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Group Threat and Racial Disparities in Police-Caused Killings

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I am submitting herewith a dissertation written by Ruben A. Ortiz entitled "Group Threat and Racial Disparities in Police-Caused Killings." I have examined the final electronic copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Sociology.

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Group Threat and Racial Disparities in Police-Caused Killings

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ABSTRACT

Blacks, Latinos, and American Indians are killed by police at a disproportionately higher rate than whites and Asians, but whether racial discrimination accounts for these killings remains disputed. I contribute to this debate by assessing whether group threat theory is associated with the overall, and race-specific count of police-caused killings at the metropolitan and county level across the US. Furthermore, I assess whether there is evidence of racial bias in police-caused killings, and if county-level measures of threat are associated with measures of racial bias at the individual level.

Using data from the Census Bureau, American Community Survey, *The Washington Post*, *The Guardian*, and Uniform Crime Reporting program, my results indicate that the size of the black and Latino population relative to the white population is consistently associated with a higher expected count of police-caused killings of blacks and Latinos at the metropolitan and county level. Moreover, I find that an increase in the size of the black and Latino population relative to the white population across US counties is associated with decreases in the expected count of police-caused killings of all people and white people.

I find that regional differences exist in the expected count of police-caused killings across metropolitan areas, and counties. Moreover, my results provide evidence of racial bias in police-caused killings. Among people who were shot and killed by police, Latinos were 1.26 times as likely as whites to have not been attacking people when killed, and blacks were 1.38 times as likely as whites to have been unarmed prior to getting shot by police. deaths. In developing solutions to reduce police-caused killings, researchers should look beyond the proximal causes of death (i.e. the police) to the distal factors operating across metropolitan areas and counties that predict the expected count of police-caused killings of minorities.

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Chapter One

INTRODUCTION

In the United States, far more US residents die at the hands of police officers than in any other developed country. In fact, in the first 24 days of 2015, fatal police shootings were responsible for 59 civilian deaths in the United States, compared to 55 fatal police shootings in England and Wales over the last 25 years (Lartey 2015). It is unclear how enduring the problem of police-caused killings is, but recently, the highly publicized and controversial deaths of unarmed people of color at the hands of the police have resulted in public demands for action in order to address the disproportionate killing of minorities (Kos 2020; Campaign zero 2020), and researchers have shown renewed interest in examining the possibility of discrimination in police use of lethal force (c.f. Ross 2015; Tregle, Nix, and Alpert 2019; Nix, Campbell, Byers, and Alpert 2017). Responding to public interest, some news organizations have collected verified, crowd-sourced data on police-caused killings and published analyses of those data that highlight the glaring racial disparities among victims. Adjusting for population size, the British newspaper *The Guardian's* data show that the rate at which blacks were killed by police was more than twice the rate of whites in 2015 and 2016 (Swaine 2016), and *The Washington Post* reports, "When adjusted by population, unarmed black men were seven times as likely as unarmed white men to die from police gunfire" (Lowery 2016). However, despite evidence that victims of police violence are disproportionately people of color, findings of racial prejudice in police-caused killings, to date, are mixed (c.f. Zane 2017; Cesario, Johnson and Terrill 2018).

Ross (2015), using multilevel Bayesian analysis and national-level data, shows that armed black and Latino residents have 2.94 and 1.57 times the probability of being shot by the police, respectively, than armed white residents. Examining death certificates, Buehler (2017) also finds that black and Latino men were 2.8 and 1.7 times more likely than whites, respectively, to die from legal intervention. whites, on average, lost about 169 years of expected life from police violence in 2015 and 2016, while blacks lost 674 years (Bui, Coates, and Matthay 2018). Scott, Ma, Sadler and Correll (2017) find that officers are more likely to shoot black suspects, even when racial differences in crime are held constant. Using data made available by *The Washington Post*, Nix and his colleagues (2017:325) find evidence of racial bias in their study, stating that non-white US residents "were significantly more likely than whites to have been fatally shot because of an apparent threat perception failure." Their findings point to evidence of racial bias, aligning with previous police shooting simulation studies indicating that officers are more likely to employ fatal force on unarmed minority residents (c.f. Correll, Park, Judd, and Wittenbrink 2002; Cox, Devine, Plant, and Schwartz 2014; Payne 2001).

However, in other experimental simulations with police officers, James and her colleagues (2016) found that officers were less likely to shoot armed and unarmed black suspects than whites. Using data from 11 cities, Fryer's (2019) fixed-effects analysis of police violence, conditioned on being stopped by the police, reveals that blacks and Latinos are no more likely than whites to be shot by police, but they are fifty percent more likely to experience non-lethal violence at the hands of police officers. Similarly, Cesario, Johnson and Terrill (2018) find no substantial evidence of racial disparities between blacks and whites at the national level when benchmarking deaths against race-specific violent crimes[1] rather than population. In sum, there is no clear consensus

[1] Race specific crime refers to arrest rates, disaggregated by race. In this case, race-specific violent crimes refer to violent crime arrest rates for black and white people separately.

among scholars regarding the role that racial discrimination plays in explaining police-caused killings.

This dissertation reconciles many of the contradictory findings in the previous studies on racial bias in police-caused killing by using novel approaches and data that remove many of the limitations of prior research. Here, I use data from *The Washington Post*, *The Guardian* and the Supplementary Homicide Reports (SHR hereafter) to examine police-caused killings at the US metropolitan area and county level in 2015, 2016 and 2017. Using Blumer (1958) and Blalock's (1967) structural theories of racism, I examine how the macro-social context in which police kill US residents influences the overall and race-specific count of police killings. Moreover, given that researchers have demonstrated how minority threat effects can be manifested at multiple levels of aggregation (Wand and Mears 2010), I evaluate whether county-level measures of minority threat affect individual level measures of racial bias. Lastly, my work addresses recent arguments (Cessario, Johnson, and Terrill 2018) that question the way in which researchers assess racial disparities by determining whether police-caused killings are better predicted by race-specific population or race-specific crime. In all, I determine whether the overall composition and change of minority populations relative to the white population predicts the expected count of police-caused killings at the metropolitan, county, and (within limits) individual level across the United States. In using several data-sources at multiple levels of aggregation, my study provides one of the most comprehensive tests of minority threat theory and police-caused killings to date.

LIMITATIONS OF PREVIOUS WORK

Understanding racial disparities observed in police-caused killings necessitates not only considering the specific characteristics of those who were killed by the police (e.g., their race, armed status, and behavior), but the geographic context in which the killing occurs (Sinyangwe 2016), making macro-structural approaches to racial prejudice well-suited for this endeavor. Criminologists interested in context, like me, frequently turn to the minority threat hypothesis. The minority threat hypothesis frequently derives from Blumer's (1958) notion that racial prejudice stems from real or perceived challenges to group position. When people of color are perceived to be in a position to challenge the structures that assure that whites unfairly accrue economic and power resources, racial prejudice and discrimination are employed to reduce that threat. Studies have shown that the size of the minority population, relative to whites, impacts prejudice (Fossett and Kielcolt 1989; Quillian 1996; Bobo and Hutchings 1996) and racially motivated violence (Tolnay, Beck, and Massey 1989).

Criminological studies testing the minority threat hypothesis show that the size of the minority population predicts the distribution of police resources, arrests rates, use of excessive force (Stults and Baumer 2007), and discriminatory policing practices (e.g. Holmes 2000; Jackston 1989; Jackson and Carroll 1981; Kent and Jacobs 2005; Liska and Chamlin 1984; Liska, Chamlin, and Reed 1985; Smith and Holmes 2014). Similarly, I hypothesize that as the size of the minority population increases, relative to whites, the more likely it is that police will kill US residents of color.

Studies of police-caused killing, to date, have serious limitations, and these limitations have likely accounted for disparate findings regarding the potential for racial bias in police-caused killing. First, a focus on the police in investigating racial disparities in police-caused killings limits analyses to the proximate causes of death, ignoring distal[2] causes that may be operating at the societal level. In fact, many emerging studies of police-caused killings tend to focus almost exclusively on officer characteristics (Ridgeway 2020) or interventions at the departmental level (Sherman 2020), and these interventions have proven to be ineffective at reducing the incidence of police-caused killings (Smith 2003, 2004). Thus, examining police-caused killings using the minority threat hypothesis (and its variants, economic competition and power threat) to interrogate observed racial disparities allows for a closer examination of distal causes of death that are related to inter-group processes. Additionally, examining the effects of minority population growth on police-caused killings is paramount as current demographic trends indicate that the white share of the population is declining as Latino, Asian and black populations continue to grow (Krosgstad 2019).

Second, almost all the work, to date, focuses on police-caused *shootings* (i.e. Holmes, PainterII and Smith 2018; Bejan, Hickman, Parkin and Pozo 2018), ignoring the myriad other ways that people can be killed by the police. Using several data sources, my study analyzes all deaths at the hands of the police and determines whether the data source used has implications for the conclusions reached assessing group threat theory empirically.

Third, many studies examine only a few geographies (i.e. Fryer 2019) or look at the nation as a whole (e.g., Cesario et al. 2018), ignoring the fact that deaths at the hands of the police are not randomly distributed across the country. For example, the Los Angeles metropolitan area is less than twice the size of the Boston metropolitan area, but police in Los Angeles killed 66 US

[2] Proximate causes refer to factors that immediately precede an event (i.e. characteristics of the officer, or person who was shot that could be conceptualized as being immediately responsible for the shooting. Distal causes refer to the ultimate, or real reason something has occurred.

residents in 2016, while police in Boston killed 10. My analyses examine killings at two levels of aggregation, thus ensuring that findings are closely associated with the places where harm occurs.

Fourth, researchers have not, to date, investigated claims by Cesario and his colleagues (2018) and Tregle and his colleagues (2019) that rates of police-caused killings should be adjusted by race-specific crimes, rather than population. Instead, researchers have avoided addressing the issue of benchmarking by focusing on approaches that bypass the need to benchmark in assessing whether there is evidence of racial disparities in police-caused killings. Johnson and his colleagues (2019) for example, test for evidence of racial disparities by attempting to predict the race of people killed by police using county level characteristics such as crime, population, and characteristics of police officers. Their analysis shows the importance of the context in which shootings take place, as any significant association between officer characteristics and racial disparities disappear once county level predictors are included in the analysis. Moreover, they conclude that while race-specific violent crime rates explain about 44 percent of the variance in determining the race of the victim shot, race-specific population also explains about 43 percent of this variance (Johnson, Tress, Burkel, Taylor, and Cesario 2019).Clearly, the context in which these killings take place is important. My analyses add to the conversation by evaluating such claims and assessing whether it is better to benchmark police-caused killings on population or race-specific crime rates by looking at model information criteria.

Fifth, borrowing from literature on minority threat and sentencing disparities (Wang and Mears 2010a; 2010b; 2015) and pre-trial detention (Zane 2018), I include "threat triggers" as operationalized by Kane, Gustafson and Bruell (2013) in my models to explore if increases in the size of minority groups in historically white areas are associated with increases in police-caused killings. Similarly, like many researchers studying group threat in sentencing contexts (Wang and Mears 2015; Feldmeyer et al. 2015) and responding to calls for multilevel models (Holmes et al. 2018) within the police-caused killing literature, I test whether county-level measures of threat affect individual-level measures of bias.

RESEARCH QUESTIONS AND ANSWERS

In my analyses I examine how the macro-social context in which police kill US residents influences the overall and race-specific count of police-caused killings. Broadly, I ask whether the overall composition and change of minority populations relative to the white population predicts the expected count of police-caused killings at the metropolitan, county, and individual level across the United States. Moreover, I assess whether police-caused killings are better predicted by race-specific population or race-specific crime and whether there is evidence of racial disparities in police-caused killings when benchmarking on population and crime.

My results indicate that the size of the black and Latino population relative to the white population is consistently associated with a higher expected count[3] of police-caused killings of blacks and Latinos at the metropolitan and county level. Moreover, I find that an increase in the size of the black and Latino population relative to the white population across US counties is associated with decreases in the expected count of police-caused killings of all people and white people. I find that regional differences exist in the expected count of police-caused killings across metropolitan areas, and counties. Metropolitan areas in the Northeast have a lower expected count of police-caused killings of all, minority and whites (relative to the Southwest), and metropolitan

[3] Expected counts refer to predicted counts from a multivariate regression model dealing with discrete counts such as the number of police-caused killings.

areas in the Midwest have lower expected count of all killings (relative to the Southwest). Moreover, counties in the South (relative to the Southwest) have lower expected killings of minorities and Latinos. Counties in the Northeast have lower expected killings across all models, and counties in the Midwest have lower expected killings across all models except for models predicting black deaths. These regional differences are consistent with findings from previous studies of police-caused killings (c.f. Smith 2003,2004; Willits and Nowacki 2014; Holmes et al. 2018).

Like previous research (Nix et al. 2017) I find clear evidence of bias in police-caused killings using data from *The Washington Post*. That is, among people who were shot and killed by police during this time period, Latinos were 1.26 times as likely as whites to have not been attacking police officers or other people prior to getting killed. Similarly, blacks were 1.38 times as likely as whites to have been unarmed prior to being killed by police. Such findings provide further evidence of bias in police-caused killings and establishes a clear pattern of bias when looking at police-caused killings using data provided by *The Washington Post.*

In evaluating the relationship between population and police-caused killings, there is clear evidence that the size of the black and Latino population relative to the white population matters when predicting police-caused killings of black and Latino residents regardless of the data source used. However, as I will show in upcoming chapters, official estimates of killings provided by the SHR severely undercount the overall and race-specific incidence of police-caused killings. For example, the reported number of black deaths provided by *The Counted* in 2015 and 2016 is eleven times higher than the estimates provided by the *SHR* for the same period. The differences in estimates by data source used are especially of concern when attempting to assess whether racial disparities exists in police-caused killings given population estimates and race-specific crime estimates. As shown in Chapter Six, there are clear patterns of racial disparities in police-caused killings when benchmarking estimates provided by The *Washington Post* and *The Guardian* on population estimates as well as race-specific arrest data. However, these racial disparities disappear when using estimates provided by the *SHR*. Clearly, official estimates of police-caused killings provided by the *SHR* do not capture the total number of police-caused killings and it is particularly problematic that deaths to people of color appear to be systematically missing in these data. Until the *SHR* can provide more reasonable estimates of police-caused killings, these data should not be used.

IMPLICATIONS

This dissertation makes several noteworthy contributions toward gaining a deeper understanding of racial disparities in police-caused killings using group threat theory. First, my analyses incorporate all conceptual models as described by Blalock (1967) by fitting linear, logged, and quadratic functions of threat. Based on model information criteria, it seems clear that economic competition models fit the relationship between population and police-caused killings better than minority threat or power threat models. Until now, only Liska and Yu (1992) have assessed all functional forms of group threat in the context of police-caused killings. My results differ from previous analyses (Liska and Yu 1992) in that economic competition, not minority threat models, seem to provide a better model fit when attempting to predict police-caused killings.

Second, in my analyses I incorporate dynamic measures of threat that are better aligned with Blalock's (1967) concerns over the effect of a growing minority population on discriminatory behavior. Although no significant effects were found, I believe it is important to test for such effects as others (Jacobs and O'Brien 1998) have found a significant relationship between minority population growth and police-caused killings and to keep true to the theoretical perspective I engage. Third, I use several novel measures of racial threat that have not been used within the context of police-caused killings. That is, I measure threat using several black to white and Latino to white ratios in measuring population and income differentials. Moreover, I introduce different measures of residential segregation that account for minority-majority interactions. To date, this is the first study that has used such measures in the context of police-caused killings. It could be that by using measures more closely aligned with group threat theory and the correct model specification (economic competition), I find significant results between measures of group threat and police-caused killings of blacks and Latinos when recent studies have failed to find a significant relationship (c.f. Holmes et al. 2018).

Moreover, this is the first study of group threat theory that accounts for the effect of segregation on police-caused killings by operationalizing residential segregation as exposure to whites for blacks and Latinos instead of using evenness (c.f. Liska and Yu 1992; Holmes et al. 2018). Although previous analyses have evaluated the relationship between segregation and police-caused killings (c.f. Liska and Yu 1992; Holmes et al. 2018), to date, measures of exposure have been absent from analysis of police-caused killings thus far, despite the fact that exposure is the dimension of segregation most closely aligned with group threat theory. My analysis finds that black-white exposure is related to an increase in the expected count of white killings, which is counter to what I would expect under group threat theory. However, this relationship demonstrates the need for further study of the role of residential segregation in police-caused killing.

Lastly, this study used several data sources in order to assess whether using different data-sources would influence the conclusions reached when empirically testing group threat theory in police-caused killings. As my analyses will show, there is a clear association between measures of group threat and police-caused killings of minorities at the metropolitan and county level, regardless of the data source used.

Implications for Public Policy

This dissertation has several implications for public policy. First, like many researchers have previously stated (Nix et al. 2017; Planty et al. 2015; Ross 2015), the US government needs to act swiftly and establish a reliable and timely account of police-caused killings. The severe undercounting of police-caused killings by our government is problematic and certainly does not help their legitimacy in communities of color which is a foundational feature of effective and just policing (Peyton, Sierra-Arévalo, and Rand 2019). In fact, without police legitimacy, residents are less likely to call the police (Carr, Napolitano, and Keating 2007), report crimes (Desmond Papachristos, and Kirk 2016) and support their local law enforcement agencies (Sunshine and Tyler 2003). Without legitimacy, police-public interactions are more likely to escalate into contests for dominance and result in injury or deaths of police and citizens alike (Peyton et al. 2019). As former FBI director James Comey stated "It is unacceptable that *The Washington Post* and *The Guardian* newspaper from the U.K. are becoming the lead source of information about violent encounters between police and civilians. That is not good for anybody" (Davis and Lowery 2015: Online).

Moreover, as I show in this dissertation, it is important to focus on the distal causes of police-caused killings that may influence the overall, as well as the race-specific incidence of police-caused killings. For majority-minority group relations to improve, minority presence needs to be normalized, a difficult task in such polarizing times. However, research has pointed to some promising ways of doing so. For example, Pettigrew and Tropp (2006) find that intergroup contact can reduce prejudice. Moreover, Park and Glaser (2011) find that after police officers performed modified police-shooting simulations they tend to demonstrate less bias on subsequent simulations. Lastly, procedural justice training in police departments could be a fruitful avenue of intervention as researchers have consistently pointed to an increase in legitimacy and trust between police and the public (Wolfe, Nix, Kaminski and Rojek 2016). However, police officers, administrators, and politicians must realize that in order to reduce disparities in police-caused killings, and improve police-community relations, a multi-faceted approach is needed. There are no simple solutions to the racial disparities observed in police-caused killings and the legitimacy crisis that police departments face after controversial killings.

Chapter Two

GROUP THREAT THEORY AND POLICE-CAUSED KILLINGS

Broadly speaking, research on police use of force has focused on four points of view. *Individual perspectives* focus on the characteristics of police officers that make them more or less likely to use force (Paoline and Terril 2004). *Situational perspectives* shift the focus to the suspect and specific characteristics making them more likely to become a victim of police use of force (McCluskey and Terrill 2005). *Organizational perspectives* seek to explain variation in police use of force by looking at agency-specific characteristics, such as formal policies and operating procedures guiding the use of force of their officers (Smith 2004). Lastly, *ecological perspectives* focus on the effect of the context and how levels of crime, disadvantage, and racial composition affect use of force broadly (Fridell and Lim 2016; Nix et al. 2017). Using group threat theory, this study builds on *ecological perspectives* by focusing on the overall composition and growth of minority populations and its relationship to police-caused killings.

The disproportionate surveillance and control of racial and ethnic minorities by the state is well documented outside the realm of police use of force. Researchers have examined the size and growth of minority populations as predictors of arrests and tickets (Kochel, Wilson and Mastrofski 2011; Langton and Durose 2013), searches (Eith and Durose 2011; Engel and Johnson 2006, pedestrian and motor vehicle stops (Langton and Durose 2013; Lundman and Kaufman 2003), and more general forms of surveillance (Meehan and Ponder 2002). Consistently, the evidence seems to suggest that social control activities disproportionately affect people of color and are often concentrated in minority communities. For example, as part of the "stop and frisk" program in the late 2000s, the New York Police Department made roughly 1.6 million stops of people on the street, with approximately eighty percent of these stops involving blacks and Latinos despite the fact that these racial groups comprise only about 58 percent of NYCs total population (Center for Constitutional Rights 2009). Moreover, the data shows that these stops were concentrated in minority neighborhoods, and as a result, black and Latino new yorkers were nine times as likely to be stopped by police compared to white New Yorkers (Southall and Gold 2019).

Empirical studies at the individual level often focus on various characteristics of police officers such as their race (Gau, Mosher, and Pratt 2010), gender (e.g. Bazley, Lersch and Mieczkowski 2007), experience, and education (e.g. Paoline and Terrill 2007), attitudes (e.g. Worden 1995), and training (e.g. Lim and Lee 2015). Taken together, findings show that police officers who engage in use of force have relatively low experience and education. Among veteran officers and those with higher levels of education it is more common to hold attitudes that are less favorable to using force. Such findings may indicate that more experienced officers are better prepared to de-escalate a situation without having to resort to force. Moreover, more experienced police officers not only use force less often, but when they do, they do so with less lethal options such as Tasers (Fridell and Lim 2016). Additionally, officers' gender and race do not seem to be associated with use of force (Lim, Fridell and Lee 2014; Paoline and Terrill 2007). While these insights are useful in understanding officer characteristics that are often associated with use of force, they do little to inform policy that may help counter the disproportionate use of state violence directed at racial and ethnic minorities. Moreover, looking at individual factors would give credence to explanations that focus on removing "bad apples" or certain kinds of officers from police departments without problematizing the role of policing and state violence in American society. In fact, focusing on the individual characteristics of police officers takes attention away from the institution as a whole and its role in maintaining and promoting a racialized order in which minorities often face the brunt of state violence (Durr 2015).

Situational perspectives are often focused on the individual characteristics of suspects such as their race, (Engel, Sobol and Worden 2000; Klinger and Brunson 2009), gender (Garner, Maxwell and Heraux 2002), social status (Birbeck and Gabaldon 1998), and mental health (Best and Quigley 2003). In fact, recent data provided by *The Guardian* documents the prevalence of police use of deadly force against blacks, Latinos, and suspects who seem to struggle with mental health disorders and are experiencing an episode. In other words, descriptive interpretations of the available data seem to lend support to assertions that have been empirically established. For example, using data form the *Washington Post*, Nix and his colleagues (2017) show how most people killed by police are men (96 percent) and between the ages of 25 and 34 (31 percent). Similarly, Edwards, Lee and Esposito (2019) find that African American men and women as well as Latino men face a higher lifetime risk of being killed by police than do their white peers, with the highest risk for black men. In fact, they claim that police use of force is among the leading causes of death for young men of color (Edwards, Lee and Esposito 2019). Moreover, situational level research has also taken into account the nature of events leading to use of force, such as the seriousness of the offense (white 2002), whether the subject was resisting arrest (Klinger and Brunson 2009; Lim and Lee 2015), and the number of officers on the scene (Durán and Loza 2017; Lim and Lee 2015). The evidence is less straightforward when it comes to gender, with some scholars arguing that police are more likely to use force against men (Engel and Calnon 2004; Kaminski, Digiovanni and Downs 2004; Lim and Lee 2015) and others claiming women (Engel, Sobol, and Worden 2000; Lawton 2007).

In assessing the relationship between race of suspects and police use of force, some have found that minority suspects are more likely to have force used on them (Engel and Calnon 2004; Terrill and Mastrosfski 2002) and others have found no relationship between suspect race and use of force (McCluskey, Terrill, and Paoline III 2005; Sun and Payne 2004). Additionally, researchers suggest that the relationship between suspect race and use of force disappears when accounting for variables such as compliance (Garner, Maxwell, and Heraux 2002). Lastly, some argue that black suspects seem more likely to resist arrest and/or be combative when compared to white suspects (c.f. Belvedere, Worral, and Tibetts 2005; Engel 2003).

However, these arguments not only ignore the troubled past and present between police agencies and communities of color (Durr 2015), but they take resistance, combative behavior, and "*threat*" as an objective measure that is not deeply influenced by racial stereotypes linking racial and ethnic minorities with violence and criminality (Kahn and Martin 2016). There are plenty of instances in which police officers justify using force based on their perception of resistance or combative behavior. For example, when Alton Sterling was murdered by police officers in Baton Rouge, Louisiana the officers not only threatened Sterling with lethal force prior to actually deploying it, but officers repeatedly ordered Sterling to stay on the ground despite that fact that police were on top and in control of Sterling. In this situation, Sterling was clearly perceived as a threat to be neutralized regardless of his actual behavior and compliance.

Threat itself within the context of policing is "the category that animates all police power. To engage in policing is to engage in threat management through identifying, responding, containing, and eradicating various threats to a propertied, always racialized order" (Correia and Wall 2018:232). Threat, in this case is always "operative in official justifications of police violence, as the cop only has to claim that he or she felt threatened or that he or she felt the suspect posed a threat to the public to justify the violence" (Correia and Wall 2018:232). Thus, claims of resistance or threat are inherently beneficial for the state and do not acknowledge the power differential in these claims. So long as an officer invokes threat, he or she is immune or excused from accountability even in egregious acts and questioning the validity of threat claims is nearly impossible (Kindy 2015). For example, Walker Scott was fatally shot in the back several times by a South Carolina police officer after getting pulled over for a non-functioning brake light. Initial reports stated that Scott had taken the Taser away from the officer which caused the officer to fear for his life, leading to the shooting. This assertion would not have been challenged if it wasn't for a video that emerged showing the officer aiming and shooting at Scott as he ran away.

While the stories presented here are clearly the most extreme cases selected to illustrate how police can justify use of force by claiming that there was a threat to be neutralized in cases where any reasonable person would not feel threatened, in most cases, regardless of the circumstances, police officers rarely face prosecution when using fatal force (Kindy 2015). As part of their coverage on police-caused killings, *The Washington Post* details how few police officers have been charged with wrongdoing when using fatal force on civilians. Their reporting indicates that since 2005 only 54 officers have been charged. Moreover, the reporting illustrates time and time again how police officers have shot civilians in ways that are problematic. As criminologist (and former police officer) Philip M. Stinson argues, "To charge an officer in a fatal shooting, it takes something so egregious, so over the top that it cannot be explained in any rational way" (Kindy 2015). Lastly, the report indicates that while almost all suspects were unarmed at the time of their deaths, most of suspects were black and the officers pulling the trigger were white (Kindy 2015).

Organizational perspectives seek to account for variations in officer use of force by looking at agency-level characteristics. The main argument from this line of research indicates that restrictive deadly force policies and comprehensive training results in fewer instances of police use of deadly force (c.f. Alpert 1989; Alpert and MacDonald 2001). However, just like the previous perspectives discussed, the evidence in support of *organizational perspectives* is mixed with some finding that agencies with more written policies did not have significantly fewer cases of deadly force (Smith 2004). In fact, Smith finds that the number of field training hours officers receive after completing the police academy is positively related to the number of civilians killed by police (2004). Thus, the relationship between agency characteristics and use of force may be more complex than currently theorized. This specific relationship can point to the nature of training that is given to police officers, which often emphasize threat neutralization and control as opposed to de-escalation (Smith 2004). More recently, Nowacki (2015) finds that departmental policies limiting officers' discretion as well as the size of the police department are associated with reduced lethal force incidents for all and black killings but did not influence white killings.

