate that dropping any one incident has an indistinguishable effect on both the magnitude of the estimate and its statistical significance.

In summary, results in Table 5 provide strong evidence that white officers scale up use of force much more than black officers as they are dispatched from white to black neighborhoods. Estimates indicate that this additional force used by white officers is three to five times the average rate of use of force with a gun observed in the data. We also show that estimates are unaffected by any one incident of gun force observed in the data, and report empirical p-values that suggest the estimates of that magnitude are unlikely due to chance ($p=0.0715$ for the specification with beat-by-time effects, and $p=0.0000$ for the specification with officer fixed effects). In addition, as shown in Figure 3, effects seem largely driven by the much higher rates of force with a gun used by white officers in mostly-black neighborhoods, compared to black officers.

4.4 Individual Officer Effects

Given the findings discussed above, it is natural to ask whether the differences by race are due to only a handful of officers, or if they are more systemic. Put differently, are the effects we find due to differences in the middle of the distribution, or are they due to differences in the tails? To address this question, we estimate an individual officer random effects model, and then compute and graph the distribution of (shrunken) effects for white and black officers. We begin by regressing use of force on beat-by-time fixed effects as in column 4 of Table 3, and keep the residuals. We then use those residuals and the Stata command *mixed* to estimate a random effects model, and then compute the (shrunken) random effect for each officer. We do so only for officers who respond to at least 500 calls, which limits the sample to 46% of officers in our sample, though those officers respond to 91% of the calls. After estimating officer effects, we trim the 5% most extreme officers in the far tails so the reader can visualize any differences in the distributions.[12]

Results comparing black and white officers are shown in Figure 5a, which shows a kernel density plot of both white and black officers. We also show a histogram version of this plot in Appendix Figure A3, the main advantage of which is it enables a viewer to see approximately how many officers are in different parts of the distribution. Both figures show a rightward shift in the distribution of white officers, compared to black officers. This suggests that the increased propensity to use force by white officers is not driven by a handful of officers, but is rather due to an increased propensity across a significant fraction of white officers.

Figure 5b shows the distributions of white officers in white and black neighborhoods, while Figure 5c shows the distribution of black officers in white and black neighborhoods. In each case we define white neighborhoods as those in the bottom quartile of neighborhoods in the call data by percent black, and define black neighborhoods as the top quartile. Figure 5b shows a slight leftward shift of the distribution of white officers compared to black officers in white neighborhoods. Figures 5b and 5c both show a wider distribution for calls in black neighborhoods. However, while Figure 5c shows at most a modest rightward shift among black officers in black versus white neighborhoods, Figure 5b shows a much larger rightward shift for white officers. The shift in the distribution suggests the opposite-race effects documented in Table 4 appear to be due to differences among more than a handful of officers.

In summary, Figure 5 suggests that the increased overall propensity of white officers to use force is due to differential behavior by a nontrivial number of white officers, rather than a few. Similarly, the differential scaling up of use of force by white officers in black-versus-white neighborhoods, compared to black officers, also seems to be driven by the mass of the distribution, rather than the tails.

4.5 Results from the Second City: Whites and Hispanics

In addition to studying the effects of police officer race in a city in which citizens and officers are mostly white or black, we also study it in the context of a second city composed primarily of whites and Hispanics. Figure 6 shows the distribution of calls across neighbor-

[12] Without trimming, the resulting figures are so "zoomed out" that it is impossible to distinguish between the distributions even when there are meaningful differences.

hoods of differing race. As shown there, the vast majority of 911 calls in this city originate from neighborhoods in which at least half of the population is Hispanic. However, within those neighborhoods, minority (i.e., mostly Hispanic) and white officers are dispatched to neighborhoods that range from 50 to 100 percent Hispanic.

The correlation between call characteristics and officer race are shown in Appendix Table A5, which follows the form of Table 2. Panel A shows unconditional correlations, while Panels B and C include beat and beat-by-time fixed effects respectively. Estimates in Panel A suggest there is more nonrandom sorting of officers in this city across police beats compared to the first city. Six of the 12 estimates in Panel A are significant at the 10 percent level, and five are significant at the one percent level. However, Panels B and C show that once we condition on beat fixed effects, or beat-by-time fixed effects, there is little correlation between officer race and neighborhood and call characteristics. In each panel, only two of the twelve coefficients are significant at the 10 percent level, one at the five percent level, and one at the one percent level. Moreover, even the largest and most significant coefficients are of small economic significance. For example, the coefficient of 0.000494 in column 5 of Panel B suggests that white officers are dispatched to neighborhoods where the unemployment rate is 0.05 percentage points higher. Collectively, results in Table A5 indicate that while including beat or beat-by-time fixed effects may not be so necessary in the first city, it may be necessary in the second city. Indeed, given the dispatch protocol in the second city, we would expect that controlling for only beat fixed effects would be sufficient in the absence of systematic sorting of officers by race within beat over different time periods within neighborhoods of the same racial composition, and beat-by-time fixed effects should be sufficient under any such non-random sorting.

Perhaps more meaningfully, we also graph predicted use of force against the proportion of minorities in the Census Block Group. Specifically, we regress residual use of force (after removing beat-by-time fixed effects) on every call characteristic that we observe, except for the race of the officer dispatched. Results in Figure 7 indicate that white and Hispanic officers were dispatched to calls that had a similar underlying level of danger. This provides evidence that the variation in officer race across calls is as good as random, consistent with the identifying assumption. We note also that the range of the x-axis in Figure 7 goes from 60 percent minority to 100 percent minority. This reflects the fact there are few mostly-white neighborhoods in this particular city.

Figure 8 shows actual use of force by officer race. Panel a shows the raw data, while panel b shows residualized use of force after removing beat-by-year-by-month and beat-by-shift fixed effects. Two main findings are evident in Figure 8. The first is that white and Hispanic officers do not seem to differ in their overall propensity to use force. This contrasts with Figure 3, which showed that white officers were much more likely to use force than black officers.

The second finding is that as officers are dispatched to more minority/Hispanic neighborhoods, white officers seem to increase their use of force more than Hispanic officers. This is particularly evident in panel b of Figure 8, which shows residualized use of force. This suggests that dispatching officers to a different-race neighborhood results in increased use of force.

Corresponding estimates are shown in Tables 7 and 8, which follow the same form as Tables 5 and 6. Estimates in Table 7 confirm the visual evidence in Figure 8 that there is almost no difference between the rate at which force is used by white and Hispanic officers. Logit estimates indicate that white officers are 1.0 to 1.1 times more likely to use force than Hispanic officers; none of the logit or OLS estimates are significant at conventional levels.

In contrast, Table 8 provides evidence that while overall rates of use of force may be similar across officer race, that force is disproportionately used in neighborhoods of a different race. Estimates in columns 1 through 4 of Panel 1 indicate that white officers scale up their use of force by 0.084 to 0.101 percentage points more than Hispanic officers. This is just more than a 100 percent increase relative to the mean of 0.094 percent. Estimates in columns 1 through 3 are significant at the one percent level, while the estimate in column 4 is significant at the five percent level. Estimated odds ratios in columns 1 through 4 of Panel B range from 2.6 to 2.8, all of which are significant at the five percent level.

Adding beat-by-year-by-month and beat-by-shift fixed effects in column 5 reduces the OLS estimate slightly to 0.0694 percentage points, and the odds ratio to 2.2 in Panel B. Both estimates are significant at the 10 percent level. Adding call controls (column 6) and a control for home beat and home beat interacted with officer race leaves the estimates and statistical significance unchanged. Finally, in column 7 of Panel A we report results from a specification where we also include interactions between officer race and call characteristics.

The estimate is reduced by approximately half to 0.0306 percentage points.

As with the results for the first city, we also compute empirical two-sided p-values based from randomization inference, where we randomly assign the race of each officer in the data in a way that preserves the overall racial composition of officers we observe. These empirical p-values are 0.015, 0.0171, 0.0165, 0.1076, and 0.1808 for the estimates in columns 1 through 5 of Panel A. This exercise suggests that we are especially unlikely to see estimates of this magnitude for models that include somewhat less restrictive sets of fixed effects.

In summary, our analysis of the second city that is populated primarily by Hispanics and whites yields two findings. First, white and Hispanic officers use force at similar overall rates. Second, that overall similarity in the use of force disguises the fact that force is disproportionately used in different-race neighborhoods. Results suggest that the rate at which white officers use force increases by more as those officers are dispatched to more Hispanic neighborhoods, compared to Hispanic officers. As a result, we estimate that minority citizens are roughly twice as likely to experience use of force when interacting with a white officer, compared to if white officers were to scale up use of force similarly to Hispanic officers.

5 Discussion and Conclusion

In this paper, we examine whether officer and civilian race matter when it comes to police use of force. We do so by exploiting as-good-as-random variation in the race of officers dispatched to more than two million 911 calls in two different cities. In doing so, we answer two questions: Do white officers use force more often than minority officers in otherwise-similar situations? And do white officers scale up their use of force more than minority officers as they go from white to minority neighborhoods?

Results provide strong evidence that race matters in a systematic way. We show that white officers use force 55 to 65 percent more than black officers when responding to similar calls, and that these differences are due differences in behavior by a substantial proportion of white officers, rather than only a few. Moreover, we show that while white officers increase their use of force significantly as they are dispatched to more black neighborhoods, black officers do so at most modestly. This is true both in the aggregate, and when comparing within individual police officers using officer fixed effects. As a result, we estimate that interacting with an opposite-race officer results in a 30 to 60 percent increase in the use of force. We find somewhat larger effects in a second city consisting primarily of whites and Hispanics. There, interacting with an officer of a different race roughly doubles the likelihood force will be used.

The importance of race in white versus black neighborhoods is even more pronounced when it comes to the likelihood the officer will fire his gun. While black and white officers use force with a gun at approximately the same rate when responding to calls in white neighborhoods, white officers are five times as likely to fire their gun when dispatched to a call in predominantly black neighborhoods. As a result, we estimate that white officers are twice as likely to use gun force overall, and that dispatching an opposite race officer increases the probability he will fire his gun by a factor of five. This finding contrasts significantly with previous work by Fryer (forthcoming) and Weisburst (2019), who find black civilians are no more likely to experience use of force with a gun. There are several explanations for the difference. The first is that effects may be present when police respond to 911 calls, but not when police interact with civilians in other contexts. Alternatively, results could differ due to differences in the cities and neighborhoods studied. As shown in Figure 4, our findings on force with a gun are driven by police behavior in the most black neighborhoods of the city, where a disproportionate amount of crime occurs and 911 calls originate. In addition, the city itself has a homicide rate that ranks in the top 20 nationwide among large cities, a set that does not include any of the cities studied by this previous work. This suggests that just as effects are not evident in many of the neighborhoods shown in Figure 4, effects may also not be present in the black neighborhoods found in less dangerous cities. Finally, the contrast in findings could be due to differences in research design. Fryer (forthcoming) uses arrests as a benchmark and a selection-on-observables design to account for differences across interactions, while we exploit exogenous interactions and compare changes in force across neighborhood race for the same black and white officers. The former approach could understate effects in our context if white officers were to arrest at higher rates in the predominantly black neighborhoods in Figure 4, or if white officers described those encounters as more dangerous than black officers would have described identical situations. It is also possible that a selection-on-observables approach understates the impact of race compared to the difference-in-differences approach used here, perhaps because the context of the encounter is described in the arrest report after force is used. In this way, we believe an important contribution of our study is that it provides researchers and policymakers with a roadmap for how to analyze whether race matters when it comes to use of force in a given city, without making benchmarking and selection-on-observables assumptions. While the 911 call dispatch system in some cities may not be suitable for an analysis like this, others certainly are. In those cases, the only roadblock to documenting the impact of race on force in this way is access to the necessary data.

Overall, our findings indicate that race is an important determinant of use of force. While it is difficult to know if these findings extend beyond the two major cities studied here, the findings do corroborate the distrust of police among minorities in the U.S. In particular, this study shows that white officers are more likely to use force than black officers when dispatched to otherwise-similar situations. In addition, it shows that white officers scale up their use of force—including and especially lethal force—more than minority officers as they go from white to minority neighborhoods. This raises an important question for white police officers: if calls in minority neighborhoods were that much more dangerous than calls in white neighborhoods, why don't black officers increase their use of force as much as white officers do? We believe the only reasonable interpretation of these findings is that race matters in a systematic way with respect to police use of force. As a result, much work remains to develop the civilian trust and cooperation necessary to maximize policing effectiveness.

References

Antonovics, Kate and Knight, Brian G. A new look at racial profiling: Evidence from the Boston Police Department. *The Review of Economics and Statistics*, 91(1):163–177, 2009.

Anwar, Shamena and Fang, Hanming. An alternative test of racial prejudice in motor vehicle searches: Theory and evidence. *American Economic Review*, 96(1):127–151, 2006.

Anwar, Shamena, Bayer, Patrick, and Hjalmarsson, Randi. The impact of jury race in criminal trials. *The Quarterly Journal of Economics*, 127(2):1017–1055, 2012.

Arnold, David, Dobbie, Will, and Yang, Crystal S. Racial bias in bail decisions. *The Quarterly Journal of Economics*, 133(4):1885–1932, 2018.

Ayres, Ian and Siegelman, Peter. Race and gender discrimination in bargaining for a new car. *The American Economic Review*, pages 304–321, 1995.

Bielen, Samantha, Marneffe, Wim, and Mocan, Naci H. Racial bias and in-group bias in judicial decisions: Evidence from virtual reality courtrooms. Working Paper 25355, National Bureau of Economic Research, December 2018.

Center, Pew Research. The racial confidence gap in police performance, 2016. `https://www.pewsocialtrends.org/wp-content/uploads/sites/3/2016/09/ST_2016.09.29_Police-Final.pdf`, Last accessed on 2019-12-2.

Chamberlain, Gary. Analysis of covariance with qualitative data. *The Review of Economic Studies*, 47(1):225–238, 1980.

Dahl, Gordon B and Moretti, Enrico. The demand for sons. *The Review of Economic Studies*, 75(4):1085–1120, 2008.

