

Everyday Risk Exposure Disproportion and Racial Disparities in Police Violence *

Laurel Eckhouse [†]

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Abstract

Scholarship on police violence has focused largely on the moment of decision, identifying cognitive and psychological causes of implicit bias among police officers. This research strategy poses both analytical and policy problems: it separates police violence from the remainder of the carceral state and identifies causes of racial inequality with no well-substantiated remedies. I argue that what I call *exposure disproportion* is a more significant cause of racial disparities in police violence. Using national data on police killings, I show that racial differences in exposure to police contact produce larger racial differences in police killings. I examine a novel data set in South Carolina which shows that this is also true for non-lethal shootings.

Finally, using an interrupted time series from New York City, in which a procedural change reduced the exposure of black civilians to casual stops, I show that reductions in exposure can diminish racial disparities in police violence *even when* the probability of violence within a stop rises. Reductions in Stop, Question, and Frisk eliminated 50 incidents of use of force against African Americans *per day* in a 30-day bandwidth; estimates on the full data set show a reduction of as many as 185 uses of force against black civilians *each day* from stopping fewer civilians.

After Mike Brown's death in Ferguson in 2014, scholars and journalists hastened to develop adequate measures of police violence, and to understand the nature and origins of the disproportionate risk of police violence faced by black and Native Americans, especially young men. The findings were stark. Federal records drastically understate the number of police killings (Ball, 2016). As

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[†]Doctoral Candidate, Department of Political Science, University of California at Berkeley. eckhouse@berkeley.edu. Prepared for New Faces in Political Methodology, April 2017.

many as one in five homicides committed by strangers are committed by police (Ball, 2016) and the death rates for black and Native Americans are over twice the death rates for white Americans. Unarmed black Americans are nearly five times as likely to be shot by police as unarmed white Americans (Zimring, 2017)). As Zimring writes, “the circumstances of the Michael Brown killing were typical of police killings rather than singular” (Zimring, 2017)). Shaw’s introduction to the Department of Justice investigation of the Ferguson police department makes a similar point:

Ferguson did not happen in a vacuum. Police killings of unarmed individuals are, unfortunately, not uncommon. While the facts of each case are different, there is a numbing familiarity when an unarmed black boy, teenager, or man is killed by a police officer. A well-worn script unfolds after each death: The police officer recounts a threat to his life, which allegedly includes a weapon. The dead black man is dehumanized and demonized through the release of any record of past wrongdoing in an attempt to implant the worthy-of-death notion in the public’s mind. In most instances, state and local authorities do not bring charges against the officer. In the rare instances when there is an indictment, the officer is, more often than not, cleared of wrongdoing. In many cases, the family of the decedent and community activists seek federal review and prosecution, usually without success. (Shaw et al., 2015))

And yet, when scholars, journalists, and activists tried to unpack the causes of this racial disproportion in police violence, they focused largely on the moment of decision. Police, like other Americans, are more likely to see “gun” in an ambiguous object if the person carrying it is black (Greenwald, Oakes and Hoffman, 2003)). Police, like other Americans, are more likely to shoot in experimental situations when the target is black (Greenwald, Oakes and Hoffman, 2003; Plant and Peruche, 2005; Correll et al., 2007)). These biases are a substantial concern, but they are also difficult to change (Sim, Correll and Sadler, 2013; Eberhardt et al., 2004). Moreover, as I show in this chapter, equalizing the probability of being shot in any given interaction would still leave

tremendous racial disparities.

In focusing on *how police react* in highly charged situations where they are considering using force, scholars separate police violence, and especially police homicide, from the rest of the carceral state. In fact, these extreme instances of state violence have everything to do with the normal operation of the criminal justice system, and with the most mundane interactions between citizens and the criminal justice system.

On July 6, 2016, Philando Castile was pulled over in Falcon Heights, Minnesota. With him in the car were his girlfriend and her four-year-old daughter. Within minutes of the beginning of the stop, police officer Jeronimo Yanez had shot Castile four times (LaFraniere and Smith, 2016). Nationwide protests erupted in the wake of Castile's death – and the previous day's shooting of Alton Sterling in Louisiana. Dozens of protesters were arrested in the Twin Cities, and at protests around the country. As reporters uncovered the details, they learned that it was at least the 49th time Castile had been pulled over in 13 years (LaFraniere and Smith, 2016).

Journalists reported on Castile's many stops as an indicator of the heavy weight of law enforcement borne by low-income black communities. Castile's encounters with law enforcement, almost entirely over minor issues such as broken taillights, cost him over six thousand dollars in fines (Peralta and Corley, 2016). An extensive literature documents the consequences of these routine stops for African Americans: fines and fees paid, but also time spent in court and waiting rooms, suspended licenses, warrants, civic disengagement, lost jobs, and evictions (Goffman, 2014; Weaver and Lerman, 2010; Brayne, 2014; Lerman and Weaver, 2014a; Harris, 2016).

Each encounter, though, also carries the risk of escalation. Castile was stopped for a broken taillight; the officer also thought he resembled a robbery suspect. Castile was carrying a firearm – one he was licensed to carry under Minnesota state law, and one which he informed the officer of immediately (LaFraniere and Smith, 2016). Despite the law-abiding character of his behavior and the minor causes of the stop, Castile lost his life.

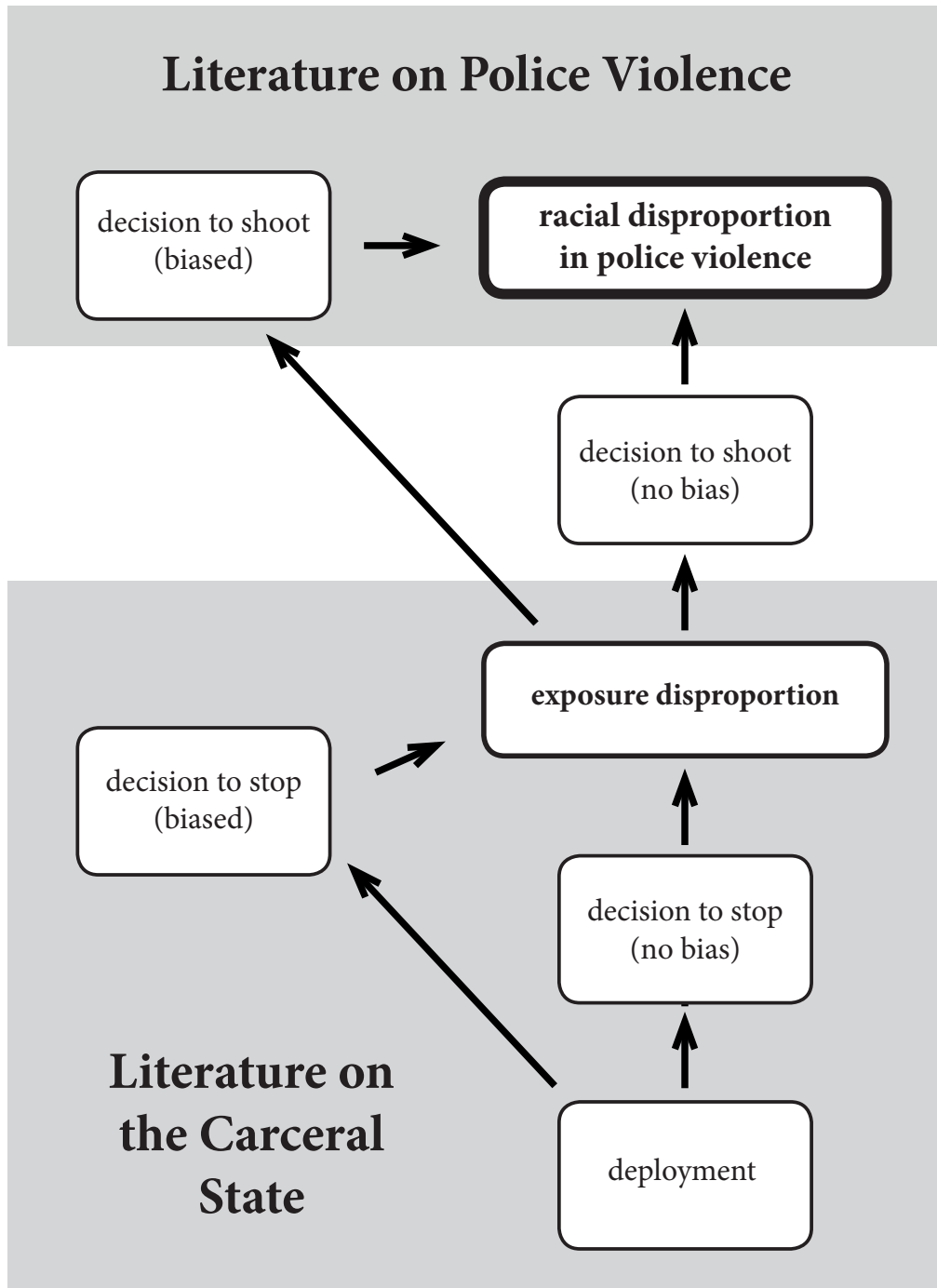
Given a fixed risk of being shot in any encounter with police – even a risk that is fixed at the individual level, rather than at the population level – any individual person’s lifetime risk of being shot is a function both of the probability of being shot in any given encounter and the number of total encounters. Thus, racial disproportion in the probability of being shot by police can originate not only in the decision to shoot, but in any of the prior steps that led to the interaction.