Lastly, the *ecological perspective* attributes variations in police use of force, or police behavior more generally, to the broader context in which police officers operate (Klinger 1997; Terrill and Reisig 2003). According to this line of inquiry, officers are more likely to use force in dangerous areas, with high levels of disadvantage, and high homicide rates (Terrill and Reisig 2003; McCluskey et al. 2005; Lim et al. 2014). More recent research by Klinger and his colleagues (2016) does not find disadvantage or the racial composition of neighborhoods to influence police shootings, however, according to their results, the rate of firearm violence does influence police shootings. More specifically, neighborhoods with moderate levels of firearm violence exhibited the highest number of shootings by police (Klinger et al. 2016; Nix et al. 2017). As with all perspectives, support for *ecological perspectives* is mixed regarding police use of force.

Police Deadly Force

Previous scholarship examining black-white differences in police-caused killings suggest that police "have one trigger finger for whites and another for blacks" (Takagi 1974:30). Durán and Loza (2016) provide one of the most detailed and extensive analyses of the two-trigger-finger hypothesis, analyzing three decades of data between 1983 and 2012 in the city and county of Denver, Colorado. Their study highlights differences in 218 police-caused shootings involving whites, blacks, and Latinos by focusing on three main themes (suspect characteristics, officer characteristics, and contextual factors). Regarding the first theme (suspect characteristics), the results show that black and Latino suspects who were shot were significantly younger than white suspects, with median ages of 18 for blacks and 22 for Latinos, compared to 26 for whites. Alcohol and drug use, as well as mental health, was reported at significantly higher rates for whites compared to blacks and Latinos. Gang membership was only reported for black (15.7 percent) and Latino (6.6 percent) suspects, who are often labeled dangerous regardless of actual gang affiliation (Kahn and Davies 2011). Lastly, officers showed more compassion and restraint toward white suspects even when they were in possession of a firearm. However, officers were disrespectful and confrontational toward black and Latino suspects and fired their weapons more quickly (Durán and Loza, 2016). Overall, the narratives presented by Durán and Loza's (2016) study show a lack of compassion by officers toward minorities both before and after the shootings took place.

In terms of officer characteristics, Durán and Loza (2016) found that when the shooting involved a white suspect, the number of officers at the scene and the number of officers who fired their weapon was higher than when the suspect was black. Moreover, the number of shots fired along with the number of times the suspect was hit was also higher for whites, indicating a higher potential for death. However, although officers were hardly injured in these incidents, they were more likely to be victimized by white suspects than by black or Latino suspects. When police-caused shootings involved more than one officer, the officers were usually men, white, and relatively new, with a median of four years on the force (ibid). The results regarding contextual factors indicate that the area in which suspects were shot matched the racial and ethnic characteristics of the suspect in question. However, there were significant differences in how long it took officers to shoot their weapon upon arriving to the scene. Officers took longer before shooting white suspects (median of 4 minutes) compared to black suspects (median of 2 minutes). Lastly, almost every shooting in the three-decade period that was analyzed in their study was deemed legally justifiable (ibid). Recent reporting by *The Washington Post* seems to confirm such findings (Kindy 2015) and show how police departments actively try to control their appearance in order to protect themselves against public complaints and accusations of favoritism (see also Manning 1978)

Previous ethnographic studies on police use of deadly force provide rich details in understanding the disproportionate use of deadly force toward black and brown bodies. They also point to the difficulties in obtaining data at a national level, which can, in turn, be used to provide a general understanding across localities in the United States. In studying police use of deadly force, particularly police-caused killings, the biggest impediment has been the lack of reliable data. Researchers looking at police-caused killings have used three main data sources. 1) The National Center for Health Statistics (NCHS) National Vital Statistics System (NVSS), which keeps a count of deaths by legal intervention; 2) The Supplementary Homicide Report (SHR) program provided by the FBI's UCR, this includes counts of "justifiable homicides" by law enforcement officers; and 3) the US Bureau of Justice Statistics (BJS) Death in Custody Reporting Program (DCRP), which, beginning in 2003, attempts to provide information about every citizen death (regardless of cause) at the hands of the police at any stage in the criminal justice program (Klinger 2012).

There are several problems in using these sources, but the biggest drawback is the unreliability and undercounting of deaths by as much as 51 percent (c.f. Sherman and Langworthy 1979; Klinger 2012; Planty, Burch, Banks, Couzens, Blanton, and Devon 2015). In fact, some of the most controversial deaths in recent years (i.e. Tamir Rice, Eric Gardner) have not made it to official counts. Thus, results from these data should be interpreted with caution.

No matter the data source used, a problem that plagues all studies is the way researchers account for deadly force with available data that do not capture the entirety of state violence. While several data sources track deaths at the hands of the police, this alone is not an accurate measure of deadly force use. As Fyfe (1978:32) notes, it is the pulling of the trigger that matters, not the result of those shots. He states:

> Deadly force is physical force *capable* of or *likely* [emphasis in original] to kill; it does not always kill. The true frequency of police decisions to employ firearms as a means of deadly force, therefore, can best be determined by considering woundings and off-target shots as only fortuitous variations of fatal shootings. All are of a kind.

My study uses data that is superior to much of the data used to study police use of deadly force at the national level, but my findings are also limited to only accounting for those who are killed.

With the emergence of new data sources, researchers are rapidly adding to the body of knowledge regarding police-caused killings and racial bias. However, as expected, the evidence is also mixed. For example, using several multilevel Bayesian models and national-level data, Ross

(2015) finds that armed black and Latino/a residents have 2.94 and 1.57 times the probability of being shot by the police than their armed white counterparts (Ross 2015). Looking at death certificates, Buehler (2017), in line with Ross, finds that black and Latino men are 2.8 and 1.7 times more likely than whites to die from legal intervention by the state. Edwards, Lee and Esposito (2019) find that the risk of being killed by police is highest for black men, who face about 1 in 1,000 chance of being killed by police over the life course. In fact, they argue that for young men of color, police use of force is among the leading causes of death (Edwards, Lee and Esposito 2019).

These studies point to racial bias in police-involved violence by focusing on the disproportionate effect that police use of lethal force has on minorities. Experimental research has also contributed to our understanding of racial bias, particularly biases that operate subconsciously, and are not easy to recognize, or "implicit bias" (for a detailed review of implicit bias in policing see Staats 2014). Through a series of simulation experiments, Payne (2001) demonstrates that participants in his study were faster to identify an object as a gun when they were shown a black face than when they were shown a white face. Participants also mistakenly identified tools as guns when they were shown a black face (Payne 2001). Further experimental studies by Correll and his colleagues (2002) also provide support for the implicit bias perspective. Their study demonstrates that participants shot more quickly at armed black suspects than armed white suspects (Correll, Park, Judd, and Wittenbrink 2002). They also found that participants tend to respond faster and more accurately when the target is consistent with prevailing cultural stereotypes—unarmed white and armed black targets. Glaser and Knowles (2008) find similar support, in that stereotypic associations between blacks and weapons predict the intensity of shooter bias.

Other experimental studies have tested the implicit bias perspective argument using real police officers. For example, Cox and his colleagues (2014) had police officers' complete shooter simulations in different realistic scenarios, such as responding to different types of service calls. Their findings show that officers were faster to shoot black suspects than white suspects. However, this was only the case when officers went through picture simulations, with the effect disappearing after going through video simulations (Cox et al. 2014). Although useful for understanding the association between bias and police shooting decisions, such studies are hard to replicate in other situations, and their findings are often specific to the sample employed. Moreover, these experimental studies claim that bias is present in the decision to shoot suspects, but that such bias is implicit or unconscious. In making this claim, researchers often argue that violent responses reflect automatic processes and do not reflect overt and or explicit bias. For the purposes of this study, measures that have been traditionally associated with "implicit bias" will be referred to as measures of bias, but I will not label this as "implicit." In failing to do so, I argue that it is impossible to really know whether the decision to shoot or the process of misattribution that leads officers to mistakenly shoot unarmed minority civilians is informed by more than just unconscious responses. It could very well be the case that an officer has explicitly held views that black and Latino suspects are inherently more dangerous than white suspects.

Other researchers have found no racial bias in the shooting of civilians using quantitative methods as well as experimental ones. For example, using data from police departments in cities located in Texas, Colorado, and Washington, Fryer's (2019) analysis of police violence reveals that blacks and Latinos are no more likely than whites to be shot by police, but they are fifty percent more likely to experience violence not leading to death when stopped by police. However, Fryer's analysis was conditioned on suspects being stopped by police, which has been shown to be racially biased (Ferrandino 2015). Using police officers in highly realistic simulations, James and his colleagues (2014, 2016) show that police are more hesitant to use force against minorities for fear of backlash or criminal prosecution. Responding to previous literature on implicit bias, they argue that police officers display a "counter bias" effect, that is, police officers are less likely to shoot black civilians than white civilians. Although they theorize that such a counter-bias effect stems from fear of prosecution, police officers who use force rarely get charged, regardless of how problematic the circumstances surrounding the shooting are (Kindy 2015). Additionally, while I do not doubt Kindy's findings, it is worthwhile to note that police shooting simulations are subject to the Hawthorne effect, a type of participant bias where research subjects change their behavior because they know they are being watched.

USING GROUP THREAT THEORY TO UNDERSTAND POLICE-CAUSED KILLINGS

From the existing literature, it seems clear that there are more questions than answers in assessing the relationship between bias and police-caused killings. Recent quantitative studies continue to offer conflicting results regarding the role of race in police-caused killings. For example, using data from the *Washington Post*, Nix and his colleagues (2017) find that among those who were shot by police, people from "other" minority groups were less likely to have been attacking the police, and that blacks were more than twice as likely as whites to have been unarmed when they were shot by police. Nix and his colleagues claim that their findings suggest "evidence of implicit bias in real-world scenarios" although, as I mention above, the bias could also be explicit

(2017:329). Benchmarking police-caused killings against race-specific crime rates Cesario and his colleagues (2018) claim that blacks are not more likely to be killed than whites, and in fact, when looking at police-caused killings benchmarked on race-specific weapons violation data, a clear pattern of anti-white bias emerges.

The approach taken by Nix and his colleagues (2017) and Cesario and his colleagues (2018) in assessing whether bias is present in police-caused killings is quite different. They use different data for their studies, different methodology, and not surprisingly, they reach different conclusions. Cesario and his colleagues (2018) imply that police officers objectively measure threat and only deploy force when necessary (or when people are being arrested for violent crimes and weapons violations). It has long been established that use of force is always present in any police-citizen interaction, regardless of whether citizens demonstrate an objective or perceived threat (Bittner 1970). Cesario and his colleagues (2018) attempt to assess whether racial disparities exists when conditioning deaths on violent crime data, while Nix and his colleagues (2017) attempt to determine whether there is evidence of bias in police-caused killings by determining whether minorities are as likely as whites to have been shot due to bias. In order to provide more clarity, I follow this line of inquiry and investigate both claims.

In investigating what drives observed racial differences in police-caused killing, I am particularly interested in evaluating whether there is evidence of racial prejudice using an approach grounded in a conflict perspective that views prejudice as a sense of group position (Blumer 1958). That is, instead of being preoccupied with individual lines of inquiry (such as assuming that police may be racially biased), I am concerned about collective processes "by which a racial group comes to define and redefine another racial group" (Blumer 1958:3). The proposed approach to study the relationship between race and police-caused killings is congruent with the approach taken by Blumer (1958) in theorizing prejudice as a sense of group position. In discussing how racial prejudice emerges, Blumer points to a collective process of defining ones' racial group vis-à-vis defining the other group. Thus, the sense of social position that emerges from this process serves as the basis for racial prejudice (Blumer 1958). He describes four important feelings that create race prejudice. These are 1) a feeling of superiority, 2) a feeling that the subordinate race is intrinsically different and alien, 3) a feeling of proprietary claim to certain areas of privilege and advantage, and 4) a fear and suspicion that the subordinate race harbors designs on the prerogatives of the dominant race (Blumer 1958). For the purposes of this study, I am more concerned with the fourth feeling described by Blumer (1958) or acts that are threatening or perceived to threaten the position of dominant groups.

Blalock (1967) built on Blumer's (1958) theory and specified distinct forms in which whites use their disproportionate power to implement state control over minorities. In a nutshell, racial threat theory proposes that in the face of large or growing minority populations, whites feel threatened and engage in racialized social control activities in order to protect their existing power and privilege (Blalock 1967). Blalock proposes three distinct forms of racial threat that encourages white support for state-sanctioned racialized social control practices. *Economic threat* refers to a threat to whites' job availability, stability and wages. *Political threats* are fears associated with losing political power and representation in the face of a large or growing minority population. Lastly, *symbolic threats* refer to whites perceiving non-whites as inherently more dangerous or deviant (Blalock 1967).

Blalock (1967) argued that the size of the minority population relative to the majority provides a necessary indicator of macro-structural threats that is needed in order to test hypotheses empirically. It should be pointed out that the majority of studies testing some variant of the racial threat hypothesis often rely on simple percentages of the black population as measures of racial threat (c.f. Liska and Yu 1992; Sorensen Marquart, and Brock 1993; Smith 2003, 2004; Jacobs and O'Brien 1998; Holmes et al. 2018), and more recently percentages of the Latino population as measures of ethnic threat (c.f. Holmes, Smith, Freng and Muñoz 2008; Holmes et al. 2018; Zane 2018; Feldmeyer, Warren, Siennick and Neptune 2015; Charmichael and Kent 2014; Kent and Jacobs 2005; Stucky 2005). I argue that tests of the threat hypothesis should employ a measure that is true to how Blalock conceived it, and I use relative, rather than absolute, population size.

Overall, racial threat theory has received ample support in studies looking at the effect of threat on police resource allocation and sentencing and mixed support in studies looking at the effect of threat on arrests, sentencing and police-caused killings. Research on police resource allocation usually examines whether racial threat indicators influence police expenditures as a measure of police force. Since police are often called to deal with various forms of conflict (Turk 1969), there is an expected relationship between growing minority populations and police force size (Kent and Jacobs 2005). Empirical analyses show that the size of the minority population is associated with measures of police force. For example, Juff and Stahura (1980) find that the percentage of black residents significantly predicts the number of officers employed in suburban areas when controlling for violent and property crime. Jackson and Carroll (1981) conclude that the size of the minority population is associated with policing expenditures (c.f. Jackson 1989). Such findings remain consistent, even when conducting cross-national analyses. Kent and Jacobs

(2004) note that out of 11 developed nations, the United States is the only nation where they can be certain that there is a relationship between minority population and police size. More recently, Carmichael and Kent (2014) find that racial threat and economic inequality work independently and jointly to produce shifts in the size of police forces even after accounting for levels of crime. Their results also suggest that racial threat effects appear to have expanded between 1980 and 2010.

Studies looking at the relationship between racial threat and police resource allocation have also incorporated measures of the size of the Latino population. Similarly, researchers tend to be supportive of an association between the large and growing Latino populations and police force size (Holmes et al. 2008; Stucky 2005). For example, Holmes and his colleagues (2008) find that Latino population size has a positive effect on police expenditure and that proximity to the US-Mexico border is an important consideration when modeling the effect of Latino threat on police expenditures. They find that states closer to the border have higher police expenditures. Stucky (2005) also finds a positive relationship between percent Latino and police strength, stating that the effect of Latino presence on police strength was consistent with a classic racial threat argument. More recently, Zhao and his colleagues (2012) find a negative relationship between the size of the Latino population and police force in their longitudinal study of police employment across cities; and Carmichael and Kent (2014) find no relationship between a growing Latino population and the size of police departments across the United States. Despite recent lack of evidence for a Latino threat and the size of police departments, it seems clear that racial threat—at least as measured by the percent of the population that is black—is among the strongest predictors of the size of municipal police departments. Such findings are consistent with reviews provided by Sever

(2003) in which he finds that only 5 percent of the 28 empirical analyses he reviewed failed to support the racial threat perspective.

Studies assessing the relationship between racial threat effects and arrests rates have been less conclusive in their support for the theory. Liska and Chamlin (1984) find considerable variation in arrest rates between cities and that the racial composition of cities substantially accounts for this variation independent of crime rates. Specifically, they find that while the size of the non-white population is associated with increases in total arrest rates, it does not influence white arrest rates. Chamlin and Liska (1992) do not find support for the racial threat perspective and find that percent non-white has a negative relationship to arrest rates for personal and property crime rates. Using multilevel models of violent crime rates reported to police in 182 cities during the 2000s, Stolzenberg and his colleagues (2004) find that black residents have a lower probability of arrest in cities with a relatively large black population. Similarly, Parker and his colleagues (2005) find that the size of the black population and black immigration patterns have a negative effect on black arrest rates in urban cities. More recently, Eitle and Monahan (2009) find that racial threat measures are associated with race specific crime arrests. Moreover, they find that organizational characteristics of police departments largely moderate the relationship between racial threat and race-specific drug arrests. That is, police departments with more formal rules demonstrate a weak or null relationship between economic competition variables and black arrest rates, whereas departments with no or few rules exhibit the strongest associations between economic competition and black drug arrests rates (Eitle and Monahan 2009).

In the context of sentencing decisions, there has been mixed results in terms of empirical support for racial threat. Studies providing support for racial threat find that the percentage of blacks in the population is related to increased racial and ethnic disparities in imprisonment (Myers and Talarico 1987; Wang and Mears 2010; Weidner, Fase and Schultz 2005). For example, using data from the Bureau of Justice Statistics, Wang and Mears (2010) find that increases in racial threat were associated with tougher sentencing of convicted violent and drug offenders. Moreover, they find that in areas where the baseline minority population was high, further increases in ethnic threat were associated with tougher sentencing of offenders. Similarly, the percentage of blacks in the population has been linked to increases in sentence length (Ulmer and Johnson 2004), departure from sentencing guidelines (Johnson 20005), and judges' willingness to withhold adjudication for black defendants (Bontrager, Bales and Chiricos 2005). More recently, Feldmeyer and his colleagues (2015) find that while black defendants were more likely to be sentenced to prison and receive longer sentences in counties with a growing black population, similar results do not hold for Latino defendants.

Other studies find mixed or no evidence supporting the racial threat hypothesis. For example, Ulmer and Johnson (2004) find that county-level black and Latino populations have a positive effect on racial disparities in sentence length but not in incarceration. Similarly, some research finds that minority population size has no effect on racial disparities in state sentencing (Britt 2000; Weidner, Frase and Schultz 2005) or in federal sentencing for drug cases (Kautt 2002). Moreover, some actually find a negative relationship between minority group size and sentencing (Britt 2000) and sentence length (Feldmeyer and Ulmer 2011). For example, Britt (2000) finds that larger black population size is associated with shorter sentences and Feldmeyer and Ulmer (2011) find that the size of the minority population has no effect on sentence length for blacks and is linked to shorter sentences for Latinos.

Much like the racial threat literature on police-resource allocation, arrests, and sentencing decisions, empirical studies looking at police-caused killings have offered mixed support. Using vital statistics data to examine predictors of police-caused killings, Kania and MacKey (1977) and Liska and Yu (1992) find that variations in police-caused killings are a function of police officers' perception of threats associated with a large non-white population. Liska and Yu (1992) conclude that threat is perceived as coming from the proportion of non-whites as well as violence in the community. Liska and Yu (1992) note that the proportion of non-whites in cities is related to the killing of both whites and nonwhites.

Other analyses have largely relied on Supplementary Homicide Reports (SHR) data on police-caused killings from the Uniform Crime Reports (UCR). These analyses are mostly supportive of the racial threat argument. Using four years of data, Sorensen and his colleagues (1993) find that the proportion of black residents in cities with 100,000 and 250,000 or more residents is associated with higher rates of police-caused killings. Similarly, using six years of data, Jacobs and O'Brien (1998) find that the percent of black residents in cities with 100,000 or more residents is related to police-caused killings of blacks. However, unlike Liska and Yu (1992) their analysis does not find an association between percent black and the total rate of police-caused killings. Furthermore, Jacobs and O'Brien (1998) show that change in the percentage of blacks in cities, as well as the ratio of black to white income is significantly associated with increases in the killing of blacks. Smith (2003, 2004) and Willits and Nowacki (2014) found a significant relationship between the proportion of black residents and overall police-caused killings. Similarly, Lautenschlager and Omori (2018) find a significant relationship between percent black and police-caused killings.

Most of these studies also look at the effect of percent Latino on police-caused killings, however their results are largely non-significant (Smith 2004; Willits and Nowacki 2014; Holmes et al. 2018). Only Holmes and his colleagues (2018) claim to have found a statistically significant effect for percent Latino. However, this effect is only significant when using $p<.10$ as their level of significance. In using race-specific models, both Smith (2004) and Holmes and his colleagues (2018) found that percent black was a strong predictor of police-caused killing of blacks. Interestingly, Holmes and his colleagues (2018) find a statistically significant negative relationship between percent black and the overall incidence of police-caused killings. Others find no relationship between racial composition and deadly force (Eitle, D'Alessio and Stolzenberg 2014; Parker et al. 2005), and Klinger and his colleagues (2016) find an initial effect between percent black and police-caused killings, but this effect is completely mediated by firearm violence in neighborhoods. Considering the discrepancies between studies on the relationship between threat and police-caused killings, more work is needed.

SITUATING THE CURRENT STUDY

I believe there are four main discrepancies that may be influencing the varying results across studies. First, few of the analyses test all the conceptual models described by Blalock (1967) and specifying the wrong functional form of the relationship between variables can, in turn, influence the conclusions drawn from results. Second, most recent analyses do not account for the growing minority populations or changes in the minority population over time, which has been shown to be influential in accounting for police-caused killings (c.f. Jacobs and O'Brien 1998). Third, studies have relied almost exclusively on the proportion of black and Latino residents as measures of threat, thus ignoring relative (to white) population size, as theorized by Blalock. Lastly, studies have not tested whether using different data and units of analyses matter in finding support (or not) for racial threat theories.

Most analyses have modeled threat linearly (Smith 2003, 2004; Sorensen et al. 1993; Jacobs and O'Brien 1998; Willits and Nowacki 2014) under a minority threat model. Liska (1992) hypothesized a linear relationship between threat and social control, arguing "The greater the number of acts and people threatening to the interests of the powerful, the greater the level of deviance and crime control" (18). As such, a linear relationship would imply that social control efforts could be deployed against minorities regardless of how large the minority group size becomes. However, Blalock has interpreted threat to imply a non-linear relationship between percent non-white and social control outcomes. In economic competition models, threat posed by minorities is positively associated with racial domination activities, but with a decreasing slope. The slope increases rapidly past a tipping point in power-threat models. Thus, in order to fully test threat models as conceptualized by Blalock (1967), researchers should fit threat models as linear, logged, and quadratic functions rather than assuming a single form to see which best fit their data.

Only Liska and Yu (1992) assessed all three models and found no support for economic or power threat models. More recently, Holmes and his colleagues (2018) test power-threat models and claim support for power threat models using $p<.10$ as their level of significance. Although non-linear specifications of minority threat have been used in the context of sentencing (c.f. Feldmeyer et al. 2015; Wang and Mears 2010), prison admissions (Keen and Jacobs 2009), and police force size (Stults and Baumer 2007) to date, no study has evaluated how well these different models fit the relationship between threat and police-caused killings using race-specific models for whites, blacks and Latinos. Assessing how well these different models fit the data is essential in order to further develop models that can accurately account for the expected count of police-caused killings.

Another puzzling issue with recent studies has been the lack of attention paid to changes in the minority population and how such changes are associated with formal social controls. Chamlin (1989) has argued that the exclusion of dynamic indicators (or changes in the minority population over time) of threat may lead to model misspecification and spurious results. In fact, Chamlin (1989) does not find significant support for racial threat in predicting police force size using static indicators but does find a significant relationship using changes in the minority population over time. Similarly, Caravelis and his colleagues (2011) find the same when looking at sentencing outcomes using multilevel analyses. Their results indicate that an increasing black population over time is associated with increasing odds of being sentenced as a habitual offender for black and Latino defendants (Caravelis, Chiricos, and Bales 2011). Although earlier studies have examined the relationship between changes in the minority population over time and police violence (Jacobs and O'Brien 1998) and punitive attitudes (King and Wheelock 2007), most recent studies of police-caused killings have only used static measures of threat (c.f. Holmes et al. 2018; Lautenschlager and Omori 2018).

Blalock (1967) emphasized the importance of the relative size of the minority population as well as its growth over time. Similarly, Liska (1992) states that "changes in the absolute or relative size of certain classes and distributions of people and of certain acts may be as or even more threatening than their absolute or relative size" (186). We see this, for example, with hostile state legislative responses to increases in the Latino population, such as Alabama's HB56, despite that fact that Latinos, overall, comprise only 4 percent of Alabama's population (Arrocha 2011). Given the stated theoretical importance of a *growing* minority population in measuring threat, it seems surprising that much of the research assessing the relationship between threat and police-caused killings has not incorporated dynamic measures of threat.

A third issue plaguing recent studies of racial threat and police-caused killing is the lack of innovation in terms of measuring threat as well as other constructs that have traditionally been incorporated in models predicting social control outcomes. What I mean by this is that in analyzing the relationship between threat and police-caused killing, researchers have mainly used the number of black (Smith 2003,2004; Willits and Nowacki 2014), minority (Liska and Yu 1992), and Latino residents (Smith 2003,2004; Willits and Nowacki 2014; Holmes et al. 2018) as a proportion of the overall population as measures of threat. However, Blalock (1967) was concerned with the size of minority populations relative to whites which would imply a more robust measure as opposed to simple proportions. For example, knowing that 20 percent of the population in a city is black does not account for the majority group size, since there is no reason to assume that the rest of the population is white. Instead, using a ratio of black (or Latino) to white residents should better capture the "threat" size of minority populations, thus more adequately tapping into the arguments put forward by Blalock (1967).

For measures of threat beyond population proportions, researchers have used race-specific ratios in order to better account for economic or political threats to the majority. For example, Jacobs and Woods (1999) use the black to white ratio of unemployment to measure economic competition and find that in cities where the unemployment of blacks approaches that of whites, killings of blacks by whites increase. Similarly, Eitle and his colleagues (2002) use a ratio of black to white votes cast in the general election to measure political threat, stating that "this measure is a better indicator of black political threat than black population size because voting requires an expenditure of time and effort on the part of the individual" (564). Other studies use similar measures to capture black voting and political representation and find an association between black voting and political representation and the likelihood of jail and prison sentences (Wang and Mears 2010) as well as police force size (Stults and Baumer 2007). Only Jacobs and O'Brien (1998) operationalize racial inequality as the ratio of black to white mean family income in studies looking at police-caused killings, while some (Smith 2004) use ratios to account for the racial representation of police departments. Overall, using ratios instead of simple population proportions, or economic characteristics of minorities better accounts for the standing of minorities relative to the majority, which, was of crucial concern for Blalock (1967). Thus, studies assessing the relationship of group threat and police-caused killings will benefit from using ratios to account for the various ways in which minorities could be perceived as threatening by whites.

Another key concern for Blalock was the effect of segregation on racial discrimination more broadly. Blalock (1967) argued that segregation maintains clear divisions between whites and minorities more broadly in US society. As such, changes in segregation "entails direct and obvious affront not only to white individuals, but to the entire dominant group. The symbolic value of the segregation, whatever its specific nature may be, means that dominant-group mobilization is easy to arouse" (1967:125). Similarly, researchers such as Liska and Chamlin (1984), hypothesized segregation to have a "benign neglect" effect. In studying the effect of segregation on arrest rates, they find that segregation can lead to reduced levels of arrests "by increasing the ratio of intraracial to interracial crime for nonwhite offenders, the segregation of nonwhites decreases the pressure on police to control crime, thereby decreasing the arrest rate, especially that of nonwhites" (385).