Feld, Jan, Salamanca, Nicolás, and Hamermesh, Daniel S. Endophilia or exophobia: Beyond discrimination. *The Economic Journal*, 126(594):1503–1527, 2016.

Flanagan, Francis X. Race, gender, and juries: Evidence from North Carolina. *Journal of Law and Economics*, 61(2):189–214, 2018.

Fryer, Roland G. An empirical analysis of racial differences in police use of force. *Journal of Political Economy*, 127(3):1210–1261, 2019.

Goldin, Claudia and Rouse, Cecilia. Orchestrating impartiality: The impact of "blind" auditions on female musicians. *American Economic Review*, 90(4):715–741, 2000.

Grogger, Jeffrey and Ridgeway, Greg. Testing for racial profiling in traffic stops from behind a veil of darkness. *Journal of the American Statistical Association*, 101(475):878–887, 2006.

Haseman, Janie, Zaiets, Karina, Thorson, Mitchell, Procell, Carlie, Petras, George, and Sullivan, Shawn. Tracking protests across the usa in the wake of george floyd's death, 2020. `https://www.usatoday.com/in-depth/graphics/2020/06/03/map-protests-wake-george-floyds-death/5310149002/`.

Heckman, James J. *The incidental parameters problem and the problem of initial conditions in estimating a discrete time-discrete data stochastic process and some Monte Carlo evidence*. University of Chicago Center for Mathematical studies in Business and Economics, 1987.

Horrace, William C and Rohlin, Shawn M. How dark is dark? Bright lights, big city, racial profiling. *Review of Economics and Statistics*, 98(2):226–232, 2016.

Johnson, David J, Tress, Trevor, Burkel, Nicole, Taylor, Carley, and Cesario, Joseph. Officer characteristics and racial disparities in fatal officer-involved shootings. *Proceedings of the National Academy of Sciences*, 116(32):15877–15882, 2019.

Knox, Dean and Mummolo, Jonathan. Making inferences about racial disparities in police violence. *Available at SSRN 3431132*, 2019.

Knox, Dean, Lowe, Will, and Mummolo, Jonathan. The bias is built in: How administrative records mask racially biased policing. *Available at SSRN*, 2019.

Lavy, Victor. Do gender stereotypes reduce girls' or boys' human capital outcomes? Evidence from a natural experiment. *Journal of Public Economics*, 92(10-11):2083–2105, 2008.

Moss-Racusin, Corinne A, Dovidio, John F, Brescoll, Victoria L, Graham, Mark J, and Handelsman, Jo. Science faculty's subtle gender biases favor male students. *Proceedings of the National Academy of Sciences*, 109(41):16474–16479, 2012.

Neumark, David, Bank, Roy J, and Van Nort, Kyle D. Sex discrimination in restaurant hiring: An audit study. *The Quarterly Journal of Economics*, 111(3):915–941, 1996.

Parsons, Christopher A, Sulaeman, Johan, Yates, Michael C, and Hamermesh, Daniel S. Strike three: Discrimination, incentives, and evaluation. *American Economic Review*, 101 (4):1410–35, 2011.

Persico, Nicola and Todd, Petra. Generalising the hit rates test for racial bias in law enforcement, with an application to vehicle searches in Wichita. *The Economic Journal*, 116 (515):F351–F367, 2006.

Price, Joseph and Wolfers, Justin. Racial discrimination among NBA referees. *The Quarterly Journal of Economics*, 125(4):1859–1887, 2010.

Sloan, CarlyWill. Racial bias by prosecutors: Evidence from random assignment. 2019.

Tuttle, Cody. Racial disparities in federal sentencing: Evidence from drug mandatory minimums. 2019.

Voigt, Rob, Camp, Nicholas P, Prabhakaran, Vinodkumar, Hamilton, William L, Hetey, Rebecca C, Griffiths, Camilla M, Jurgens, David, Jurafsky, Dan, and Eberhardt, Jennifer L. Language from police body camera footage shows racial disparities in officer respect. *Proceedings of the National Academy of Sciences*, 114(25):6521–6526, 2017.

Weisburst, Emily K. Whose help is on the way? The importance of individual police officers in law enforcement outcomes. 2017.

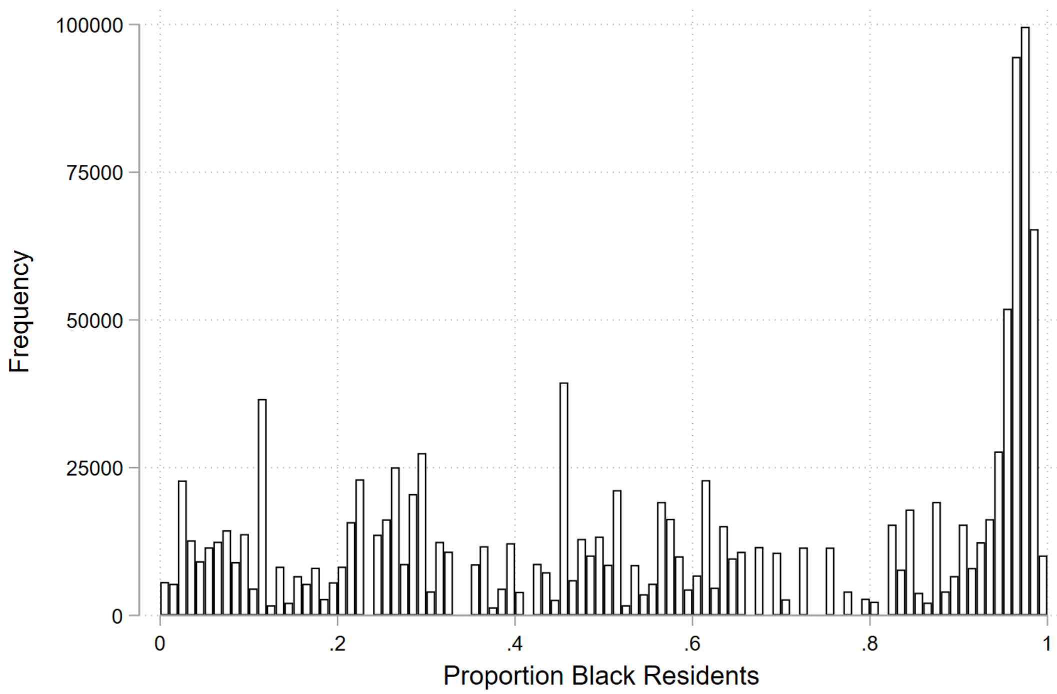
Weisburst, Emily K. Police use of force as an extension of arrests: Examining disparities across civilian and officer race. *AEA Papers and Proceedings*, 109:152–56, 2019.

West, Jeremy. Racial bias in police investigations. 2018.

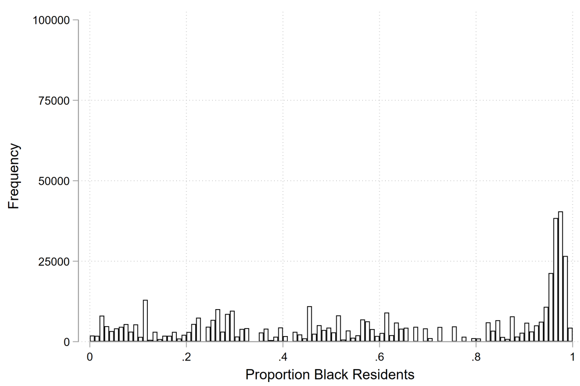
6 Tables and Figures

Figure 1: Distribution of 911 calls across Census Block Groups

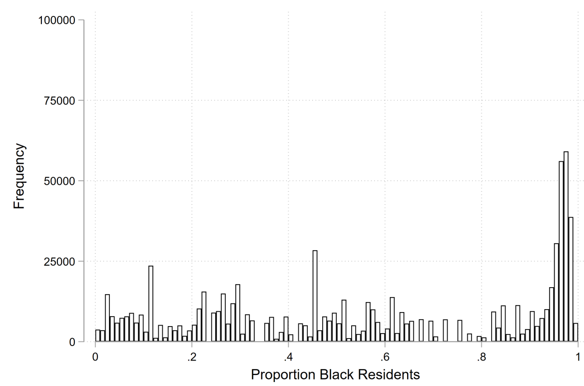
(a) Entire Sample



(b) Black Officers



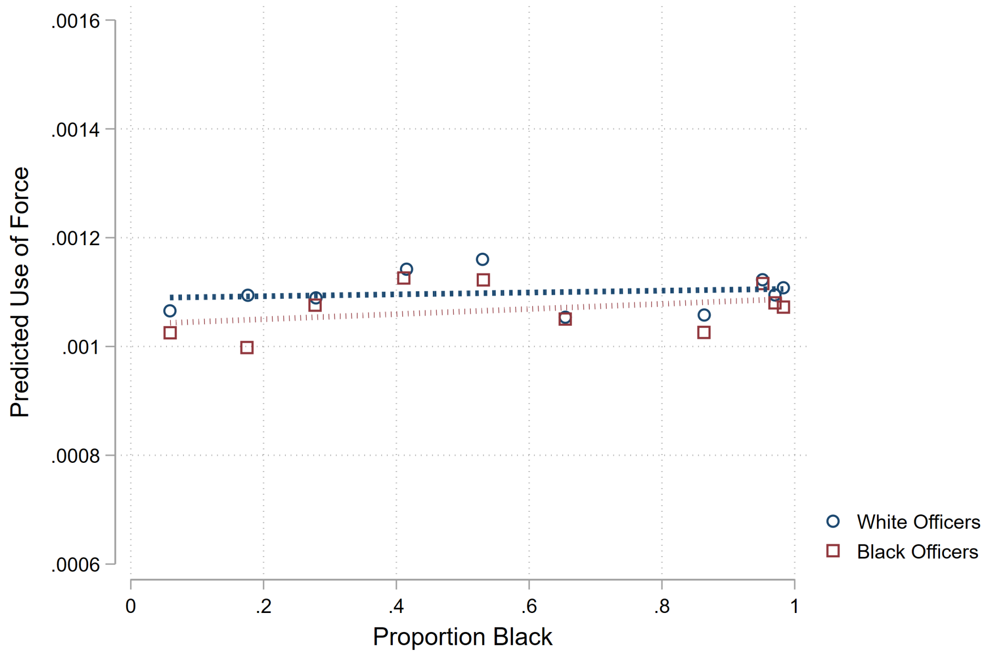
(c) White Officers



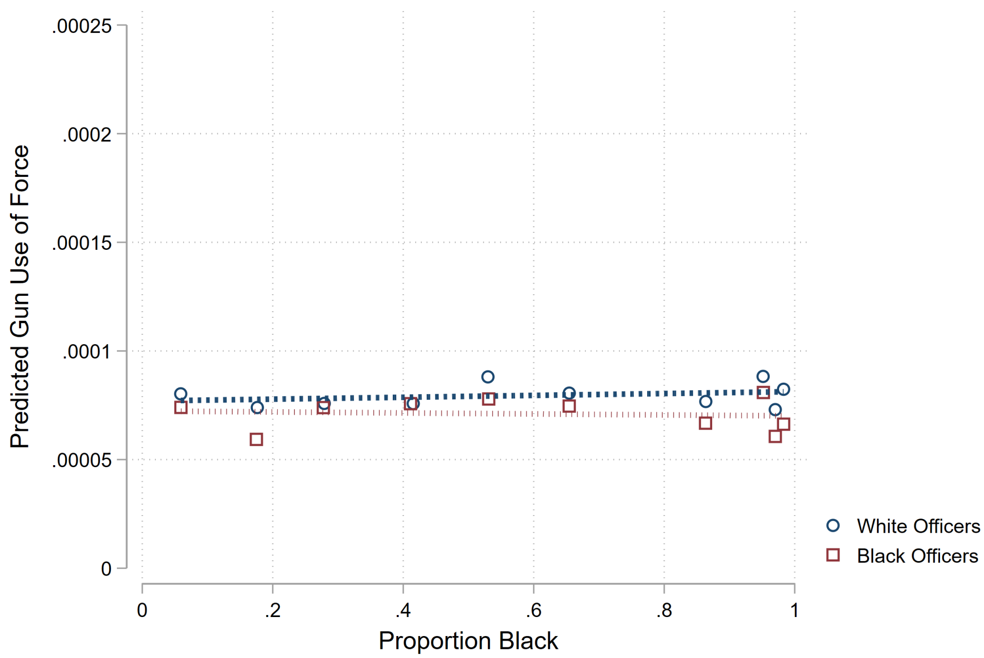
Notes: These figures report the distribution of proportion black residents for calls for service. Each histogram uses 0.01 size bins. Panels (b) and (c) report the histogram for calls where only black or white officers are dispatched, respectively.