1 The Origins of Disproportion

The ultimate outcome of interest is the severe racial disproportion in police violence. Overall, the risk of being shot by police for an unarmed black civilian is between 3.49 and 5 times as high as the risk for an unarmed white civilian (Ross, 2015; Zimring, 2017). In many US counties, the risk for black civilians approaches 20 times the risk of white civilians (Ross). The death rates for African Americans are 2.3 times the death rates for whites. Native Americans are killed at 2.06 times the rate at which whites are killed (Zimring, 2017). These figures are much higher than the figures for other, comparable countries (Zimring, 2017).

What causes racial disproportion in shootings? One possible cause – the central one in the emerging scholarship on police violence – is that police make biased decisions about whom to shoot (or, less frequently, tase). By the time the officer is choosing whether to shoot a particular individual, the officer has gone through many steps – training, deployment, and the decision to stop – that can produce racial disproportion *with no bias in the shooting decision*. Figure 1 summarizes this process, which I describe in more detail below. At three key steps – deployment, engagement, and the decision to use force – opportunities for disproportion enter and compound. Deployment and engagement together produce what I call *exposure disproportion*. As I show later in the paper, bias in the shooting decision accounts for far less of the racial disproportion in police violence than exposure disproportion.

First, officers are deployed to a particular location. Ample evidence suggests that police are



disproportionately deployed to low-income black and brown neighborhoods (Goel, Rao and Shroff, 2016; Moskos, 2009; Fagan et al., 2009). Drug enforcement and discretionary stops, in particular, are concentrated in what Soss and Weaver call “race-class subjugated communities” (Soss and Weaver, 2017; Lum and Isaac, 2016). Over the last four decades, “public authorities poured their expanded policing resources into a suite of new techniques such as zero tolerance policing, ‘command and control’ operations, order maintenance, ‘hot spots’ policing, saturation policing, and interventions based on the SARA (Scanning, Analysis, Response, and Assessment) model, all of which gave rise to a higher volume of lower quality arrests and convictions. Thus, as new policing models proliferated in the 1980s, 1990s, and 2000s trumpeted through a string of tactical campaigns titled ‘Operation’ such and such the core preoccupation of policing consolidated around the elimination of disorder and the regulatory enforcement of codes against disordered people and places” (Soss and Weaver, 2017). In the 1990s, the nationwide spread of Compstat led to a further increase in minor arrests. Like other, similar technocratic interventions, Compstat was justified by the Broken Windows theory of policing, which held that policing disorder would reduce serious violent crime. More recent evidence, though, suggests that crime arises from private conflict, not public disorder (O’Brien and Sampson, 2015). Moreover, Compstat did not lead to a significant decline in crime (Eckhouse, 2016).

Today, intensive policing is the norm in race-class subjugated communities (Goffman, 2014; Soss and Weaver, 2017) but rare in wealthier neighborhoods (Moskos, 2009; Goel, Rao and Shroff, 2016). These deployment patterns mean that police officers are more likely to be working near black and brown Americans than near whites. Police training, management, and promotion expectations teach officers to see stops and arrests as indicators of productivity (Goel, Rao and Shroff, 2016). Proximity to race-class subjugated communities means not only that police may be more likely to observe any illegal behavior, but that as they seek out occasions to show productive work according to their organizational benchmarks, they will disproportionately engage with and arrest

people of color – with no individual personal bias at play. Deployment itself can lead to exposure disproportion with no bias.

Second, police officers must choose whether to engage with a particular individual, with or without bias. Assessing bias in the decision to stop is complex, but the available evidence suggests that, even accounting for neighborhood deployment patterns, police are more likely to stop people of color and especially African Americans (Goel, Rao and Shroff, 2016). In a study of Kansas City area traffic stops, black drivers were 270% more likely to be stopped for “investigatory” purposes. These stops are often not the result of overt racial animosity. Rather, they are planned opportunities to investigate, understood by many police officers as “among their most effective tools for finding and arresting criminals and preventing crimes.” Since “even people who are opposed to racism often implicitly perceive others in racially stereotypical ways.... implicit negative racial stereotypes help to support punitive practices like the investigatory stop. In turn, these practices contribute to racial disparities in who is stopped[.]” (Epp, Maynard-Moody and Haider-Markel, 2014). Racial differences in stops – whether they originate in institutional recommendations, implicit bias, or overt animosity – also contribute to exposure disproportion.

While black Americans are 2.3 times as likely to be killed by police as whites generally, after accounting for exposure disproportion (using arrest data, which likely underestimates the extent of exposure disproportion) they are only 1.6 times as likely to be killed. Setting the number of police killings per arrest – the measure of bias in shootings – equal for blacks and whites would have meant 101 fewer deaths of black Americans in 2015. Fixing the black arrest rate at the white arrest rates, with no change to the race-specific ratio between killings and arrests, would eliminate twice as many police killings: 209 deaths. Exposure disproportion plays a very important role in predicting racial disproportion in police shootings.

For police reformers, this is encouraging news. Changing individual-level officer attitudes and behaviors, especially those held at the subconscious level, is a difficult task. We have little

evidence that training programs effectively diminish implicit bias, especially given the socializing effects of police work environments (Moskos, 2009; Christie, Petrie and Timmins, 1996; Paluck and Green, 2009). Even diversifying police forces – a major push over the last several decades, and one supported by black mayors (Saltzstein, 1989) – has not solved racial disparities in policing.

Changing deployment is a different matter. Historically, scholars have expressed concern that police activities are difficult to monitor. Police are street-level bureaucrats at the end of a loosely coupled chain of policy makers and implementers, and must engage independently with situations that remain unobserved by voters, police chiefs, and even their direct supervisors (Wilson, 1978). When policies direct police to change their observed work products, though, the evidence suggests that police often do so. They make more or fewer stops (Moskos, 2009; Mummolo, 2017) and change the composition of those stops to reflect departmental priorities (Eckhouse, 2016).

In the sections that follow, I discuss the challenges of measuring both police violence and exposure effectively. These limitations are substantial, and the product of political and institutional challenges to collecting adequate information about police behavior. I identify two data sets – one on South Carolina, another on New York City’s Stop, Question, and Frisk (*Terry* stops) program – which help overcome different data problems. The South Carolina dataset includes both lethal and non-lethal police shootings, which improves on the inherently sparse data on police killings as well as reducing measurement error caused by the randomness in whether people survive a police shooting. The New York City data helps show that *changes* in police practice which reduce exposure reduce racial disparities in the use of force on people of color. A discontinuous, policy-driven change in the frequency of SQF/*Terry* stops massively reduced the exposure of black civilians in New York City to police use of force – even though the probability of force being used in any individual stop actually rose. Taken together, these findings suggest that scholars, activists, and policymakers concerned about police violence should make solving exposure disproportion, and reducing the exposure of black civilians to police interactions, a high priority.