Some researchers have found a negative relationship between segregation and arrest rates (Liska and Chamlin 1984; Parker et al. 2005) as well as arrest likelihood (Stolzenberg et al. 2004). To date, no studies looking at the relationship between racial threat and police-caused killings have used measures of exposure, and studies that have used measures of segregation often find that black-white segregation measures increase the expected counts of police-caused killings (Liska and Yu 1992; Holmes et al. 2018). Recently, Latino-white segregation measures have also been incorporated into models. For example, Holmes and his colleagues (2018) look at the effect of Latino-white segregation, but do not find any significant results.

In extending previous knowledge on this issue, I incorporate Latino-white measures of segregation consistent with other scholars (Holmes et al. 2018; Smith and Holmes 2014) but opt for measures that better capture minority group exposure to the majority. Among all measures of residential segregation, the commonly used index of dissimilarity fails at capturing potential interaction between minority and majority group members, which is key in understanding how minorities may be perceived as threats across communities. Blalock (1967) was concerned with the perceived threat that interactions between majority and minority group members could elicit, and as such, an appropriate measure of residential segregation would be a measure of exposure, not evenness.

Lastly, despite serious concerns with the validity of official estimates of police-caused killings (Planty et al. 2015; Williams et al. 2016), researchers have not thoroughly investigated whether using different sources that document police-caused killing influence the results of analyses testing racial threat theory. For example, Holmes and his colleagues (2018) dismiss the potential issue of undercounting by using SHR estimates on police-caused killings but fail to consider the potential effects of using other data sources. To date, no study assessing the relationship between racial threat and police-caused killings have used multiple data sources to evaluate whether "official" sources (SHR or Vital Statistics Program) or "unofficial" sources (Fatal Force, The Counted) provide similar conclusions in analyses. If the unofficial sources—which document as many as twice as many annual killings—provided different results than official sources, then the claims that the killings missing from the SHR data are missing at random should be reconsidered.

Similarly, choosing the "right" unit of analysis in evaluating the effect of threat on social control outcomes may account for some of the inconsistencies in current studies. Most structural analyses attempting to explain police-caused killings use large cities as their unit of analysis (Smith 2003, 2004; Liska and Yu 1992; Holmes et al. 2018), metropolitan areas (Jacobs 1979), counties (Ross 2015), or census tracts (Klinger et al. 2016). Although researchers often run separate analyses for large and small cities (defined as cities with at least 100,000 residents and cities with 250,000 residents or more; Smith 2003,2004; Jacobs and O'Brien 1998), to date, no studies have used different geographical areas in their evaluation of group threat in order to assess whether findings are contingent on the unit of analysis selected. I assess the relationship between group threat and police-caused killings using metropolitan areas, counties, and individual level data on people killed by police to assess whether group threat effects are contingent on the unit of analysis used.

Moreover, while some acknowledge the need for multi-level tests of racial threat in police-caused killings (see Holmes et al. 2018), few studies have used multi-level models in looking at police-caused killings (Ross 2015). Researchers have stated the importance of assessing threat effects at multiple levels of aggregation (Feldmeyer et al. 2015; Wang and Mears 2015). For example, Felmeyer and his colleagues (2015) find that black defendants are more likely to be sentenced to prison and are given longer sentences in counties with growing black populations. Similarly, Wang and Mears (2015) find that state-level racial context amplifies the threat effects of county-level racial context. Acknowledging that threat may be operating at multiple levels and responding to calls for multi-level models of police-caused killings (Holmes et al. 2018), this study will use multi-level models to assess whether county-level measures of threat affect individual-level measures of racial bias.

Overall, this study makes several noteworthy contributions to research on racial threat and police-caused killings. First, my analyses incorporate all conceptual models as described by Blalock (1967) by fitting linear, logged, and quadratic function of threat. Second, I use dynamic measures of threat that are better aligned with Blalock's (1967) concerns over the effect of a growing minority population. Third, I use several novel measures of racial threat that have not been used within the context of police-caused killings. That is, I measure threat using several black to white and Latino to white ratios in measuring population and income differentials. Moreover, I introduce different measures of residential segregation that account for minority-majority interactions. Lastly, this study not only uses both "official" and "unofficial" data sources to evaluate whether this decision affects my results, but I also evaluate the relationship between threat and police-caused killings using metropolitan areas and counties, as well as multi-level models accounting for threat effects at different levels.

Chapter Three

DATA SOURCES

The data used for this project comes from several sources. Data on police-caused killing come from three sources. The first source is The Counted, a project of the British newspaper *The Guardian*, which provides verified, crowd-sourced information on "any deaths arising from police encounters with law enforcement" in the United States (Swain et al. 2016: online). The second source is Fatal Force, a project of *The Washington Post*. Their database "contains records of every fatal shooting in the United states by a police officer in the line of duty since Jan. 1, 2015" (Tate et al. 2016: Online). The third source is the Supplementary Homicide Reports (SHR), which include the incidence of felons killed by police and reported to the FBI by local police agencies as part of the Uniform Crime Reporting (UCR) program. The bulk of demographic data used as predictors in this project comes from the American Community Survey (ACS), a federal data collection project that replaces the US Census long form and is the premier source for detailed population and housing information about the United States (Census n.d.). Additionally, data used to calculate population growth comes from the 2000 and 2010 decennial census. Lastly, crime data for this project come from the UCR program, which gathers crime statistics from law enforcement agencies across the nation and reflects the total number of crimes known to law enforcement agencies, and data on those arrested by age, sex, and race across multiple levels of aggregation.

The Counted

The Counted database combines reporting by *The Guardian* with verified crowdsourced information on people killed by the police and other law enforcement agencies in the United States throughout 2015 and 2016 and reports demographic data and a narrative on how the incident took place. Unlike the sources to follow, these data attempt to account for all people killed by police, regardless of the way in which people were killed. In other words, although the sources listed below account for the number of people shot by police, these data include all people killed by police (i.e. Tasered to death, choked to death, bludgeoned to death, etc.; Swain et al. 2016). This effort emerged as an attempt to provide the public with reliable information on the total number of people killed by the police and their demographic information as the US government has no comprehensive record of people killed by police.

The Counted is not the only crowd-sourced account of police-involved killings, but it has the highest production value, probably because at least 21 paid *Guardian* staffers worked on the project. The Counted does not include *all* police-involved killings. By their own account, The Counted excludes deaths in custody that are self-inflicted, such as overdoses or suicides. For example, Sandra Bland—a women whose alleged 2015 suicide in a Texas jail following a minor traffic violation led to an FBI investigation and a conclusion of improper policing—is not among The Counted. Deaths in accidents during police chases are also excluded, unless a police vehicle was directly involved in the collision. Also missing are complicated and speculative death accounts, such as the deaths from a May 20, 2015 shootout between rival motorcycle gangs and police in a restaurant in Waco, Texas, where it remains unclear who was killed by police and who was killed by bikers, although, presumably, these cases will be added as that information becomes known. Publicly posted decision rules on The Counted website oversimplify the process by which *The Guardian* staff determine inclusion and exclusion, according to a conversation with a *Guardian* staffer who wished to remain off the record.

According to the database, there were a total of 1,146 people killed in 2015 and 1,093 killed in 2016. Out of these deaths, 584 victims were white, 307 were black, and 195 were Latino in 2015 and 574 victims were white, 266 were black, and 183 were Latino in 2016. Although Native Americans and Asian/Pacific Islanders were also included in these data, I classify them under minority deaths since the focus of this project is on blacks, Latinos, and non-white citizens in the aggregate. Thus, in 2015, 562 victims of police-caused killings were non-white citizens and in 2016, 519 victims of police-caused killings were non-white. So far, The Counted only provides data on police-caused killings for 2015 and 2016.

My analyses do not include the total number of people killed by police as reported by The Counted due to missing data on other sources and the fact that I am conducting analyses at multiple levels of aggregation. For example, analyses of deaths in metropolitan areas do not include those killed in non-metropolitan areas. Table 3-1[4] shows the total number of killings reported by The Counted and the total number of cases used for the analyses using these data. After losing cases to non-metropolitan areas and missing data on other indicators used in the analyses, the total number of deaths is 862 police-caused killings in 2015 and 803 in 2016. Of these killings, 486 were non-white victims, 250 blacks, 174 Latinos and 376 whites in 2015. In 2016, out of the total number of police-caused killings, 418 were non-white, 208 black, 162 Latino and 385 white. On average, I use 78 percent of total cases reported by The Counted[5].

[4] All tables and figures are located in the appendix.
[5] Average number of cases retained is calculated as the average percentage of cases retained for all categories in 2015 and 2016.

Fatal Force

In 2015, *The Washington Post* began tracking more than a dozen details about each death at the hands of the police in the United States, including the race, age, and sex of the person killed and whether they were armed and/or experiencing a mental health crisis. Information was compiled by searching local news reports, law enforcement websites, social media platforms, and other independent databases such as Fatal Encounters and Killed by Police (Tate et al. 2016).

Data provided by *The Washington Post* through Fatal Force differs from data provided by The Counted in two important ways. First, the data provided by Fatal Force reports only police-caused killings due to shootings. Second, unlike The Counted, Fatal Force has continued collecting data on the number of people shot by police in the United States and has detailed information on these killings starting in 2015, with the latest data available for 2019. Although these data do not capture deaths that were not the result of police shootings, most deaths at the hands of police are due to shootings. For example, out of all cases presented by The Counted, 89 percent of reported deaths in 2015 were due to shootings and 92 percent in 2016 were due to shootings (Swaine et al. 2016). Given that some researchers insist that police-caused killings are rare and scattered events (Holmes et al. 2018; Cesario et al. 2018), having more years of data allows me to explore that assertion more fully (see Chapter six).

Table 3-2 displays the total number of killings reported by Fatal Force and the total number of cases used for the analyses using these data. In my analyses I use estimates provided by Fatal Force for 2015, 2016, and 2017, as more recent years of data were not available until after my analyses were completed. In 2015, 994 killings were reported. Of those dead, 497 were non-white, 258 black, 172 Latino and 497 white. In 2016, 962 killings were reported. Of these deaths, 497 were non-white, 234 black, 160 Latino and 465 white. Lastly, in 2017, 986 killings were reported. Of those dead, 527 were non-whites, 223 black, 179 Latino and 459 whites. My analyses do not include the total number of people killed by police as reported by Fatal Force due to missing data on other sources (especially crime, see below). After counties with missing data are removed from the overall analyses, the models presented here include 975 killings for 2015, 940 killings for 2016 and 971 killings for 2017. On average, I retain about 97 percent of total reported cases by Fatal Force.

Supplementary Homicide Reports

As part of the UCR program, the Federal Bureau of Investigation (FBI) collects detailed information about each homicide included in the UCR aggregate counts. SHR data provide details about each homicide reported by law enforcement agencies such as the jurisdiction, month, year, victim and offender demographics, weapon, circumstances surrounding the event and details regarding the relationship between the victim and offender, if reported (Bureau of Justice Statistics 2014). The SHR is comprised of two sections, one for all murders and non-negligent manslaughters, which includes justifiable homicides and one for negligent manslaughters. Murder and non-negligent manslaughter include cases that are "suspected" to be murders, homicides committed in self-defense, and law-enforcement related killings. Negligent manslaughter cases on the other hand, include cases that are determined to be unintentional killings of a person by another, which exclude motor-vehicle accident deaths (Bureau of Justice Statistics 2014).

SHR data include the number of felons killed by police and reported to the FBI by local police agencies. These data only include cases that are determined to be legally justified, which, is almost always the case in police-caused killings (Kindy and Kelly 2015; Stinson 2017). In fact, Fyfe (1988) argues that even in cases where people of color are killed by police and the circumstances involving the shooting are questionable, these cases are often ruled justifiable. One does not need to look very hard for these cases. For example, the death of Eric Garner was deemed justifiable, despite the fact that Garner was not armed, had his hands in the air, told police he couldn't breathe, and pleaded for his life 11 times as a Staten Island police officer choked him to death using an illegal chokehold (Benner 2016). However, the biggest issue with using SHR data is the fact that some agencies under-report, or fail to report data altogether (Loftin, Wiersema and McDowall 2003; Banks and Planty 2015).

The problem of underreporting has been thoroughly discussed in the literature and it is a well-known issue with SHR data (c.f. Klinger 2012; Loftin et al. 2003; Banks and Planty 2015). In fact, these estimates are so bad that researchers at times have stated "at present reliable estimates of the number of justifiable homicides committed by police officers in the United States do not exist" (Loftin et al. 2003:1121). The main issue with SHR estimates is the fact that these data are voluntarily submitted by agencies across the nation on a monthly basis. Because of the voluntary nature of the reporting, the SHR misses cases because some jurisdictions fail to file reports altogether or omit cases in which police kill civilians (Loftin et al. 2003). Moreover, cases where people die in areas falling under federal jurisdiction (i.e. national parks, federal prisons) are also not reported to the SHR, adding to the issue of underreporting. Some studies have found that SHR estimates only report about 49 percent of expected cases (Loftin et al. 2003), while other studies have found that in some cases SHR estimates only capture about 34 percent of total cases (Banks and Planty 2015).

In fact, there are entire states that have not submitted information to the SHR in years, thus raising concerns regarding the actual incidence of police-caused killings. For example, Alabama and Florida did not report data to SHR from 2011 to 2016 (National Center for Juvenile Justice 2020). According to estimates by The Counted, between 2015 and 2016, 46 people were killed by police in Alabama and 144 people were killed by police in Florida. Such figures have led researchers to state that SHR estimates under-report in systematic ways (Loftin et al. 2003), thus making use of this data problematic. Speaking on research conducted using SHR estimates, Klinger (2012:82) states, "To summarize about previously published studies of national-level data on citizen deaths at the hands of US police officers, the available evidence indicates that none of the three data collection systems provides counts of police-caused deaths that researchers should trust."

According to the SHR data there were a total of 452 police-caused deaths in 2015, 439 in 2016 and 428 in 2017. Table 3-3 present the total number of cases retained for the supplementary analyses at the metropolitan area level and Table 3-4 presents the total number of cases retained for the supplementary analyses at the county level. Overall, I use, on average, about 88 percent of cases made available by SHR in the analyses presented in this dissertation. Although these data are not used as the main source in answering my research questions, it is worthwhile presenting the data and evaluating whether my results are altered by using these data since these data has been used almost exclusively in studies of police-caused killings (Liska and Yu 1992; Sorensen et al. 1993; Jacobs and O'Brien 1998; Smith 2003,2004; Holmes et al. 2018).

Table 3-5 helps us understand the extent of this underreporting. For example, The Counted reported a combined total of 2,239 deaths for 2015 and 2016 whereas the SHR reported a combined total of 891 deaths in the same period. Thus, The Counted estimates report 2.51 times as many deaths as the SHR estimates, and all of The Counted's reports have been verified by a team of professional journalists. This underreporting of the SHR estimates is much more pronounced when looking at race-specific deaths. For example, the SHR reported a total of 44 black deaths for 2015 and 2016 combined, whereas The Counted reported a total of 573 black deaths, or 13 times as many deaths as the SHR reported. These disparities persist when comparing reported deaths by the SHR to reported deaths by Fatal Force. For example, the SHR reported a combined total of 1,319 for 2015-2017, whereas Fatal Force reported 2,942 deaths during the same period of time, or 2.23 times as many deaths, even though Fatal Force only includes shooting deaths. Again, the discrepancy between estimates is especially of concern when looking at black deaths, with Fatal Force estimates being 11 times higher than SHR estimates (715 vs 64) in this three-year period.

Clearly, the data used in the analyses can have major implications for the conclusions reached in assessing whether there are racial disparities in police-caused killings. Table 3-6 presents the rate of residents killed by police per million people, and the black-white ratio by source in order to assess whether blacks are disproportionately killed by the police given their population proportion. I report that the rate of police-caused killing of blacks is consistently higher than that of whites. Consequently, the black-white ratio indicates that blacks are over twice as likely as whites be killed by police when using The Counted and Fatal Force estimates. However, when using the SHR estimates, the estimates indicate that blacks are around 0.5 times as likely as whites to be killed by police. Clearly, the data used in the analyses have implications for the conclusions reached.

In fact, despite serious concerns raised regarding the use of SHR data (Davis and Lowery 2015; Klinger, Rosenfeld, Isom and Deckard 2016; Nix et al. 2017; white 2016; Klinger 2012;

Loftin et al. 2003), some (Holmes et al. 2018) have continued using these estimates. Holmes and his colleagues (2018) argue that underreporting or undercounting do not threaten the validity of these data as long as the errors in measured values are random, uncorrelated, and unbiased (see Neter, Kutner, Nachtsheim and Wasserman 1996:164-166). These authors are correct in asserting that data missing completely at random (CAR) do not affect analyses; however characterizing SHR police-caused killing data as CAR is optimistic at best. In fact, this assertion has already been addressed by Loftin and his colleagues (2003) who state that data on police-caused killings are often underreported in systematic ways, and, as such, should be used with caution.

Other researchers have made similar points about UCR data more broadly. For example, Maltz and Targonski (2003: 201) argue that errors in the UCR data (especially at the county level) are "non-independent, non-normal, and virtually all negative; i.e., crime counts for specific months or for specific agencies within a county have been omitted, not overstated." Given the limitations of the UCR more generally (Skogan 1987; Maltz and Targonski 2003), but especially SHR data on police-caused killings (Davis and Lowery 2015; Klinger, Rosenfeld, Isom and Deckard 2016; Nix et al. 2017; white 2016; Klinger 2012; Loftin et al. 2003), this project focuses on "unofficial" data sources on police-caused killings (i.e., The Counted and Fatal Force), but I include analyses of SHR data as a point of comparison.

Decennial Census and the American Community Survey

Demographic data for this study are taken from the 2000 and 2010 Decennial Census and the 2015 American Community Survey. The US government, through the Census Bureau, conducts a census every ten years in order to determine the number of people living in the United States. The data collected by the census are used to determine the number of seats each state has in the US House of Representatives. The ACS is an ongoing nationwide survey also conducted by the Census Bureau to collect and produce information on social, economic, housing and demographic characteristics of the US population. In 2005, the ACS replaced the decennial census “long form.” Every year the Census Bureau contacts over 3.5 million households nationwide to participate in the non-voluntary survey. The Census Bureau selects a random sample of addresses to be included in the ACS so that the sample ensures good geographic coverage. In doing so, the ACS produces a representative sample of the population that can be used for many purposes (Census n.d.).

Uniform Crime Reporting (UCR) Program

The primary objective of the UCR program is to “generate reliable information for use in law enforcement administration, operation, and management” (Federal Bureau of Investigation n.d.). The program is widely used by law enforcement officials, criminology students, researchers, and members of the media and public seeking information on crime in the United States. The program produces four annual publications comprised of data submitted by more than 18,000 city, university and college, county, state, tribal, and federal law enforcement agencies voluntarily participating in the program. Participating agencies typically submit crime data through a state UCR program or directly to the FBI’s UCR program (FBI n.d.).

The four data collections included in the UCR are 1) The National Incident-Based Reporting System (NIBRS), 2) The Summary Reporting System (SRS), 3) The Law Enforcement Officers Killed and Assaulted (LEOKA) Program, and 4) The Hate Crime Statistics Program. The UCR program makes all data accessible on an annual basis, and one can easily access these data online through an interactive Crime Data Explorer tool (FBI n.d.). Crime data used in this project come from the Summary Reporting System and can be broken down into offenses known to police and arrests made by age, sex, and race.

Offenses known to police can be obtained at various levels of geography and include all offenses that come to the attention of police in a given time period. These offenses include Part I offenses and are deemed the most serious type of crimes. The reporting of Part I offenses to the UCR follows the hierarchy rule. The hierarchy rule applies when multiple offenses are committed at the same time. When this happens, law enforcement agencies must classify the offenses, and only submit information on the most serious offense. For example, if a person steals a car, uses the car to kidnap someone, and then shoots a police officer, only the most serious offense would be reported for this case. That is, the responsible law enforcement agency would submit information on the murder of the police officer, and not the other crimes. While this affects reporting of crime more generally, it does not affect how suspects get charged or prosecuted.

The UCR program collects data on all Part I offenses that become known to law enforcement whether they involve arrest, as these crime totals are essential to measuring the level and scope of crimes occurring across the country. Although the UCR program collects arrest data for both Part I and Part II offenses, agencies only report arrest data for Part II offenses, so information on Part II offenses differs from Part I offenses in that this information only represents the number of arrests for those offenses, not the number of offenses that have come to the attention of the police. Table 3-7 provides a detailed description of all Part I and Part II offenses as reflected by the latest Department of Justice offense definitions in 2018. I use violent crime estimates in all chapters, which is composed of four offenses: murder and non-negligent manslaughter, rape, robbery, and aggravated assault. I use race-specific arrest data for all Part I and Part II offenses in analyses reported in Chapter Six.

DEPENDENT VARIABLES

In assessing whether there is a relationship between racial threat and police-caused killings, I create models for the total incidence of police-caused killings and race-specific models. Thus, I use five different aggregations of dependent variables in my analyses. Specifically, I use the total number of police-caused killings and the total number of police-caused killings of non-whites, blacks, Latinos and whites (separately). This allows me to assess whether my variables of interests (i.e., measures of threat) are related to the total incidence of police-caused killings or only related to the killing of minorities, blacks, Latinos or whites. This approach is consistent with existing research on police-caused killings and racial threat theory more broadly (c.f. Liska and Yu 1992; Sorensen et al. 1993; Jacobs and O'Brien 1998; Smith 2003, 2004; Holmes et al. 2018).

In determining whether to use a single or multiple year of data, I choose to follow the approach taken by criminologists (c.f. Holmes et al. 2018) and model two years of data in Chapter Four and three years of data in Chapters Five and Six. I use two years of data for the analyses using The Counted, because only two years of data were collected. I added a third year in the analyses using Fatal Force and the SHR, because a more recent year of data (2017) were available when I completed the analyses. Criminologists often argue for the use of multi-year counts of police-caused killings in order to reduce random fluctuations in the data, given that police-caused killings are relatively rare events (Liska and Yu 1992; Sorensen et al. 1993; Jacobs and O'Brien 1998; Smith 2003, 2004; Ross 2015; Holmes et al. 2018). Although many statistical models would produce misleading results if rare event data are used, the methods used in this dissertation to model count data are appropriate for rare events, making the use of multi-year estimates unnecessary. Interestingly, many criminologists who use multiple years of data because of potential rare-event bias actually use count models that account for rare events (Smith 2003,2004; Holmes et al. 2018). As Piza (2012:2) states, "The 'rare events' nature of crime counts is controlled for in the formulas of both Poisson and negative binomial regression." Additionally, the more complete counts included in The Counted and Fatal Force also show that criminologists are wrong when they assert that there is considerable annual fluctuation in *where* police-caused killings occur. In fact, looking at simple correlations between estimates taken from The Counted, Fatal Force, and SHR reveals a strong relationship between yearly estimates as seen in Tables 3-8 through 3-10. In fact, I would argue that it was unnecessary to use more than one year of data in my analyses, but multiple years of data makes my findings more comparable with other available research on police-caused killings.

Tables 3-8, 3-9, 3-10 show correlations across years by place for police-caused killings reported in The Counted, Fatal Force, and SHR estimates, respectively. The highlighted cells show the Pearson's correlation between the same type of death from year to year. Estimates for all deaths in 2015 and 2016 taken from The Counted at the metropolitan area level (Table 3-8) have a Pearson correlation coefficient of 0.91. Estimates for all deaths in 2015, 2016 and 2017 taken from Fatal Force at the county level have a Pearson correlation coefficient of 0.82 (2015-2016), 0.83 (2015-2017) and 0.87 (2016-2017; Table 3-9). Similarly, estimates for all deaths in 2015, 2016 and 2017 taken from the SHR at the county area level have a Pearson correlation coefficient of 0.85 (2015-2016), 0.87 (2015-2017) and 0.88 (2016-2017). Race-specific estimates follow a similar pattern, although the correlation coefficients seem to be slightly lower for The Counted and Fatal Force.

Interestingly, although all correlations are high for most of the data presented, correlations for death counts of blacks taken from the SHR are quite low. For example, black death counts in 2015, 2016 and 2017 from the SHR at the county level have a Pearson correlation coefficient of 0.38 (2015-2016) and 0.23 (2016-2017), suggesting that not only do these incidents fluctuate more, but that there is considerably more variation in reporting of black killings. In sum, I follow the approach taken by other researchers and use multi-year estimates as my dependent variable for all years of data available.[6] The dependent variable used across chapters is the multi-year estimates of all, minority, black, Latino, and white deaths.

In Chapter Five, after running models to assess the relationship between group threat and police-caused killings at the county level, I seek to determine whether there is evidence of racial bias in the data provided by *The Washington Post* for 2015-2018. This is the only analysis where I use data for 2018 since it was made available by *The Washington Post* and I am not comparing results to data provided by the SHR as they do not provide the same details made available by *The Washington Post*. In order to assess whether there is evidence of racial bias in the data, I follow an approach taken by Nix and his colleagues (2017) and use two indicators of threat perception failure.

Overall, threat perception failure refers to the lack of accuracy by a police officer in identifying a threat accurately. In other words, if a police officer believes that a person is armed and kills them to save his or her life or those of others, but it turns out that the suspect is not

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[6] I use 2 years of data from The Counted as they only provide data for 2015 and 2016. I use 3 years of data from Fatal Force and SHR because the latest estimates provided by the SHR do not include estimates for 2018.

armed, then the police officer failed at determining the threat accurately. Correll and his colleagues (2002), though simulations, show that police officers are more likely to believe minorities are armed when they are holding objects such as cellphones, wallets, and similar items. They argue that implicit biases against blacks are present in split-second judgements, and these judgements cause police officers to mis-identify everyday objects as weapons when they deal with black suspects as opposed to white suspects (Correll et al. 2002). Other researchers investigating whether there is evidence of bias in police-caused killings tend to use similar measures (c.f. Correll et al. 2007; James et al. 2016). Nix and his colleagues (2017: 317) argue that "if minorities are more likely than whites to have not been attacking the police/other civilians, or more likely to have been unarmed, this would indicate that the police exhibit implicit bias by falsely perceiving minorities to be a greater threat to their safety (i.e. threat perception failures; see Fachner and Carter 2015)."

I followed the approach taken by Nix and his colleagues (2017) and created two dichotomous variables that serve as indicators of threat perception failure. First, "attacking" was coded 1 if the civilian killed was firing a weapon at a person (civilian or police officer), attacking with something other than a weapon, pointing or brandishing a weapon (anything from a knife to a stapler), refusing to drop a weapon, or lunging toward an officer in any way. All other cases were coded 0. It is worth noting that whether or not a person was "attacking" is based on police reporting, which may or may not be accurate, and as there is some motivation to falsely report, my analysis will overestimate the true probability of attacking. Second, in order to assess whether a civilian was shot while armed, I coded 1 for cases where it was determined that the person had a weapon on them and 0 for all other cases. Although I acknowledge that what constitutes a deadly weapon, or a weapon for that matter, can be debatable, I stick to the information provided by *The Washington Post* team and code my variables accordingly. Again, results would suggest bias if minorities who are killed are more likely to have not been attacking people, or more likely to have been unarmed when they were shot by police. Although researchers (c.f. Correll et al. 2007; Nix et al. 2017) often make the claim that it is implicit bias that is at play, there is really no way of determining whether the bias is implicit or not, therefore I conceptualize these measures as indicators of bias without ascribing a source.