Figure 2: Predicted Outcomes for Black and White Officers

(a) Predicted Use of Force



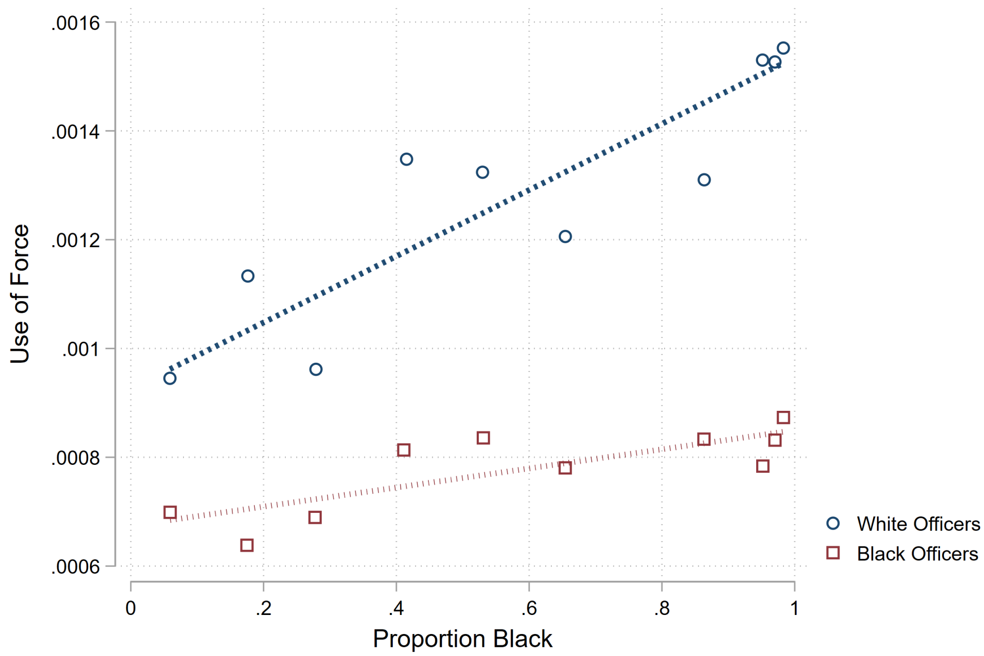
(b) Predicted Use of Force with a Gun



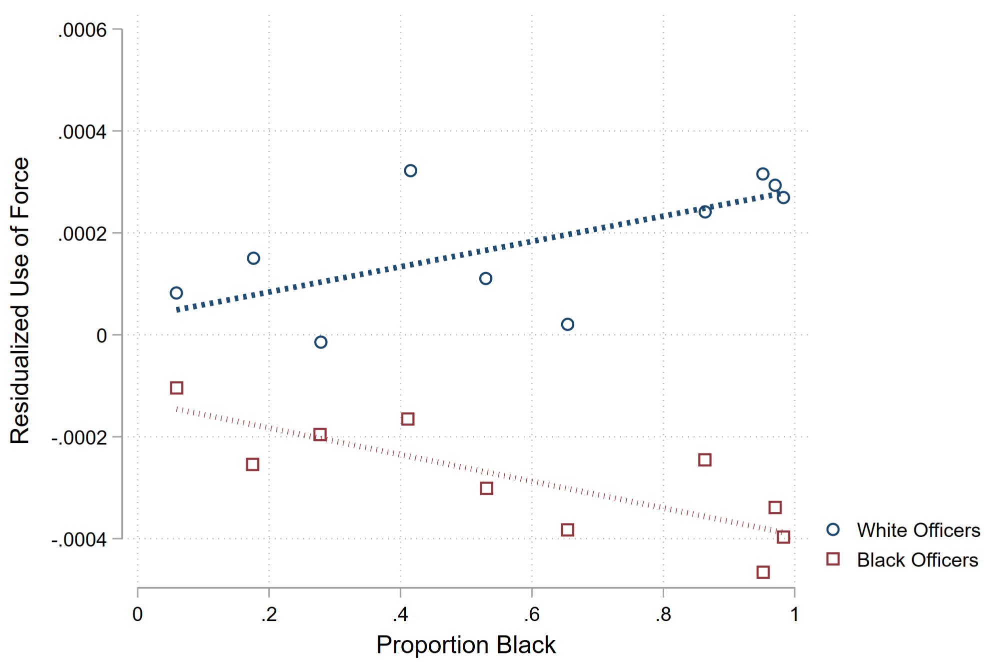
Notes: In Panels (a) and (b), we predict probability of use of force or use of force with a gun using all observable call characteristics for each call for service. Specifically, we predict (after removing beat-year-week-shift fixed effects) using proportion black civilians, unemployment, per capita income, proportion high school drop outs, call priority, latitude, longitude, and time between call and dispatch, as well as fixed effects for day of the week, call description, home beat, and call taker using a linear probability model. Observations are grouped so that each point includes an equal number of calls. The fitted line is a linear fit across all predicted use of force rates.

Figure 3: Actual Use of Force for Black and White Officers

(a) Use of Force



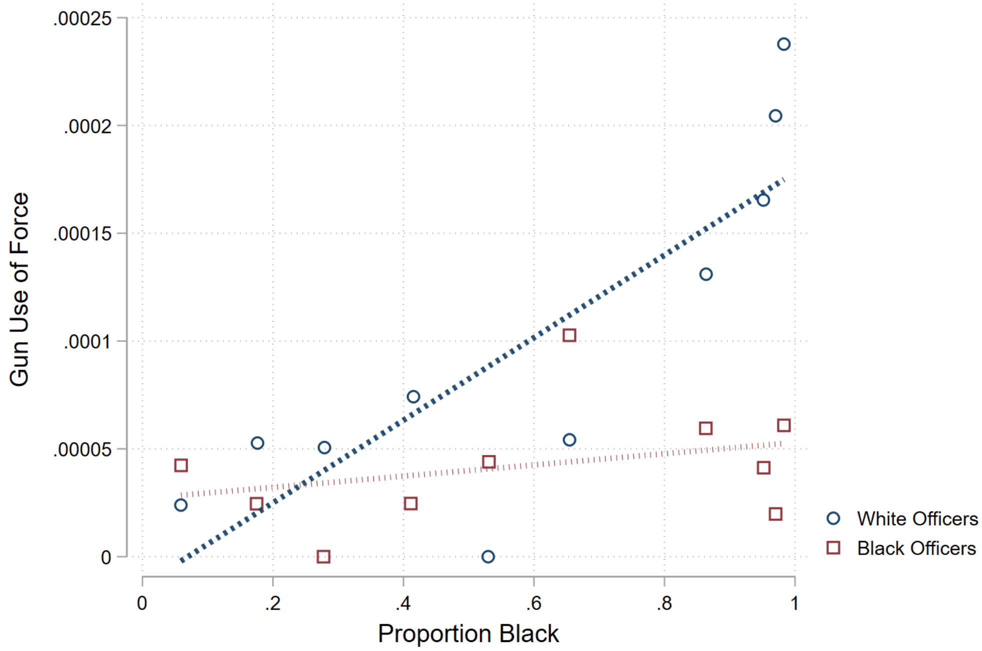
(b) Residualized Use of Force



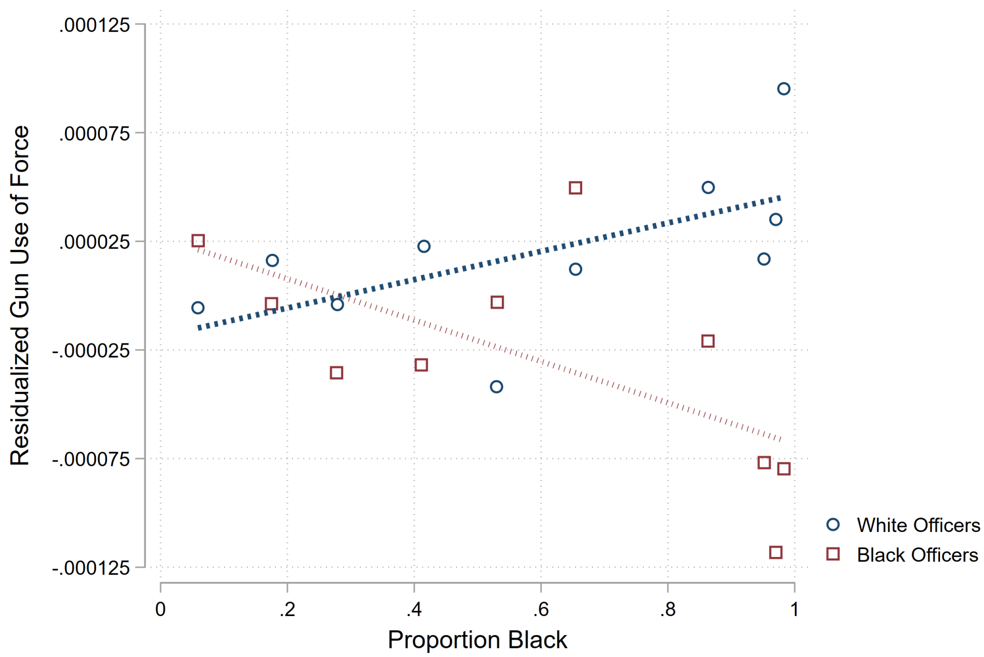
Notes: In Panel (a) we plot use of force. In Panel (b) we plot residualized (beat-year-week-shift fixed effects are removed) use of force. The fitted line is a linear fit across all use of force rates. Observations are grouped so that each point includes an equal number of calls.

Figure 4: Actual Use of Force with a Gun for Black and White Officers

(a) Use of Force with a Gun



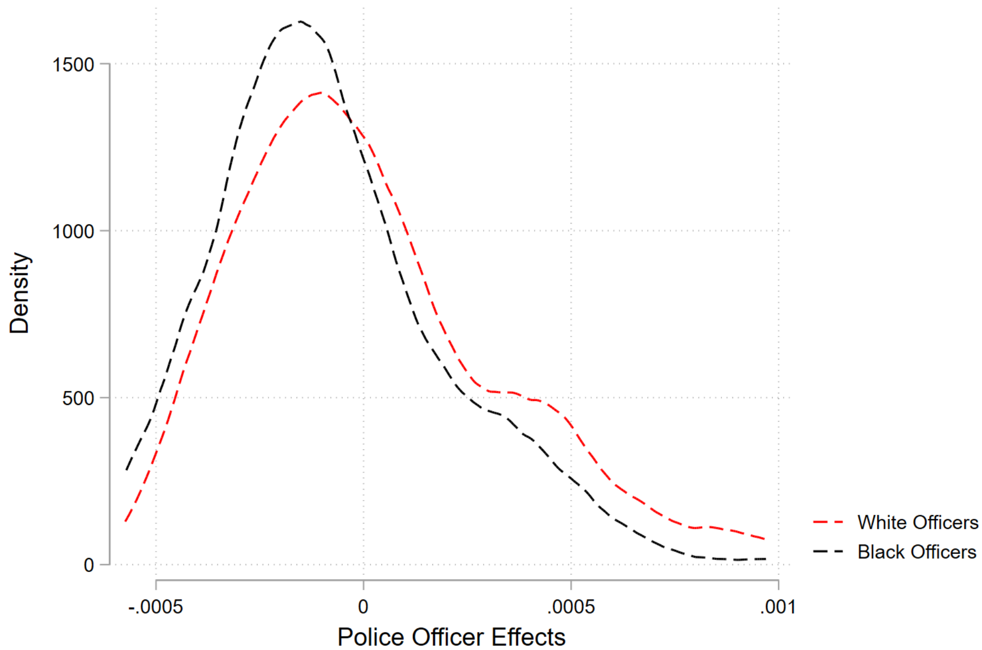
(b) Residualized Use of Force with a Gun



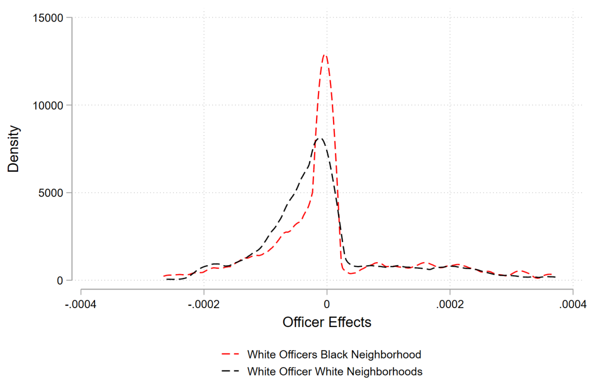
Notes: In Panel (a) we plot use of force with a gun. In Panel (b) we plot residualized (beat-year-week-shift fixed effects are removed) use of force with a gun. The fitted line is a linear fit across all gun use of force rates. Observations are grouped so that each point includes an equal number of calls.

Figure 5: Distribution of Individual Police Officer Effects by Officer Race

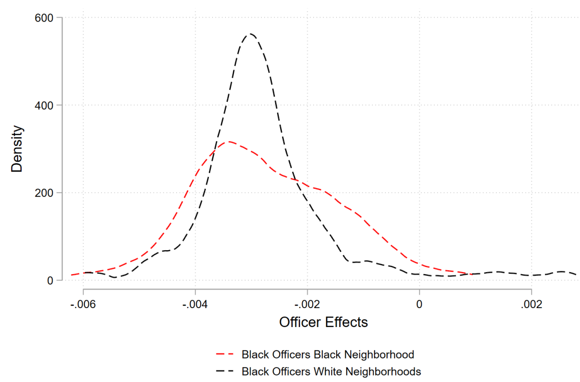
(a) White and Black Officer Effects



(b) White Officer Effects by Neighborhood

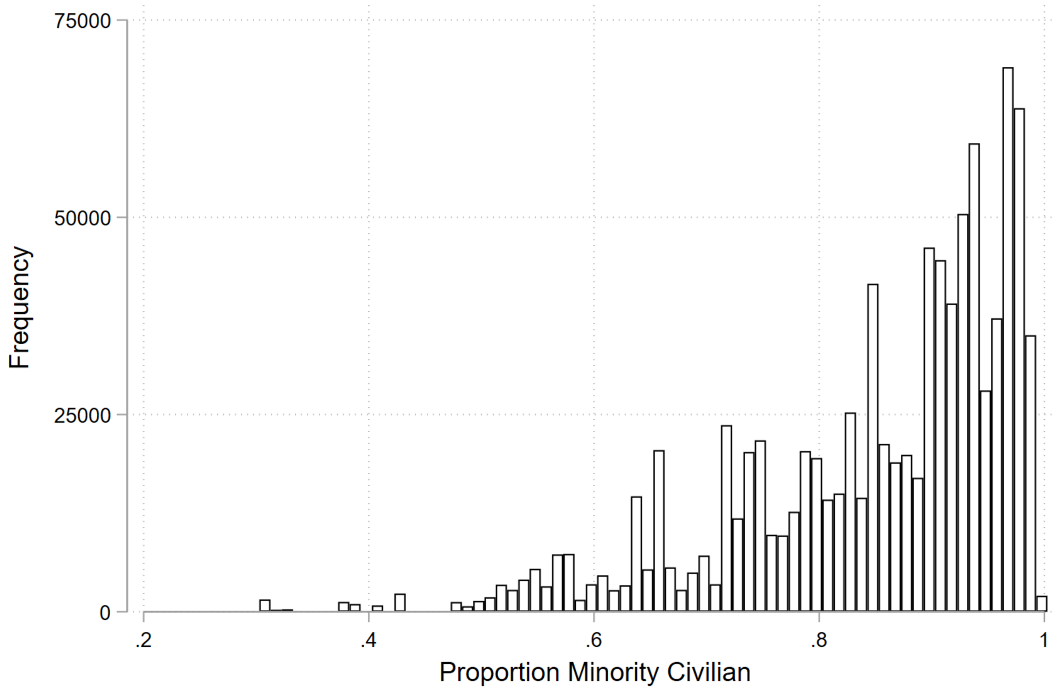


(c) Black Officer Effects by Neighborhood

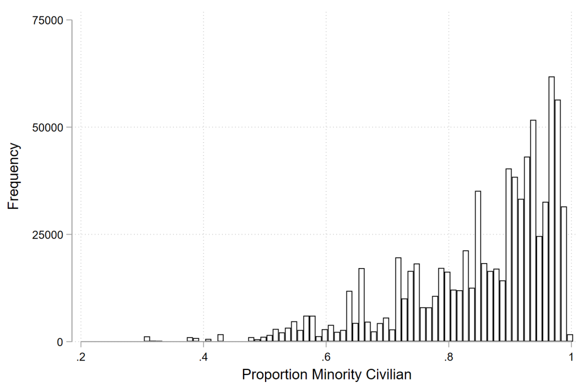


Notes: Figures represent the distribution of individual police officer effects (Bayes shrinkage) by police officer race and neighborhood race.