2 Data Problems in Documenting Police Violence

In the years since Michael Brown's killing in Ferguson touched off extended protests and political activism, various organizations have begun to compile data sets on police killings. These data sets typically share certain characteristics: they are crowd-sourced (Mapping Police Violence, Fatal Encounters) or maintained by journalists (The Counted, a data set by The Guardian). They rely on local journalists' reports of police killings, and occasionally on public records requests.

In addition, the Bureau of Justice Statistics collects information about police killings in the Arrest-Related Deaths database, while the FBI collects data about whether homicide victims were killed by police in the Supplementary Homicide Reports. While the Centers for Disease Control and Prevention collect information about homicides by law enforcement in the National Violent Death Reporting System, only 17 states participate in the NVDRS. In 2013, the NVDRS reported 212 deaths in which the victim was injured by law enforcement. In contrast, Fatal Encounters lists 1271 deaths in 2013.

2.1 Missing records

These data sets are tremendously important, but they also have major limitations. There are substantial problems with missingness in the data. Using multiple systems estimation, the Human Rights Data Analysis Group estimates the true number of police killings in the range of 1500 per year, a substantially higher number than found in any of the existing data sources. For 2015, for example, Mapping Police Violence reports 1152 killings, while The Counted reports 1146. This missingness is almost certainly higher for incidents that do not involve the use of guns: over 90% of reported lethal incidents are shootings. This may reflect the true distribution of types of lethal violence, but it is also possible that police killings that do not involve guns are easier to conceal: for example, someone who dies in a chokehold without video evidence may be listed as having died of an asthma attack. Such deaths would be disproportionately easy to conceal when victims

or their families are reluctant to involve police for the reasons described above.

While no policy can eradicate the ability of police and other data collectors to manipulate data, imposing legal obligations for disclosure can increase the risk to police of concealing such incidents.

2.2 Non-Lethal Use of Force

The information we have is almost entirely about police killings rather than non-lethal gun violence by police, which raises problems about both the coverage of the data and the characteristics of the distribution. The difference between a non-fatal shooting and a killing can be as little as three minutes on the way to the emergency room. Perhaps more importantly, police killings are rare events at the top of the continuum of police violence. Because they are sparsely represented in the data, making inferences about the true risk of killings is challenging. We need information about shootings and other types of use of force because the characteristics of the data lend themselves more easily to statistical analysis.

Data sources on other types of police use of force cover even less ground. The Police-Public Contact Survey asks a nationally representative sample of the public about their experiences with police. Unfortunately, because it comes from a national sample, it cannot effectively provide information about local variation. Data from the NYPD Stop, Question, and Frisk program does include information about use of force; however, New York is already the best studied example of US policing. These data cover only one city, and one with a very particular history. We need broader, national data about police use of force.

2.3 Putting Use of Force in Context

Many police killings begin as ordinary interactions between police and civilians. What characteristics of interactions predict escalation? Do racial disparities arise because of differences in the

risk of being killed in a given encounter, or because of the massively larger exposure to police enforcement that people of color (and especially young black and Latino men) face? How do these two risks interact? We lack an adequate national data set of stops. While Goel et al are in the process of developing such a data set for state highway patrols, much policing is the province of a patchwork array of local police departments, sheriffs, and tribal police.¹ The extent to which state highway patrol stops proxy for stops in general varies substantially both within and among states. In particular, in dense urban areas like New York City, pedestrian rather than traffic stops are the central mode of enforcement. Even in lower-density areas where more traffic happens by car, in larger incorporated municipalities local police conduct the majority of stops and enforcement.

In addition, we lack access to adequate historical data. Consider James Baldwin, writing in 1960: "Rare, indeed, is the Harlem citizen, from the most circumspect church member to the most shiftless adolescent, who does not have a long tale to tell of police incompetence, injustice, or brutality."(Baldwin, 1966) Black newspapers from the middle of the 20th century repeat these themes, arguing that police kill black Americans with impunity. How has that risk changed over time? Funders should consider contributing to historical as well as contemporary data collection efforts. This analysis of exposure disproportion, however, suggests that the vast increase in policing over the last four decades may have increased the use of force *even if* the enfranchisement of African Americans has reduced the risk of violence during any particular police encounter.

3 Police Shootings in South Carolina

The South Carolina Law Enforcement Division, SLED, investigates every police shooting in the state of South Carolina (Post and Courier, Shots Fired, 2015). The Charleston Post and Courier used a combination of public records from SLED and supplementary court records to create a data

¹<http://www.knightfoundation.org/grants/201652100/>

set of SLED records from 2009 to 2014, which they generously shared with me.²

This dataset catalogues 235 shootings by police officers between 2009 and 2014, for a yearly average of 39 shootings. (In 2015, there were 48 officer-involved shootings.) The data are missing a substantial amount of information: 30 incidents are missing victims' names, making it impossible to use newspaper sources to confirm details. Twenty-nine are missing information about race as well. Of the 234 incidents, 102 were fatal, and thus provide a useful check on the completeness of crowd sourced data sets. Sheriff's departments accounted for 116 shootings, while police were responsible for 96. White victims were more likely than black victims to be shot by sheriffs: 62% of white victims were shot by sheriffs, compared to 47% of black victims. This is most likely related to differences in relative urbanization, since sheriffs typically provide services to rural communities with a larger share of white residents.

The SLED data set includes more entries than Mapping Police Violence or Fatal Encounters for the total time frame: 102, rather than 64 for the Fatal Encounters data set. However, all of the fatalities in the SLED data set for 2013 and 2014 are covered in the crowd-sourced data; the SLED data set has about the same number of killings, but the missing names and other information make it impossible to fully confirm that the incidents are the same.

Overall, African Americans in South Carolina had a risk of being shot 2.92 times the risk for whites in South Carolina. As I show below, this disparity arises largely from the greater exposure to the risk of shootings faced by African Americans in South Carolina.

3.1 Reference Populations

Fryer's recent working paper on police use of force attempts to place police gun violence in the context of other police actions. In order to determine whether police are more or less likely to shoot black suspects, he compares shootings in Houston to a reference set of other types of interactions.

²The data have more recently been published on github, and are available at <https://github.com/postandcourier/shots-fired>

This raises a key issue: what is the appropriate reference population? Fryer argues for a focus on specific, serious incidents.

”The other two datasets were assembled for the purposes of this research. We use event summaries from all incidents in which an officer discharges his weapon at civilians including both hits and misses from three large cities in Texas (Austin, Dallas, Houston), six large Florida counties, and Los Angeles County, to construct a dataset in which one can investigate racial differences in officer-involved shootings.... To supplement, our fourth dataset contains a random sample of police-civilian interactions from the Houston police department from arrests codes in which lethal force is more likely to be justified: attempted capital murder of a public safety officer, aggravated assault on a public safety officer, resisting arrest, evading arrest, and interfering in arrest.” (Fryer Jr, 2016)

However, many of the high profile shootings we have recently observed do not arise from this reference population. Philando Castile in particular – who was shot minutes into a traffic stop for a broken taillight – would not have been charged with any of these based on the observed interactions. The same is true for many other high-profile police shootings. Moreover, the final three – ‘resisting arrest’, ‘evading arrest’, and ‘interfering in arrest’ – are highly fungible categories to which people can easily be assigned based on the outcome of the interaction. Police can list someone as having ‘resisted arrest’ to justify a shooting. For this reason, researchers are likely to observe many cases of ‘resisting’ or ‘evading’ arrest among individuals who are shot, but these designations can be selected *after* the shooting does or does not take place. That is, the civilian may resist arrest and thus be shot; or the causal arrow may run the other direction, so that the civilian is shot and, as a result, recorded as resisting arrest.

Considering all police contacts as the reference population provides one solution. Every contact carries with it the possibility of escalation, and numerous police killings which have received national attention began as traffic stops or interactions over non-serious offenses. Since no com-

prehensive dataset of stops exists, I use the total number of arrests for both black and white South Carolinians (data on people with other racial identifications is insufficient to draw conclusions in South Carolina). Most people who are shot by police but survive are later arrested, though of course this creates the same bias as that described above. However, with 241,645 total yearly arrests in South Carolina in 2011, the number of arrests is large enough that adding 235 shootings – 0.016% of the 6-year arrest figure – does not materially change the population’s characteristics. Black South Carolinians are much more likely to be arrested than white South Carolinians. Figure 1 shows the density of arrest to population ratios for South Carolina counties by race.