INDEPENDENT VARIABLES

Broadly, I am interested in assessing the relationship between group threat and police-caused killings. In order to examine this relationship, I focus on indicators of group threat theory at two different levels of aggregation. Traditionally, criminologists assessing the relationship between group threat and police caused killings measure threat as the proportion of the population that is non-white, black or Latino (Liska and Yu 1992; Jacobs and O'Brien 1998; Sorensen et al. 1993; Smith 2003, 2004; Nowacki 2015; Holmes et al. 2018). Race scholars on the other hand, tend to measure threat by including population ratios (c.f. Fossett and Kiecolt 1989; Tolnay, Beck, and Massey 1989; Quillian 1996; DeFina and Hannon 2009; Alba, Rumbaut, and Marotz 2005). I follow the approach taken by race scholars, and measure threat as the black-white and Latino-white population and income ratios, given that Blumer (1958) was concerned with the relative—rather than absolute—size of the minority population. Thus, in using a ratio as opposed to simple population proportions, I can better capture the "threat" size of minority populations, which more adequately aligns with Blumer (1958) and Blalock's (1967) arguments.

Researchers testing the relationship between minority threat and police-caused killings often model this relationship linearly. That is, as the size of the black or Latino population increases, social control efforts to counter this threat increase as well (Liska and Yu 1992; Sorensen et al. 1993; Smith 2003,2004). However, Blalock (1967) conceptualized threat in a more comprehensive fashion. Although he did argue that under minority threat, the size of the minority population would be linearly related to social control outcomes, he also offered alternative theories and appropriate mathematical functions by which threat could influence social control. Blalock's (1967) political threat hypothesis, or power threat hypothesis, states that as the percentage of blacks in the population grows larger, social control efforts employed to combat the threat increases dramatically, suggesting an increasing slope or quadratic relationship. Power threat models have been tested, with mixed support (Liska and Yu 1992; Holmes et al. 2018). For example, Liska and Yu (1992) find no support for a quadratic relationship, while Holmes and his colleagues find support in a race-specific model of police-caused killings.

Moreover, placing emphasis on the potential economic threat posed by minorities, Blalock (1967) formulated the economic competition hypothesis. This hypothesis asserts that competition between whites and blacks for finite economic resources results in an increase in social control efforts directed toward blacks. However, unlike the power threat hypothesis, economic competition states that the social control directed at minorities will increase with a positive but declining slope (Blalock 1967). The hypothesized reason for this declining slope is that other economic exclusion strategies (such as discrimination in the workplace) will emerge, rendering social control efforts less effective (Blalock 1967; Eitle et al. 2002). Researchers have tested this economic competition variant by looking at causes of riots (Olzak, Shannahan, and McEneaney

1996) lynchings (Beck and Tolnay 1990; Olzak 1990, 1992) and interracial killings (Jacobs and Wood 1999), suggesting that economic competition is related with non-state sanctioned forms of social control against blacks.

Economic competition models as theorized by Blalock (1967) refers to a log-linear function as an explanation of discrimination. That is, in contrast with minority threat models, the percentage of the non-white population or "threat" will be linearly associated with increases in discriminatory outcomes, but once a tipping point is reached, discriminatory outcomes will increase at a decreasing rate. Once minorities can wrest power from whites, or at least share the economic and political power associated with more balanced population composition, discriminatory outcomes should increase but at a decreasing rate. Some studies of policing have found support for such a function of threat (c.f. Jackson and Carroll 1981; Eitle et al. 2002; Stucky 2005).

Lastly, power threat models refer to the hypothesis that the relationship between percent minority and social control efforts will display an initial positive parabolic (quadratic) relationship, which eventually could turn negative as the percent of minorities reaches a level where minorities can wield political power (Blalock 1967). Researchers have provided mixed support for the power threat hypothesis, with most studies not finding support for this quadratic relationship (c.f. Liska and Yu 1992; Smith and Holmes 2014) while some studies find support for models predicting the death of Latinos (Holmes et al. 2018). In order to determine whether minority threat, economic competition, or power threat are better suited at predicting overall, and race specific counts of police-caused killings I fit all models and examined fit statistics to determine which models best suited the data used in this study. In this dissertation, I present results from the best fitting models.

Specifically, I fit linear, quadratic, and logged functions of my predictors in order to test whether minority threat, power threat, or economic competition best explains the relationship between the size of the black and Latino population relative to whites and police caused killings. In my study, I include three measures of threat. First, I measure black and Latino threat as the natural log of the black and Latino population divided by the white population. Secondly, I measure threat as the ratio of the natural log of average per-capita income, calculated as the black and Latino per capita income (separately) divided by mean white per capita income. Using this measure as opposed to a more general measure of income inequality (such as the Gini coefficient) is in line with previous research (Holmes 2000; Jacobs and O'Brien 1998; Nowaki 2015) and allows for a racialized understanding of economic inequality. For example, Jacobs and O'Brien (1998) argue that greater differences in the economic resources of blacks and whites reduces the black population's political influence. Furthermore, Nowaki (2015) states that using these measures introduces racial dynamics into measures of inequality, which accounts for the effect of threat on the distribution of resources.

The third measure used to capture group threat is a measure of residential exposure. Specifically, I use an interaction index to determine the degree of potential contact, or the possibility of interaction between black, Latino, and white people who live in the same neighborhoods (measured as Census tracts). While the focus is often on relative group size, the geographic distribution of minorities has also received plenty of attention in the group threat scholarship (Stults and Baumer 2007). Blalock (1967) argues that residential segregation operates as an effective way to reduce minority threat by minimizing interpersonal contact between majority and minority group members. Put another way, it matters less how many minorities are in a place if white people never see them. Therefore, high levels of segregation may reduce the perceived threat directly, thus reducing the effect of racial composition on perceived threat (Blalock 1967; Burr, Omer and Fossett 1991; Taylor 1998). Previous studies of group threat and police-caused killings have accounted for the effect of segregation using the index of dissimilarity, finding mixed support. For example, Liska and Yu (1992) find that "segregation may increase the perceived threat for those who work in racially segregated ghettos, while decreasing it for those working outside of these ghettos" (56). Willits and Nowacki (2014) find no effect between segregation and police-caused killings using an index of dissimilarity for Latinos. Lastly, Holmes and his colleagues (2018) find a positive relationship between segregation and police-caused killings of all civilians, and of blacks, meaning that segregation is working differently than assumed.

In using an index of exposure as opposed to using an index of dissimilarity as a measure of segregation, I am able to model threat as conceptualized by Blalock (1967) more accurately as "exposure measures the degree of potential contact, or possibility of interaction, between minority and majority group members" (Massey and Denton 1988:287). Moreover, as Massey and Denton (1988) point out indexes of evenness and exposure are correlated but measure different things. Exposure is better suited than evenness as the exposure index depends on the relative size of the two groups being compared, which better fits the group threat hypothesis. Exposure measures, specifically an interaction index, captures the degree to which blacks and Latinos experience segregation, assuming they are equally likely to interact with each person in their respective census tract (Bohon and Nagle 2019). Because interaction indices depend on the size of the two groups being compared, the measure ranges from 0 to the highest proportion of the majority group, with higher values indicating higher interactions. For example, since whites comprise 98 percent of the population in some counties, the highest value for the interaction index would be .98.

Although accounting for the residential distribution is important for group threat processes, the index of dissimilarity does not capture the potential interaction between majority and minority group members, which is essential in understanding threat more generally. Dissimilarity, a measure commonly used in the study of residential segregation, examines only one dimension of segregation—evenness. It looks at the degree to which all neighborhoods (operationalized as census tracts) have the same racial distribution as the city. Stults and Baumer (2007) state that using the index of dissimilarity is not ideal for measuring the concept of exposure to minority group members as outlined in Blalock's (1967) theory of minority group relations and that a different dimension of segregation—exposure—would be more appropriate. However, because indicators of exposure are highly collinear with common measures of threat (i.e. percent black and percent Latino) most studies opt for an index of dissimilarity instead (Stults and Baumer 2007). I can use a measure of exposure, which is better aligned with group threat theory and is not collinear with my measures of group threat, as I do not use the proportion of residents that is black and Latino and use a measure of relative size instead. Thus, the way I operationalize threat more generally allows me to fit a model that emphasizes the distribution of the minority population, and the potential interaction between majority and minority group members. To the best of my knowledge, mine is the first study in the group threat literature that uses a measure of exposure instead of evenness.

I also measure threat as the increase in the proportion of the population that is black and Latino in a fifteen-year period. Given that threat theories suggest that law enforcement should be more coercive in places with a large proportion of black and Latino residents, this could also be the case in places that experience a growth in the minority population, as this growth can also trigger an increased concern over the prevalence of crime. Jacobs and O'Brien (1998) find that the growth in the black population is associated with an increase in the killing of blacks. Nowacki (2015) also accounts for black population growth but does not find any effects associated with this measure. Researchers using minority threat theories to understand sentencing outcomes usually look at changes in the black and Latino population as dynamic measures (c.f. Wang and Mears 2015).

Lastly, following the approach taken by Kane and his colleagues (2013) I look at the effect of minority population growth using threat trigger variables. Kane and his colleagues (2013) incorporate threat triggers into a minority group threat model of misdemeanor arrests. In their study, they find that increases in the population percent black were associated with increased black misdemeanor arrests, but only in historically white census tracts. Thus, they argue that threat trigger variables "should be measured in terms of difference scores and weighted by initial dominant group representation" (Kane et al. 2013:957). In essence, accounting for changes in racial composition, weighted by initial dominant group representation may tap into challenges to power differentials between racial groups (c.f. Green, Strolovich and Wong 1998), which Blalock's (1967) model suggests. Kane and his colleagues (2013) measure threat triggers as difference scores for both Latino and black populations, where each group group's tract-level values at 1990 were subtracted from their 2000 values, multiplied by a dummy variable indicating which census tracts were at least 51 percent white in 1990. By doing so, they assess the independent effect of black and Latino population growth in historically white communities while controlling for such changes across all tracts (Kane et al 2013:968).

I follow the method of Kane and his colleagues (2013) and incorporate threat triggers into my models in Chapter 5 by calculating a dummy variable that measures whether a county lost a 51 percent majority in a fifteen-year period. That is, counties where whites comprised 51 percent or more of the population in 2000, but not in 2015, were coded as 1 and all others were coded as 0. This coding allows me to assess the effect of minority population growth in all counties, and in counties where whites lost majority status in the last 15 years. I then create an interaction term between black and Latino population growth and my dummy variable indicating whether whites lost their demographic majority in the fifteen-year period. About 3 percent of counties used in my county-level analyses had a white majority (at least 51 percent) in 2000 and not in 2015. Surprisingly, while these counties only represented 3 percent of total counties in my sample (94 out of 2712), 12 percent of all people killed by police between 2015 and 2017 (361 out of 2886) died in these counties. In my models, I include the dummy variable alone to assess whether changes in the majority population have an independent effect on the expected count of police-caused killings and an interaction between the dummy variable and my growth measures in order to determine if threat triggers moderate the relationship between minority population growth and the expected count of police-caused killings.

In sum, in my structural analyses of group threat theory, I measure group threat using 1) the 2015 black and Latino population divided by the white population, 2) the black and Latino per-capita income divided by the white per-capita income in 2015, 3) the black-white exposure index and Latino-white exposure index in 2015, 4) black and Latino population growth between 2000 and 2015, and 5) threat trigger indicators. Although most of my analyses are at the metropolitan area level and the county area level, I also assess the effect of civilians' race on the likelihood of being shot and killed while unarmed and shot and killed while not attacking police officers and others. I report descriptive statistics and results from my analyses in the following three chapters.

CONTROL VARIABLES

Several control variables are incorporated in my analyses. These control variables are selected based on previous studies of police-caused killings and literature on group threat and social control outcomes more generally. In my structural-level analyses of police-caused killings I am controlling for population size, police per capita, the violent crime rate, and regional differences (using the Southwest as the reference category). Furthermore, I am controlling for individual-level characteristics of residents shot by police in multi-level analyses in Chapter Five, which resemble the controls used by Nix and his colleagues (2017) in their analysis of racial bias in police-caused killings. Below is a brief description on how these variables were computed and how they are used in research that is often conducted on police use of force.

Population

All models in this project control for the 2015 population in thousands. Controlling for population is important because cities with larger populations are expected to have more encounters between police and residents (probably because larger cities have more potential killing "opportunities"). Furthermore, most studies evaluating structural predictors of police-caused killings tend to find a significant and positive relationship between killings and population (c.f. Liska and Yu 1992; Sorensen et al. 1993; Smith 2003,2004; Holmes et al. 2018). Population estimates are taken from the 2015 ACS 5-year estimates and divided by 1,000. Furthermore, I take the natural log of this variable in order to help with skewness and fit the model better. In Chapter Six, I use the overall 2015 population in thousands, and the black and white population in thousands for race-specific models. Again, I use the natural log of these control variables as there is a tipping point above which increasing population size has less impact on changes in the dependent variable.

Police

I control for the population per capita in all models, consistent with other studies of police-caused killings (c.f. Liska and Yu 1992; Jacobs and O'Brien 1998; Smith 2003, 2004; Holmes et al. 2018). Moreover, police size is included because conflict theory suggests that it affects the level of crime control activities and is related to percent non-white, segregation, and income inequality (Liska and Yu, 1992). Moreover, including the number of police per capita provides a simple measure of potential resident exposure to police. Simply put, there cannot be a police-caused killing without police officers, thus I include police per thousand residents in all analyses. Arguably the control for population size is a proxy for the number of police officers, but tests of multicollinearity do not confirm this. My measure is taken from the Crime Explorer data tool, which is part of UCR program. I again use the natural log of police per capita, as this transformation helps with skewness and provides a better fit overall for my models. I calculate police-per thousand residents by dividing the total number of sworn police officers by the total population in 2015 and multiply this by 1,000. This measure is consistent with previous studies of police-caused killings (see Smith 2003, 2004).

Crime rate

The violent crime rate for 2015 is used in all analyses in order to control for the potential effect of threatening acts on police officers' decisions. Previous studies of police-caused killings have included a measure of the violent crime rate or murder rate (Liska and Yu 1992; Smith 2003; Smith 2004; Sorensen et al. 1993; Jacobs and O'Brien 1998; Holmes et al. 2018; Nowacki 2015; Ross 2015). Crime or violent acts are usually conceptualized as main drivers of police-caused killings under the community violence perspective (Sorensen et al. 1993; Fyfe 1980). Patterns of police-caused killings are said to occur in response to overall violence in cities (Fyfe 1980; Cesario et al. 2018; Smith 2003; Smith 2004). The violent crime rate used in these analyses includes murder, rape and sexual assault, robery and aggravated assault. In keeping with most other studies (Holmes et al. 2018; Smith 2003, 2004), I use the violent crime rate per 100,000 residents. I also take the natural log of the violent crime rate as it provides a better model fit.

Regional controls

In order to control for regional variations in the incidence of police-caused killings, I include indicator variables for the South, West, Northeast, Midwest, Southwest, and Northwest region using the Southwest as the reference category. Historical variations in racial tensions and stratification that vary across geographic regions may influence levels of coercive crime controls (Jackson 1992), thus it is imperative to include such controls. Regional variables were constructed in a fashion similar to work by Smith and Holmes (2014) who separate the United States into five regions instead of the four delineated by the US Census Bureau. Specifically, I used US Census region designations for the Northeast and Midwest. The South region is the same as the Census Bureau's designations except for Texas, which was included in the Southwest category. The Northwest resembles the US Census regional designation of West, except for Arizona, California, and New Mexico. These states, along with Texas are included in my Southwest regional indicator. By using an indicator for the Southwest, I am able to account for potential tensions between police and Mexican immigrant populations that are largely located in these states (Bender 2003).These regional controls are consistent with prior research on police-caused killings (Smith 2003, 2004; Holmes et al. 2018), civil rights violations (Holmes 2000), and police use of excessive force (Smith and Holmes 2014), which occur at greater rates in the Southwest.

Urban-Rural Designations

Some of my analyses are limited to metropolitan areas, but I use counties as my unit of analyses in Chapter Five, so I think it is important to account for potential urban-rural differences in police caused killings in the county-level analyses. Researchers have argued that police kill more people in large cities and metropolitan areas as social control is harder to exercise in urban areas (Jacobs and Britt 1979). Moreover, most police-caused killings occur in metropolitan areas (about 85 percent) and police in general are more likely to use force or excessive force in urban areas (Holmes et al. 2008). Furthermore, because stereotypes associating racial and ethnic minorities with serious criminality (Bender 2003; Quillian and Pager 2001) and urban violence (Chiricos, Welch, and Gertz 2004) are prevalent, distinguishing any urban-rural differences is important. Additionally, given that black and Latino populations are overwhelmingly urban, these associations may vary between urban and rural settings. I account for rural-urban differences using the Office of Management and Budget's (OMB) delineation of metropolitan statistical areas and micropolitan statistical areas. All counties that were designated metropolitan in the 2010 Census are coded 1, and all other counties are coded 0.

I also include a set of individual-level control variables in my multilevel analyses (Chapter Five). Controls for this analysis includes indicator variables for other (non-black, non-white, non-Latino) race, fleeing, body camera presence, mental illness, and attacking. I also control for the age of the resident killed. I mean center age in order to aid with interpretation. "Other race" is coded as 1 when the civilian killed was not categorized as black, Latino or white. In my individual-level analyses, white serves as the reference category. "Fleeing" is coded 1 if the civilian was fleeing at the time of death. "Body camera" is coded 1 if the officers involved in the incident were wearing body cameras at the time of the incident[7]. "Mental illness" was coded as 1 if the resident killed displayed any signs of mental illness when they were killed by police. Lastly, "attacking" was coded as 1 if the resident killed by the police was attacking others or the police when they were killed by police, according to police accounts. I only control for whether a resident was attacking officers or others when I also include my "unarmed" threat perception failure variable. Again, I do not make my own determinations as to what constitutes signs of mental illness, being armed, or fleeing; these were determined by *Washington Post* staff using their decision rules. For detailed information on how these coding decisions were made see Fatal Force's website.

[7] Although it is possible that officers can wear body-cameras while the cameras are off, the data available only distinguishes between officers wearing body-cameras and those who were not wearing cameras.

Chapter Four

GROUP THREAT IN METROPOLITAN AREAS

In order to assess the relationship between group threat theory and police-caused killings, in this chapter I addresses whether the size and composition of the minority population predict the expected count of police-caused killing at the metropolitan area-level. Specifically, I ask:

RQ1a. Do minority threat, economic competition, or power threat models better fit the relationship between population and police-caused killings?

RQ1b. Do dynamic measures of population better predict killings than static measures?

RQ1c. Do the models better predict the expected count of all deaths, black deaths, Latino deaths, or deaths to all people of color?

Overall, I am interested in broadly assessing whether the size and composition of the minority population is related to police-caused killings at the metropolitan area level. This line of questioning is rooted in a long history of studies examining the relationship between minority populations and various forms of state social control (Liska and Yu 1992; Liska 1992; Feldmeyer et al. 2015; Durso and Jacobs 2013; Holmes et al. 2018)).I n line with the conflict perspective, I assume that an increase in the non-white population will be associated with an increase in state social control efforts, as this relationship has been established in previous studies. Several authors in the 1990s point to a significant positive relationship between the size of the minority population and police-killings (Sorensen et al. 1993; Liska and Yu 1992; Jacobs and O'Brien 1998). More recent studies also demonstrate a significant relationship between the size of the minority population and police killings. For example, Smith (2003; 2004) finds that the percent of the population that is black is associated with an increase in the number of police killings in cities with 100,000 residents or more. However, Holmes and his colleagues (2018), using official data sources from the Supplemental Homicide reports from 2008-2013, find a significant negative relationship between the percent of the population that is black, and the overall number of civilians killed by police.

This chapter replicates previous research in that it examines the relationship between the size of minority populations and police-caused killings but departs from previous studies by using more complete data and developing models more closely aligned with minority threat theory. As indicated in Chapter Three, this analysis uses data from The Counted, which I compare to models using data from the Supplementary Homicide Reports (SHR) which is consistent with previous studies (c.f. Smith 2003; 2004; Holmes et al. 2018). Data taken from the SHR has been shown to vastly undercount the overall incidence of police killings (Williams, Bowman and Jung 2016). Thus, in using two different data sources, I can not only include more cases for the analysis (allowing me to assess the argument that multiple years of data are necessary), but I can assess whether the data used has implications for the conclusions drawn from the analysis. In developing models more closely aligned with minority threat theory. I opt to measure threat by employing black to white ratios for population and income instead of crude percentages as measures of threat (see Smith 2003, 2004; Holmes et al. 2018) which have dominated previous analyses. Additionally, previous studies have used an index of dissimilarity in accounting for the effect of residential segregation (see Liska and Yu 1992; Holmes et al. 2018) on police-caused killings. I on the other hand, account for the effect of residential segregation by using an index of exposure, which more adequately measures potential interaction between minority and majority group members.

ANALYTICAL STRATEGY

In deciding which unit of analysis would be more appropriate for evaluating the relationship between group threat and police-caused killings, I chose metropolitan statistical areas for theoretical and methodological reasons. Previous research has indicated that tensions between police and minority communities are particularly acute in large urban areas (c.f. Jacobs and O'Brien; Liska and Yu 1992; Holmes et al. 2018). Moreover, researchers have also stated that small cities have 1) few police-caused killings, and 2) that introducing small cities into analyses may introduce systematic measurement error into statistical models (c.f. Smith 2003; Liska and Yu 1992; Holmes et al. 2018). In fact, about eighty-five percent of the police-caused killings in 2015 and 2016 happened in metropolitan areas across the US.

Traditionally, researchers have looked at cities with at least 100,000 residents or more and cities with 250,000 residents or more in assessing the relationship between several social indicators like crime, inequality, racial threat measures, population change and police-caused killings (Sherman and Langworthy 1979; Liska and Yu 1992; Sorensen et al. 1993; Jacobs and O'Brien 1998; Smith 2003,2004; Nowacki 2015; Willits and Nowacki 2015; Holmes et al. 2018). Others have looked at counties (Ross 2015), states (Jacobs and Britt 1979; Kania and Mackey 1977; Shane, Lawton and Swenson 2017), police departments (Klinger, Rosenfeld, Isom and Deckard 2016; Fryer 2019), and some have looked at the nation as a whole (Cessario et al. 2018; Edwards, Lee and Esposito 2019). Only Scott, Ma, Sadler and Correll (2017) use metropolitan statistical areas as their unit of analyses in understanding racial disparities in police shootings. Surprisingly, metropolitan areas have received little attention in studies of group threat and police-caused killings. Seeking to remedy this lack of attention to metropolitan areas, I start my analysis using metropolitan areas as my unit of analysis. Moreover, using metropolitan areas to study inter-group processes makes sense as whites tend to segregate from minorities outside metropolitan areas. Referencing Massey and Hajnal (1995) “Using lower levels of aggregation [than the metropolitan area] as the contextual unit of analysis tends to underestimate minority group size because of the increased tendency of Anglos to segregate themselves in different cities and neighborhoods." (Rocha and Espino 2009:418).

Before determining which specification of group threat better fits the relationship between population and police-caused killings, it is necessary to determine whether it is better to model these data using Poisson, negative binomial, or zero inflated count approaches. Different count models are appropriate for different data characteristics, and it is not always easy to determine, on their face, which data are appropriate for which type of model. In determining which model is better suited for this data, I fit all models and determine which model is best by evaluating which model provides an adequate account of the data, while using the minimum number of parameters possible (Wagenmakers and Farrell 2004; Myung, Forster and Browne 2000). In determining which model provides an adequate account of the data, I take a two-prong approach. First, I run all models and store Akaike Information Criteria (AIC) for each model. Then, I calculate Akaike weights, which can be directly interpreted as “conditional probabilities for each model” (Wagenmakers and Farrell 2004:192) and select the model with the lowest AIC. Secondly, I evaluate my candidate models graphically, by looking at the observed versus expected counts for competing models. This approach allows me to assess models comprehensively by looking at numerical and graphical evidence.

The objective of an AIC model selection procedure is “to estimate the information loss when the probability distribution f associated with the true (generating) model is approximated by probability distribution *g*, associated with the model that is to be evaluated" (Wagenmakers and Farrell 2004). So, by looking at AIC differences and AIC weights in determining which model better fits the data, we can assess the relative performance of competing models, as opposed to absolute AIC values alone. This proves useful as AIC weights provide a straightforward interpretation as the probabilities of each model being the best model or the model that has the smallest Kullback-Leibler distance, given the data and the set of candidate models (Wagenmakers and Farrell 2004). Thus, taking this information and graphically assessing how well the models predict the observed counts of police-caused killings provides evidence to decide how to best model the data.

In selecting among count models, I began by fitting Poisson models. Poisson regression requires that the variance equal the mean of the dependent variable, an assumption that often does not hold for most crime data (Osgood 2000). In fact, the variance is significantly larger than the mean, creating a condition statisticians refer to as *over-dispersion*. Table 4.1 shows the mean, variance and percent of cases without any deaths for all dependent variables used in the analyses. The average number of deaths at the metropolitan area level is about 5 deaths and the variance is over 100. Race specific deaths show the same pattern, that is, variances are considerably larger than the average number of deaths. Deviance and chi-square statistics also indicate that the conditional variance is much larger than the mean, indicating that Poisson models are inappropriate for these data.

When the variances are larger than the mean, negative binomial models are often employed as these models allow for over-dispersion, by directly estimating this over-dispersion with a dispersion parameter (Osgood 2000). Thus, I also fitted negative binomial regression models.

However, there is a concern with the number of metropolitan areas and counties without any deaths at all. Table 4.1 displays the percent of cases without any deaths at the metropolitan area level. About 21 percent of metropolitan areas in my sample experienced no police-caused killings. However, when looking at race-specific killings, the proportion of metropolitan areas without any deaths is much larger. For example, when looking at Latino deaths, almost 75 percent of metropolitan areas have no Latino deaths at the hands of police. So, in attempting to account for the high proportion of cases without any deaths, I fit zero-inflated models.

Other researchers have had to deal with the same kind of problem. For example, in their structural analysis of police-caused killings, Jacobs and O'Brien (1998) used Tobit regression models because 18 percent of the cities used in their analysis had no police-caused killings. As they claimed, they "capitalize on the properties of this estimator because it is the most appropriate when the dependent variables are censored rates with large number of zeros (Roncek 1992; Greene 1993)" (Jacobs and O'Brien 1998). In other words, they fit a model (Tobit) that assumes that an event will happen, but it hasn't happened yet. However, for count data, count models are more appropriate than Tobit models, which require a 0 (no killing yet), 1 (killing has happened) distribution. Zero-inflated models are adjustments to count models that account for the presence of two kinds of zeroes--*true* zeroes that occur because no one was killed by the police and *excess* zeros that occur because police had no opportunities to kill residents. For my purposes, it seems hard to imagine that there would be cases were police had no opportunities to kill anyone as they are legally allowed to use discretion in using fatal force. However, I fitted zero-inflated Poisson and zero-inflated negative binomial models to assess which count model better models the data.