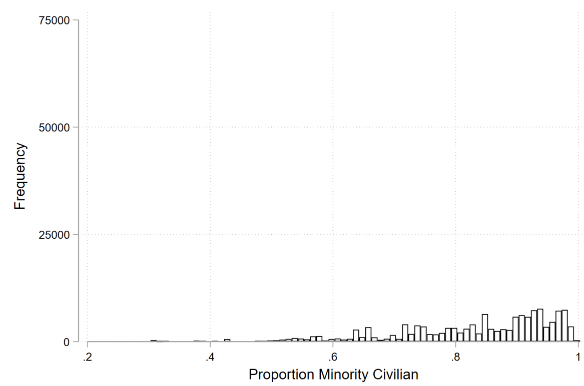
Figure 6: **Second City** Distribution of 911 calls across Census Block Groups



(a) Entire Sample



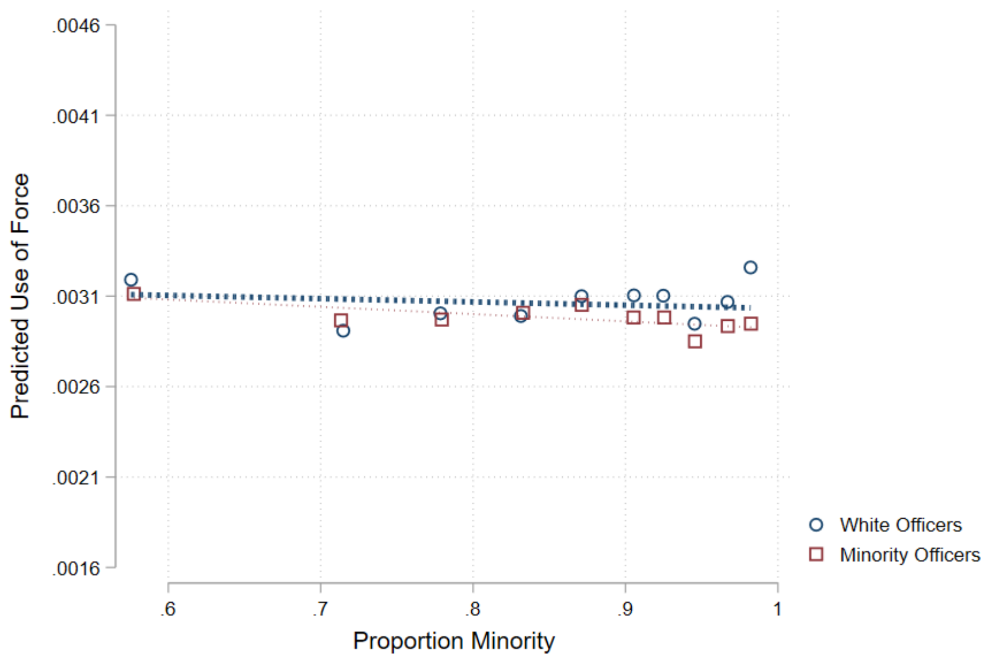
(b) Minority Officers Only



(c) White Officers Only

Notes: These figures report the distribution of proportion minority civilians for calls for service. Each histogram uses 0.01 size bins. Panels (b) and (c) report the histogram for calls where only minority or white officers are dispatched, respectively. 97% of minority officers are Hispanic. 96% of minority civilians are Hispanic.

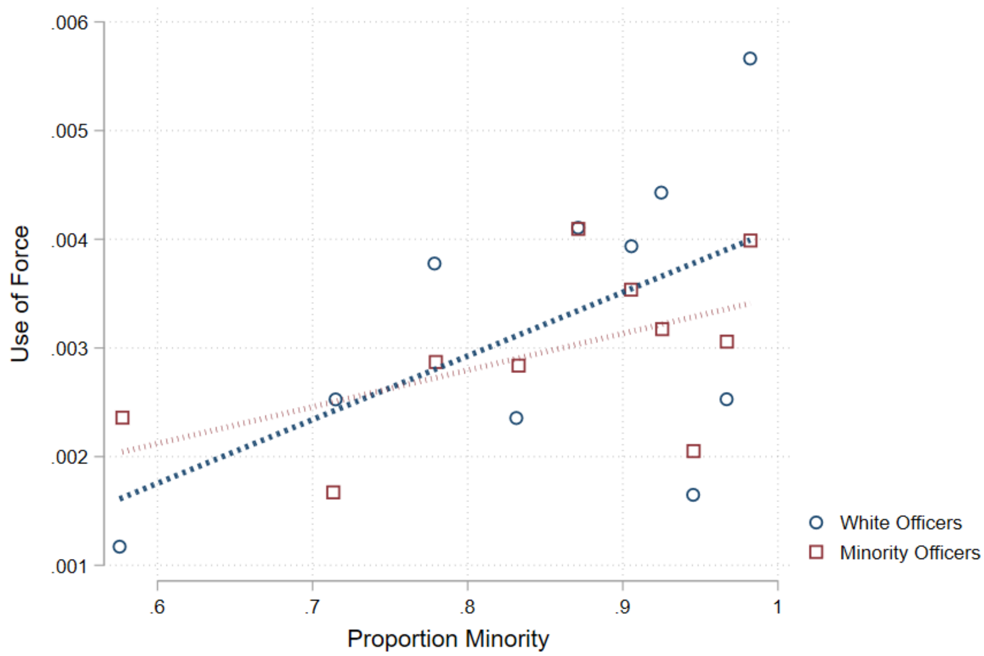
Figure 7: **Second City** Predicted Use of Force for Minority and White Officers



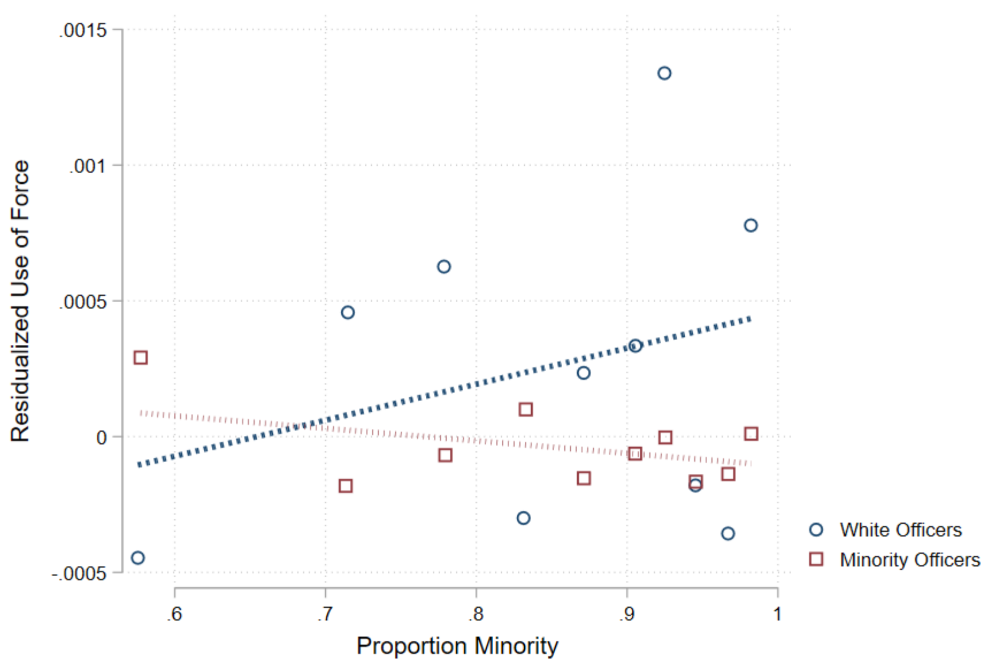
Notes: Here we predict probability of use of force using all observable call characteristics for each call for service. Specifically, we predict (after removing beat-year-month and beat-shift) using proportion minority civilians, call priority, latitude, longitude, per capital income, unemployment, and proportion with less than a high school degree, and dispatch time, as well as fixed effects for day of the week, call description, officer's home beat and call source. Observations are grouped so that each point includes an equal number of calls. The fitted line is a linear fit across all predicted use of force rates. 97% of minority officers are Hispanic. 96% of minority civilians are Hispanic.

Figure 8: **Second City** Actual Use of Force for Minority and White Officers

(a) Use of Force



(b) Residualized Use of Force



Notes: Here we plot the average use of force for 10 bins in panel (a) or residualized (beat-year-month, and beat-shift are removed) use of force in panel (b). The fitted line is a linear fit across all use of force rates. Observations are grouped so that each point includes an equal number of calls. 97% of minority officers are Hispanic. 96% of minority civilians are Hispanic.

Table 1: Summary Statistics

	(1) Entire Sample	(2) Black Officers	(3) White Officers
Outcomes			
Use of Force	0.00109	0.000780	0.00128
Gun Use of Force	0.0000762	0.0000426	0.0000969
Call Characteristics			
Proportion Black Civilians	0.586	0.603	0.575
	(0.333)	(0.333)	(0.333)
Per Capita Income	23280.1	22796.2	23577.3
	(14849.2)	(14559.2)	(15016.8)
Proportion Unemployed	0.139	0.143	0.137
	(0.111)	(0.112)	(0.110)
Proportion Less than HS Degree	0.185	0.187	0.184
	(0.118)	(0.117)	(0.118)
Priority of Call	2.838	2.838	2.838
	(0.757)	(0.754)	(0.758)
Time Between Call and Dispatch	6.479	6.509	6.460
	(63.24)	(23.06)	(78.28)
Black Officer	0.380	1	0
Female Officer	0.160	0.189	0.142
Years of Experience	10.11	10.52	9.858
	(7.980)	(7.969)	(7.976)
Call from Home Beat	0.180	0.183	0.177
Observations	1233139	469170	763969

Standard deviations in parentheses

Notes: This table reports mean, standard deviation, and number of observations for each variable. Use of force and use of force with a gun are measured at the call level and take on values of one if the call ended in a use of force or use of force with a gun. Overall, 7 percent of use of force involves a gun, 38 percent involves a taser, and the remaining 55 percent is a category that includes the officer using his things like his hands, feet, baton, mace, or nightstick to subdue the civilian. Priority, latitude, and longitude have been altered (multiplied by a random number) to protect the anonymity of the city.

Table 2: Correlation Between Call Characteristics and Officer Race

	(1) Proportion Black Civilians	(2) Per Capita Income	(3) Proportion Unemployed	(4) Proportion Less than HS Degree	(5) Call Priority	(6) Time Between Call and Dispatch	(7) Call from Home Beat	(8) X Coord	(9) Y Coord
Panel A: Unconditional									
White Officer	-0.0279	788.8*	-0.00598	-0.00346	-0.000808	-0.0464	-0.00546	-9785.9	-231576.1
	(0.0185)	(447.9)	(0.00394)	(0.00309)	(0.0122)	(0.143)	(0.00502)	(48828.7)	(217728.2)
Observations	1233139	1233139	1233139	1233139	1233139	1233139	1233139	1233139	1233139
Outcome Mean	0.586	23281.7	0.139	0.185	2.839	6.490	0.180	87304866.5	202240062.9
Panel B: Beat FE									
White Officer	-0.00260	110.2	-0.000581	-0.000217	-0.00468	0.0693	-0.00711*	-4273.8	-231576.1
	(0.00183)	(107.8)	(0.000399)	(0.000732)	(0.0106)	(0.117)	(0.00430)	(7245.0)	(217728.2)
Observations	1233139	1233139	1233139	1233139	1233139	1233139	1233139	1233139	1233139
Outcome Mean	0.586	23281.7	0.139	0.185	2.839	6.490	0.180	87304866.5	202240062.9
Panel C: Beat-year-week-shift FE									
White Officer	-0.00128*	52.40	-0.000247	-0.000208	-0.0105	-0.108	-0.00582	758.7	339.2
	(0.000662)	(43.57)	(0.000246)	(0.000254)	(0.00857)	(0.144)	(0.00374)	(1103.3)	(1157.6)
Observations	1233139	1233139	1233139	1233139	1233139	1233139	1233139	1233139	1233139
Outcome Mean	0.586	23281.7	0.139	0.185	2.839	6.490	0.180	87304866.5	202240062.9

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the coefficient on $White\ Officer$ from separate regressions of call characteristics on a binary variable representing officer race. Panel B includes beat fixed effects and Panel C includes beat-year-week-shift fixed effects. Standard errors are clustered at the officer level. Priority, latitude, and longitude have been altered (multiplied by a random number) to protect the identity of our city.