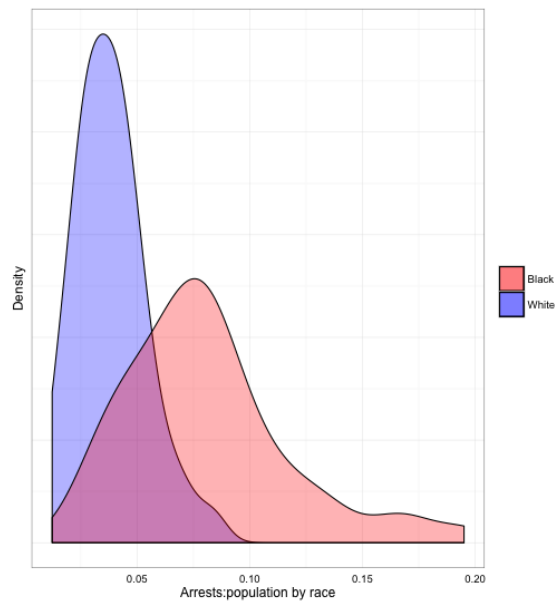


Figure 1: Arrests by Population for South Carolina counties

Fryer argues that his analysis identifies bias in officer decision-making: that is, are officers more likely to shoot black people than white people, conditional on the same level of threat within a specific interaction? In examining the consequences of this finding, we might ask: if officers were not influenced by racial bias within interactions, would racial disparities in shootings disappear?

My results suggest that it would not: this analysis confirms the important role of exposure to police interaction in producing disproportionate shootings of black Americans.

However, if people of color are more likely to experience police contact for the same behavior, as national survey data on drug use suggests, the population *arrests of black Americans* may in fact be a less ‘dangerous’ population than *arrests of white Americans*; the same is true for any individual category of arrests. This type of bias in the composition of the population would make the estimates here a lower bound on the influence of individual, internal racial bias on racial bias in shootings.

3.2 Measuring Exposure Disproportion and Racial Disparities in Police Shootings

I combine data on shootings from SLED with FBI data on arrests from the 2011 Uniform Crime Reports³ and American Community Survey 5-year population estimates for 2011. I multiply the arrest and population figures by 6 to account for the time covered by the SLED dataset.⁴ Between 2009 and 2014, there were 1.61 shootings for every 10,000 arrests of black South Carolinians; for white South Carolinians, there are 1.22 shootings per 10,000 arrests. Compare this to the population figures: 1.42 shootings per 100,000 black residents per year, versus 0.49 shootings per 100,000 white residents. Conditioning on arrest shows that exposure to the criminal justice system is a key driver of racial bias in police shootings.

Figures 2 and 3 show the density of the ratio of shootings to population or arrest, by race, for South Carolina counties. These density graphs provide a readable alternative to histograms. The distribution of shootings to arrest by race are much more similar. (The outlier bumps for police shootings to arrests in the Figure 2 are small counties with only one police shooting and a small

³91% of US law enforcement agencies submit UCR reports.

⁴In an extension of this project, I plan to add arrest data by year. I welcome thoughts about the importance of adding year-specific population data. The smallest South Carolina county, McCormick County, had 10,091 residents according to the 2011 5-year estimates from the ACS; this is too small to get accurate 1-year or 3-year estimates, so additional data would come from 5-year rolling estimates and might add little information.

number of white arrests.)

Table 1: Incidence of Shootings by Population and Arrest, by Race

| | White | Black | difference | <i>p</i> (Wilcoxon) | <i>p</i> (t test) |
|---------------------------------|-------|-------|------------|------------------------|----------------------|
| <i>statewide</i> | | | | | |
| Shootings:Population | 0.488 | 1.42 | .932 | | |
| Shootings:Arrests | 12.3 | 16.1 | 3.8 | | |
| <i>mean by county</i> | | | | | |
| Shootings:Population | .563 | 1.22 | .657 | 0.0104** | 0.0106** |
| Shootings:Arrests | 15.9 | 21.69 | -5.82 | 0.672 | 0.459 |
| <i>without outlier counties</i> | | | | | |
| Shootings:Population | 1.27 | 0.46 | 0.653 | 0.00118** | 0.0105** |
| Shootings:Arrests | 16.6 | 13.3 | 3.30 | 0.309 | 0.429 |

Notes:

All quantities are per 100000.
 Welch's two-sample paired t test and Wilcoxon signed-rank test.
 * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 1 shows the mean shooting to population and shooting to arrest ratios for South Carolina counties. These numbers differ from those described above because they are the mean figures for counties, while the above figures pool all counties statewide. Two small counties with few arrests skew the distribution for white South Carolinians: each has a very small number of arrests (363 and 81 total arrests, 119 and 61 respectively of white individuals) and only one police shooting, of a white person. Excluding those counties, the county-level results are similar to the state-level results.

Paired t tests and Wilcoxon signed rank tests confirm that the differences between black and white shootings:arrest ratios are not statistically significant, while differences in shootings:population ratios are highly statistically significant. Intensive contact with the criminal justice system, and with police, poses a risk that goes far beyond the direct consequences of any given arrest: the repetition

of contact means that people of color experience tremendous exposure to the life-threatening risk of police gun violence.

The implications of this are substantial. If African-Americans in South Carolina were shot at the same rate per arrest as whites in South Carolina, the relative risk ratio would decline only from 2.92 to 2.22. However, if the arrest rate for black South Carolinians matched the arrest rate for white South Carolinians, leaving racial disparity in shootings per arrest unchanged, the relative risk ratio would decline to 1.32 – cutting the disparity more than in half.

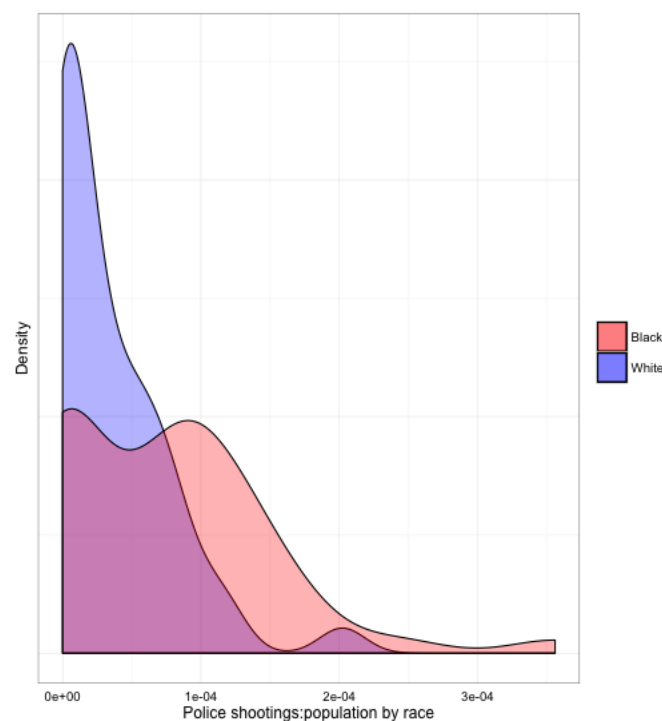


Figure 2: Police shootings to race-specific population by county for white and black South Carolinians

This analysis treats police shootings as a binomial distribution: the cumulative probability of being shot is a function of both p and the number of draws from the distribution. This is not an argument that racial bias is not a factor in police shootings: rather, it is an argument that we need

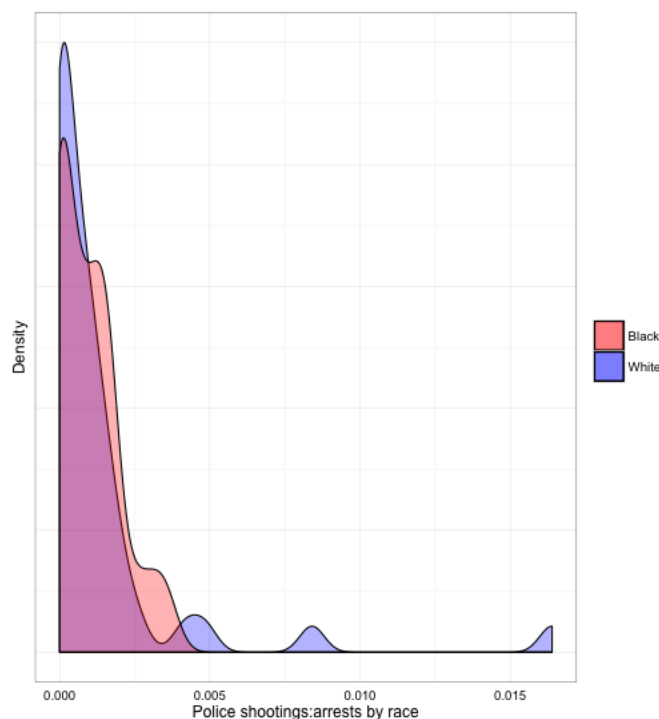


Figure 3: Police shootings to race-specific arrests by county for white and black South Carolinians

to think systemically about the massively greater exposure to criminal justice contact that people of color face, and the major risks of state violence that that carries with it. Bias in policing is not exclusively or a consequence of police officer bias within the interactions: focusing on it omits important information about how police are deployed and whom they are expected to arrest.