I make the final determination of the model used to test my hypotheses by a) comparing the Akaike information criterion (AIC) among competing models, b) determining whether the model captures the zeros accurately, and c) evaluating model parsimony. Table 4.2 shows the AIC, Δ AIC, and AIC weights for all count models at the metropolitan level. It seems clear that Poisson and zero-inflated Poisson models do not fit the data well as anticipated, given that the data are over-dispersed. At the metropolitan level, the Δ AIC or difference from the best fitting model is 520 and 377 respectively. The likelihood that these models are better than the zero-inflated negative binomial models is essentially zero. Next, the difference between negative binomial models and the zero-inflated negative binomial model is 3. While this is a small difference, the AIC weight indicates that the zero-inflated model is still a better fitting model.

However, when graphically assessing how well the models fit the data, it seems like the negative binomial models do a better job for my purposes. Figure 4.1 displays the observed versus predicted probabilities for each count model. When looking at the fit, it appears that the zero-inflated model over-estimates the zeroes and under-estimates the counts. Since I am interested in accurately predicting counts, and I am less concerned with predicting zeroes I opt for negative binomial models that do not adjust for zero-inflation. Also, given that the use of zero-inflated models remains under-studied (Allison 2012) and my results remain consistent whether I use negative binomial models or zero-inflated models, I use negative binomial models in all analyses where I am modeling the expected count of police-caused killings.

Before conducting the analysis, I performed preliminary diagnostic tests for the presence of multicollinearity or influential cases. I assess whether multicollinearity would be a problem in my models by estimating variance inflation factors (VIFs). The estimated VIFs, indicating the inflation of the variances of regression coefficients compared with those for linearly independent variables were small and below the level (10) at which significant distortion of findings is likely to occur (Neter, Kutner, Nachtsheim and Wasserman 1996). Moreover, I assessed whether outliers would be a problem for the analysis. After calculating Cook's distance (D) for all cases, four metropolitan areas (Dallas, Los Angeles, Phoenix, and Riverside) exceeded values greater than (4/323). However, none of these cases exceeded the value of Cook's D (1.0) at which changes in estimates may occur (Weisberg 1985). Moreover, I estimated all models without these metropolitan areas to see if the overall estimates changed. Given that the estimates remained relatively the same, I proceeded with these metropolitan areas in the overall sample.

In sum, my initial analysis indicates that the data is better modelled by fitting negative binomial regression models. As such, I fit negative binomial regression models to assess the relationship between measures of group threat and police-caused killings of all, minority, black, Latino, and white people. This approach allows me to assess the effect of group threat indicators on the overall and race-specific count of police-caused killings.

RESULTS

Descriptive Statistics

Table 4-3 illustrates the reported deaths at the metropolitan area level for 2015-2016 by source. The SHR data consistently under-reports deaths when compared to The Counted data. For example, if we look at deaths reported by the SHR as a percentage of deaths reported by The Counted, we see that the SHR data only captures 44.92 percent of all deaths, 8.95 percent of black deaths, 32.74 percent of Latino deaths, 38.37 percent of white deaths and 50.44 percent of minority deaths. Under-reporting is especially problematic when looking at black deaths, with The

Counted estimates being eleven times higher (458 vs 41) than the SHR estimates. Moreover, beside the serious problem of under-counting, the SHR data can reflect selection effects that are often made worse by using multiple years of data as most researchers do (c.f. Holmes et al. 2018; Smith 2003,2004). These potential selection effects threaten the confidence in results from studies using these data, as the under-counting seems to vary systematically, especially when it comes to race-specific estimates. Holmes and his colleagues (2018) dismiss potential problems with SHR estimates by arguing that potential under-counting varies randomly, a claim that is hard to accept given the serious differences in total and race-specific police-caused killings between sources.

Table 4.4 presents descriptive statistics for all variables used to address the research questions listed above. Overall the average number of deaths for 2015-2016 across 323 metropolitan areas reported by The Counted is 5.15 for all deaths, 2.8 for all minority deaths (including black, Latino and other non-white race combined), 1.42 for black deaths, 1.04 for Latino deaths and 2.36 for white deaths. The average number of deaths reported by the SHR in the same time period is 2.32 for all deaths, 1.41 for minority deaths, 0.13 for black deaths, 0.34 for Latino deaths and 0.9 for white deaths. As The Counted includes only deaths that have been independently verified by professional journalists, clearly the SHR data vastly undercount the incidence of police-caused killings.

Looking at measures of relative population, blacks have an average ratio of .17 black residents for every white resident and Latinos have an average ratio of .42 Latino residents for every white resident indicating that, on average, there are more Latinos than blacks relative to whites in metropolitan areas, consistent with the fact that Latinos are the largest and most urban US minority group. There are also metropolitan areas where blacks and Latinos outnumber whites. For example, blacks outnumber whites in Albany, GA where they comprise 52.67 percent of the population compared to 42.42 percent for whites. Latinos far outnumber non-Hispanic whites in Laredo, TX where they comprise 95.30 percent of the population compared to 3.66 percent for whites.

When it comes to the relative per-capita income, blacks fare slightly better than Latinos with an average ratio of $0.56 in black income for every dollar of white income compared to $0.50 to the dollar for Latinos compared to whites. blacks also fare slightly better than Latinos in areas where their average per-capita income is larger than that of whites. For example, blacks have a higher per-capita income than whites in Farmington, NM with a per-capita income of $38,221 compared to $32,653 for whites. Latinos have a larger per-capita income than whites in Altoona, PA with an income of $25,321 compared to $24,009 for whites. As expected, in areas where blacks and Latinos have a higher per-capita income than whites they represent a small percentage of the population. For example, blacks comprise less than 1 percent of the population in Farmington, NM and Latinos comprise just over 1 percent of the population in Altoona, PA. It is possible that blacks or Latinos moving to such areas do so because of lucrative employment opportunities that are different from traditional employment in more diverse areas. For example, a Latino executive might move to Altoona because he or she is transferred by their employer, or a Latino entrepreneur may see an opportunity for a lucrative business venture there. This is quite different from seeking employment in the larger labor market of a place where one happens to find themselves.

The exposure index is a measure of interaction intended to "measure the degree of potential contact, or possibility of interaction, between minority and majority group members

(Massey and Denton 1988:287). In this index, a larger value means that the average group member lives in a tract with a higher percentage of people from the other group (Massey and Denton 1988). The exposure index measures the degree to which blacks or Latinos in a neighborhood experience segregation, assuming they interact with each neighbor about the same (Bohon and Nagle 2019). Also, because the index depends on the size of the two groups being compared, the maximum value will depend on the proportion of white residents. As such, places with relatively small proportions of black and Latino residents will have a high index of exposure. For example, the exposure index for blacks is 94.39 and for Latinos is 95.35 in Parkersburg, WV-OH where blacks comprise just over one percent and Latinos comprise less than one percent of the population. On average, blacks seem to live in areas with a lower percentage of whites with a score of 57.36, than Latinos with a score of 63.12.

Measures of population growth consistently show that on average, the growth of Latino and black populations surpasses the growth of the overall population. For example, between 2010 and 2015, metropolitan areas had an average population growth of 2.23 percent for all people, 5.62 percent for blacks, and 10.92 percent for Latinos. Similarly, between 2000 and 2015 metropolitan areas had an average population growth of 14.05 percent for all people, 33.6 percent for blacks, and 102.81 percent for Latinos. In the 2010-2015 period, the highest overall population growth occurred in Midland, TX and the highest overall population loss occurred in Pine Bluff, AR. In the same 5-year period, the highest population increase for blacks occurred in Wenatchee, WA where the share of blacks increased by 62.78 percent. The largest population loss for blacks occurred in Lewiston, ME where the share of blacks decreased by 63.78 percent. The highest population growth for Latinos occurred in Bismarck, ND where the share of Latinos increased by

62.47 percent and the largest population loss for Latinos occurred in Montgomery, AL where the share of Latinos decreased by 2.9 percent. It is worth noting that the largest increases and decreases in the share of blacks and Latinos occurred in majority white areas as small population changes would necessarily result in large percentage gains or losses.

Population changes were much more pronounced between 2000 and 2015. Overall population growth was highest in St. George, UT with a population increase of 64.07 percent and the highest population loss occurred in Pine Bluff, AR with a decrease of 10.45 percent. black population growth was highest in St. George, UT with an increase of 392.47 percent and lowest in Carson City, NV where the share of black residents decreased by 46.62 percent. Lastly, Latino population growth was highest in Winchester, VA with an increase of 300.78 percent and lowest in Saginaw, MI with an increase of just 12.69 percent. Although some metropolitan areas experienced a decline in the overall, as well as the black population, no metropolitan area had a decrease in the Latino population.

Focusing on the last couple variables used in this analysis, we see that metropolitan areas in this sample have an average of 693,370 people. The largest metropolitan area is Los Angeles, CA with 13,154,457 residents and the smallest metropolitan area is Carson City, NV with a total of 54,482 residents.

Metropolitan areas had an average violent crime rate of 375.22 violent crimes per 100,000 residents. In terms of police presence, metropolitan areas have an average of 2.3 police officers per 1,000 residents. Most metropolitan areas in this sample are in the South (36 percent) followed by the West (24 percent), Midwest (23 percent), Southwest (18 percent), Northwest (13 percent) and Northeast (11 percent).

Evaluating Different Specifications of Threat

Before I evaluate the relationship between threat and police-caused killings, I assess whether minority threat, economic competition, or power threat models better fit the relationship between threat and police-caused killings. Group threat and minority threat theories often draw from the foundational work of Blumer (1958) on racialized group dynamics. Blalock (1967) specified specific functions by which threat would bring about discriminatory activities. Although the basic notion of group threat is that as the size of the minority population increases, so will instances of racial discrimination or discretionary social control, Blalock devoted time elaborating on what this threat process might look like. He specified three testable models of threat: minority threat, economic competition, and power threat models. Minority threat, which is often tested in the criminological literature (c.f. Liska and Yu 1992; Sorensen et al 1993; Smith 2003,2004; Holmes 2000; Holmes et al. 2018) refers to models in which the percentage of non-whites is strongly and linearly associated with increases in police-caused killing (Liska and Yu 1992).

In order to test minority threat theory, I fit all independent variables in my model as linearly related to the expected count of police-caused killings. I then re-fit the model using the same predictors but taking the natural log of the predictors to test economic competition. Lastly, I fit a model with all predictors as well as quadratic terms for these predictors in order to test whether quadratic effects are present or not. I then estimate predicted counts of police-caused killings for all models as well as AIC scores for all models. It is worth noting that in these competing models, all control variables are logged transformed, which aids in model fit. Figure 4-4 presents AIC scores for all models estimated. Clearly economic competition, followed by power threat models, consistently provide better fitting models than minority threat models, which are commonly employed in studies of police-caused killings (c.f. Liska and Yu 1992; Smith 2003;2004).

When looking at the predicted counts, modeling police-caused killings using a linear function drastically over-estimates the incidence of killings. For example, in estimating the total incidence of police-caused killings, the minority threat model predicts an average of 28 police-caused killings, the economic competition model predicts an average of 5 killings and the power threat model predicts an average of 6 killings per metropolitan area. As shown in Table 4-4, the average number of all people killed across 323 metropolitan areas is around 5. Figure 4-3 displays the average number of police-caused killings for all models by dependent variable. Overall, it seems clear that a minority threat model over-estimates the average counts of police-caused killings, while the economic competition model consistently estimates counts that are close to the observed data. Thus, the economic competition models better fit the relationship between population and police-caused killings and is used in subsequent analyses in this chapter.

Is There a Relationship Between Threat and Police-Caused Killings?

Table 4-5 shows the results of five negative binomial regression models used to test economic competition. Models 1-5 represent all, minority, black, Latino and white deaths, respectively. Table 4-5 reports standardized logged coefficients, but I use standardized percent change values in my discussion of results as it yields a more meaningful interpretation of significant terms (see Long 1997). This approach is often used in studies of police-caused killings (c.f. Smith and Holmes 2014; Holmes et al. 2018), and the resultant coefficients can be interpreted as the percent change in killings associated with a one standard deviation increase from the average of a predictor when all other predictors are held constant. Since all the predictors in the models are logged, when I discuss percent change, I am reporting the change from an unlogged variable to increase comprehension.

Table 4-5 shows that the ratio of black to white people is significantly associated with an increased expected count of police-caused killings for blacks (one standard deviation increase is associated with a 7 percent increase in black deaths), but not for the other groups. The ratio of Latino to white people is significantly associated with a 92 percent increased expected count of police-caused killings for Latinos but not for other groups. Both measures of "threat" are associated with increases in the expected count of police-caused killings for black and Latinos but not for all, white, or minorities more generally. This is consistent with the group threat hypothesis and partially supports the findings of previous research as it relates to Latinos. For example, Holmes and his colleagues (2018) find that Latino "threat" is unrelated to police-caused killings in all models except for the Latino specific model. However, they find no effect for black "threat" on black-specific models of police killings, but a significant and negative effect in total incidence-models (Holmes et al. 2018). Insofar as the relative population of blacks and Latinos is associated with police-caused killings, it is only associated with the expected count of killings for blacks and Latinos but not associated with an overall increase in killings.

In all, it seems that by using better data sources and measures of threat that account for the relative size of the white population I can get results that are more consistent with the work of Blalock (1967). That is, unlike Holmes and his colleagues (2018), I find that the measures of black "threat" are associated with increases in the expected count of police-caused killings of blacks and unrelated to the overall count of police-caused killings. Similarly, as the theory would predict, increases in the Latino population are associated with increases in the expected count of police-caused killings of Latinos, but unrelated to the overall count of killings. These disparate findings highlight the benefits of using different operationalizations of threat as well as different data sources that may capture the race-specific count of police-caused killings more accurately. In fact, the differences in estimated deaths by source are far greater when looking at race-specific counts. As previously noted, when compared to the SHR estimates for 2015 alone, The Counted reports more than eleven times as many deaths of black civilians as the SHR does. As such, it is not surprising that my results differ from previous analyses (c.f. Holmes et al. 2018) that have used unreliable data sources and crude measures of threat.

I find no relationship between relative income and police-caused deaths; recent work on minority threat and police caused killings using different measures of income inequality also yield non-significant findings (Smith and Holmes 2014). However, older studies report that large differences between majority and minority income are associated with more "threat" (Jackson and Carroll 1981; Holmes 2000), including police-caused killings (Jacobs and O'Brien 1998). These older findings are counter to Blalock's (1967) economic competition theory that suggests that people of color may be perceived as threatening when the income gap is reduced, meaning when the income of blacks and Latinos approaches that of whites.

The black-white exposure index is significantly and positively related to a 4 percent increase in the expected count of police-caused killing of whites, but insignificant in all other models. No significant effects are found for the Latino-white exposure index. Previous research using the index of dissimilarity (a measure of evenness rather than exposure) indicate a significant relationship between black-white segregation and police-caused killings (c.f. Liska and Yu 1992; Holmes et al. 2018). Although the theory would lead us to expect a significant positive relationship between exposure and police-caused killings for blacks and Latinos, my results are not consistent with theory. It could be that whites living in areas with higher scores for the black-white exposure index are viewed as threats and are often policed more than other metropolitan areas leading to an increased risk of death. Alternatively, it could be that because the exposure index is artificially inflated when there is a large proportion of majority group members, an increase of white deaths could be expected in the presence of a small, almost negligible minority population. For example, metropolitan areas with the highest scores for black and Latino-white exposure indices were areas with a black and Latino population of under 2 percent. Clearly, more work is needed to fully understand this finding.

In examining the controls, I find that larger metropolitan population sizes are significantly associated with higher expected counts of police-caused killings for all (19 percent), minority (19 percent), black (25 percent), Latino (32 percent), and white (19 percent) deaths. This relationship is consistent with previous research using different data and models (Jacob's and O'Brien 1998; Smith 2003; Holmes et al. 2018).

The violent crime rate in metropolitan areas is significantly and positively associated with increased expected counts of police-caused killings for all (3 percent), minority (3 percent), (Latino (5 percent), and white (4 percent) deaths, but not related to increases in black deaths. Previous studies of group threat yield mixed results regarding the role of crime in these killings. For example, Sorensen and his colleagues (1993), find that crime is associated with police-caused killings in cities with 100,000 or more residents, but in their analysis of larger cities (250,000 or more residents) crime lost statistical significance. Similarly, Smith (2003) finds the same effect in cities with 100,000 residents or more, but the effect disappears when looking at larger cities with

250,000 residents or more. Moreover, Smith (2004) finds crime to be associated with an increase in all killings in large cities, but this effect is not associated with increases in black or white deaths specifically. These findings are similar to Smith's (2004) findings in that violent crime is not associated with an increase in the expected killing of blacks.

Police per-capita does not seem to be significantly related to the expected number of police-caused killings for any model. This is consistent with other research in that organizational characteristics of police departments in which these killings occur (percent of female officers, percent of non-white police officer, among others—see Smith 2003,2004; Holmes et al. 2018) or police officers per capita (Jacobs and O'Brien 1998) have no statistically significant association with the expected number of people killed by police.

Lastly, in assessing whether there are regional differences in police-caused killings, Table 4-2 indicates that, compared to the Southwest, metropolitan areas in the Northeast region have lower expected killings for all (2 percent), whites (2 percent) and minorities (2 percent). Moreover, metropolitan areas in the Midwest have 2 percent lower expected counts of police-caused killings for all deaths. No regional differences are found in the killing of blacks and Latinos.

In sum, in determining whether or not the size and composition of the minority population is associated with the expected count of police-caused killing at the metropolitan area level, I find that the size of the black and Latino population relative to the white population is associated with an increase in the expected count of police-caused killings of blacks and Latinos, respectively. Moreover, I find that black-white exposure is associated with an increase in the expected count of killings of whites, but it is not significant in any other models. In that "threat" is occurring at the metropolitan level as measured by the black and Latino population relative to the white population, this "threat" is manifesting in race-specific ways consistent with the minority threat hypothesis. That is, although variables used to measure "threat" are not associated with an overall increase in the incidence of police killings, it certainly does influence the expected count of killings of blacks and Latinos, as the minority threat hypothesis suggests. Interestingly, while the effect of threat seems to be much larger for Latinos than it is for blacks, seemingly unrelated regression techniques fail to detect any statistically significant difference between estimates.

Is it Population? Or Population Growth?

Having established a relationship between minority threat and police-caused killing, my focus now turns to population dynamics and the effect on expected counts of police-caused killings. Specifically, I ask, do dynamic measures of population better predict killings than static measures of "threat"? In examining whether there is a threat effect playing into a wide array of social control outcomes, researchers (c.f. Smith 2003, 2004; Holmes et al. 2018, Sorensen et al. 1993; Rocha and Espino 2009) typically look at the percentage of the population that is black or Latino using a single year or snapshot. I argue that, consistent with Blalock's formulation, a closer understanding of his theory would take into account the changes in the non-white population as this may get closer to the process of inter-group conflict detailed by Blumer (1958). In fact, some researchers have incorporated these measures into their models in hopes of providing a more comprehensive understanding on the process of threat (Chamlin 1989; Jacobs and O'Brien 1998; Feldmeyer et al. 2015).

Jacobs and O'Brien (1998) argue that threat theories suggest that law enforcement should be more coercive in cities that have recently experienced growth in the percentage of black residents. They elaborate further by claiming that if the black population grows within a given city, so would concerns about the prevalence of crime. In their models they found that measures of threat were significantly associated with the rate of killings of blacks by police, but not the overall rate of killings by the police (Jacobs and O'brien 1998). More importantly, they find that both the percent of the population that is black as well as the 10-year growth in the black population were significantly associated with increases in the rate of killing of blacks by police. In sum, they point to the importance of accounting for changes in the black population when looking at inter-group conflict and studying racial threat more generally.

More recently, researchers have followed such an approach pointing to the importance of accounting for black and Latino population growth in investigating racial threat. Kane and his colleagues (2013) examined whether increases in black and Latino representation in communities were associated with increased misdemeanor arrests. They find that increases in the percent black population were associated with increases in black misdemeanor arrests, but only in historically majority white census tracts. Furthermore, they find that increases in Latino populations were associated with increased minority misdemeanor arrests across all tracts, as well as in historically majority white tracts (Kane et al. 2013). They argue that threat trigger variables should be measured in terms of difference scores and weighted by initial dominant group representation.

In discussing threat triggers, they draw from the defendant neighborhood literature, arguing like Suttles (1972) that communities in which residents perceive racial encroachment may mobilize informally and formally against minorities. As Kane and his colleagues argue "It seems a reasonable theoretical leap to argue that communities characterized by racial change may encourage the police to use their discretionary authority in ways that attempt to control and/or contain the threat (2013:964)." Although Kane and his colleagues (2013) focused on neighborhoods, I argue that the same process may be taking place in metropolitan areas across the United States since the view of minority criminality and its respective threat has become institutionalized, widespread, and reinforced through a variety of media (Chiricos and Eschholz 2002; Smith and Holmes 2014; Ferrandino 2015). For example, when Latinos began moving to Georgia in large numbers in the 1990s, reporting on "illegal" immigrants in the *Atlanta Journal-Constitution* typically was accompanied by a chart or graph showing the size and growth of the Latino population, thus conflating Latino presence with supposed criminal activity (Bohon and Parrot 2009). Thus, in metropolitan areas with higher than expected minority population growth, members of the majority may feel threatened and demand more policing activities to qualm this perceived threat.

In analyzing changes in the minority population and its effect on police killings and discretionary arrests, a 10-year change score is commonly employed (c.f. Jacobs and O'brien 1998; Kane et al. 2013). However, because of metropolitan area-level data limitations in the 2005 American Community Survey, I run models examining 5-year and 15-year population increases to ascertain whether short- or long-term changes in the black and Latino population is related to police-caused killings. I use county-level information for 2000, 2010, and 2015 and match these counties to their specific metropolitan areas in order to obtain population growth rate measures at the metropolitan area level. In matching counties to metropolitan areas, I use the latest metropolitan statistical area delineations consistent with the Office of Management and Budget (OMB) definitions of metropolitan-micropolitan statistical areas. This allows me to look at the changes in population using the same metropolitan statistical areas across time. This allows me to look at changes in areas that are defined metropolitan by today's guidelines.

The analytical strategy mirrors that of previous models. I estimate all models and compare competing models' AIC and overall prediction accuracy. Table 4-6 shows the results of five negative binomial regression models used to determine whether 5-year changes in the black and Latino population is associated with the expected count of police-caused killings. Although I anticipated that growth in the minority population would be positively related to police-caused killings of people of color, I find no evidence of this. However, all significant predictors in the previous models remain significant in this model. Overall population growth seems to only matter in the model predicting the expected count of police-caused killings of whites with a standard deviation increase being associated with a 21 percent increase in the expected count of white deaths.

Next, I re-estimate models using the 15-year population growth variables instead of 5-year population growth variables in order to assess whether long-term changes in the percentage of residents that are black, and Latino are associated with changes in the expected count of police-caused killings. Overall, when comparing short-term growth estimates to long-term growth estimates in Table 4-5 all significant predictors remain unchanged with a few exceptions. When looking at the black deaths model, I see that a 15-year increase in the percent of the population that is Latino is associated with a 28 percent increase in the expected number of blacks killed by police. However, this predictor is insignificant in all other models. In making sense of this finding, it could be that in areas where there is a large non-white increase in the population, groups that have traditionally been associated with criminality will suffer the consequences in terms of police surveillance. That is, in areas where whites are potentially losing their status as a majority, police may be more likely to act in stereotypical consistent ways, thus potentially killing more black residents. More work is needed in explaining why an increase in the Latino population may be associated with increases in the expected count of black deaths but not Latino deaths.

While it appears that population change is associated with police-caused killing only insofar as long-term Latino population change is associated with black deaths, other findings from Table 4-5 are worth touching upon. In the white deaths model, regional differences disappear once 15-year population growth variables are included as predictors. Moreover, while the overall 5-year population growth was associated with an increase in the expected count of white deaths in Table 3, no effects are significant in Table 4-2. Lastly, when modeling minority deaths and including 15-year growth measures, there seems to be a significant effect for the Latino population relative to whites. In this model, a one standard deviation increase in the Latino population relative to whites is associated with a 47 percent increase in the expected count of Latino deaths. Overall, population change has little impact on police-caused killing.

Are there differences between models?

The last research question addressed in this chapter seeks to determine if the models used here are better able to predict the expected count of all deaths, minority deaths, black deaths, Latino deaths, or white deaths. In order to answer this question, I use seemingly unrelated regression techniques which allows me to assess whether effects are statistically different from each other when the dependent variable is related, but not the same. This will allow me to test whether the effect of a single predictor is statistically different between models with different dependent variables. Overall, not a single seemingly unrelated regression post-estimation test indicated that predictors are different between models. Although "threat" indicators were only significant for black and Latino death models, they do not seem statistically different from each other from model to model.

Lastly, I wanted to determine if the findings obtained would vary from data-source to data source. Although SHR data has been used almost exclusively in past studies of minority threat and police-caused killings (c.f. Smith 2003, 2004; O'Brien and Jacobs 1998; Holmes et al. 2018) there have been constant critiques of the value of SHR data due to issues of underreporting. For example, Klinger (2012) states that the issue of underreporting in SHR data makes cross-city comparisons of killings essentially worthless. Williams and his colleagues (2016) also point to the flaws of these data claiming that they may undercount the overall incidence of police killings by as much as 75 percent. Others, such as Holmes and his colleagues (2018) continue to use the SHR data and argue that while undercounting may be an issue, it would not lead to biased results so long as underreporting does not vary systematically across observations. However, researchers have long argued that the underreporting in official estimates of police-caused killings may indeed vary systematically (Loftin et al. 2003,2017). Indeed, I show in the previous chapter that SHR data are not missing at random. Since there is no clear consensus on the appropriateness of the data, and because table 4-1 shows substantial differences between estimates, I wanted to see for myself if the data changed the overall results.

Table 4-8 presents results for five negative binomial regression models using the SHR data. The models employed are identical to models used to obtain the data presented in Table 4-5. Despite data differences, the results in Table 4-5 and 4-8 are quite similar. Population has a statistically significant positive effect on the expected count of all, black, Latino, white and minority deaths. Violent crime has a statistically significant positive effect on the expected count of all, Latino, and white deaths. In these models, crime is not associated with increases in the expected count of black and overall minority deaths calling into question arguments stating that the killing of black residents is a function of black criminality (c.f. Cesario et al. 2018). In terms of group threat indicators, the black population relative to whites is still significantly and positively associated with increases in the expected count of black deaths. However, the Latino population relative to whites is not significantly associated with increases in the expected count of Latino deaths, suggesting, possibly that Latino deaths are less likely to be reported in the SHR or are reported as white deaths (Eppler-Epstein et al. 2016). Moreover, while black-white exposure has been consistently associated with increases in the expected count of white deaths in previous models, that is not the case using the SHR. In these models, black-white exposure is associated with increases in the expected count of Latino deaths. Overall, there are some small differences between regional controls, but overall findings remain consistent.

Despite Klinger (2012) and Williams and his colleagues (2016) arguing that the SHR data is essentially useless due to under-counting, I am surprised to find that for the most part, findings are not that different. However, with the availability of other data sources which clearly do a much better job at keeping track of these deaths, I do not see the point in continuing to use the SHR data when underreporting is such an issue. Also, descriptive statistics using the SHR provide a false picture of the true incidence of police-caused killing. Moreover, the SHR data provides less information on the overall incidents than data from The Counted or Fatal Force provide, which allows the researcher to make better choices about which cases to include in counts.