Table 3: Estimated Difference in Use of Force by Officer Race

	(1)	(2)	(3)	(4)	(5)
	Use of Force	Use of Force	Use of Force	Use of Force	Use of Force
Panel A: OLS					
White Officer	0.000480***	0.000485***	0.000485***	0.000507***	0.000428***
	(0.0000792)	(0.0000796)	(0.0000796)	(0.0000819)	(0.0000791)
	[0.0000775]	[0.0000775]	[0.0000776]	[0.0000798]	[0.0000789]
Observations	1233139	1233139	1233139	1233139	1233139
Outcome Mean	0.00106	0.00106	0.00106	0.00106	0.00106
Beat FE	-	Y	-	-	-
Beat & Month FE	-	-	Y	-	-
Beat-year-week-shift FE	-	-	-	Y	Y
Call Controls	-	-	-	-	Y
Panel B: Logit (Odds Ratio)					
White Officer	1.637***	1.658***	1.658***	1.665***	1.615***
	(0.135)	(0.136)	(0.136)	(0.137)	(0.130)
Observations	1233139	1233128	1233128	1230375	1230353
Outcome Mean	0.00109	0.00109	0.00109	0.00109	0.00109
Police Area FE	-	Y	-	-	-
Police Area & Month FE	-	-	Y	-	-
Police Area-x-Year FE	-	-	-	Y	Y
Additional Controls	-	-	-	-	Y

Exponentiated coefficients; Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the coefficient on $White\ Officer$ from the regression of $Use\ of\ Force$ on an indicator for officer race. For OLS specifications, column 5 add controls for latitude, longitude, time between call and dispatch, per capita income, unemployment, and proportion with less than a high school degree, as well as fixed effects for day of the week, priority of call, call description, call taker, officer gender, officer years of experience, and officer home beat, as proxied by the beat to which the officer responded to the most calls. For Logit specifications, column 5 add controls for per capita income, unemployment, and proportion with less than a high school degree, as well as fixed effects for officer gender and priority of call. Police areas are roughly five times as large as a beat and are used instead of police beats to attain convergence of the logit model. Robust standard errors clustered at the officer level are reported in parentheses and two-way clustered (officer and beat) standard errors are reported in brackets. The empirical two-sided p-values from randomization inference for the estimates in columns 1 through 4 of Panel A are 0.001, 0.001, 0.001, and 0.000, respectively.

Table 4: Estimated Difference in Use of Force with a Gun by Officer Race

	(1) Gun Use of Force	(2) Gun Use of Force	(3) Gun Use of Force	(4) Gun Use of Force	(5) Gun Use of Force
Panel A: OLS					
White Officer	0.0000506**	0.0000540**	0.0000540**	0.0000518**	0.0000460**
	(0.0000208)	(0.0000219)	(0.0000218)	(0.0000219)	(0.0000228)
	[0.0000218]	[0.0000227]	[0.0000227]	[0.0000224]	[0.0000234]
Observations	1233139	1233139	1233139	1233139	1233139
Outcome Mean	0.0000710	0.0000710	0.0000710	0.0000710	0.0000710
Beat FE	-	Y	-	-	-
Beat & Month FE	-	-	Y	-	-
Beat-year-week-shift FE	-	-	-	Y	Y
Call Controls	-	-	-	-	Y
Panel B: Logit (Odds Ratio)					
White Officer	2.272***	2.372***	2.374***	2.238**	2.049**
	(0.717)	(0.765)	(0.766)	(0.723)	(0.645)
Observations	1233139	1208695	1208695	998518	905261
Outcome Mean	0.0000762	0.0000778	0.0000778	0.0000941	0.000104
Police Area FE	-	Y	-	-	-
Police Area & Month FE	-	-	Y	-	-
Police Area-x-Year FE	-	-	-	Y	Y
Additional Controls	-	-	-	-	Y

Exponentiated coefficients; Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the coefficient on $White\ Officer$ from the regression of $Gun\ Use\ of\ Force$ on an indicator for officer race. For OLS specifications, column 4 add controls for latitude, longitude, time between call and dispatch, per capita income, unemployment, and proportion with less than a high school degree, as well as fixed effects for day of the week, priority of call, call description, call taker, officer gender, officer years of experience, and officer home beat, as proxied by the beat to which the officer responded to the most calls. For Logit specifications, column 4 add controls for per capita income, unemployment, and proportion with less than a high school degree, as well as fixed effects for officer gender and priority of call. Police areas are roughly five times as large as a beat and are used instead of police beats to attain convergence of the logit model. Robust standard errors clustered at the officer level are reported in parentheses and two-way clustered (officer and beat) standard errors are reported in brackets. The empirical two-sided p-values from randomization inference for the estimates in columns 1 through 4 of Panel A are 0.0698, 0.0655, 0.0648, and 0.0629, respectively.

Table 5: The Effect of Opposite-Race Police Officers on Use of Force

	(1) Use of Force	(2) Use of Force	(3) Use of Force	(4) Use of Force	(5) Use of Force	(6) Use of Force	(7) Use of Force
Panel A: OLS							
Opposite Race Officer							
(White Officer*Pr Black Civilian)	0.000426**	0.000441**	0.000443**	0.000566***	0.000613**	0.000629**	0.000400
	(0.000191)	(0.000192)	(0.000192)	(0.000194)	(0.000277)	(0.000275)	(0.000303)
	[0.000179]	[0.000179]	[0.000178]	[0.000183]	[0.000284]	[0.000270]	[0.000859]
Observations	1233139	1233139	1233139	1233139	1233139	1233139	1233139
Outcome Mean	0.00106	0.00106	0.00106	0.00106	0.00106	0.00106	0.00106
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	Y
Beat FE	-	Y	-	-	-	-	-
Beat & Month FE	-	-	Y	-	-	-	-
Beat-year-week-shift FE	-	-	-	Y	Y	Y	Y
Officer FE	-	-	-	-	Y	Y	Y
Call Controls	-	-	-	-	-	Y	Y
Interactions	-	-	-	-	-	-	Y
Panel B: Logit (Odds Ratio)							
Opposite Race Officer							
(White Officer*Pr Black Civilian)	1.294	1.298	1.300	1.297	1.598*	1.434	
	(0.265)	(0.268)	(0.269)	(0.267)	(0.454)	(0.398)	
Observations	1233139	1233128	1233128	1230375	829428	829428	
Outcome Mean	0.00109	0.00109	0.00109	0.00109	0.00162	0.00162	
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	
Police Area FE	-	Y	-	-	Y	Y	
Police Area & Month FE	-	-	Y	-	-	-	
Police Area-x-Year FE	-	-	-	Y	-	-	
Officer FE	-	-	-	-	Y	Y	
Additional Controls	-	-	-	-	-	Y	

Exponentiated coefficients; Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the coefficient on the interaction of *White Officer* and *Proportion Black Civilian* from the regression of *Use of Force* on indicators for officer race, proportion citizen black, and the interaction term. For OLS specifications, column 6 adds controls for home beat, latitude, longitude, time between call and dispatch, per capita income, unemployment, and proportion with less than a high school degree, as well as fixed effects for day of the week, priority of call, call description, and call taker. For Logit specifications, column 6 adds controls for per capita income, unemployment, and proportion with less than a high school degree, and fixed effects for priority of call. Police areas are roughly five times as large as a beat and are used instead of police beats to attain convergence of the logit model. Robust standard errors clustered at the officer level are reported in parentheses and two-way clustered (officer and beat) standard errors are reported in brackets. The empirical two-sided p-values from randomization inference for estimates in columns 1 through 5 of Panel A are 0.1878, 0.1782, 0.1766, 0.1011, and 0.0000, respectively.

Table 6: The Effect of Opposite-Race Police Officers on Use of Force with a Gun

	(1) Gun Use of Force	(2) Gun Use of Force	(3) Gun Use of Force	(4) Gun Use of Force	(5) Gun Use of Force	(6) Gun Use of Force	(7) Gun Use of Force
Panel A: OLS							
Opposite Race Officer							
(White Officer*Pr Black Civilian)	0.000156***	0.000157***	0.000158***	0.000171***	0.000379***	0.000368***	0.000299***
	(0.0000556)	(0.0000565)	(0.0000566)	(0.0000601)	(0.000133)	(0.000133)	(0.000105)
	[0.0000540]	[0.0000547]	[0.0000547]	[0.0000564]	[0.000143]	[0.000198]	[0.0000560]
Observations	1233139	1233139	1233139	1233139	1233139	1233139	1233139
Outcome Mean	0.0000710	0.0000710	0.0000710	0.0000710	0.0000710	0.0000710	0.0000710
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	Y
Beat FE	-	Y	-	-	-	-	-
Beat & Month FE	-	-	Y	-	-	-	-
Beat-year-week-shift FE	-	-	-	Y	Y	Y	Y
Officer FE	-	-	-	-	Y	Y	Y
Call Controls	-	-	-	-	-	Y	Y
Interactions	-	-	-	-	-	-	Y
Panel B: Logit (Odds Ratio)							
Opposite Race Officer							
(White Officer*Pr Black Civilian)	5.708*	5.391*	5.444*	5.195*	14.86**	10.24**	
	(5.273)	(4.705)	(4.753)	(4.666)	(15.78)	(11.69)	
Observations	1233139	1208695	1208695	998518	71418	71418	
Outcome Mean	0.0000762	0.0000778	0.0000778	0.0000941	0.00132	0.00132	
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	
Police Area FE	-	Y	-	-	Y	Y	
Police Area & Month FE	-	-	Y	-	-	-	
Police Area-x-Year FE	-	-	-	Y	-	-	
Officer FE	-	-	-	-	Y	Y	
Additional Controls	-	-	-	-	-	Y	

Exponentiated coefficients; Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the coefficient on the interaction of *White Officer* and *Proportion Black Civilian* from the regression of *Gun Use of Force* on indicators for officer race, proportion citizen black, and the interaction term. For OLS specifications, column 3 adds controls for home beat, latitude, longitude, time between call and dispatch, per capita income, unemployment, and proportion with less than a high school degree, as well as fixed effects for day of the week, priority of call, call description, and call taker. For Logit specifications, column 6 adds controls for per capita income, unemployment, and proportion with less than a high school degree, and fixed effects for priority of call. Police areas are roughly five times as large as a beat and are used instead of police beats to attain convergence of the logit model. Robust standard errors clustered at the officer level are reported in parentheses and two-way clustered (officer and beat) standard errors are reported in brackets. The empirical two-sided p-values from randomization inference for estimates in columns 1 through 5 of Panel A are 0.0994, 0.0971, 0.0955, 0.0715 and 0.0000, respectively.

Table 7: **Second City** Estimated Difference in Use of Force by Officer Race

	(1) Use of Force	(2) Use of Force	(3) Use of Force	(4) Use of Force	(5) Use of Force
Panel A: OLS					
White Officer	0.0000575	0.0000774	0.0000790	0.0000956	0.0000729
	(0.0000836)	(0.0000831)	(0.0000832)	(0.0000824)	(0.0000783)
	[0.0000854]	[0.0000839]	[0.0000841]	[0.0000826]	[0.0000783]
Observations	938562	938562	938562	938562	938562
Outcome Mean	0.000940	0.000940	0.000940	0.000940	0.000940
Beat FE	-	Y	-	-	-
Beat & Month FE	-	-	Y	-	-
Beat-year-month, Beat-shift FE	-	-	-	Y	Y
Call Controls	-	-	-	-	Y
Panel B: Logit (Odds Ratio)					
White Officer	1.046	1.102	1.103	1.149*	1.130*
	(0.0767)	(0.0783)	(0.0781)	(0.0826)	(0.0782)
Observations	938562	937796	937796	937796	937796
Outcome Mean	0.003	0.003	0.003	0.003	0.003
Police Area FE	-	Y	-	-	-
Police Area & Month FE	-	-	Y	-	-
Police Area-x-Year FE	-	-	-	Y	Y
Additional Controls	-	-	-	-	Y

Exponentiated coefficients; Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the coefficient on $White\ Officer$ from the regression of $Use\ of\ Force$ on an indicator for officer race. For OLS specifications, column 5 adds controls for latitude, longitude, per capita income, unemployment, proportion with less than a high school degree, as well as fixed effects for years of experience, day of the week, hour of dispatch, priority of call, call description, call source, multi-agency call, officer gender, and officer home beat, as proxied by the beat to which he responded to the most calls. For Logit specifications, column 5 adds controls for per capita income, proportion with less than a high school degree, and officer hire date, as well as fixed effects for officer gender, call priority, and day of week. Police areas in this city are ten times as large as a beat and are used instead of police beats to attain convergence of the logit model. Robust standard errors clustered at the officer level are reported in parentheses and two-way clustered (officer and beat) standard errors are reported in brackets. The empirical two-sided p-value from randomization inference for the estimate in column 3 of Panel A is 0.1488.