4 New York: Exposure Disproportion and Policy Change

On March 5, 2013, the New York City Police Department undertook a reform that massively reduced the exposure of black civilians to the NYPD. For the preceding two decades, since William Bratton brought Compstat and intensive policing to New York, the NYPD made tens to hundreds of thousands of stops each year. These stops were legally justified by the Supreme Court's decision

in *Terry v. Ohio* (1968), which held that “police can stop a citizen based on founded suspicion that crime may be ‘afoot.’ The encounter would proceed to increasing levels of intrusion if suspicion was determined to be credible or reasonable. Reasonable suspicion would permit pointed questioning and frisk or pat down to look for weapons, drugs or other contraband” (Fagan et al., 2009).

Stop, Question, and Frisk became a cornerstone of the NYPD’s investigative and crime prevention practice, and officers reported organizational pressure to keep their stop rates high (Lerman and Weaver, 2014b; Rayman, 2013). These stops disproportionately targeted black and brown civilians, and especially teenage boys (Gelman, Fagan and Kiss, 2007; Fagan et al., 2009; Goel, Rao and Shroff, 2016). SQF reached a high of nearly 700,000 stops in 2011. That same year, there were 112,115 stops of male black and Latino teenagers between 14 and 18; at the time, about 177,000 New York residents were black and Latino boys between 14 and 18. Even accounting for repeat stops, Fagan et al estimate the probability of being stopped for eighteen and nineteen black and Latino males in 2008 at .79 (Fagan et al., 2009). Research has consistently found lower success rates (measured by arrest or finding a weapon) for *Terry* stops of black and Latino civilians, Goel finds that stops of black civilians typically have less *ex ante* justification (Goel, Rao and Shroff, 2016).

Taken together, this evidence suggests that the SQF program produced disproportionate exposure to police contact among black and brown civilians in New York. The SQF program also led to political outcry and legal challenges. In 2008, the Center for Constitutional Rights filed a case challenging Stop, Question, and Frisk as racially biased and unconstitutional.

As the case went to trial five years later, in a memo dated March 4, 2013, the plaintiffs requested a procedural change to the “UF-250” forms which officers use to record stops: “the UF-250 form should be modified to: (i) include a narrative portion for police officers to justify the basis for stops, frisks and searches. . . .” (Mummolo, 2017) The UF-250 form had previously included “the date,

time and location of each stop, as well as the reason (suspected crime and other circumstances), suspect attributes and various outcomes such as whether a weapon was found or an arrest was made,” but “critics of SQF had long alleged that this form was insufficient to establish the legality of a stop” (Mummolo, 2017). The NYPD also required officers to “enter details” in their patrol notebooks, but officers were not required to submit these notes and plaintiffs argued that audits of officer notes revealed that they frequently were not recorded.

4.1 A Sudden Change to Stop, Question, and Frisk

In a move that surprised the plaintiffs, the NYPD in fact adopted that reform on the following day. In a memo dated March 5, 2013, Chief of Patrol James P. Hall issued “a new order requiring officers to photocopy and submit these narrative descriptions of the reasons they stopped suspects to supervisors after each shift.” Mummolo finds that this memo led officers to believe that their decisions to implement SQF would be under increased supervision (Mummolo, 2017).

The sudden change in SQF procedure provides a temporal discontinuity in police practice, and an opportunity to observe the consequences of changes in police exposure in the city. Police stops fell dramatically after the memo was adopted, and continued to decline over the following year. Mummolo’s paper provides extensive evidence from both plaintiffs and interviews with officers that this intervention was a surprise to officers, possibly adopted as a strategic move to defang the upcoming court case. This suggests that anticipatory changes by police are not likely to be a confounding factor. (Mummolo, 2017) ⁵

Mummolo’s interviews with officers confirm that the memo had a significant effect on officers’ perception of how closely their stops would be scrutinized. Despite the high volume of memos in the NYPD, officers told Mummolo that this memo stood out.

“Theyre really watching us now,” one officer recalled thinking when the memo was

⁵I am particularly grateful to Mummolo for sharing the well-formatted public data and code appendix to his paper, which greatly facilitated the development of this section of the paper.

released (Officer 2). Another officer added that before the memo, supervisors, “would only look at [memo book entries] if someone made an allegation. . . or you had to go to court. . . Now. . . its basically like theyre looking at it. . . without any sort of allegation being made. . . Theyre trying to find a reason to penalize us,” (Officer 1). Supervisors, “obviously look at these things with a fine-tooth comb,” said another officer. “We need to protect ourselves, (Officer 3). (Mummolo, 2017)

Before the memo, officers believed themselves to be accountable for delivering a large number of stops (Rayman, 2013). After, they instead believed that stops would need to be carefully justified, and that their supervisors would be proactively monitoring their justifications for stops (Mummolo, 2017).

The effect of this memo was an immediate and substantial reduction in stops. As interviews I have conducted with police confirm, paperwork requirements are a major barrier to police use of particular tactics.⁶ Moreover, “the perception of increased risk led some officers to aggressively forego making stops unless they observed something highly incriminating. ‘Its forcing people to not get involved in things that otherwise, a few years ago, they would have,” said one officer (Officer 3).” (Mummolo, 2017)

This section of the paper examines the consequences of this change in exposure for police violence. In doing so, it overcomes several limitations of existing work on police violence. First, while most of this data draws on inherently sparse records of individual killings, I use NYPD’s public release Stop, Question, and Frisk data. I examine records that provide continuous, highly granular data on police-civilian interactions. The NYPD SQF data from 2008-2015 describes over 3.2 million stops, including 701,989 instances of use of force by police. Second, this data set provides comprehensive measures of a set of police interactions with their outcomes, avoiding the problem of constructing a reference population. The data cover *all* stops, not just stops which turn

⁶An officer from an unrelated department said that when the department wanted to reduce the use of tasers, they added paperwork requirements and watched the frequency of taser use fall immediately.

out to have force, so they are not skewed by the reverse causality described above.

Finally, studies of police violence have rarely used experimental or quasi-experimental techniques, leaving substantial problems with omitted variable bias in the literature. By leveraging a temporal discontinuity, I can compare the same officers in the same city policing the same population, with different levels of exposure. This study is the first to my knowledge to examine the effect of a procedural change that reduces exposure – and exposure disproportion – on racial disparities in police violence. Because the data are coded with precise dates, rather than by month or year, the change induced by the stop can be identified more accurately.

How does this reduction in exposure affect racial disparities in police use of force? I find that the probability that force would be used in any specific stop does not change. Using formal tests of equivalence (Hartman and Hidalgo, 2016), I find no significant changes in the rate of use of force at any point within a 30-day bandwidth. However, the number of stops falls substantially at the discontinuity (and continues to decline). Both the daily incidence of use of force on black civilians and the racial disparity in police violence declined significantly.

Could reporting bias explain this effect? Officers reported that they expected their new, more detailed reports to be heavily scrutinized: gone over “with a fine-tooth comb”, in one officer’s words. Combined with the existence of ongoing litigation and the public salience of the issue, this procedural change seems, if anything, more likely to produce an increase in reporting of police violence as officers grow more concerned that any omissions might be challenged by supervisors, civilians, or the courts.