CONCLUSION

Overall, in this chapter I ascertained whether the size and composition of the minority population predicted the expected count of police-caused killing at the metropolitan area. Moreover, I determined which model of group threat best describes the relationship between minority population size and police-caused killings. Economic competition models do a much better job at modeling these data than minority threat or power threat models. Economic competition models consistently show a better fit based on AIC and the prediction seem to be more consistent with the observed data than predictions from other models. It seems clear that logging predictors produces models with the best fit. Moreover, I find that the size of the black population relative to whites is associated with increases in the expected count of black deaths and the size of the Latino population relative to whites is associated with the expected count of Latino deaths. Thus, it seems like "threat" as measured in this chapter is associated with race-specific killings, but not with the overall incidence of killings, consistent with theory. These findings are consistent with studies from two decades ago or longer but stand in sharp contrast to more recent studies using poorer quality data and models that find no association between "threat" and the deaths of people of color.

Surprisingly, the black-white exposure index is associated with increases in the expected count of white deaths. Moreover, population size is associated with increases in the expected count of all, black, Latino, white, and minority deaths. Violent crime is significantly associated with increases in the expected count of deaths of all groups across all models, except for the expected count of black deaths. Violent crime rates are not related to the killing of blacks by police. Lastly, there are some regional variations in the expected count of all, white and minority deaths but no regional differences in the killing of blacks or Latinos.

In testing if dynamic measures of population better predict the expected count of killings than traditional static measures of threat, it seems like the overall change of the population is not related to changes in the expected count of killings. Moreover, there does not seem to be a benefit in including such predictors based on model information criterion or model parsimony.

Chapter Five

GROUP THREAT IN COUNTIES ACROSS THE US

Building on the findings from the previous chapter, in this chapter I assess the relationship between group threat theory and police-caused killings at the county level. Focusing on a smaller level of geography will allow me to 1) include more deaths that were previously excluded because they did not occur in a metropolitan area, 2) determine whether there are "threat" differences between rural and metropolitan counties, 3) examine whether significant relationships at the metropolitan level remain significant at the county level, and 4) assess whether threat effects are more prevalent in counties where there has been a shift in the majority population. Specifically, I ask:

RQ2a. What is the relationship between threat, threat triggers, and police-caused killings at the county-level?

RQ2b. Do threat triggers moderate the relationship between threat and police-caused killings?

RQ2c. Among residents who were shot and killed, were black and Latino residents less likely than white residents to have been attacking the police officers or others?

RQ2d. Among residents who were shot and killed, were black and Latino residents more likely to have been unarmed than white residents?

RQ2e. What is the relationship between threat at the county level and the likelihood of being shot and killed while unarmed?

RQ2f. What is the relationship between threat at the county level and the likelihood of being shot and killed while not attacking the police officer or others?

Although scholars often look at large cities in studying the relationship between group threat and social control outcomes (c.f. Sorensen et al. 1993; Jacobs and O'Brien 1998; Holmes et al. 2018) doing so ignores the reality that social control, and in this case police-caused killings, is not solely a metropolitan problem. By using metropolitan areas across the United States as the unit of analysis in the previous analysis, I lose about 15 percent of killings since they do not occur in metropolitan areas. Thus, by looking at counties as my unit of analysis I can retain a higher proportion of cases in my analysis and look at differences between metropolitan and rural counties. Moreover, using different unit of analysis to evaluate findings is well within the scope of studies looking at the relationship between group threat and police-caused killings. Additionally, since there is no clear consensus on the level of geography that should be used in testing group threat effects, it is worth evaluating whether threat effects are contingent on the geographical level used.

Many scholars have investigated the relationship between group threat and police-caused killings using different sized cities as their unit of analysis (c.f. Sorensen et al. 1993; Jacobs and O'Brien 1998; Smith 2003, 2004) and have found contradictory findings. For example, Smith (2003) finds the effect of racial threat (measured as the percent of the population that is black) to be much stronger in large cities (250,000 or more residents) when compared to small cities (100,000 or more residents). Additionally, he finds that although crime is a significant predictor of police-caused killings in small cities, this relationship disappears when looking at large cities (Smith 2003). Moreover, Smith (2004) finds a statistically significant positive effect between percent black and the number of people killed by police in cities with 100,000 or more residents, but not in cities with 250,000 or more residents. Clearly, the unit of analysis used makes a difference in the results obtained. As such, this analysis builds on the previous one, by replicating the analysis using counties instead of metropolitan areas to assess whether the relationship between group threat and police-caused killings holds at both levels.

Additionally, this analysis uses three years of police-caused killings to measure the dependent variable, whereas the previous analysis only used two years. The use of single year, two years, or three years of data should not change the results. However, the main difference in the availability of data for various years is the source of the data. *The Washington Post* provides data on police-caused killings starting in 2015 and continues to track these cases to this day. The Counted stopped tracking and collecting information on police-caused killings in 2016. Thus, this analysis uses *The Washington Post* and the Supplementary Homicide Reports data on the number of people killed by police from 2015 to 2017.

Following the analytical strategy of Kane and his colleagues (2013), this analysis seeks to examine the relationship between group threat and police-caused killings, while considering the transformation of counties from historically white communities to more racially diverse communities. As Suttles (1972) has argued, the relative population of minorities may mean less in areas that are predominantly African American than in areas that are predominantly or mostly white. Thus, by looking at the relationship between black and Latino populations relative to white populations and taking into account demographic shifts that have altered the majority group representation across counties, I can test whether “threat triggers” moderate the relationship between minority populations and police-caused killings.

In this analysis, threat trigger refers to the change in majority white counties. Specifically, threat may be felt more strongly in counties that have turned from majority white (51 percent or more) counties to minority white (under 51 percent white) counties in a 15-year time period.

Lastly, the last set of questions that guide this chapter are grounded on the work of Nix and his colleagues (2017) and Wang and Mears (2015). Nix and his colleagues (2017) used one year of data published by *The Washington Post* to assess whether there was evidence of implicit bias in police-caused killings by using multivariate regression models to predict two measures of threat perception failure. They measure threat perception failure as 1) whether a resident was attacking an officer or resident or not prior to being killed by police, and 2) whether residents were unarmed when fatally shot. Nix and his colleagues (2017) found that residents from other minority groups (they classified "other" as all residents who were not classified as white or black) were more likely than whites to be shot by the police when not attacking the officer or residents and that black residents were more than twice as likely as white residents to have been unarmed at the time of their death. Thus, they conclude that such findings are evidence of implicit bias (Nix et al. 2017). This analysis replicates their analysis using four years of data[8] and county-level predictors to determine whether there is evidence of racial bias in police-caused killings, and whether county-level measures of threat are associated with measures of bias.

[8] The county level analysis uses three years of data from *The Washington Post* because I am comparing my results to models using data from the SHR (SHR data is only available for 2015-2017). The multi-level analysis uses 4 years of data (2015-2019) as I am only using data provided by *The Washington Post*.

ANALYTICAL STRATEGY

I follow the same analytical strategy as the previous chapter in order to assess the relationship between threat, threat triggers, and police caused killings at the county-level. That is, I regress counts of all, minority, black, Latino and white deaths on measures of threat and threat triggers on all, minority, black, Latino and white deaths using negative binomial models. Before fitting the models and presenting results, I run Poisson, negative binomial, and zero inflated variants of these models and determine which one best fit the data. Table 5-1 displays the mean and variances for all dependent variables in the analyses. Given that the variances are much larger than the mean in all cases, Poisson regression would not be suited for these data. I fit all remaining models and assess how well the predicted probabilities match the observed probabilities in the data. Figure 5-1 displays the observed versus predicted probabilities of my models. As shown, the negative binomial and zero-inflated negative binomial models seem to fit the observed data quite well. Thus, in keeping with the previous analysis, I model the overall, as well as race-specific count of police-caused killings using negative binomial models.

Moving on to the next set of analyses, I assess whether there is evidence of racial bias in the data provided by *The Washington Post* from 2015-2018. I can use an additional year of data in determining whether there is evidence of racial bias, as I am not comparing results between these data and the data provided by the SHR. Given that I am assessing whether there is evidence of racial bias using two dichotomous variables – not attacking and unarmed – I begin by estimating multivariate logistic regression models. However, given that I am assessing whether county-level indicators of threat influence individual measures of bias, a better approach would be to fit the models using hierarchical logistic regression models, which assess whether there is between county level variation in measures of bias.

I start the analyses by regressing individual-level characteristics on measures of bias to assess whether the race of residents killed by police was associated with the likelihood of a resident being killed while not armed and not attacking the police or other residents. If blacks, Latinos, or residents categorized as other race (i.e. people killed who were not categorized as white, black or Latino) are more likely than whites to die while not attacking or being armed, then this would be indicative of bias (see Nix et al. 2017). I then include county-level measures of group threat to assess whether the size of the black and Latino population relative to the white population influences measures of bias.

RESULTS

Descriptive Statistics

Table 5-1 presents descriptive statistics for all variables used to assess the relationship between group threat, threat triggers and police-caused killings at the county level (RQ2a and RQ2b). On average, 1.06 people were killed in each US county during the three-year period, and the average number of minorities killed was 0.55. On average, 0.25 blacks, 0.19 Latinos, and 0.52 whites were shot to death in US counties between 2015 and 2017 as reported by *The Washington Post*. The average number of deaths reported by the SHR in the same time period is much smaller: 0.48 for all deaths, 0.29 for minority deaths, 0.02 for black deaths, 0.06 for Latino deaths and 0.19 for white deaths. As noted in the previous chapter, clearly the SHR data vastly underreports the incidence of police-caused killings.

On average, county populations have about 0.19 black residents and 0.26 Latino residents for every white resident, consistent with the fact that there are more Latino than black people in the United States. There are also counties where blacks and Latinos outnumber whites. For example, blacks outnumber whites in Claiborne County, MS where they comprise 84 percent of the population compared to 14 percent for whites. Latinos far outnumber whites in Starr County, TX where they comprise 99 percent of the population and whites comprise about 1 percent of the total population.

When it comes to relative per-capita income, blacks across counties make an average of $0.57 for every dollar whites earn, and Latinos make an average of $0.53 for every dollar whites earn. However, there are counties where the average income of blacks and Latinos far exceeds the average income of whites. For example, blacks have a higher per-capita income than whites in Teton County, WY with a per-capita income of $532,063 compared to $47,903 for whites. Latinos have a higher per-capita income than whites in West Carroll Parish, LA with an income of $44,474 compared to $20,689 for whites. As in the previous chapter, in areas where blacks and Latinos have a higher per-capita income than whites they represent a relatively small percentage of the population. For example, in blacks comprise less than 1 percent of the population in Teton County, WY (home to the resort town, Jackson Hole) and Latinos comprise less than 3 percent of the population in West Carroll Parish.

On average, blacks and Latinos have similar levels of exposure to whites across counties as indicated by the index of exposure. blacks and Latinos seem to fare about the same in terms of exposure to whites across counties with average scores of 0.70 and 0.73 respectively. Thus, blacks and Latinos across counties have about a 70 percent chance of encountering a white resident in their county, assuming they have the same chances of encountering all residents in their county.

Looking at population, counties in the sample have an average population of 112,400 residents. The least populated county in this sample is Bristol Bay Borough, AK with just under

1,000 residents and the most populated county in this sample is Los Angeles County, CA with a population of over one million residents. On average, there is just over 1 violent crime per thousand residents in 2014. There are a lot of places where the reported violent crime is 0, however whether the zero represents a lack of crime or a lack of reporting is unknown. Davidson County, TN has the largest violent crime per thousand residents in 2014 with over eight violent crimes per thousand residents.

On average, there are just over 2 police officers per thousand residents in this sample. No police officers were reported for the Yukon-Koyukuk census area in Alaska for 2014 and over 70 police officers per thousand residents were reported for Yancey County, NC. Compared to other counties Yancey has an unusually high proportion of residents working in law enforcement. In fact, Data USA's profile on Yancey county states that the county has more than two times the expected number of police officers per capita (https://datausa.io/profile/geo/yancey-county-nc). However, such a high number of police officers per-capita may simply indicate that there are few people living in Yancey county (about 17,000 people) and that most people are probably employed as forest rangers (Yancey county is in the middle of the Pisgah National Forest).

Moving on to the threat trigger indicator, on average, only 3 percent of counties that were comprised of at least 51 percent white residents in 2000 lost that majority by 2015. This may be indicative of insularity in these counties and relatively low in-migration of blacks and or Latinos. About 41 percent of counties in the sample are considered metropolitan according to the latest OMB rural-metropolitan classification scheme for counties. Lastly, 12 percent of counties are in the Southwest, 40 percent of counties are in the South, 8 percent of counties are in the Northeast, 32 percent in the Midwest and 8 percent in the Northwest. The regional classification used in these analyses' mirrors that of Smith and Holmes (2014) and the analyses in Chapter Four.

Assessing the Relationship between Group Threat, and Killings across Counties

Table 5-3 shows the results of five negative binomial regression models used to test economic competition models. Models 1-5 represent all, minority, black, Latino, and white deaths, respectively. I report standardized logged coefficients, but I used standardized percent change values in my discussion of results as it yields a more meaningful interpretation of significant terms (see Long 1997). This approach is the same one used in Chapter Four, and consistent with studies of police-caused killings (c.f. Smith and Holmes 2014; Holmes et al. 2018).

Table 5-3 shows that the ratio of black to white people is significantly associated with a lower expected count of police-caused killings for all and white people, but an increased expected count of police-caused killings for blacks. A one standard deviation increase in the black-white population ratio is associated with a 214 percent and a 216 percent decrease for all and white deaths, but a 475 percent increase in expected killings of blacks. The ratio of Latino to white people is significantly associated with a lower expected count of police-caused killings for all, black, and white people, but an increase in the expected count of police-caused killings of Latinos. A one standard deviation increase in the Latino-white population ratio is associated with a 235 percent, 214 percent, and 221 percent decrease for all, black and white deaths, but a 998 percent increase in the expected count of Latino deaths. In so far as relative (to white) black and Latino populations influence the expected count of police-caused killings, it seems like these measures of threat have a negative effect on all deaths as well as white deaths; but are associated with increased black and Latino deaths. The finding that when the black population increases, relative to whites, the number of black people dying at the hands of police increases is consistent with the threat hypothesis, as is the finding regarding the relative Latino population.

A surprising finding is that as the Latino population grows (relative to the white population) the expected count of black deaths is lower. It could be that when the Latino population increases relative to whites, social control efforts are redirected toward Latinos away from blacks, reducing the expected count of black deaths. Future studies should examine changes in neighborhood policing patterns with population change. These findings are also consistent with previous research. For example, Holmes and his colleagues (2018) find that the proportion of black residents in large cities is associated with lower expected count of police-caused killings for all people.

I find no relationship between relative income and police-caused killings. Recent work on minority threat using different measures of income inequality also yield non-significant findings (Smith and Holmes 2014). However, older studies report that large differences between majority and minority income that benefit the majority are associated with more “threat” (Jackson and Carroll 1981; Holmes 2000), including police-caused killings (Jacobs and O’Brien 1998). These older findings are counter to Blalock’s (1967) economic competition theory that suggests that people of color may be perceived as threatening when the income gap is reduced, meaning when the income of blacks and Latinos approaches that of whites. Given these outcomes, it seems likely that Blalock (1967) is wrong or that scholars have not yet discovered the best means of measuring the type of economic disparities that lead to perceptions of threat, which in turn, lead to increases in social control efforts toward minorities. It could also be that threat effects depend on the overall economic conditions, meaning that threat effects could be strong during economic uncertainty (i.e. high unemployment, recession), but weak during economic prosperity.

The black-white exposure index is significantly and negatively related to the expected count of police-caused killings for all deaths and minority deaths. A one standard deviation increase in the black-white exposure index is associated with a 192 percent and 166 percent lower expected count of police-caused killings for all and minority residents. However, the black-white interaction index is significantly and positively related to the expected count of police-caused killings of Latinos. That is, a one standard deviation increase in black-white exposure (as measured by this index) is associated with 820 percent higher count of police-caused killing of Latinos. It is important to note that while previous analysis used both black-white and Latino-white exposure indices, I removed the Latino-white interaction index due to high collinearity with the black-white interaction index. These measures had a correlation of .75 and the Latino-white interaction index had a variance inflation factor (VIF) of 10.23. Since these measures are related to one another, it seems like counties are either racially integrated or they are not.

In that the level of interaction between blacks, Latinos, and whites is the same (as measured by the black-white exposure index) these findings are consistent with theory. In other words, higher exposure between minority and majority group members would be associated with more discriminatory acts. This seems to be the case for Latino deaths. However, the decrease in all deaths and minority deaths could be a statistical artifice of the fact that the exposure index is inflated in counties with very large white populations. Counties with the highest black-white interaction index values are roughly 98 percent white, thus, it could be that in these counties an overall minority presence is not “threatening” enough to engender an increase in calls for more social control (as measured by police-caused killings). However, if this were the case, then we could expect a lower expected count of killings for Latinos, too. More work is needed to fully understand the relationship between black-white exposure and police-caused killings, especially police-caused killing of Latinos, and a more nuanced measure of residential segregation may need to be developed.

Moving on to the controls in the model, I find that larger county populations are significantly associated with higher expected counts of police-caused killings for all (470 percent), minority (520 percent), black (1,890 percent), Latino (1,380 percent) and white (490 percent) deaths. Previous studies of group threat consistently find a relationship between population size and police-caused killings (Jacobs and O'Brien 1998; Smith 2003; Holmes et al. 2018) if for no other reason that there are more people to kill.

The violent crime rate in counties is significantly and positively associated with increased expected counts of police-caused killings for all (316 percent), minority (328 percent), black (305 percent), and white (307 percent) deaths, but not related to increases in Latino deaths. Previous studies of group threat yield mixed results regarding the role of crime in police-caused killings. For example, Sorensen and his colleagues (1993) find that crime is associated with police-caused killings in cities with 100,000 or more residents, but in their analysis of larger cities (250,000 residents or more residents) crime loses statistical significance. Moreover, Smith (2004) finds crime to be associated with increases in all killings, but not with increases in black or white deaths specifically. It is also possible that fluctuations in crime rates are felt more strongly in smaller geographies (i.e. counties, census tracts) than larger geographies such as metro areas.

Police per-capita is significantly and positively associated with increased expected counts of police-caused killings for all (301 percent), minority (325 percent), black (309 percent) and Latino (357 percent), but not for white deaths. These findings are in line with studies evaluating the effect between organizational characteristics of police departments in which these killings occur (percent of female officers, percent of non-white police officers, among others—See Smith 2003, 2004; Holmes et al. 2018) or police officers per capita (Jacobs and O'Brien 1998) and police-caused killings. Previous studies have found no statistically significant relationship between police per-capita and police-caused killings. association with the expected number of people killed by police.

Lastly, in assessing whether there are regional differences in police-caused killings the results indicate that, compared to the Southwest, counties in the South have lower expected killings of minorities (206 percent) and Latinos (159 percent). Moreover, counties in the Northeast region have lower expected killings for all (136 percent), minorities (135 percent), blacks (181 percent), Latinos (135 percent), and whites (130 percent). Counties in the Midwest region have lower expected killings of all (168 percent), minorities (168 percent), Latinos (140 percent) and whites (158 percent), but not of blacks. Lastly, metropolitan counties have a higher expected count of police-caused killings of minorities (368 percent).

In sum, I find that the size of the black population relative to the white population is associated with a decrease in the expected count of police-caused killings overall, as well as of whites, but it is associated with an increase in the expected count of police-caused killing of blacks. Similarly, I find that the size of the Latino population relative to the white population is associated with a decrease in the expected count of police-caused killings overall, as well as of blacks and whites, but it is associated with an increase in the expected count of police-caused killing of

Latinos. The black-white interaction index is associated with decreases in the overall expected count of police-caused killing as well as minorities but is associated with an increase in the expected count of police-caused killing of Latinos. Counties with larger populations have higher expected count of police-caused killings in all models. Police per-capita is associated with increases in the expected count of police-caused killings in all models except the white death model. The results indicate that overall there are less expected counts of police-caused killings in the South (minorities and Latinos), the Northeast (all models) and the Midwest (all, minority, Latino and white) compared to the Southwest. Lastly, metropolitan counties seem to have a higher expected count of police-caused killings of Latinos.

When examining threat triggers, I ask whether threat triggers have an independent effect on the expected count of police-caused killings and whether threat triggers moderate the relationship between threat and police-caused killings. Table 5-4 shows the results of five negative binomial regression models used to assess the main effect of triggers on police-caused killings. Overall, table 5-4 indicates that threat triggers do not have an independent effect on killings. In fact, the results are nearly identical to results shown in table 5-3. The only difference being slight changes in coefficients. Given that threat triggers do not have an independent effect on police-caused killings, they cannot moderate the relationship between measures of threat and police-caused killings.

Is There Evidence of Racial Bias in Police-Caused Killings?

The remaining questions in this chapter are grounded on the work of Nix and his colleagues (2017) and Wang and Mears (2015). Nix and his colleagues (2017) used one year of data published by *The Washington Post* to assess whether there was evidence of racial bias in police-caused killings by using multivariate regression models to predict two measures of threat perception failure. They measure threat perception failure as a) whether a resident was not attacking an officer or resident prior to being killed by police, and b) whether residents were unarmed when fatally shot. Nix and his colleagues (2017) found that police were more likely to kill a minority group member who was not attacking an officer or residents than a white group member and that black residents were more than twice as likely as white residents to have been unarmed at the time of their death. According to Nix and his colleagues (2017) these findings are indicative of implicit bias in the shooting of residents by the police.

In this section, I replicate Nix and his colleagues' work using four years of data and county level measures of threat to determine if county level threat influences outcomes indicative of implicit bias. Although Nix and his colleagues (2017) control for city and agency level characteristics in their analysis, they did not account for the lack of independence among observations. In my analysis, I also model these data using multilevel models that better address the lack of independence among observations. That is, because there are data on suspects and data on the counties in which these killings take place, we can no longer assume independence of observations given that police actions in the same county are more likely to be similar than police actions from different counties. Thus, I present results using regular logistic regression models as Nix and his colleagues (2017) did, to reestablish their baseline, and hierarchical logistic regression models that are more appropriate for these data. Furthermore, by using four years of data in assessing whether there is evidence of racial bias in police-caused killings, I can determine whether previous evidence of implicit bias remains consistent across years.

Additionally, incorporating measures at the county level can help us understand whether county level measures of threat are influencing measures of bias. Multilevel models of minority threat have shown that threat may be operating at more than one level of aggregation (c.f. Wang and Mears 2015; Ulmer and Johnson 2004). Investigating whether county-level threat is associated with bias as operationalized by Nix and his colleagues (2017) seems like a reasonable next step and contribution to the body of knowledge on minority relations and racial bias.

The data used for these analyses provide 3,943 observations at the incident level (i.e. 3,943 resident deaths) nested in 1,138 counties. Table 5-5 provides descriptive statistics on the variables used to answer the last four research questions. The table shows that out of all people killed by the police between 2015 and 2018, 10 percent were unarmed at the time of their death and 37 percent were not attacking the officer or residents prior to getting shot by police. Among the victims, 24 percent were black, 17 percent were Latino, 48 percent were white, and 4 percent were classified as "other race." Moreover, 32 percent of victims were fleeing when they were shot by police (i.e., they were not posing a threat to police officers). On average, people killed by the police were 36 years of age (the oldest person killed by the police was 91-year-old Franky W. Wratney who was armed but had no criminal record). Police officers had body cameras in 11 percent of shootings and reported signs of mental illness in 24 percent of shootings. Lastly, in 63 percent of cases the suspect shot by police was either attacking a police officer or resident. Overall, it seems as though most suspects were armed at the time of being shot, however what classifies as "armed" in these cases is subject to debate; different operationalizations may alter estimates (see Chapter Three for more information).

Two facts stand out when looking at county-level characteristics in which these killings took place. First, black and Latino measures of threat indicate that in counties where police killed civilians, the black and Latino population relative to the white population was larger than in the overall sample of counties. That is, in the previous analysis (where the sample of counties used included counties with no deaths as well as counties with deaths) the black and Latino population relative to the white population in counties was lower than in the counties where police killed people (.19 vs .26 for blacks and .26 vs .58 for Latinos). Second, the average violent crime rate in these counties (where deaths occurred) is higher in the overall sample of counties used in the previous analysis. Lastly, most deaths occurred in metropolitan counties (84 percent).

Table 5-6 provides results for six models predicting whether the resident who was fatally shot was unarmed. Three models are regular logistic regression models (logistic base, logistic level1 and logistic level2) and three models are hierarchical regression models (ML base, ML level1 and ML level2). All results are interpreted in odds ratios and white residents are used as the reference category. The initial hierarchical level analysis indicates that only 1 percent of the variance in the dependent variable is due to county-level differences (ICC=.012), thus it would be safe to interpret the logistic regression models. That is, there does not seem to be evidence of between county differences in racial bias measured by being shot while unarmed. Overall, blacks killed by police were significantly more likely than whites to have been unarmed when they were killed. Specifically, unarmed blacks were 1.4 times greater odds to be shot than unarmed whites. Across models, I find that unarmed residents who fled have 1.5 times greater odds of being shot as those who did not flee from police. Results also indicate that age was significantly and negatively related to being shot while unarmed.

The presence of a body camera does not seem to be associated with the odds of being shot while unarmed, calling into question how useful calls for more body cameras would be in reducing police-caused killings (Ariel, Farrar, and Sutherland 2015). Moreover, residents who displayed signs of mental illness had 34 percent lower odds of being shot while unnamed than residents without signs of mental illness. These findings could be indicative of police officers being more sympathetic to residents displaying signs of mental illness compared to residents who do not display signs of mental illness. Maybe officers are less threatened when the potential dangerousness of a situation can be attributed to mental illness or may be more concerned with potential backlash stemming from shooting a person with mental illness.

Lastly, residents who were attacking an officer or resident had 79 percent lower odds of being shot while unarmed compared to residents who were not attacking an officer or resident. County level predictors do not seem to be associated with the odds of being shot while unarmed in these models. My findings are consistent with the findings of Nix and his colleagues (2017) in that there seems to be evidence of racial bias in police-caused killings. That is, blacks killed by police were significantly more likely than whites to have been unarmed when they were killed. Despite previous evidence suggesting that threat operates at multiple levels of aggregation (c.f. Wang and Mears 2015; Ulmer and Johnson 2004), the results in this chapter do not provide evidence that county-level threat is associated with measures of racial bias at the individual level.

Table 5-7 provides results for six models predicting whether the resident who was fatally shot was not attacking an officer or resident at the time of death. Three models are regular logistic regression models (logistic base, logistic level 1 and logistic level 2) and three models are hierarchical regression models (ML base, ML level 1 and ML level 2).

The results indicate that Latinos were 1.2 times more likely than whites to have not been attacking an officer or resident when they were shot by police. Again, age is significantly and negatively associated with the odds of being shot while not attacking an officer or resident. Interestingly, the use of a body-camera is significantly and positively associated with higher odds of residents getting shot while not attacking an officer or resident. This certainly runs counter to arguments pointing to body cameras as a potential deterrence of lethal force (c.f. Kahn and Davies 2016). Unlike the previous analysis, mental illness is significantly and positively associated with higher odds of getting shot while not attacking an officer or resident. Specifically, residents displaying signs of mental illness were 1.2 times as likely to die while not attacking an officer or resident than residents who were not displaying signs of mental illness. Unlike previous logistic regression models in table 5-6, there also seems to be regional differences in the odds of being shot while not attacking an officer or resident. Specifically, residents in the South had 24 percent lower odds and residents in the Midwest had 21 percent lower odds of being killed while not attacking an officer or resident.