Table 8: **Second City** The Effect of Different-Race Police Officers on Use of Force

	(1) Use of Force	(2) Use of Force	(3) Use of Force	(4) Use of Force	(5) Use of Force	(6) Use of Force	(7) Use of Force
Panel A: OLS							
Opposite Race Officer							
(White Officer*Pr Minority Civilians)	0.00101***	0.000991***	0.000996***	0.000840**	0.000694*	0.000716*	0.000306
	(0.000373)	(0.000381)	(0.000381)	(0.000393)	(0.000398)	(0.000392)	(0.000542)
	[0.000397]	[0.000407]	[0.000407]	[0.000407]	[0.000407]	[0.000411]	[0.000586]
Observations	938562	938562	938562	938562	938562	938562	938562
Outcome Mean	0.000940	0.000940	0.000940	0.000940	0.000940	0.000940	0.000940
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	Y
Beat FE	-	Y	-	-	-	-	-
Beat & Month FE	-	-	Y	-	-	-	-
Beat-year-month, Beat-shift FE	-	-	-	Y	Y	Y	Y
Officer FE	-	-	-	-	Y	Y	Y
Call Controls	-	-	-	-	-	Y	Y
Interactions	-	-	-	-	-	-	Y
Panel B: Logit (Odds Ratio)							
Opposite Race Officer							
(White Officer*Pr Minority Civilians)	2.576**	2.628**	2.611**	2.770**	2.231*	2.228*	
	(1.104)	(1.089)	(1.083)	(1.129)	(0.962)	(0.984)	
Observations	938562	937796	937796	937796	863920	863920	
Outcome Mean	0.003	0.003	0.003	0.003	0.003	0.003	
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	
Police Area FE	-	Y	-	-	Y	Y	
Police Area & Month FE	-	-	Y	-	-	-	
Police Area-x-Year FE	-	-	-	Y	-	-	
Officer FE	-	-	-	-	Y	Y	
Additional Controls	-	-	-	-	-	Y	

Exponentiated coefficients; Standard errors in parentheses

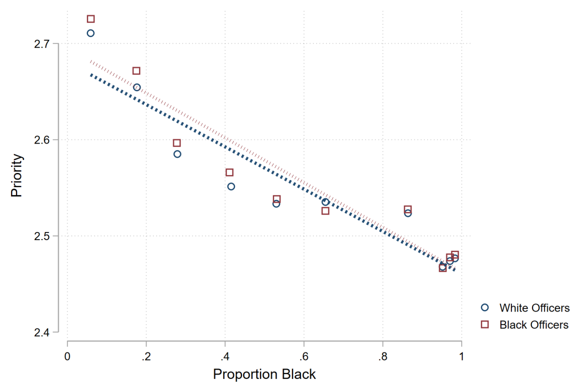
* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the coefficient on the interaction of $White\ Officer$ and $Proportion\ Minority\ Civilians$ from the regression of $Use\ of\ Force$ on indicators for officer race, proportion citizen black, and the interaction term. For OLS specifications, column 6 adds controls for latitude, longitude, per capita income, unemployment, proportion with less than a high school degree, date, as well as fixed effects for years of experience, day of the week, hour of dispatch, priority of call, call description, call source, and multi-agency call. For Logit specifications, column 6 adds controls for per capita income, proportion with less than a high school degree, and officer hire date, as well as fixed effects for call priority and day of week. Column 7 adds interactions for every call characteristic added in column 6 interacted with officer race. Police areas in this city are ten times as large as a beat and are used instead of police beats to attain convergence of the logit model. Robust standard errors clustered at the officer level are reported in parentheses and two-way clustered (officer and beat) standard errors are reported in brackets. The empirical two-sided p-values from randomization inference for estimates in columns 1 through 5 of Panel A are 0.015, 0.0171, 0.0165, 0.1076 and 0.1808, respectively.

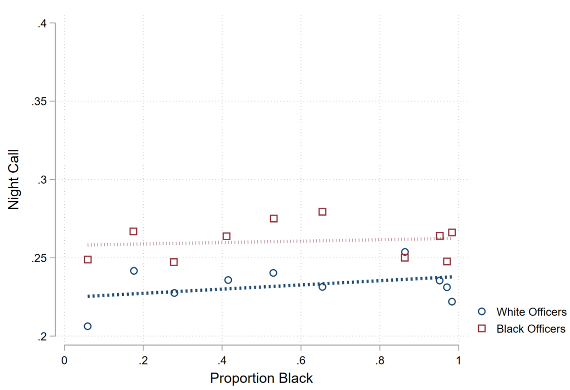
A Appendix (For online publication)

Figure A1: Call Priority, Night Calls, and Number of Calls per Beat by Officer Race & Civilian Race

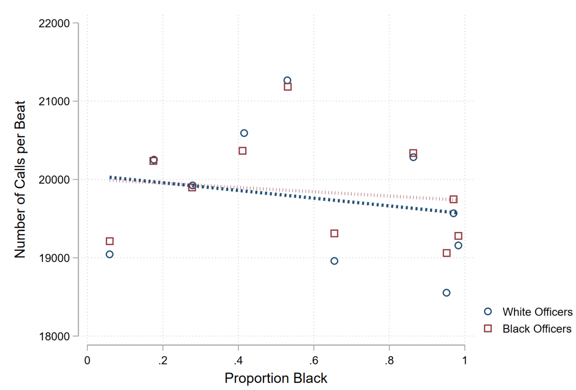
(a) Priority



(b) Night Calls



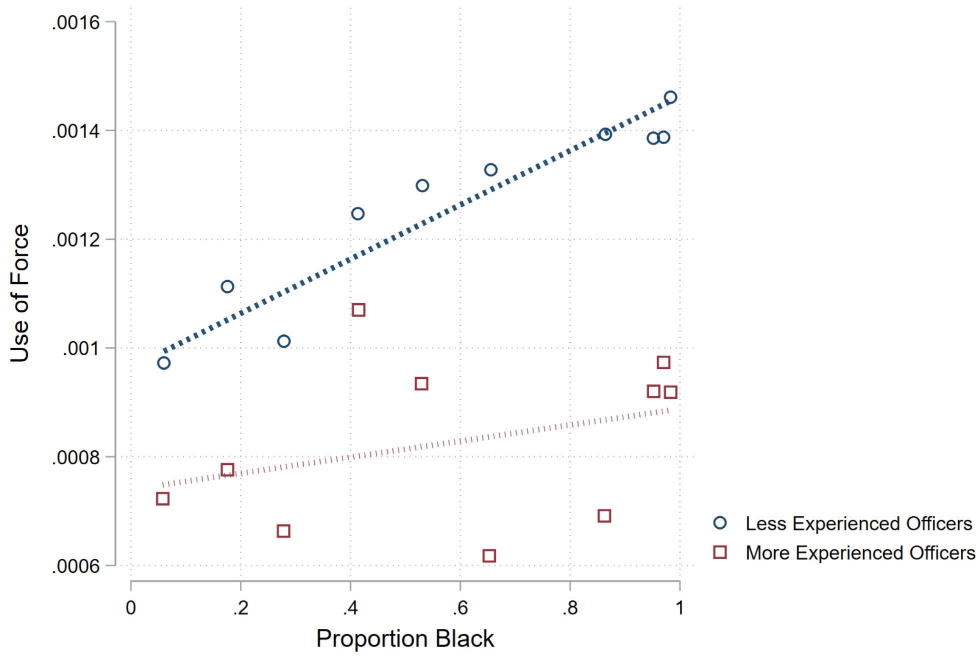
(c) Number of Calls per Beat



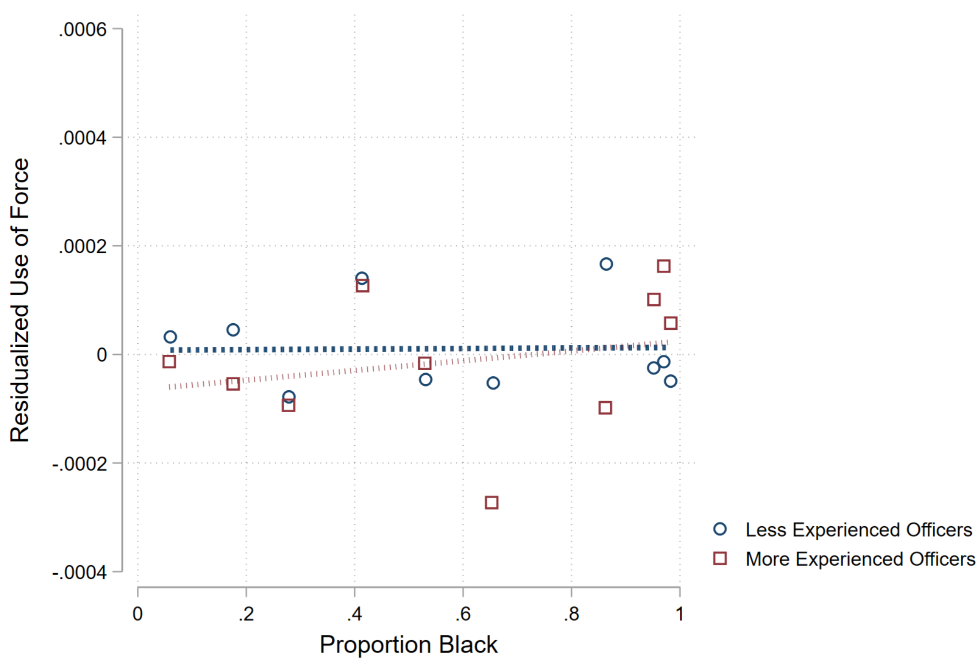
Notes: In Panels (a), (b), and (c) we plot call priority,night call (whether a call was made between 10 pm and 5 am), and number of calls per beat. Observations are grouped so that each point includes an equal number of calls. The average number of calls per beat is 19800 and 24% of calls occur between 10 pm and 5 am.

Figure A2: Actual Use of Force by Officer Experience & Civilian Race

(a) Use of Force



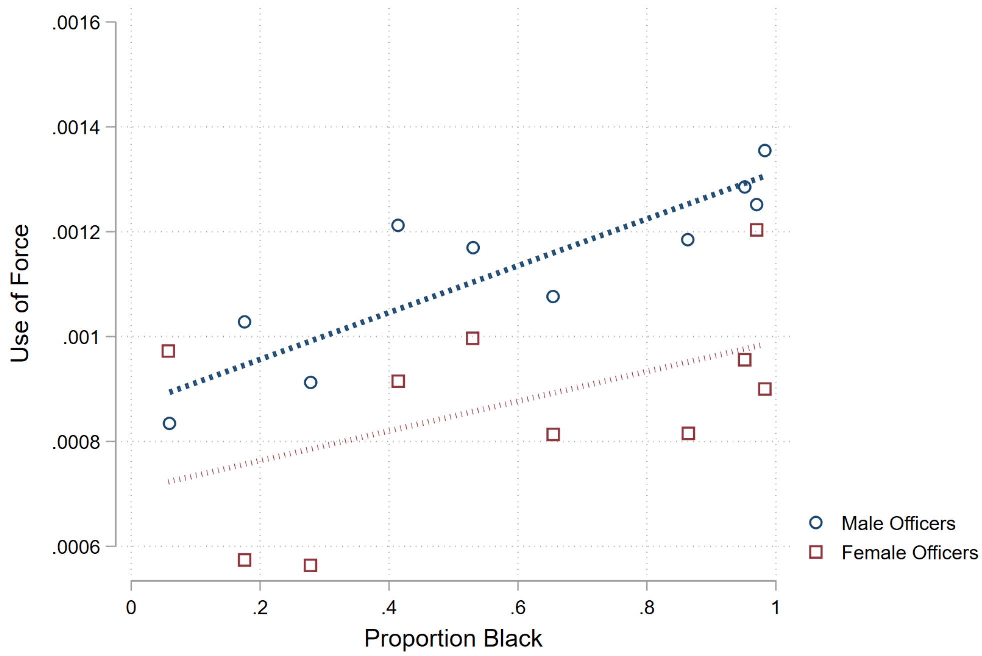
(b) Residualized Use of Force



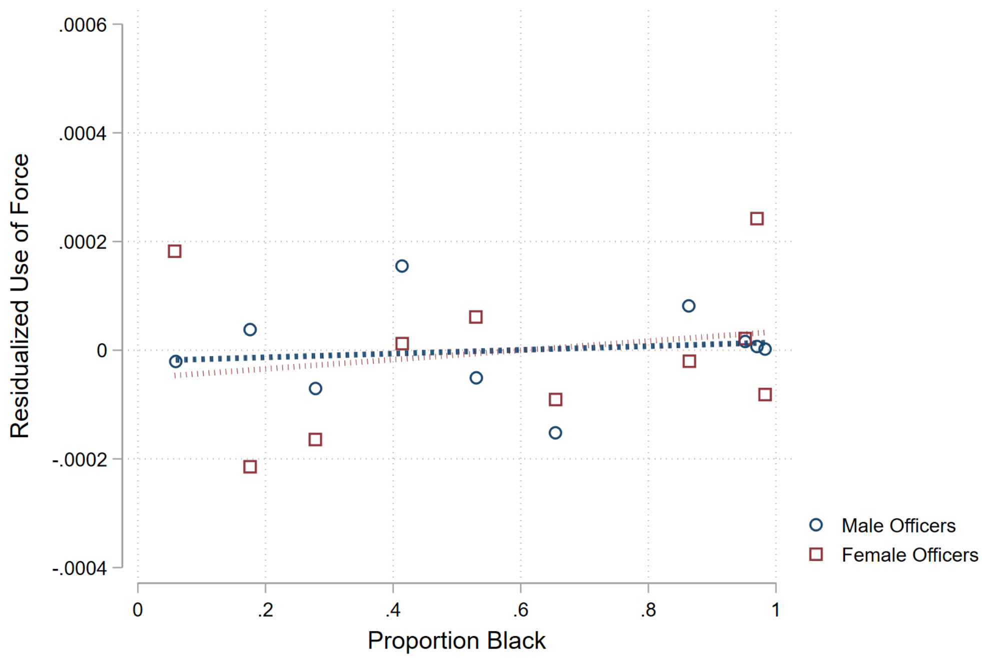
Notes: In Panel (a) we plot use of force with a gun. In Panel (b) we plot residualized (beat-year-week-shift fixed effects are removed) use of force with a gun. The fitted line is a linear fit across all gun use of force rates. Observations are grouped so that each point includes an equal number of calls.