4.2 Analysis and Results

The outcome of interest is the number of stops in which force is used on a civilian. The incidence of use of force is particularly important because it is these occasions – not merely the rate – which produce negative consequences for individuals, communities, and state legitimacy. Figure 4 shows

the effect of the memo on the number of stops, both total and, for white and black civilians, broken down by race. There is a clear discontinuity at the memo in the number of stops. Dividing the stops by race, though, reveals that this discontinuity results from a decrease in the number of stops of African Americans, not of whites.

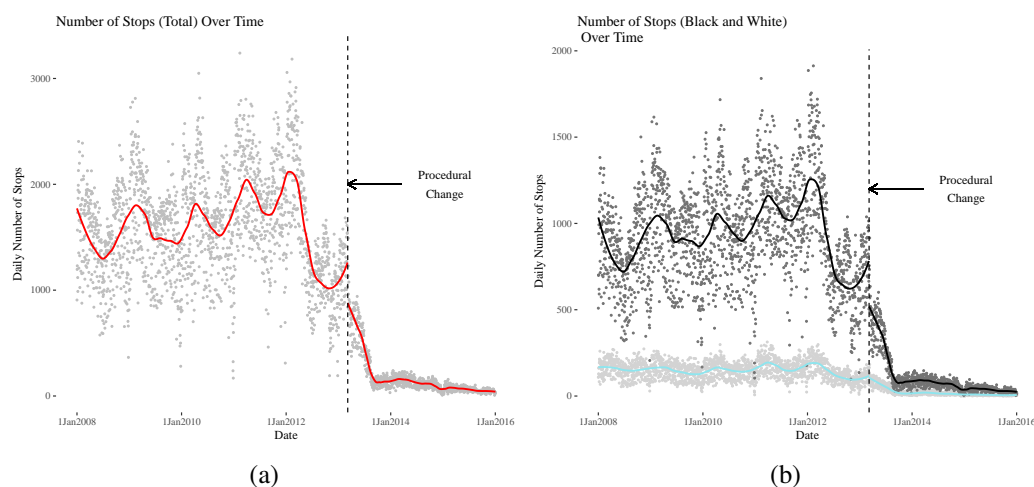
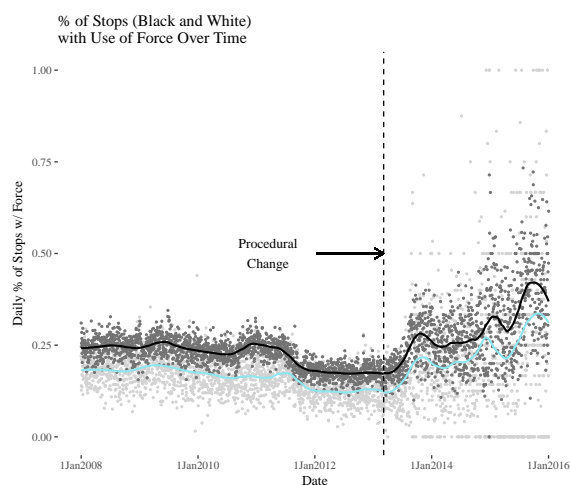


Figure 4: This figure shows the daily number of stops from 2008 to 2015. Panel (a) shows all stops (pooled). Panel (b) shows stops for black and white civilians separately. The solid line shows a locally weighted LOESS regression predicting the number of stops, without adjustment for covariates.

The research on the cognitive mechanisms behind implicit bias suggests that officers have a lower threshold for stopping black civilians; analysis of stop, question, and frisk data in New York confirms that officers typically have less evidence *ex ante* when they stop black civilians than when they stop whites (Goel, Rao and Shroff, 2016). As the officer quoted above said, “Its forcing people to not get involved in things that otherwise, a few years ago, they would have” (Mummolo, 2017). When police believe they need more concrete justification for stops, they may forego the stops of precisely the groups of people for whom they previously needed less justification. Regardless of the precise mechanism, this shows a substantial decrease in exposure. Exposure disproportion also declined over this period: in the 30 days before the memo, the risk of being stopped for black

Figure 5: This figure shows the percent of stops with use of force for white and black civilians from 2008 to 2015. Here the stop is the unit of analysis/prediction for the LOESS regression.



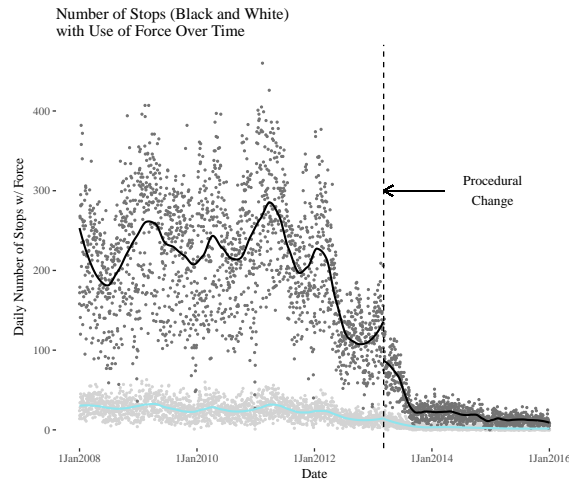
civilians was 8.14 times that for whites, while in the 30 days post-memo the risk disparity fell to 6.11.

Figure 5 shows the change in the rate of use of force by race. There is no discontinuity in the share of stops which include force for white or black civilians. Over the entire time period, stops of black civilians are between 4 and 9 percentage points more likely to involve force than stops of whites.

Figure 6 shows the change in the total number of stops which included use of force. There is no discontinuous change for white civilians; in contrast, for black civilians, the daily number of incidents with use of force drops precipitously, mirroring the decline in stops almost exactly. The magnitude of this change is substantial.

These visualizations convey the scale of the change in exposure disproportion and the accompanying change in use of force on civilians by NYPD, but I also use formal discontinuity tests to estimate the effect of the procedural change on the rate at which force is used, the total number of stops (exposure), and the number of stops where force is used. I use an interrupted time se-

Figure 6: This figure shows the number of stops with use of force for white and black civilians from 2008 to 2015, aggregated by day.



ries, a form of regression discontinuity where the discontinuity is temporal. As Mummolo writes, “The SQF data are ideal for this approach because of the high frequency of measurement and well-defined moment of the intervention the former alleviates concerns about unobserved confounders which change levels during long intervals between observations, and the latter guards against researcher discretion in coding treated and untreated units” (Mummolo, 2017).

To estimate changes in the rate of use of force, I estimate the following equation:

$$force_i = \alpha + \beta memo_i + s_j(d_i) + \epsilon_i \quad (1)$$

Here, $force_i$ is the (0, 1) assessment of whether any type of force was used in the encounter. α is the intercept, $memo_i$ indicates whether the incident fell before or after the memorandum, $s_j(d_i)$ is a function, and ϵ_i is the error term.

There are substantive debates about the best strategies to estimate regression discontinuity models (Imbens and Lemieux, 2008; Eggers et al., 2015; Gelman and Imbens, 2014; de la Cuesta and Imai, 2016), and I therefore estimate multiple models, including difference in means as well as

linear, quadratic, and cubic time trends with and without controls. The identifying assumption required to assign the change in outcomes to the memo itself is continuity in potential outcomes: that no other factor which affects the outcomes changed at that precise moment (de la Cuesta and Imai, 2016).

I estimate the change in rate of use of force at a variety of bandwidths up to 30 days, and separately for the data as a whole and for black civilians. This 30-day bandwidth covers a total of 61,754 stops over 60 days, including 10,238 uses of force, 6380 on black civilians and 760 on white civilians. Figure 7 shows the point estimates for these tests.

Absence of statistically significant differences does not, per se, demonstrate equivalence. Following Hartman and Hidalgo's recommendations, I use 0.2 times the pooled standard deviation in rates of use of force as an equivalence threshold (Hartman and Hidalgo, 2016). The vast majority of outcomes fall within the equivalence range; in 180 estimates, only one is (very marginally) below the equivalence range. This suggests that we can establish that rates of use of force in the post-memo period, conditional on stops, were equal to or higher than rates of use of force before the memo was issued.