CONCLUSION

Overall, my findings are consistent with the findings presented by Nix and his colleagues (2017), in that my findings provide evidence of racial bias in police-caused killings using data from *The Washington Post*. However, while Nix and his colleagues find blacks were significantly more likely than whites to have been unarmed and killed by police, and residents from "other" minority groups were significantly more likely than whites to have not been attacking an officer or resident when shot, my results are slightly different. Mainly, I find that Latinos are more likely than whites to have not been attacking officers or others when shot. This may be due to minor differences in coding residents' race and ethnicity. For example, Nix and his colleagues (2017) include Latinos and Asians in the "other" category and I include an indicator for Latinos. However, these results, like the ones presented by Nix and his colleagues, offer further evidence of racial bias. That is, the race of the person killed by police is associated with measures of threat perception failure, which is often used as a proxy for racial bias. None of the county-level predictors were associated with either measure of bias.

In this chapter, I explore threat at a level of analysis that allows me to examine all police-caused killings in the United States. That is, unlike the analysis at the metropolitan level, I include all cases of police-caused killings for which data are available. In sum, results presented in this chapter indicate that the size of the black and Latino populations relative to that of whites is associated with increases in the expected killing of blacks and Latinos but associated with decreases in all and white deaths. Moreover, the black-white exposure index is associated with increases in the expected count of police-caused killings of Latinos but decreases in the expected counts of deaths of all and minorities in general. Population size and violent crime rates in counties seem to be associated with increases in the expected count of killings across models. Metropolitan counties have a higher expected count of police-caused killings of minorities than non-metropolitan counties. The takeaway is clear, more blacks and Latinos are killed by police in places where they pose the biggest threat to whites (as measured by relative population). This finding is consistent with the threat hypothesis, and simply disturbing. Lastly, although threat triggers were hypothesized to be associated with the expected count of police-caused killings, or moderate the relationship between threat and killings, these measures were insignificant in all models.

Chapter Six

RACIAL DISPARITIES AND BENCHMARKING

In this chapter, I assess whether population adjusted rates better fit the threat models than crime adjusted rates of police-caused killings. In doing so I seek to assess whether it is population more generally or crime rates that are better able to predict the overall count of police-caused killings. Police in the United States killed twice as many non-Hispanic white than non-Hispanic black civilians in 2015 and 2016 (Swain et al. 2016), but given that the US white population outnumbers the black population about 5 to 1 (US Census Bureau n.d.), it seems clear that blacks are killed at a disproportionate rate. In fact, when adjusting for population size, black residents are killed by police at twice the rate of whites (Lowery 2016). However, some researchers and pundits have argued that analysts should not adjust for population size (Cesario, Johnson, and Terrill 2018; Selby 2017). They argue that doing so assumes that everyone in the population has an equal chance of dying at the hands of police, but "surely police are more likely to use deadly force in crime-related situations than not, or in dangerous neighborhoods than safe neighborhoods, or when serving arrest warrants then when talking to a 5th-grade classroom" (Cesario n.d.). In short, these analysts argue that those who are killed by the police proportionately represent the criminal class, and it is crime, not population, that provides "more reasonable benchmarks" by which to compare racial differences in police-caused fatalities (Cesario et al. 2018:1).

In making this claim, Cesario, Johnson, and Terrill (2018) collected officially reported US race-specific murder, violent crime, and weapons crime and arrest data and used those data to adjust the number of police-caused deaths from shootings. They calculated odds ratios to compare the odds of being shot, by race, given race-specific crime data, and they find no real evidence that blacks have greater odds of being fatally shot than whites, when adjusting for race-specific crime. Their findings are in concert with work by Fryer (2019), who finds no racial bias in officer-involved shootings, conditioned on being stopped, in a study of 16 metropolitan police departments.

Fryer readily admits that his available data are problematic and that he cannot account for racial differences in the likelihood of being stopped by police [see, for example, Ferrandino (2015) on racial disparities in the likelihood of being stopped by race]. Moreover, he states that the data used only capture the police side of the story, and that "It is possible that these department supplied the data only because they either are enlightened or were not concerned about what the analysis would reveal. In essence, this is equivalent to analyzing labor market discrimination on a set of firms willing to supply a researcher with their human resources data" (Fryer 2019:1214-1215). For these reasons, he cautions that his work should be viewed as adding to the conversation about police violence. Cesario and his colleagues are more assertive in claiming police do not have "one trigger finger for whites and another for blacks" (Cesario et al. 2018 citing Takagi 1974: 30).

The analysis presented by Cesario and his colleagues' study (or Terrill 2018) does not answer the question of whether it is better to adjust police-involved killings by crime or by population. In other words, are black Americans disproportionately likely to be killed by the police (as population adjustment shows) or are they disproportionately likely to commit crimes which result in death (as race-adjusted crime data may indicate)? This question remains unanswered. Additionally, I am concerned that Cesario and his colleagues (2018), and Selby (2017) examined police-caused killings at the national level, ignoring clear and regular differences in crime rates and the number of people killed by police at smaller levels of geography (Holmes, Painter, and Smith 2018; Ross 2015). Moreover, their analyses ignore a plethora of scholarly work on policing bias and disproportionate policing that may be related to the very measure they use to benchmark police-caused killings.

Broadly, I want to know if crime is a better predictor than population of police-caused killing of residents, specifically of black and white victims. Thus, I examine all deaths at the hands of police in 2015, 2016 and 2017 (combined) as well as black and white deaths and use race-specific population and crime data to estimate negative binomial regression models that provide the expected count of police-caused deaths at the county level. In determining which model better captures the variance in the expected count of police-caused killings, I use an Akaike information criterion (AIC) which is an estimator of out-of-sample prediction error and overall quality of statistical models given a specific dataset (Burnham and Anderson 2002).

In answering this question, I estimate 35 bivariate negative binomial regression models[9] for each dependent variable using my predictors of interest, one at the time, and obtain fit statistics for each model. Once fit information was obtained, I calculate Akaike weights, which represent relative likelihood scores for the best fitting models. In order to compare the fit of non-nested count models, researchers examine the relative likelihood that a specific model (i) is minimizing the estimated information loss at the same rate as a baseline model (Burnham and Anderson 2002). (In my analysis, I calculate Akaike weights based on AIC, although a Bayesian Information Criteria (BIC) could be used.) The relative likelihood is estimated as the exponentiated average value of AIC_{min} and $AIC_{i,}$ which can then be compared to zero. Using this method, I can determine the variable that best predicts the expected count of police-caused killings (i.e., the model with the lowest AIC), and I can then examine the relative likelihood that the model with

[9] I estimate 33 models for white and black deaths as there is no information for curfew or runaway arrests.

the next lowest AIC is just as good (i.e., has a relative likelihood substantially different from zero). By using this approach, I can qualify the "best" predictors based on a continuous measure of strength of one model over others (Burnham and Anderson 2002).

There are two types of crime data included in this analysis. First, I have an overall estimate of violent crime which reflects offenses known to law enforcement and are commonly used in similar analyses (Smith 2003,2004). Violent crime estimates consist of the following offenses: murder and non-negligent manslaughter, rape and sexual assault, robbery and aggravated assault. Second, race specific arrest data comes from the UCR Arrest by Age, Sex, and Race in 2015 (ICPSR 36794). I limit the analyses to non-Latino whites and blacks because crime data are most readily available for these two groups and are less reliable for Latinos, as reporting on ethnicity is inconsistent across jurisdictions. In fact, the UCR data on arrests by age, sex, and race at the county level does not report ethnicity at all (Eppler-Epstein, Gurvis and King 2016).

Overall, the models estimated the expected count of all deaths predicted by logged measures of county-level (1) population, (2) violent crime rate, (3) police per-capita, (4) murder arrests, (5) manslaughter arrests, (6) rape arrests, (7) robbery arrests, (8) aggravated assault arrests, (9) burglary arrests, (10) larceny arrests, (11) motor-vehicle theft arrests, (12) other or simple assault arrests, (13) arson arrests, (14) forgery arrests, (15) fraud arrests, (16) embezzlement arrest, (17) stolen property arrests, (18) vandalism arrests, (19) weapons violation arrests, (20) prostitution arrests, (21) sex offense arrests, (22) drug abuse arrests, (23) drug sale arrests, (24) drug possession arrests, (25) gambling arrests, (26) family offense arrests, (27) driving under the influence (DUI) arrests, (28) liquor violation arrests, (29) drunkenness arrests, (30) disorderly conduct arrests, (31) vagrancy arrests, (32) other nontraffic arrests, (33) suspicion arrests, (34) curfew violations and loitering arrests and (35) runaway arrests. I ran separate models for overall deaths, population and arrest data, as well as race-specific models for whites and blacks.

BENCHMARKING, IS IT POPULATION OR CRIME?

Before presenting results for these models, it is important to discuss how crime data is often benchmarked by criminologists, and how the approach taken by Cesario and his colleagues is misleading and troublesome for future studies of police-caused killings.

Benchmarking is among the most common techniques used to assess racial disparities in policing outcomes. In order to benchmark outcomes, researchers typically take the number of stops, arrests, or any other outcome of interest for a specific group and then divide the outcome by that group's overall representation in the population. By doing so, researchers can easily determine whether a group is over or underrepresented in a given outcome (Glaser, 2014). As one can imagine, there is not a single "correct" denominator to use for a given racial group, as the denominator should represent the population "at risk" which can be hard to empirically determine, and choosing population or crime as the denominator in assessing whether racial disparities exists in police-caused killings has implications for interpretation and research more broadly. Cesario and his colleagues (2018) are not the only researchers who question the value of using population data alone as the denominator in policing outcomes. In fact, researchers have long argued that simple demographic data does not account for where people live, work, or decide to spend their free time, neither does it account for the driving population, or undocumented individuals (Glaser 2014; Goff and Kahn 2012). For example, does it make sense to calculate New York City's crime rate as crime per 100,000 New York City residents when many crime victims are tourists and those who commute to the city to work?

Ridgeway and MacDonald (2010) describe methods that are commonly used in assessing racially biased policing. A consistent theme is the lack of agreement on the appropriate benchmark to assess racial disparities. They classify the types of benchmarks used in assessing racial disparities in policing into a) external benchmarks, b) observation benchmarks, c) arrest and crime suspect benchmarks, d) instrumental variables, e) internal benchmarking, and f) post-stop outcomes. I will briefly discuss three of these benchmarks as they are related to the analysis in this chapter. For a detailed discussion see Ridgeway and MacDonald (2010).

External benchmarks comprise what is commonly shown in media outlets, and it entails comparing the distribution of stops (or any other policing outcome) to the racial distribution of the community in question as estimated by the Census. It is a relatively inexpensive, quick, and available method as these data are public and precise. The main criticism of this approach, and one shared by Cesario and his colleagues (2018) is that census data does not help researchers isolate the effect of racial bias from differential exposure and offending. Fridell (2004) summed up the problem of using census data by stating "this method does not address the alternative hypothesis that racial/ethnic groups are not equivalent in the nature and extent of their law-violating behavior" (106). Some researchers have developed different sets of external benchmarks.

For example, Warren and her colleagues (2004) used the race distribution of licensed drivers instead of the residential population to estimate the racial distribution of drivers at risk of being stopped. They found evidence of bias against black men, and that officers were more likely to use a discretionary reason for stopping them. Farrell and his colleagues (2000) used driving population models in order to assess whether law enforcement in Rhode Island engaged in disparate traffic enforcement practices. They found that in most communities, non-white drivers were stopped disproportionately (compared to their presence in the driving population). Despite advances in external benchmarking methods, such methods still fail to capture the race neutrality of police, or account for differential likelihood of traffic stops (Ridgeway and MacDonald 2010)

Observation benchmarks often attempt to estimate the subpopulation at risk for police behavior. This approach involves fielding teams of observers to locations of interest to observe the racial distribution of people driving and violating traffic laws (Ridgeway and MacDonald 2010). While innovative, the approach assumes that police will stop drivers for any traffic violation, which ignores the exercise of discretion in police stops and the fact that officers often target behaviors that may be indicative of other crimes. For example, officers may stop a car for speeding under suspicion that the occupants may be participating in drug transactions, fit a suspect description, or respond to calls for service and may ignore other speeders. Moreover, the expense associated with this approach makes it hard to use in estimating racial disparities in policing outcomes other than traffic stops.

Arrest and crime suspect benchmarks often use the demographic information of arrestees in hopes of providing a more "reasonable" benchmark from which the public can judge the racial distribution of a policing outcome such as stops. This is essentially what Cesario and his colleagues (2018) do in their analysis. However, this benchmark is too narrow as police often make stops for a variety of crimes or suspicious behavior that does not result in arrests. As Ridgeway and MacDonald (2010) argue "Such a benchmark could actually hide bias" (185). The crime suspect benchmark represents the public's reporting of those who are involved in suspicious activity and crime. The goal of using such a benchmark is to assess whether the police are going after suspects who fit the description provided by the public. However, since the public may have their own racial bias, they may over- or under-report certain activities depending on the policing practices and the larger demographic distribution in that area (Ridgeway and MacDonald 2010).

Overall, it is well established that the issue of appropriate benchmarks has plagued the literature on racial bias more broadly. Each method has its benefits and drawbacks as presented by Ridgeway and MacDonald (2010). But given the limitations on data availability and reliability, as well as the cost associated with observation benchmarks and other techniques, I believe the most reliable benchmark to use in assessing racial disparities will be population estimates provided by the census. The census provides the most accurate information on economic and demographic data needed by policy makers and the public on a regular basis. With a budget of nearly of nearly $4 billion dollars for 2019 alone (Census n.d.) and staff working to ensure the data is as accurate as possible, this is the most reliable data researchers have at hand when attempting to benchmark an outcome. By contrast, UCR data depends on the voluntary participation of over 17,000 police agencies across the United States. Researchers have long complained about how unreliable crime data is (c.f. Maltz and Targonski 2003; Eppler-Epstein et al. 2016).

Furthermore, although not every person in the population has the same risk of being killed by the police, those killed are certainly not limited to those arrested or suspected of crimes. And in the research trying to prove/disprove bias in police-caused killing, it makes far more sense to use a less biased measure (population reporting) than a more biased measure (police arrests).

For example, Kahn and Martin (2016) state that "only 750 out of more than 17,000 report their statistics, which makes the UCR notoriously unable to provide an accurate snapshot of the nation and any racial disparities therein" (90). Maltz and Targonski (2003) detail at length the problems associated with crime data provided by the UCR, concluding that county-level UCR crime statistics should not be used for evaluating the effects of policy initiatives on crime. Skogan (1974:25) stated that “yearly Uniform Crime Reports are reputedly invalid indicators even of the limited actions which they purport to measure directly.” However, he admits that so long as we make “modest" demands of the data, UCR data can be quite useful (Skogan 1978). Lastly, Sparrow (2015) details how crime measures are often used as measures of police performance, and how organizational pressures often affect how crime is reported, whether it is reported at all, and how it is classified. Thus, based on using reliable data as a benchmark in order to assess racial disparities in police killings, researchers are better off using the census estimates as they are far better and more reliable than UCR counts. In fact, Cesario and his colleagues (2018) themselves use crowd-sourced data estimates of police-caused killings due to the unreliability of official counts.

I argue that benchmarking on crime rates is problematic for two main reasons: First, it ignores how racism shapes the very process of identifying disorder and defining criminality in the United States (Murakawa and Beckett 2010). In discussing the erasure of racism in the study and practice of punishment, Murakawa and Beckett (2010) describe how racism and criminal justice operate in systemic and interrelated ways, and by attempting to frame disparities in policing outcomes as a consequence of either disproportionate offending or disproportionate administration of justice erases the complexity in which race shapes social relations and processes (Murakawa and Beckett 2010). This is especially notable in crime control policy and has been thoroughly discussed by scholars studying mass incarceration (Mauer 2011; Tonry and Melewski 2008; Western and Pettit 2010).

Racial disparities in criminality can be understood as a function of criminal justice decision making at various points within the criminal justice system (Mauer 2011). For example, evidence suggests that "disparity is most pronounced at the beginning stages of involvement with the juvenile justice system. When racial/ethnic differences are found, they tend to accumulate as youth are processed through the system" (Poe-Yamagata and Jones 2000:1). Thus, disparities at the beginning states of involvement with the criminal justice system alone tend to be cumulative and widen at succeeding points (Mauer 2011). Over-policing and incarceration, in turn, can have adverse effects on the well-being of communities of color through a process of coercive mobility (Clear 2007). High rates of incarceration in poor communities destabilizes social networks, which in turn, undermines informal social control leading to more crime (ibid). Since researchers have indicated that high poverty areas are disproportionately inhabited by people of color, policing practices in these areas are essentially widening the net that traps people in a cycle of criminal sanctioning by the state (Clear 2007; Becket and Sasson 2004).

As Wacquant (2007) notes, American poverty is a racial poverty that is rooted in the ghetto as a specific social form and mechanism of racial domination that is systemic in nature. Attributing racial disparities in crime data as a function of over-policing or differential involvement in crime alone ignores the historical social and political processes that explain both the uneven distribution of violence, as well as the uneven distribution of "justice" (Murakawa and Becktt 2010; Alexander 2010). Often social and political processes that explain the uneven distribution of violence and justice seem race-neutral at face value yet lead to outcomes that affect minorities disproportionately. For example, advocates of "broken windows policing" have focused their attention on manifestation of "disorder," leading to a focus on policing relatively minor offenses in hopes of preventing more serious criminal activity. However, since "disorder" is constructed through a white lens, and "order maintenance" policing enables police to make stops and searches that would otherwise be legally dubious, people of color are essentially exposed to police scrutiny so long as they are in a public space (Neocleous 2000; Clear 2007; Fagan and Davies 2000; Murakawa and Beckett 2010).

Public perceptions of danger and policy constructions of the crime problem have historically been linked to perceptions and constructions of racial order (Beckett 1997; Murakawa 2008). Scholars have found that the perception of crime and disorder in neighborhoods are affected by the racial, ethnic, and class composition of the neighborhood (Sampson and Raudenbush 2004). Quillian and Pager (2001) find that the percentage of young black men living in neighborhoods has a strong effect on perceptions of the crime problem, even after accounting for crime rates and other relevant factors. Experimental studies also indicate that the cultural association of blacks with crime has enhanced support for more punitive anticrime measures (Gilliam and Iyengar 2000; Chiricos et al. 2014; Bobo and Thompson 2011). Thus, aside from any potential individual bias or discriminatory actions by police, understanding racism as the result of individual bias ignores "how racism shapes the very process of identifying and defining criminality" (Murakawa and Beckett 2010:710).

Secondly, arguing that crime itself is responsible for putting residents in situations where they are more likely to be killed by police will be arguing for a spurious relationship between crime and police-caused killings. Cesario and his colleagues (2018) argue that adjusting by population size only makes sense insofar as black and white residents are equally likely to occupy situations in which deadly force is used. As they state it "one cannot experience a policing outcome without exposure to police, and if exposure rates differ across groups, then the correct benchmark is on those exposure rates" (Cesario et al. 2018:2). What they fail to recognize is the essential role that discretion plays in policing (Bittner 1970) and how this discretion alone (which is subject to racial bias) is essential in producing what we come to understand as crime data (Neocleous 2000) and thus, racial disparities in the data (Kahn and Martin 2016).

Racial disparities in arrest data are not measures of differential criminal involvement, rather they reflect specific priorities in policy and policing practices. For example, policing policies such as stop and frisk, the war on drugs, or order maintenance policing that have targeted racial minorities disproportionately or policing practices (e.g., giving out warnings vs. arrests) can change the scope and nature of the crime problem (Ferrandino 2015; Kahn and Martin 2016; Glaser 2014; Tonry 1995; Alexander 2010). Biased policing, which occurs throughout the entire criminal justice system, is an integral component of disproportionate minority contact (Armour and Hammond 2009; Hanes 2012; Kempf-Leaonard 2007; Piquero 2008; Thonberry 2009).

For example, in explaining racial differences in incarceration for violent crimes, Blumstein finds that 80-90 percent of race differences in imprisonment are explained by disproportionate arrest patterns (1982). Tonry (1995) argues that the bulk of the increases in arrests for drug offenses during the war on drug was attributed not by an increase in drug crime, but a focus in arresting drug offenders (1995). There is ample evidence that race profoundly shapes the likelihood of arrest and prosecution for an array of criminal offenses, particularly offenses in which police have ample discretion (Beckett, Nyrop, Pfingst and Bowen 2005; Becket, Nyrop, and Pfingst 2006; Blumstein 1993; Tonry 1995). Biased policing and proactive street policing of minor offenses, which disproportionately affect racial minorities lead to disparities across the entire criminal justice system (Kahn and Martin 2016). Thus, by ignoring clear differences in policing practices that are responsible for racial disparities in all sorts of crime data, and attributing these differences to differences in criminal offending, Cesario and his colleagues (2018) and others like Selby (2017) ignore the confounding effect disproportionate policing of minorities have on the relationship between crime and police-involved killings.

Although they do not claim causality, the underlying implication of their argument is simple to follow. It is the behavior of residents that exposes them to the police, not discriminatory policing practices more generally. Such a logic ignores the fact that policing practices could explain the effect crime may have on racial disparities in police-caused killings. Figure 6-1 illustrates the spurious nature of this relationship and how over-policing minority neighborhoods is related to race-specific crime data and racial disparities in police-caused killings. As shown in this figure, over-policing influences both race-specific crime (measured by race specific arrest data) as well as racial disparities in police-caused killings. In making their argument, Cesario and his colleagues also seem to over-simplify the role of discretion in policing practices as well as the potential for use of force in interactions between police and civilians.

Cesario and his colleagues argue that murder/nonnegligent manslaughter, violent crime, and weapons violations are three categories of crime that are appropriate benchmarks in assessing racial disparities in police-caused killings because they represent situations during which police may be more likely to use deadly force (Cesario et al. 2018). This assumes that police use deadly force only in dangerous situations in which force is required. It is true, that in a significant number of police-caused killings, the police respond to dangerous situations in which the offender is attempting to inflict harm or has already done so. As such, one could expect that police would be more fearful for their safety as well as the safety of others and employ force. However, this does not reflect exposure to police accurately, as most interactions between police and civilians (measured by arrests) are not for violent crimes and many arrests for violent crimes do not result in the police killing anyone. Table 6-1 presents the total number of arrests nationwide for 2015, 2016, and 2017 as well as a breakdown by crime category. Murder and non-negligent manslaughter comprise less than 1 percent of total arrests, arrests for violent crime offenses comprise less than 5 percent and arrests for weapons violations comprise less than 2 percent of total arrests. In 2015, 2016, and 2017 most interactions between police and civilians result in arrests for other offenses (29.81 percent, 30.53 percent, 31.17 percent), drug abuse violations (13.79 percent, 14.75 percent, 15.47 percent) and property crime (13.55 percent, 12.69 percent, 11.84 percent). Thus, most of the contact between police and residents is based on calls for services and or concerns about property and order, not violence more broadly, so the "at risk" denominator should not be the number of violent crimes.

As Bittner (1970) has argued, the discretion to use force is present in all interactions between police and the public regardless of what has caused this interaction. The capacity to use force is not only present in all interactions but is almost always justifiable. Force is a tool that is essential to policing practices thorough police agencies in the United States, and not specific to a context or crime. Thus, when police officers fail to achieve compliance by other methods, the go-to tool to achieve compliance is that of force. It is the possibility and legal authority to use force and exercise discretion that guides all police-resident interactions. Neocleous (2000:99) states "Individual police officers have the legal right and duty to enforce the law as they see fit, including whether to arrest, interrogate, and prosecute regardless of the orders of their superiors." Moreover, "…the exercise of police discretion defines who is deviant in any social context and how that deviance is controlled. Some laws may be enforced more strictly against some groups than others, while at other times certain techniques of maintaining order will be utilized for different groups." (Neoucleous 2000:99-100). Thus, not understanding the role of discretion as a fundamental feature of policing in general obfuscates the relationship between crime and use of force more generally.

In fact, some of the most controversial police-caused killings started with routine police work and not with police officers responding to imminent danger. For example, in 2014 Eric Garner died after a police officer attempted to arrest him for selling loose, untaxed cigarettes on the streets in Staten Island. The officer choked Garner to death despite Garner pleading for his life and claiming "I cannot breathe" eleven times. Before the officer tackled Garner and placed him in a chokehold, video shows Garner with his hands in plain sight without any threatening actions toward the officer. However, the officer was not able to get Garner to comply with his explicit commands and decided to use force to get him to comply instead. In 2016, Philando Castile was killed during a traffic stop in Minnesota, because he had a "wide nose" like a robery suspect. Castile was legally armed at the time; he informed the police officer that he was armed, and before he could show the officer his documentation he was shot and killed in front of his girlfriend and 4-year-old daughter.

Willie McCoy was killed while he was sleeping in his car at a Taco Bell drive-thru in 2019. Employees of the restaurant called police officers in Vallejo, CA after they were unable to wake up McCoy. Even though McCoy was sleeping in his car, police fired 55 bullets in less than 4 seconds. In 2017, Walter Scott was stopped by police for a broken taillight in Charleston, SC. Fearing that he would get arrested over unpaid child-support, Scott attempted to flee by foot before the officer fired 8 shots as he was running away. Even after Scott was killed the officer placed him under arrest and claimed that Scott had taken control of his Taser and he feared for his life. Video evidence provided by a bystander showed the officer casually stopping and taking aim as Scott was running away. There are many more examples of cases in which a death resulted from a routine interaction between police and residents, but the point is that, unlike common perceptions of police-caused killings as resulting from non-compliance by dangerous criminals, these victims were not posing an imminent threat.

Of course, upon perusing accounts of police-caused killings, one can see plenty of situations in which police put their life on the line and had to kill a suspect because he or she was putting other residents or officers in imminent danger. This is part of the sometimes-dangerous job that police officers are tasked with, and one cannot expect officers to act differently in very dangerous situations. But, framing police-caused killing as only happening when suspects occupy dangerous situations as framed by Cesario and his colleagues (2018) is naive at best. As Bittner (1970) has argued, the possibility of force (and deadly force for that matter) is always present in any interaction between police and residents. Furthermore, because discretion is central to police work, two different residents engaging in the same behavior may end up with different interactions and outcomes with the police depending on how the police chooses to exercise that discretion. As such, if Cesario and his colleagues (2018) argue for race-specific crime as the appropriate benchmark for assessing racial disparities in police-caused killings, they should at least account for all arrests, instead of picking a specific type of arrest that they deem as representative of the context in which police-caused killings take place.

RESULTS

As stated above, I estimate county-level negative binomial regression models for each dependent variable using my predictors of interest, one at the time, and obtain fit statistics for each model in order to determine which is the best predictor of police-involve killings. Before jumping to these results, I assess whether racial disparities are apparent from benchmarking police-caused killings by using population estimates, as well as crime-specific arrest data. However, in benchmarking these deaths by crime-specific data, I look at the total number of arrests instead of focusing on *rates* of arrests for violent crime, which are population-adjusted.