Figure A3: Actual Use of Force by Officer Gender & Civilian Race

(a) Use of Force



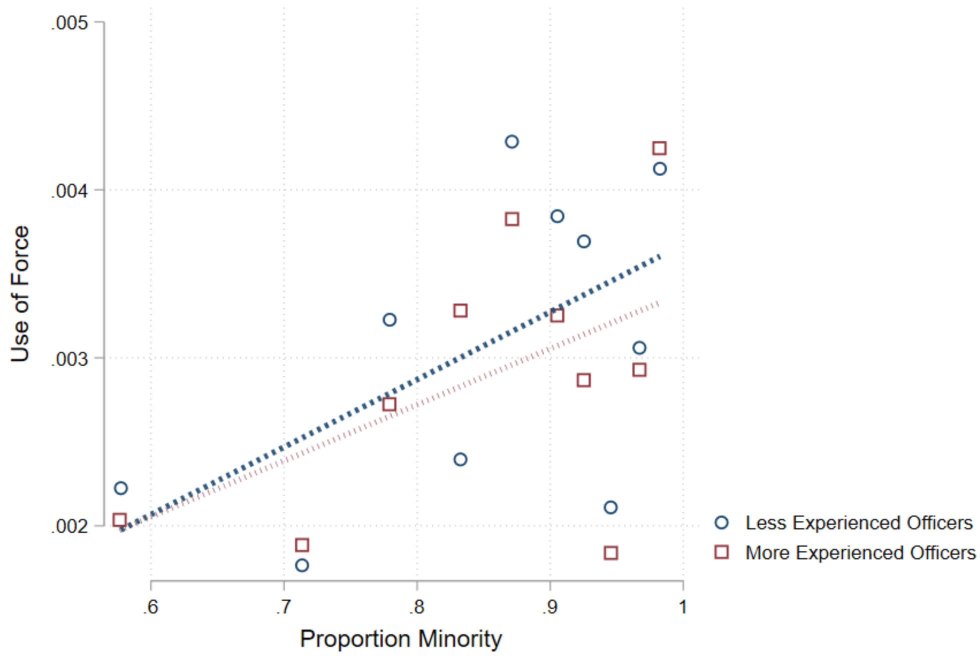
(b) Residualized Use of Force



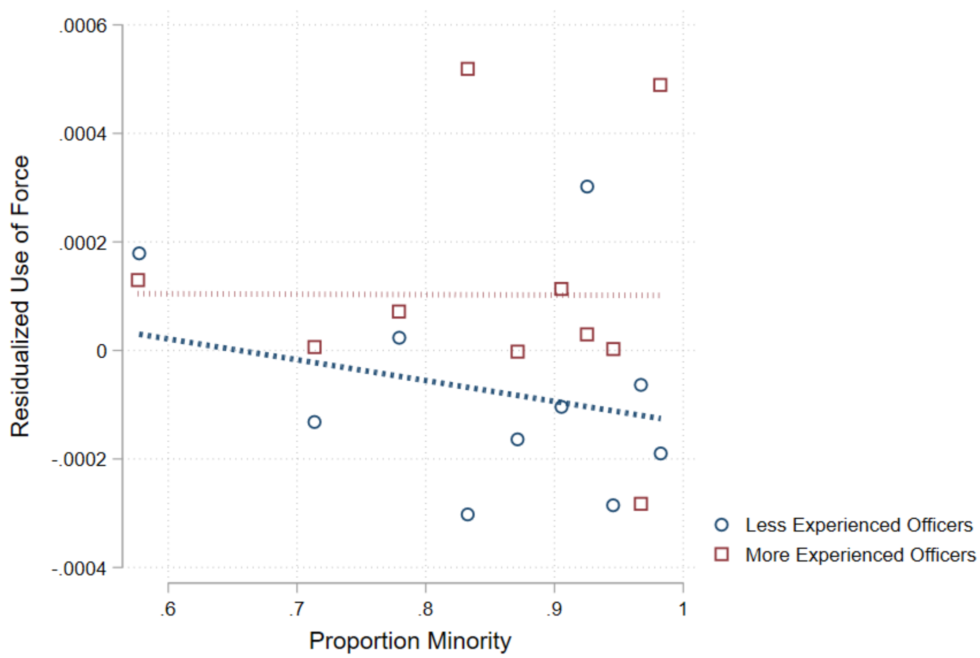
Notes: In Panel (a) we plot use of force with a gun. In Panel (b) we plot residualized (beat-year-week-shift fixed effects are removed) use of force with a gun. The fitted line is a linear fit across all gun use of force rates. Observations are grouped so that each point includes an equal number of calls.

Figure A4: **Second City** Actual Use of Force by Officer Experience & Civilian Race

(a) Use of Force



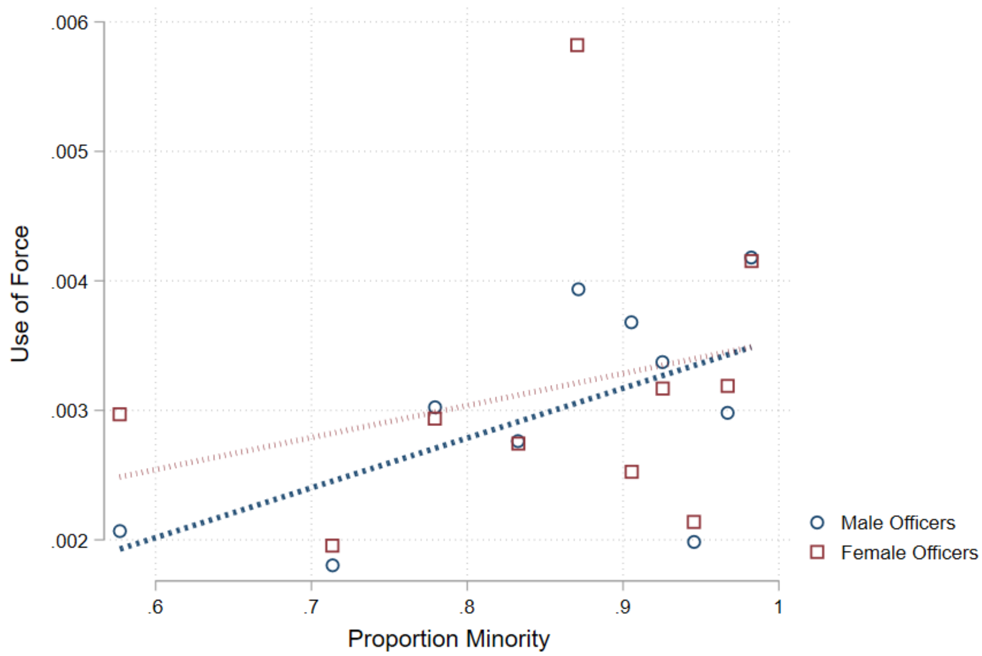
(b) Residualized Use of Force



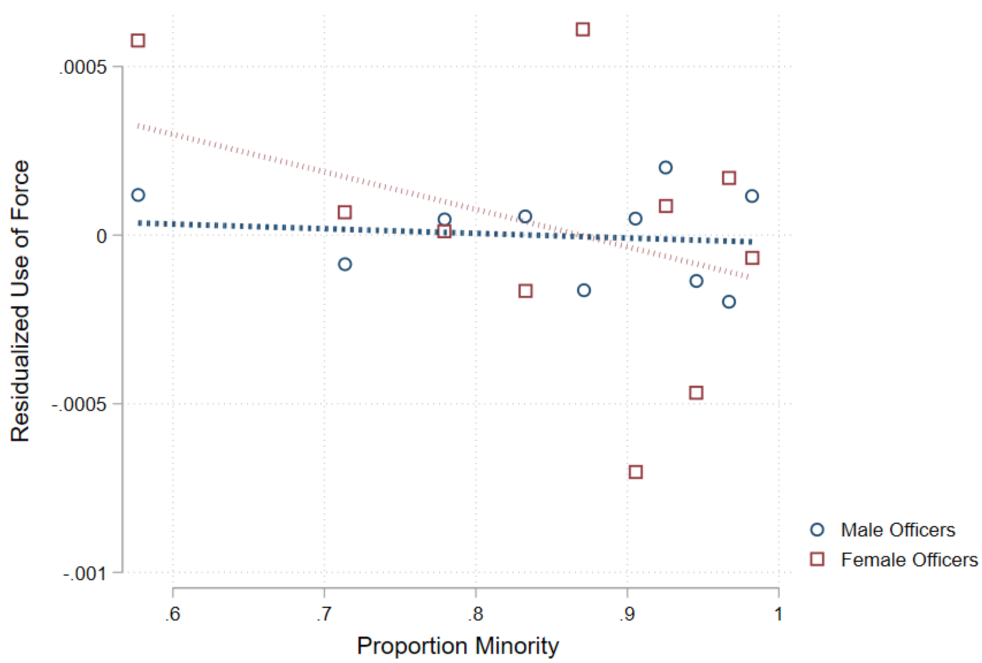
Notes: Here we plot the average use of force for 10 bins in panel (a) or residualized (beat-year-month, and beat-shift are removed) use of force in panel (b). The fitted line is a linear fit across all use of force rates. Observations are grouped so that each point includes an equal number of calls.

Figure A5: **Second City** Actual Use of Force by Officer Gender & Civilian Race

(a) Use of Force



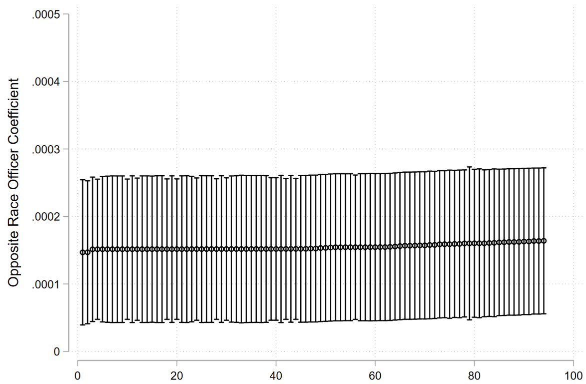
(b) Residualized Use of Force



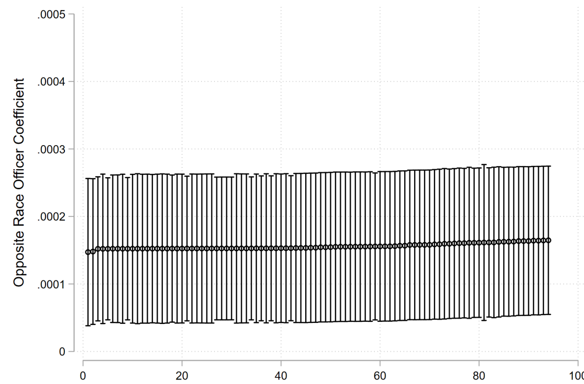
Notes: Here we plot the average use of force for 10 bins in panel (a) or residualized (beat-year-month, and beat-shift are removed) use of force in panel (b). The fitted line is a linear fit across all use of force rates. Observations are grouped so that each point includes an equal number of calls.

Figure A6: Distribution of Coefficients After Dropping One Incident of Use of Force with a Gun

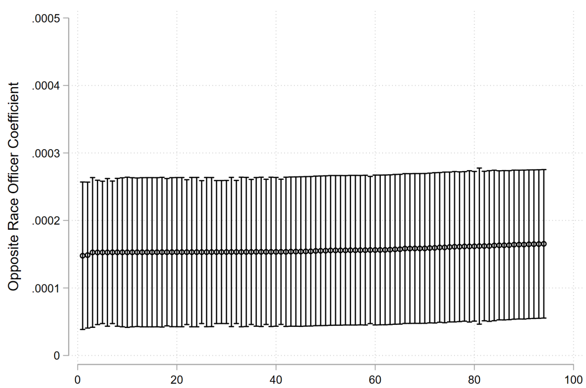
(a) Column 1 Table 6



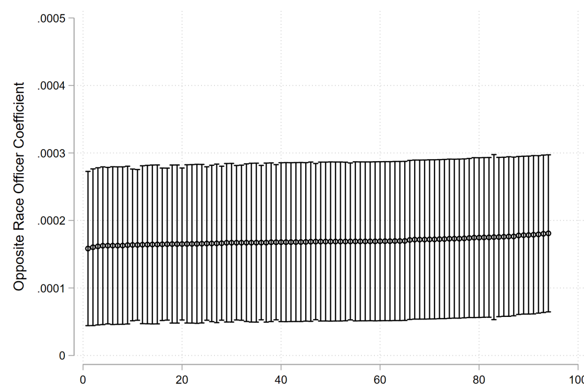
(b) Column 2 Table 6



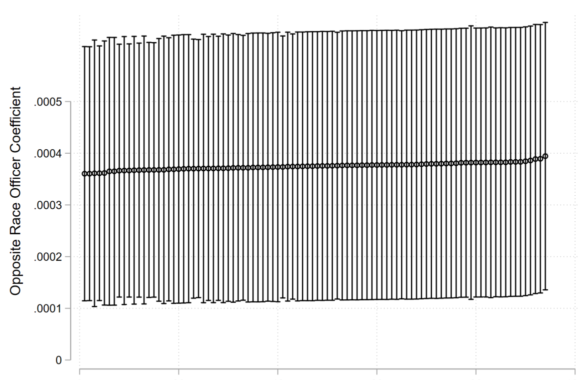
(c) Column 3 Table 6



(d) Column 4 Table 6



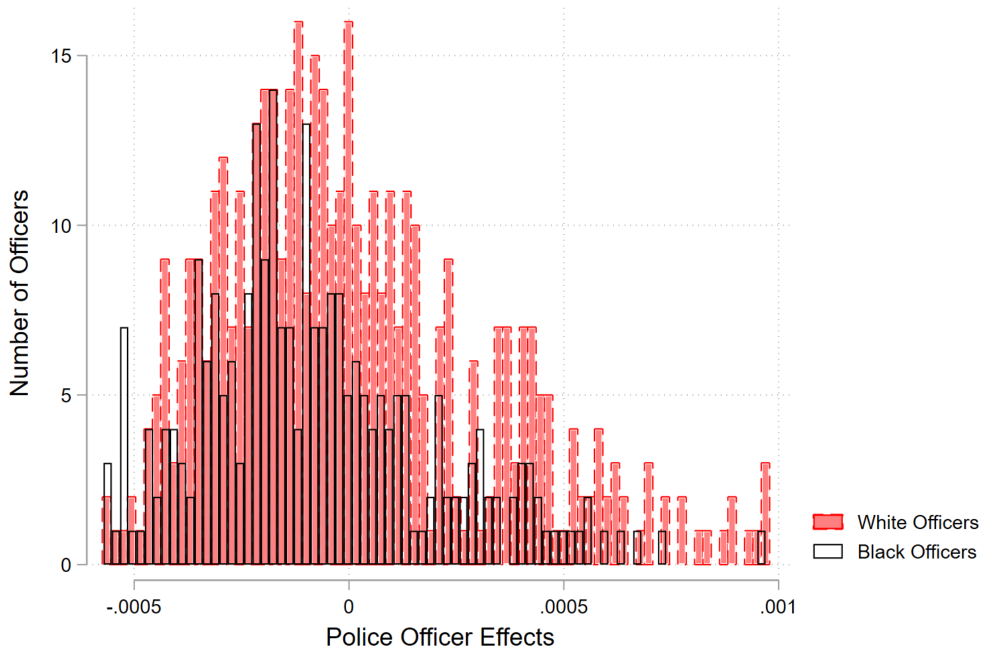
(e) Column 5 Table 6



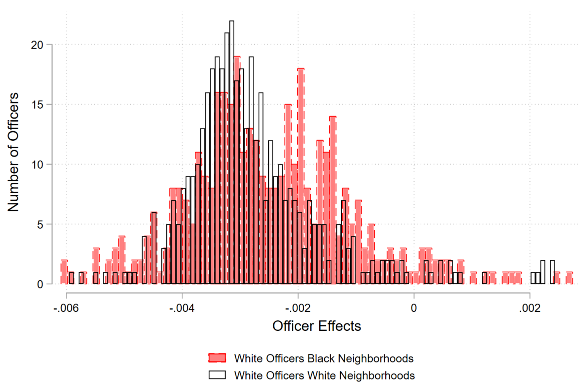
Notes: Figures represent the distribution of coefficents from dropping one incident of use of force with a gun for each of the first 5 columns in Table 6.