To estimate changes in the number of stops and incidents with use of force, I sum data by day and estimate the effect on the number of stops (or incidents with use of force) each day. Because summing by day produces a much smaller number of units, I estimate these quantities using a 100-day bandwidth. This also allows me to include optional controls for year, month, and day of the week.

$$stops_d = \alpha + \beta memo + s_j(d) + \epsilon_d \quad (2)$$

$$force_d = \alpha + \beta memo + s_j(d) + \epsilon_d \quad (3)$$

Table 2: OLS Estimates of Change in Daily Stops With Force (“force”) and Daily Stops (“stops”) at Treatment (Memo) Using 100-Day Bandwidth

| | Difference in Means | Difference in Means [†] | Linear | Linear [†] | Quadratic | Quadratic [†] |
|------------------------|----------------------|----------------------------------|------------------------|----------------------|------------------------|------------------------|
| Δ force (black) | -43.63 (5.835)* | -37.021 (17.775)* | -49.938 (11.917)* | -34.179 (18.252) | -43.781 (19.848)* | -25.525 (15.421) |
| Δ stops (black) | -261.61 (39.216)* | -123.508 (58.486)* | -303.011 (66.789)* | -111.593 (61.149) | -265.392 (104.354)* | -94.763 (71.17) |
| Δ force | -65.89 (9.299)* | -44.467 (21.498)* | -71.649 (17.633)* | -38.666 (22.189) | -63.684 (29.078)* | -28.037 (22.832) |
| Δ stops | -399.37 (67.403)* | -143.649 (73.226)* | -450.857 (109.864)* | -126.335 (74.527) | -413.552 (175.509)* | -107.474 (83.413) |
| <i>N</i> | 200 | 200 | 200 | 200 | 200 | 200 |

Includes controls for year, month, day of week, and prior days hit rate.

Maximum of homoscedastic and HAC standard errors in parentheses. * $p > 0.05$

Table 2 shows the results. All point estimates of the effect on the number of stops and amount of force used – for the total population and for African Americans as a subgroup – are negative, and most are significant.⁷ The effects estimated are largely in the range of 25 to 44 avoided uses of force on black civilians per day: an enormous substantive effect, considering the overall average. While the overall differences cannot be attributed to the memo, the overall decline in Stop, Question, and Frisk has had even more massive substantive consequences. In the overall period before the memo, from January 1, 2008 to March 4, 2013, the NYPD used force on an average of 350 civilians per day, 210 of them black. After the memo, and the concomitant procedural reduction in the use of SQF, the NYPD used force on 39 civilians, 24 of them black.

The result of this procedural change was to reduce civilian exposure to police, and hence to reduce civilian exposure to police violence. The racial disparity in risk, measured by the risk ratio for use of force, also declined: from 11.32 to 8.85 (using a 100-day bandwidth), a 22% reduction.⁸ This decline almost exactly mirrored the reduction in racial disparities in exposure, which fell 24%

⁷It is unsurprising that the most flexible models lose significance on this reduced sample size.

⁸Results are substantively identical using a 30-day bandwidth. Population data comes from the ACS estimates for New York City in 2013.

from 8.38 to 6.35.

African Americans' exposure risk remained much higher than that of whites, and the probability of force in any given stop. Still, there is robust evidence that the reduction in exposure disproportion was sufficient to massively reduce the exposure of African Americans in New York City to police violence. In the 100 days before the memo, there were 12061 uses of force on black civilians; in the 100 days after, there were 7616, nearly 5000 fewer in just a few months.

To understand the relative importance of exposure disproportion and encounter-level racial disparities, I simulate two outcomes in New York. If we kept exposure disproportion at its pre-memo level in SQF but equalized the probability of use of force when police stop black and white civilians, the number of occasions of use of force in the 100 days prior to the memo would have been 8924: a meaningful reduction from the actual number of 12061. If, instead, we equalized exposure rates, but left racial disparities within stops, the number of uses of force on black civilians would fall by an order of magnitude – to 1440.

These findings imply that exposure disproportion is a major driver of the racial disparities in the experience of police violence.

4.3 Gun Collars, Force, and Tradeoffs

One of the most common justifications for Stop, Question, and Frisk as a policy was the goal of removing guns from circulation. These results, though, suggest that the use of force on civilians was a far, far more ordinary result for a stop. In 2011, at the height of Stop, Question, and Frisk, the NYPD stopped 1087 black civilians daily. Officers used force on an average of 241 black civilians each day, while recovering only 8.5 weapons each day from the black civilians they stopped. Even worse, many ordinary folding knives of the type frequently carried for manual work are misclassified as “gravity knives” under New York law, and treated as weapons ([Campbell, 2014](#)). In 2011, NYPD recovered 591 guns from black civilians: 1.62 guns per day. A stop of a black civilian was

149 times more likely to end in the use of force than in the recovery of a gun.⁹ Table 2 shows the stop outcomes for black civilians by year.

Table 3: Outcomes of Stops of Black Civilians by NYPD, 2008-2015

| | Stops | Weapon | Gun | Force | Ratio |
|------|--------|--------|-----|-------|-------|
| 2008 | 305878 | 2682 | 616 | 75643 | 123 |
| 2009 | 343887 | 2998 | 575 | 86076 | 150 |
| 2010 | 352029 | 2970 | 601 | 82778 | 138 |
| 2011 | 396859 | 3118 | 591 | 88077 | 149 |
| 2012 | 318420 | 2476 | 540 | 56075 | 104 |
| 2013 | 116108 | 1527 | 294 | 21978 | 75 |
| 2014 | 26973 | 588 | 146 | 6979 | 48 |
| 2015 | 13267 | 481 | 130 | 4594 | 35 |

It is notable that force has continued to decline with stops. In 2011, 88077 black civilians experienced the use of force after being stopped under SQF. By 2015, that number had fallen by an order of magnitude. This is a truly major change in civilian risk. Over the same period, the ratio of use of force to guns recovered also fell – consistent with the change in exposure risk, and Mummolo’s finding that weapons recovery remained a high priority (Mummolo, 2017).

5 Significance

Why does police violence matter? On the one hand, the answer seems so obvious as to be offensive: it matters because the approximately 1500 people killed by police each year matter, as do the thousands of New Yorkers who experience force at the hands of the NYPD. The consequences of police shootings go far beyond these individuals, however. “Many black people are bone weary and cynical about a broken criminal justice system that is quick to incarcerate individuals from their communities, even while it countenances the harassment and even killing of unarmed individuals by law enforcement” (Shaw et al., 2015).

⁹This imbalance is even more severe for stops of white civilians, but their vastly lower exposure risk means the consequences for police legitimacy and public well-being are less severe.

Policing is the main way by which a core state service – justice – is delivered. Policing is inevitably an exercise in coercion, but formal justice is a valuable state service. Police resolve disputes about stolen property and public nuisances. They provide a backstop to the civil courts' enforcement of private legal arrangements: individuals who do not comply with court orders may find police enforcing them. When individuals encounter violence, police intervene to provide safety: enforcing restraining orders, identifying and apprehending those responsible for violence.

Police violence undermines access to these services. Even when committed by a small number of officers, it poisons the well, damaging trust in government and access to state services. Meares and Fagan argue that failings of *procedural justice* lead to alienation from criminal justice. Police violence and incarceration damage trust in police and government more generally (Brayne, 2014; Lerman and Weaver, 2014a,b). People who are afraid of involving police become targets for robbery, because they cannot draw on police protection (Goffman, 2014). This in turn contributes to retaliatory violence, as people defend their physical security or seek resolution to violent actions without access to legal institutions – a dynamic Miller describes as "racialized state failure." (Leovy, 2015; Miller, 2016)

The uneven distribution of both police contact and police violence – black Americans are both far more likely to be shot by police and far more likely to be arrested by police than white Americans who engage in the same behaviors – leads to spillover consequences far beyond individuals (Ross, 2015; on the States, 2009; Rodriguez and Emsellem, 2011; Brame et al., 2012). The literature on distributive justice argues that the fact of inequality in how different groups are treated shapes individuals' beliefs about the justice, efficacy, and trustworthiness of the system. Thus, even individuals who do not personally observe or experience police violence observe racial differences in how people are treated and conclude that police lack legitimacy.

Civilians who are unable to turn to police for protection turn to other techniques: carrying weapons, pursuing retaliatory violence, and asking community members to intercede in disputes.

The demographic group most likely to be killed by police is the very demographic group most likely to be murdered by civilians: young black men, who make up about 6000 of the 33000 gun deaths in the United States. Mistrust of police, low solve rates for black homicide victims, and other aspects of the carceral state exacerbated by police violence likely contribute to the use of homicide as a tool for dispute resolution and vengeance (when state intervention is lacking) (Leovy, 2015). Moreover, many of the most promising strategies for reducing civilian violence rely on building relationships between the state and those individuals most likely to victims or perpetrators of violence (Corsaro and Engel, 2015). Police violence undermines those relationships, making it more difficult for local governments to address the dangers of crime violence in their communities.

From the perspective of procedural justice, it is of course critical to ensure that police are treating white and black Americans equally during all police encounters. Focusing on the cognitive and psychological processes of individual police officers, however, leaves reformers trying to change behavior in high-intensity situations, with little certainty of success. Moreover, focusing solely on within-stop behavior ignores the critical role of exposure disproportion in producing racial inequality in police violence.

What matters most for the well-being of the families and communities at risk for violence from police is not within-stop fairness, but *reduced violence*. Each person who experiences a police stop is at risk for police violence; each person who experiences police violence adds a story the bone-weary cynicism Shaw describes, and the damage of over-policing and police violence (Shaw et al., 2015). Improving exposure disproportion may not fully address the moral responsibilities of police departments, but this evidence suggests that it will substantively change the daily experience of black Americans, and their risk of violence at the hands of state representatives.

The policy implications are clear. Exposure disproportion is a major driver racial disparities in police violence. To improve the epidemic of police violence, reformers and police departments must come to see stops not as benign, but – accurately – as carrying the constant risk of escalation

against civilians. Reducing the number of occasions on which black civilians involuntarily interact with police will reduce black Americans' exposure to police violence.

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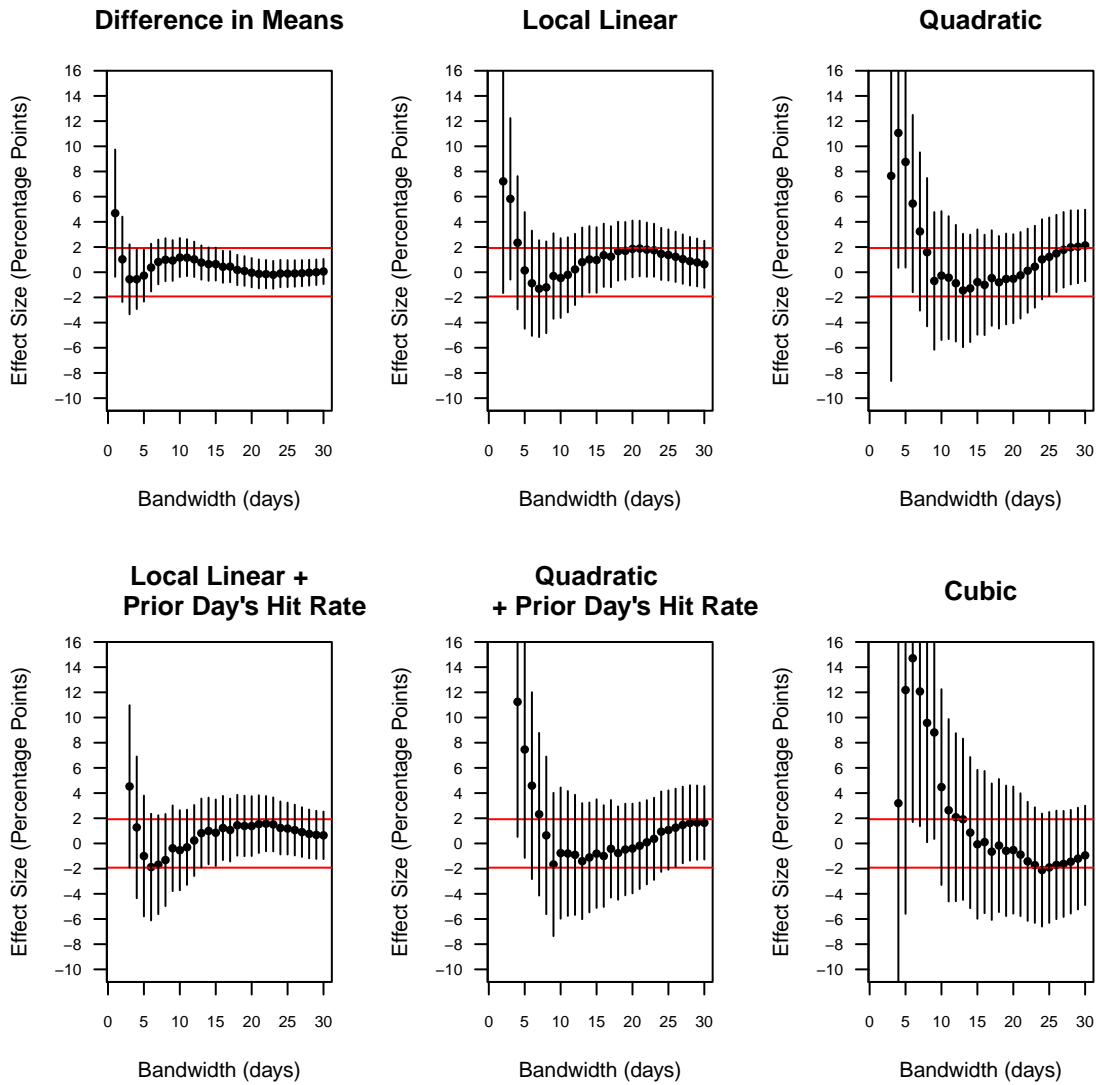
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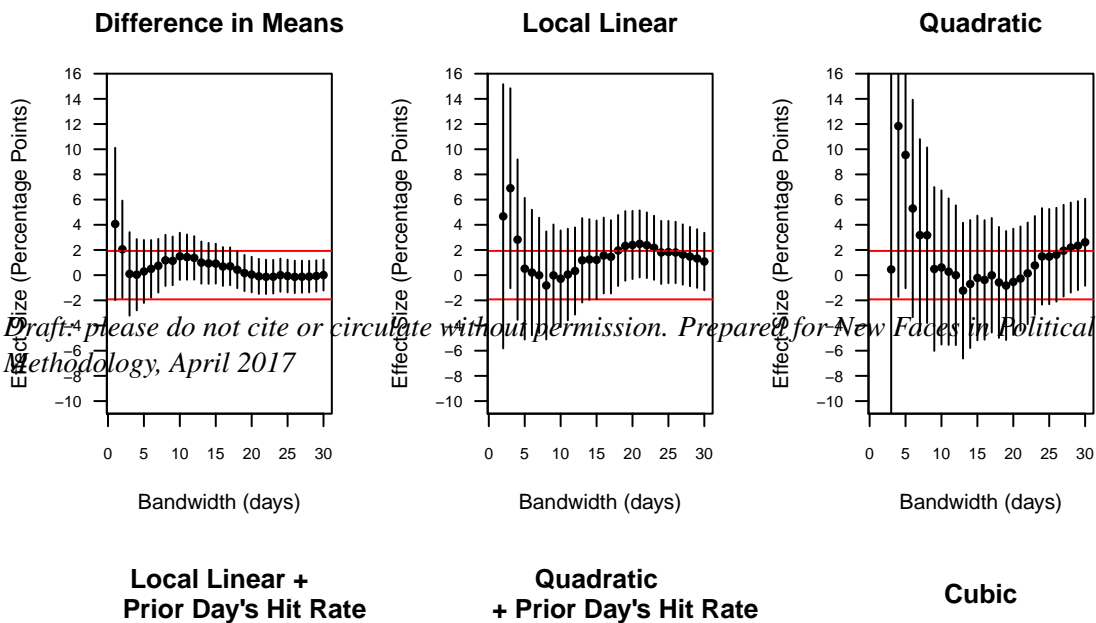
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(a) All Stops



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