Figure A7: Distribution of Individual Police Officer Effects by Officer Race

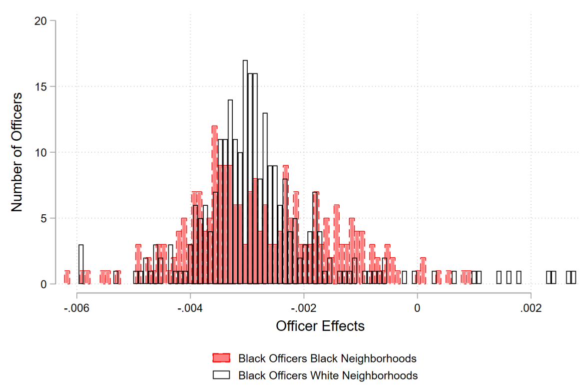
(a) White and Black Officer Effects



(b) White Officer Effects by Neighborhood



(c) Black Officer Effects by Neighborhood



Notes: Figures represent the distribution of individual police officer effects (Bayes shrinkage) by police officer and neighborhood.

Table A1: Robustness to Defining Use of Force at Individual Officer Level: The Effect of Opposite-Race Police Officers on Use of Force

	(1) Use of Force	(2) Use of Force	(3) Use of Force	(4) Use of Force	(5) Use of Force	(6) Use of Force	(7) Use of Force
Panel A: OLS							
Opposite Race Officer							
(White Officer*Pr Black Civilian)	0.000400**	0.000415**	0.000417**	0.000528***	0.000606**	0.000622**	0.000350
	(0.000191)	(0.000191)	(0.000192)	(0.000193)	(0.000280)	(0.000278)	(0.000302)
Observations	1233139	1233139	1233139	1233139	1233139	1233139	1233139
Outcome Mean	0.00105	0.00105	0.00105	0.00105	0.00105	0.00105	0.00105
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	Y
Beat FE	-	Y	-	-	-	-	-
Beat & Month FE	-	-	Y	-	-	-	-
Beat-year-week-shift FE	-	-	-	Y	Y	Y	Y
Officer FE	-	-	-	-	Y	Y	Y
Call Controls	-	-	-	-	-	Y	Y
Interactions	-	-	-	-	-	-	Y
Panel B: Logit (Odds Ratio)							
Opposite Race Officer							
(White Officer*Pr Black Civilian)	1.281	1.284	1.286	1.280	1.551	1.404	
	(0.263)	(0.266)	(0.267)	(0.264)	(0.447)	(0.396)	
Observations	1233139	1233128	1233128	1230375	819937	819937	
Outcome Mean	0.00105	0.00105	0.00105	0.00106	0.00158	0.00158	
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	
Police Area FE	-	Y	-	-	Y	Y	
Police Area & Month FE	-	-	Y	-	-	-	
Police Area-x-Month FE	-	-	-	Y	-	-	
Officer FE	-	-	-	-	Y	Y	
Additional Controls	-	-	-	-	-	Y	

Exponentiated coefficients; Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: We define use of force at the individual officer level, rather than at the call level as in the main results. This table reports the coefficient on the interaction of $White\ Officer$ and *Proportion Black Civilian* from the regression of $Use\ of\ Force$ on indicators for officer race, proportion citizen black, and the interaction term. For OLS specifications, column 6 adds controls for home beat, latitude, longitude, time between call and dispatch, per capita income, unemployment, and proportion with less than a high school degree, as well as fixed effects for day of the week, priority of call, call description, and call taker. Column 7 adds interactions for every call characteristic added in column 6 interacted with officer race. For Logit specifications, column 6 adds controls for per capita income, unemployment, and proportion with less than a high school degree, and fixed effects for priority of call. Robust standard errors clustered at the officer level are reported in parentheses.

Table A2: Robustness to Defining Use of Force at Individual Officer Level: The Effect of Opposite Race Police Officers on Use of Force with a Gun

	(1) Gun Use of Force	(2) Gun Use of Force	(3) Gun Use of Force	(4) Gun Use of Force	(5) Gun Use of Force	(6) Gun Use of Force	(7) Gun Use of Force
Panel A: OLS							
Opposite Race Officer							
(White Officer*Pr Black Civilian)	0.000146***	0.000147***	0.000148***	0.000159***	0.000381***	0.000370***	0.000273***
	(0.0000556)	(0.0000565)	(0.0000566)	(0.0000600)	(0.000138)	(0.000137)	(0.000102)
Observations	1233139	1233139	1233139	1233139	1233139	1233139	1233139
Outcome Mean	0.0000706	0.0000706	0.0000706	0.0000706	0.0000706	0.0000706	0.0000706
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	Y
Beat FE	-	Y	-	-	-	-	-
Beat & Month FE	-	-	Y	-	-	-	-
Beat-year-week-shift FE	-	-	-	Y	Y	Y	Y
Officer FE	-	-	-	-	Y	Y	Y
Call Controls	-	-	-	-	-	Y	Y
Interactions	-	-	-	-	-	-	Y
Panel B: Logit (Odds Ratio)							
Opposite Race Officer							
(White Officer*Pr Black Civilian)	4.488*	4.443*	4.483*	4.267	15.29**	10.40*	
	(3.864)	(3.797)	(3.833)	(3.767)	(17.68)	(13.20)	
Observations	1233139	1208695	1208695	998518	65584	65584	
Outcome Mean	0.0000706	0.0000720	0.0000720	0.0000871	0.00133	0.00133	
Officer and Civilian Race Controls	Y	Y	Y	Y	Y	Y	
Police Area FE	-	Y	-	-	Y	Y	
Police Area & Month FE	-	-	Y	-	-	-	
Police Area-x-Month FE	-	-	-	Y	-	-	
Officer FE	-	-	-	-	Y	Y	
Additional Controls	-	-	-	-	-	Y	

Exponentiated coefficients; Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: We define use of force at the individual officer level, rather than at the call level as in the main results. This table reports the coefficient on the interaction of $White\ Officer$ and *Proportion Black Civilian* from the regression of *Gun Use of Force* on indicators for officer race, proportion citizen black, and the interaction term. For OLS specifications, column 6 adds controls for home beat, latitude, longitude, time between call and dispatch, per capita income, unemployment, and proportion with less than a high school degree, as well as fixed effects for day of the week, priority of call, call description, and call taker. Column 7 adds interactions for every call characteristic added in column 6 interacted with officer race. For Logit specifications, column 6 adds controls for per capita income, unemployment, and proportion with less than a high school degree, and fixed effects for priority of call. Robust standard errors clustered at the officer level are reported in parentheses.

Table A3: Alternative Specifications

	Use of Force	Use of Force	Use of Force	Use of Force
Panel A: Use of Force				
Opposite Race Officer (White Officer*Pr Black Civilian)	0.000613**			
	(0.000277)			
Opposite Race Officer (Black Officer*Pr White Civilian)		0.000700**		
		(0.000297)		
Same Race Officer (Black Officer*Pr Black Civilian)			-0.000613**	
			(0.000277)	
Same Race Officer (White Officer*Pr White Civilian)				-0.000700**
				(0.000297)
Observations	1233139	1233139	1233139	1233139
Outcome Mean	0.00106	0.00106	0.00106	0.00106
	Gun Use of Force	Gun Use of Force	Gun Use of Force	Gun Use of Force
Panel B: Gun Use of Force				
Opposite Race Officer (White Officer*Pr Black Civilian)	0.000379***			
	(0.000133)			
Opposite Race Officer (Black Officer*Pr White Civilian)		0.000383***		
		(0.000141)		
Same Race Officer (Black Officer*Pr Black Civilian)			-0.000379***	
			(0.000133)	
Same Race Officer (White Officer*Pr White Civilian)				-0.000383***
				(0.000141)
Observations	1233139	1233139	1233139	1233139
Outcome Mean	0.0000710	0.0000710	0.0000710	0.0000710

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the coefficient for each possible interaction term. Every specification includes the relevant officer and civilian race controls (For example, in the second row the regression includes a control for proportion white civilians). Each column represents a separate regression. All specifications include beat-year-week-shift and individual officer fixed effects. Robust standard errors are clustered at the officer level.

Table A4: **Second City** Summary Statistics

	(1) Entire Sample	(2) Minority Officers	(3) White Officers
Outcomes			
Use of Force	0.00299	0.00297	0.00311
Call Characteristics			
Proportion Minority Civilian	0.849	0.852	0.831
	(0.125)	(0.124)	(0.130)
Proportion Hispanic Civilian	0.813	0.817	0.789
	(0.147)	(0.145)	(0.155)
Proportion Black Civilian	0.0362	0.0352	0.0423
	(0.0357)	(0.0349)	(0.0400)
Per Capita Income	19142.9	19093.2	19438.5
	(9587.6)	(9574.8)	(9658.3)
Proportion Unemployed	0.0834	0.0835	0.0824
	(0.0623)	(0.0625)	(0.0610)
Proportion Less than HS Degree	0.251	0.254	0.235
	(0.172)	(0.173)	(0.163)
Minority Officer	0.856	1	0
Hispanic Officer	0.829	0.969	0
Black Officer	0.0268	0.0313	0
Female Officer	0.0893	0.0901	0.0848
Years of Experience	8.168	8.369	6.967
	(6.725)	(6.900)	(5.421)
Longitude	-211.3	-211.3	-211.3
	(0.185)	(0.186)	(0.182)
Latitude	31.13	31.13	31.14
	(0.0584)	(0.0576)	(0.0610)
Hour Dispatched	12.93	12.98	12.62
	(7.386)	(7.311)	(7.810)
Priority	5.338	5.340	5.329
	(1.896)	(1.893)	(1.914)
Multi-Agency Call	0.852	0.852	0.854
Observations	938562	803494	135068

Standard deviations in parentheses

Notes: This table reports mean, standard deviation, and number of observations for each variable. Use of force is measured at the call level and takes on a value of one if the call ended in a use of force. Priority, latitude and longitude have been altered (multiplied by a random number) to protect the identity of our city. Multi-Agency takes on a value of one if other agencies (e.g. Fire Department) were dispatched to a call.

Table A5: **Second City** Correlation Between Call Characteristics and Officer Race

	(1) Proportion Minority	(2) Proportion Hispanic	(3) Proportion Black	(4) Per Capita Income	(5) Proportion Unemployed	(6) Proportion Less than HS Degree	(7) Home Beat	(8) X Coord.	(9) Y Coord.	(10) Time Dispatched	(11) Call Priority	(12) Multi Agency
Panel A: Unconditional												
White Officer	-0.0219***	-0.0293***	0.00734***	314.3	-0.000673	-0.0189***	-0.00472	-0.0202	0.0169***	-0.247	0.0138	0.00569*
	(0.00329)	(0.00452)	(0.00167)	(225.5)	(0.00139)	(0.00565)	(0.00521)	(0.0126)	(0.00333)	(0.381)	(0.0241)	(0.00331)
Observations	938562	938562	938562	938562	938562	938562	938562	938562	938562	938562	938562	938562
Outcome Mean	0.848	0.812	0.0365	19169.1	0.0836	0.251	0.109	-211.3	31.13	13.09	5.575	0.888
Panel B: Beat FE												
White Officer	0.000106	0.0000235	0.0000827	-23.17	0.000494***	0.000408	-0.00241	0.0000242	-0.0000189	-0.205	0.0174	0.00547*
	(0.000280)	(0.000313)	(0.0000556)	(14.39)	(0.000143)	(0.000268)	(0.00238)	(0.0000317)	(0.0000176)	(0.378)	(0.0228)	(0.00316)
Observations	938562	938562	938562	938562	938562	938562	938562	938562	938562	938562	938562	938562
Outcome Mean	0.848	0.812	0.0365	19169.1	0.0836	0.251	0.109	-211.3	31.13	13.09	5.575	0.888
Panel C: Beat-time FE												
White Officer	0.000129	0.0000693	0.0000593	-19.26	0.000432***	0.000399*	-0.00233	0.0000243	-0.0000190	-0.247	0.0138	0.00331
	(0.000228)	(0.000247)	(0.0000463)	(12.84)	(0.000139)	(0.000236)	(0.00222)	(0.0000273)	(0.0000162)	(0.381)	(0.0241)	(0.00281)
Observations	938562	938562	938562	938562	938562	938562	938562	938562	938562	938562	938562	938562
Outcome Mean	0.848	0.812	0.0365	19169.1	0.0836	0.251	0.109	-211.3	31.13	13.09	5.575	0.888

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the coefficient on $White\ Officer$ from separate regressions of call characteristics on a binary variable representing officer race. Panel C includes beat-year-month and beat-shift fixed effects. Standard errors are clustered at the officer level. Priority, latitude, and longitude have been altered (multiplied by a random number) to protect the identity of our city.