Figure 6-2 shows the odds ratio of being fatally shot for blacks relative to whites benchmarked against each group's population proportion, as well as against each group's arrest proportion. These estimates are for the entire United States used three different sources (The Counted, Fatal Force and SHR). Reading from left to right there is evidence of racial disparities in police-caused killings using population estimates as well as arrest estimates as the benchmark. Using the data provided by The Counted in 2015 and 2016, blacks were 2.61 and 2.27 times as likely as whites to be killed by the police given their population proportion. When benchmarking against total arrests for each group, blacks were 1.38 and 1.20 times as likely as whites to be killed by police given their exposure to police (measured as the total number of arrests for each group). Looking at the data provided by the Fatal Force in 2015, 2016 and 2017 blacks were 2.58, 2.48 and 2.37 times as likely as whites to be killed by police given their population proportion. When benchmarking against total arrests for each group, blacks were 1.36, 1.30, and 1.23 times as likely as whites to be killed by police given their exposure to police.

Finally, I included the data provided by the SHR to demonstrate how inaccurate "official" statistics can be, especially when attempting to assess whether racial disparities are present in the number of people killed by police. Cesario and his colleagues (2018) do not use SHR estimates as it is widely understood that police-caused killings in these data are underreported (Davis and Lowery 2015; Klinger, Rosenfeld, Isom, and Deckard 2016; Nix et al. 2017; white 2016). The under-reporting is especially of concern when looking at race-specific data. For example, when comparing data provided by the SHR against data provided by Fatal Force (used in this chapter), Fatal Force reports 11 times as many deaths as SHR estimates do (715 vs 64). Clearly, there is reason for concern when looking at estimates that are submitted voluntarily. But, if we were to use these data, we see that there is evidence of anti-white bias in police killings. Using SHR estimates for 2015, 2016 and 2017 we see that blacks are .66, .60 and .55 times as likely as whites (meaning less likely) to be killed by police given their population proportion. Moreover, when benchmarking against arrest data, blacks are .35, .32 and .29 times as likely as whites to be killed by police given their exposure to police.

By engaging in this simple analysis of whether racial bias exists by benchmarking against population and crime data for different estimates of police-caused killings, the implications of arbitrary decisions on benchmarking are obvious. If all arrests are used instead of only using arrests for violent crimes, murder/non-negligent manslaughter and weapons violations as a measure of exposure to police, there is still evidence that blacks are disproportionately killed by police. Moreover, by using SHR estimates we see the opposite, which I argue should be cause for concern, given the implications of such analyses. This also calls into question the validity of using arrests as a benchmark altogether.

Table 6-2 shows results for the negative binomial regression models used to predict the expected count of police-caused killings. The first column (all deaths) shows that the best predictor of the expected count of all police-caused killings at the county level is the overall population based on an AIC of 5,881, which was the lowest of all 35 candidate models. The second-best predictor is simple assault based on an AIC of 6,112. This initial AIC difference of 232 provides strong evidence that population is a better predictor when compared against simple assault. The third best predictor is arrests for weapons violations, with an AIC of 6,117. The AIC difference, again, provides strong evidence that population is a better predictor when compared against arrests for weapon violations. The relative likelihood estimates (or AIC weights), provide the same evidence, that is, the likelihood that model 2 or model 3 provide the best K-L model compared to the first model is essentially zero. While a "best predictor" and a benchmark do not serve the same purpose, given that it is impossible to accurately define the at-risk population, population seems to capture the greater risk of being killed by police.

Column 2 (white deaths) shows that the best predictor of the expected count of police-caused killings of white people at the county-level is the size of the white population, based on an AIC of 4,430, which was the lowest of all 33 candidate models. The second and third best predictors are larceny and simple assault arrests for whites with AIC differences of 190 and 237, respectively. This indicates that the second and third model provide essentially no evidence that the models with crime as predictors are better models than the white population model. The relative likelihood estimates provide the same information, that is, the likelihood that these predictors provide the best estimates is essentially zero when compared to the best model.

Lastly, Column 3 (black deaths) shows that the best predictor of the expected count of police-caused deaths of black residents at the county-level is the size of the black population, based on an AIC of 2,253. The second and third best predictors based on AIC are black-specific larceny arrests and simple assault arrests. These models show AIC differences of 277 and 272 respectively. The relative likelihood estimates are also essentially zero, indicating that the likelihood that using larceny or simple assault arrests of blacks as predictors provides a better fitting model than using the black population is essentially zero.

CONCLUSION

Overall, I conclude that the best predictor of the expected count of all people killed by police at the county level is the overall county population, followed by simple assault and weapon violations arrests based on AIC differences and AIC weights. More importantly for my purposes, the best predictors of the expected counts of white and black deaths at the metropolitan area level are white and black population counts, respectively. These results counter arguments by some researchers and pundits who argue that violent crime is the best predictor of who may be killed by police (Cesario et al. 2018; Selby 2017). As my analyses show, when choosing between population adjustment or crime adjustment when comparing race differences in police-caused killings, population adjustment is the best choice, since we do not know what the true at-risk population is. Additionally, in both white and black models it is larceny and simple assaults, not violent crime that better predicts the race-specific count of police-caused killings at the county level. This is not to say that crime should not be accounted for in multivariate models of police-caused killing, but these results call into question the assertion that it is inappropriate to adjust for sub-population size differences when examining police-caused killings by race.

Chapter 7

SUMMARY AND CONCLUSIONS

The purpose of this dissertation is to assess the relationship between the size of the black and Latino population relative to whites and police-caused killings. In doing so, my goal is to provide a better understanding of the racial disparities that seem to dominate much of the discussion regarding police-caused killings and the possibility of racial prejudice in these killings. As I briefly indicated in the introduction and more thoroughly discussed in Chapter 2, I argue that most of the contradictory findings in studies of police-caused killings could be attributed to five modeling choices. First, most studies investigating the association between racial prejudice and police-caused killings are focused on the proximate causes of death, thus ignoring ultimate causes that may be operating at the societal level. That is, by limiting the scope of investigation to the event in it of itself, the processes leading up to the event, or processes that allow for such event to occur are often missed in current analyses. Moving away from analyses which tend to focus on officer characteristics (Ridgeway 2020) or features of police departments (Sherman 2020), my analyses focused on the distal causes of police-caused killings using measures of group threat theory.

Second, most analyses thus far focus on police-caused shootings (i.e. Holmes et al. 2018; Bejan et al. 2018), ignoring the myriad other ways people die at the hands of police officers across the nation. Moreover, analyses of police-caused killings using group threat theory as a framework for understanding these killings rely on official data sources (c.f. Liska and Yu 1992; Smith 2003, 2004; Holmes et al. 2018), thus ignoring a clear problem of undercounting by these data-sources (Planty et al. 2015). Adding to the issue of undercounting, the common approach to using these data is to use multiple years of data to reduce random variation, which inherently compounds any issues related to systematic under-reporting, or lack of reporting altogether.

Third, most studies evaluating whether racial bias is present in police-caused killings are geographically limited. That is, most studies look at a handful of police departments (i.e. Fryer 2019; Klinger 2016), the nation as a whole (Cesario et al. 2018 Shane et al. 2017), or large cities (Holmes et al. 2018; Smith 2003, 2004) making comparisons between studies difficult. Only Ross (2015) uses all counties across the United States in his analysis and Willits and Nowacki (2014) look at various samples in their analysis of police-caused killings and departmental policies. My study followed their approach by using metropolitan areas and counties across the nation in order to test the consistency of findings and assess whether urban and rural differences exists in police-caused killings.

Fourth, researchers have not, to date, investigated claims by Cesario and his colleagues (2018), Selby (2017) and Treggle and his colleagues (2019) that rates of police-caused killings should be adjusted by race specific crimes, rather than population estimates. I evaluate such claims by assessing whether it is better to benchmark police-caused killings on population or race-specific crime rates by looking at model information criteria that assesses overall model fit.

Fifth, researchers evaluating the relationship between group threat theory and police-caused killings have not included "threat triggers" in their analyses. The inclusion of "threat triggers" allows me to assess if increases in the size of minority groups in historically white areas are associated with increases in police-caused killings. Studies evaluating disparities in sentencing (c.f. Wang and Mears 2010a;2010b) and disparities in misdemeanor arrests (Kane et al. 2013) suggest that threat effects are more pronounced in areas that are traditionally white. Similarly, like many researchers studying group threat in sentencing contexts (Wang and Mears 2015; Feldmeyer et al 2015) and responding to calls for multilevel models (Holmes, PainterII and Smith 2018) within the police-caused killing literature, I test whether county level measures of threat affect individual-level measures of bias.

In all, this dissertation seeks to thoroughly assess group threat theory and its relationship to police-caused killings using better data, multiple levels of aggregation, and different methodological choices in hopes of providing a better understanding on the distal causes that may be associated with the racial disparities in police-caused killings. In order to do so, several research questions were posed in the introductory chapter. These questions, along with the associated findings are presented below.

Question 1a. Do minority threat, economic competition, or power threat models better fit the relationship between population and police-caused killings in metropolitan areas?

In specifying the relationship between minority population size, minority population growth, and their relationship to discriminatory social control efforts, Blalock (1967) proposes three distinct empirical models. Economic competition models refer to a log-linear function as an explanation of discrimination. That is, the size and growth of the minority population relative to whites will be linearly associated with increases in discriminatory outcomes, but once a tipping point is reached, discriminatory outcomes will increase at a decreasing rate. Minority threat models refer to a linear function as an explanation of discrimination in which "The greater the number of acts and people threatening to the interests of the powerful, the greater the level of deviance and crime control" (Liska and Yu 1992:18). Lastly, power threat models refer to a quadratic function as an appropriate means of modeling discrimination. That is, the relationship between minority group size and social control efforts will display an initial positive parabolic (quadratic) relationship, which eventually will turn negative as the percent of minorities reaches a level where minorities can wield political power (Blalock 1967).

Based on model information criteria, economic competition models provide the best fitting models between minority size and police-caused killings. This finding holds for all, minority, black, Latino and white killings. Such findings are consistent with other studies testing economic competition models (Eitle et al. 2002) and its effect on black-to white arrest ratios. Overall, the analysis shows that the best fitting models are, in order, economic competition, power threat, and minority threat models. The AIC scores are consistently lower for economic competition models in predicting all, minority, black, Latino and white killings (see figure 4-4). To date, this is the only study that has tested all three variants of group threat theory in the context of police-caused killings. This implication of this finding is that researchers who are currently finding no "threat" effect may be stymied by improper model specification.

Question 1b. What is the relationship between economic competition and police-caused killings in metropolitan areas?

Overall, negative binomial regression models show that after controlling for population size, violent crime rates, income, police-per capita and regional effects, the size of the black population relative to the size of the white population is significantly associated with increases in the expected count of police-caused killings of blacks (a one standard deviation increase in relative population is associated with a seven percent increase in the expected count of police killing of black people). Similarly, holding everything else constant, the size of the Latino population relative to the size of the white population is significantly associated with increases in the expected count of police-caused killings of Latinos (a one standard deviation increase in relative population is associated with a 92 percent increase in the expected count of police killing of Latinos). The size of the black and Latino population relative to whites is unrelated to the killing of all, all minorities, and whites more generally. These findings are consistent with the group threat hypothesis, which states that large or growing minority populations will be associated with formal social control efforts to control these populations (Blalock 1967).

Another interesting finding in this analysis is the lack of significant results between violent crime rates and police-caused killings of blacks. However, the violent crime rate is significantly and positively associated with police-caused killings of all, minorities, Latinos, and whites more generally. Such findings are in direct contradiction to analyses claiming that violent crime is the best indicator of who police may come to kill in the line of duty (c.f. Cessario et al. 2018; Treggle et al. 2019; Fyfe 1980). Insofar as violent crime influences the expected count of police-caused killings, it does not seem to influence the expected count of police-caused killings of blacks, meaning that police may be especially vigilant (and, therefore, quick to pull the trigger) in white communities that they consider dangerous, but all black communities may fall under that appellation.

Measures of black and Latino income relative to whites are unrelated to the expected count of police-caused killings in all models. Moreover, measures of black-white exposure are significantly associated with increases in the expected count of police-caused killings of whites (a one standard deviation increase is associated with a 4 percent increase in the expected count of killings of whites). My findings may differ from prior studies (c.f. Smith and Holmes 2014; Holmes et al. 2018), because I use an index of exposure as a measure of segregation instead of the commonly used index of dissimilarity. These measures differ in that exposure is intended to measure the degree of potential contact, or possibility of interaction, between minority and majority group members, while the index of dissimilarity measures how evenly distributed minority and majority group members are. Insofar as higher levels of black-white exposure are associated with higher counts of police-caused killing of whites, it could be that this measure is also capturing levels of poverty, which have been associated with higher levels of crime and dangerousness (Turk 1969). For example, black-white exposure is highest in the Parkersburg, WV-OH metropolitan area where the poverty rate is around 17 percent, much higher than the national average of 13 percent (Census 2015). Moreover, it could be that in such places, there is just a majority of white residents, thus elevating the potential risk of white people being killed by police.

Overall, my findings suggest that economic competition is useful in helping researchers understand the killing of black and Latino residents at the hands of police. However, my findings also suggest that it is the mere presence of Latinos and blacks, rather than actual competition (as measured by income disparities) or contact (as measured by residential segregation) that drives racial domination outcomes.

Question 1c. Do dynamic measures of population better predict police-caused killings than static measures in metropolitan areas?

No, dynamic measures of population are unrelated to the expected count of police-caused killings. Prior researchers have argued that in evaluating the relationship between minority population sizes and social control outcomes researchers may not be accurately capturing the threat process if they do not include measures of population change (Chamlin 1989; Jacobs and O'Brien 1998). In this regard, dynamic measures of threat refer to changes in the black and Latino population. Blalock (1967) theorized that the relative size of the minority population, as well as growing minority populations would be related to increases in discriminatory behaviors. Although some have found support for this argument (c.f. Chamlin 1989; Jacobs and O'Brien 1998; Caravelis et al. 2011), my results do not point to a significant relationship between minority population growth (5-year growth or 15-year growth) and police-caused killings at the metropolitan area level.

To the extent that police-caused killing is the result of white community responses to minority presence by increasing pressure on police to surveil minority neighborhoods, my findings suggest that these white communities are responding to the presence of minorities now, rather than fears about a growing presence.

Question 1d. Do the models better predict the expected count of all killings, minority killings, black killings, Latino killings, or white killings?

Although measures of group threat are only related to the expected count of police-caused killings of blacks and Latinos, seemingly unrelated regression techniques do not show a statistically significant difference between coefficients from one model to another. That is, I am not able to conclusively state that group threat variables are statistically different from one model to the other. However, as stated earlier, the results do point that it is only in predicting police-caused killings of blacks and Latinos that measures of threat are statistically significant. Thus, although I am unable to determine whether these predictors are better able to explain the overall or race-specific count of police-caused killings, the size of the black and Latino population relative to the white population is only significant in the black and Latino models. Such findings are consistent with group threat theory, in that an increase in the size of threatening populations is associated with formal social control efforts directed at threatening populations (Liska and Yu 1992; Turk 1969; Holmes et al. 2018).

Question 2a. What is the relationship between threat, threat triggers, and police-caused killings at the county-level?

Like the analysis in chapter four, I assess the relationship between group threat and police-caused killings, but this time at the county level. I also incorporate threat triggers into the analysis to assess whether police-caused killings are more prevalent in counties that went from a majority of white residents to majority of non-white residents. Overall, I find that the size of the black population relative to the white population is associated with increases in the expected count of black killings (a one standard deviation increase in the relative population is associated with a 475 percent increase in the expected killing of black residents), and decreases in the expected count of all killings (a one standard deviation increase in relative population is associated with a 214 percent decrease in the expected count of all killings), and white killings (a one standard deviation increase in relative population is associated with a 216 percent decrease in the expected count of white killings).

I also find that the Latino population relative to the white population in counties is associated with increases in the expected count of Latino killings (a one standard deviation increase in the relative population is associated with a 998 percent increase in the expected killing of Latino residents), and decreases in the expected count of all killings (a one standard deviation increase in the relative population is associated with a 235 percent decrease in the expected count of all killings), black killings (a one standard deviation increase in the relative population is associated with a 214 percent decrease in the expected count of black killings), and white killings (a one standard deviation increase in the relative population is associated with a 221 percent decrease in the expected count of white killings).

The income of black and Latinos relative to whites is not associated with the expected count of killings in any models. Moreover, the black-white index of exposure is associated with decreases in the expected count of all killings (a one standard deviation increase in exposure is associated with a 192 percent decrease in the expected count of all killings), and minority killings (a one standard deviation increase in exposure is associated with a 166 percent decrease in the expected count of minority killings). However, the black-white index of exposure is associated with an increase in the expected count of Latino killings (a one standard deviation increase in exposure is associated with an 820 percent increase in the expected count of Latino killings).

Moreover, in assessing whether there are urban-rural differences in the expected count of killings I find that metropolitan counties have higher expected count of minority killings (a one standard deviation is associated with a 370 percent increase in the expected count of minority killings). This finding is consistent with group threat theory, as current demographic trends indicate that although the U.S. population remains majority white, whites only comprise about 44 percent of the population in urban counties (Parker et al. 2018). Thus, as group threat indicates, we would expect higher expected count of killings in areas where the minority population is larger than the white population. Lastly, the threat trigger indicator was not significant in any analysis.

Question 2b. Do threat triggers moderate the relationship between threat and police-caused killings at the county level?

No, threat triggers do not have any independent effect on the expected count of police-caused killings in any model, thus cannot moderate the relationship between threat and police-caused killings. It is possible that a lack of significance can be related to the value used in the in determining the cut-off for determining whether a county went from majority white to minority white. In determining the appropriate threshold, I followed the approach taken by Kane and his colleagues (2013) in their study of misdemeanor arrests in Washington, DC. In their analysis, they found that increases in the percent black population were associated with increased black misdemeanor arrests, but only in historically white census tracts. It could also be that analyses at the county level are not appropriate for testing such effect as it is a much larger unit of analysis. Alternatively, threat triggers may not matter.

Question 2c. Among people who were shot and killed, were black and Latino people less likely than white people to have been attacking police officers or others when they were shot and killed?

My results indicate that Latinos were 1.2 times more likely than whites to have not been attacking an officer or person before they were shot and killed. This finding suggests that Latinos have higher odds than whites of dying from a threat perception failure on the part of police officers. However, my results do not suggest that blacks are more likely than whites to be shot and killed while not attacking an officer or person.

A surprising result from this analysis is that the use of a body-camera is associated with higher odds of people getting shot due to an apparent threat perception failure. Such findings run counter to arguments pointing to cameras as a potential deterrence of lethal force (c.f. Kahn and Davies 2016). Like Nix and his colleagues (2017), my analysis indicates that people showing signs of mental illness were more likely to die from a threat perception failure. Lastly, unlike previous studies (Nix et al. 2017) I find regional differences in the odds of dying from threat perception failure. People in the South and the Midwest had lower odds (24 percent and 21 percent respectively) of being killed than civilians in the Southwest.

Question 2d. Among people who were shot and killed, were black and Latino people more likely to have been unarmed than white people when they were shot and killed?

I find that among those who were shot and killed by police, blacks were 1.4 times as likely as whites to have been unarmed, thus indicating evidence of bias in the shooting of people by police. The results indicate that Latinos are not more likely than whites to have been unarmed when they were shot and killed by police.

Question 2e. What is the relationship between threat at the county-level and the likelihood of being shot and killed while unarmed?

Responding to calls for multilevel analysis of minority threat within the police-caused killing literature (c.f. Holmes et al. 2018) I assess whether county-level measures of threat can predict indicators of racial bias. There does not seem to be a significant relationship between threat at the county level and the likelihood of being shot and killed while unarmed. Thus, despite finding evidence of bias in previous analysis, this bias is not explained by levels of threat at the county level. That is, county-level measures of group threat are not significant in predicting individual measures of bias.

Question 2f. What is the relationship between threat at the county-level and the likelihood of being shot and killed while not attacking police officers or others?

There does not seem to be a relationship between county-level measures of threat and the likelihood of being shot and killed while not attacking police officer or others. None of the county-level predictors were significant in predicting two indicators of bias. It could be that county-level measures of threat are un-related to measures of bias, or that more detailed data is needed.

Question 3a. Are police-caused killings better predicted by population counts or race-specific crime estimates?

In responding to claims made by previous researchers (c.f. Cesario et al. 2019; Treggle et al. 2019) indicating that population counts are the improper benchmark by which to assess whether there is evidence of implicit bias, I assess whether population counts provide a better model fit than race-specific arrest data in predicting police-caused killings. My analysis shows that overall population is the best predictor of police-caused killings based on AIC. Moreover, white population and black population seem to be the best predictors of police-caused killings of whites and blacks. In all, race specific population counts seem to provide the best fitting models in predicting race-specific counts of police-caused killings for whites and blacks. These models are followed by race specific larceny, and simple assault arrests.

Question 3b. Is there evidence of racial disparities in police-caused killings when benchmarking on race-specific crime counts as opposed to population?

In assessing whether there is evidence of racial disparities in police-caused killings, researchers (c.f. Cesario et al. 2019; Treggle et al. 2019) have claimed that after benchmarking police-caused killings on race-specific crime data, any evidence of racial disparities disappear. In benchmarking police-caused killings on race-specific crime data, as opposed to population, researchers have argued that using crime data provides a more reasonable benchmark than population, given that not all people are likely to occupy situations in which police may shoot them (Cesario et al. 2018). However, in selecting which crime counts to use, they focus on weapons violations, murder/non-negligent manslaughter, and violent crime, arguing that such crime data represents the true population at risk of getting killed by police. Although it is true that police kill people in order to stop them from hurting themselves or others, police also kill people who are not assaulting, murdering, or shooting at people. They also do not kill some people who are a real threat. Thus, I evaluate whether there is evidence of racial disparities in police-caused killings after benchmarking police-caused killings on race-specific crime data for all offenses, instead of violent offenses.

In benchmarking police-caused killings against population and race-specific crime data, I see a clear pattern of racial disparities with blacks being anywhere from 1.2 to 1.37 times as likely as whites to be killed by police-given their overall arrest rates. Moreover, racial disparities in police-caused killings seem to be lower when benchmarking on race-specific crime rates, than when benchmarking police-caused killings on population counts (blacks are anywhere from 2.2 to 2.6 times as likely as whites to get killed by police given their population proportions). Additionally, it is worth noting that the only instance in which evidence of racial disparities is not present in police-caused killings is when using data from the SHR. Although Cesario and his colleagues (2018) used data from The Counted for 2015 and 2016 in their analysis, and Treggle and his colleagues (2019) used data from *The Washington Post* for 2015, 2016, and 2017, I compare racial disparities using the same data used in both studies, as well as data from the SHR.

In sum, whether racial disparities are assessed by benchmarking police-caused killings on population estimates or race-specific crime estimates, there is consistent evidence of racial disparities in police-caused killings when looking at all arrests rather than cherry-picking types of crime that are less predictive of killing.

IMPLICATIONS OF THIS STUDY

This dissertation makes several noteworthy contributions toward gaining a deeper understanding of racial disparities in police-caused killings. In addressing some of the major limitations of previous studies, this dissertation finds that the size of the black and Latino population relative to whites is consistently associated with a higher expected count of police-caused killings of blacks and Latinos at the metropolitan and county level. Moreover, this dissertation finds that an increase in the size of the black and Latino population relative to whites across US counties is associated with decreases in the expected count of police-caused killings of all, and white people. I find that regional differences exist in the expected count of police-caused killings across metropolitan areas, and counties. Metropolitan areas in the Northeast have lower expected count of police-caused killings of all, minority and whites (relative to the Southwest), and metropolitan areas in the Midwest have lower expected count of all killings (relative to the southwest). Moreover, counties in the South (relative to the Southwest) have lower expected killings of minorities and Latinos. Counties in the Northeast have lower expected killings across all models, and counties in the Midwest have lower expected killings across all models except for models predicting black deaths. These regional differences are consistent with previous studies of police-caused killings (c.f. Smith 2003,2004; Willits and Nowacki 2014; Holmes et al. 2018).

Like previous research (Nix et al. 2017) I find clear evidence of bias in police-caused killings using data from 2015 to 2018. That is, among people who were shot and killed by police during this time period, Latinos were 1.26 times as likely as whites to have not been attacking police officers or other people prior to getting killed. Similarly, blacks were 1.38 times as likely as whites to have been unarmed prior to getting killed by police. These findings, like those presented by (Nix et al. 2017) provide further evidence of racial bias in police-caused killings.

In evaluating the relationship between population and police-caused killings, I provide clear evidence that the size of the black and Latino population relative to the white population matters when predicting police-caused killings of black and Latino residents regardless of the data source used. However, as shown in chapter 4 and chapter 6, official estimates of killings provided by the SHR severely undercount the overall incidence of police-caused killings as well as the race specific incidence of police-caused killings. The differences in estimates by data source used are especially concerning when attempting to assess whether racial disparities exists in police-caused killings given population estimates as well as race-specific crime estimates. As shown in chapter 6, there are clear patterns of racial disparities in police-caused killings when benchmarking estimates provided by The *Washington Post* and *The Guardian*-on population estimates as well as race-specific arrest data. However, these racial disparities disappear when using estimates provided by the SHR. Clearly, official estimates of police-caused killings provided by the SHR do not capture the total number of police-caused killings. Until the SHR can provide more reasonable estimates of police-caused killings, these data should not be used.

Group Threat Theory

Overall, this study makes several noteworthy contributions to research on racial threat and police-caused killings. First, my analyses incorporate all conceptual models as described by Blalock (1967) by fitting linear, logged, and quadratic function of threat. Based on model information criteria, it seems clear that economic competition models fit the relationship between population and police-caused killings better than minority threat or power threat models. This has implications for how researchers may come to understand the relationship between threat and social control outcomes such as police-caused killings, arrests, and proactive policing more generally.

Second, in my analyses, I incorporate dynamic measures of threat that are better aligned with Blalock's (1967) concerns over the effect of a growing minority population on discriminatory behavior. Although no significant effects were found, I believe it is important to test for such effects as others (Jacobs and O'Brien 1998) have found a significant relationship between minority population growth and police-caused killings. It is possible that the overall size of the minority population (not its growth) is related to the police-caused killings; and that regardless of fluctuations in the size of the minority population, minority populations threaten the racial hierarchy in society.

Third, I use several novel measures of racial threat that have not been used within the context of police-caused killings. That is, I measure threat using several black to white and Latino to white ratios in measuring population and income differentials. Moreover, I introduce different measures of residential segregation that account for minority-majority interactions. To date, this is the first study that has used such measures in the context of police-caused killings. It could be that by using such measures and the correct model specification (economic competition) I find significant results between measures of group threat and police-caused killings of blacks and Latinos when recent studies have failed to find a significant relationship (c.f. Holmes et al. 2018).

Moreover, this is the first study of group threat theory that accounts for the effect of segregation on police-caused killings by including measures of exposure for blacks and Latinos instead of commonly used measures of evenness (c.f. Liska and Yu 1992; Holmes et al. 2018). Although previous analyses have evaluated the relationship between segregation and police-caused killings (c.f. Liska and Yu 1992; Holmes et al. 2018), to date measures of exposure, have been absent from analysis of police-caused killings thus far. My analysis finds that black-white exposure is related to an increase in the expected count of white killings, which is counter to what I would expect under group threat theory. However, this relationship needs to be studied further in order to fully understand why higher levels of black-white exposure are associated with greater expected counts of police-caused killings.

Lastly, this study used several data sources in order to assess whether using different data-sources would influence the conclusions reached when empirically testing group threat theory in police-caused killings. In all, my analyses are supportive of the group threat hypothesis. That is, the size of the black and Latino population relative to the white population is consistently associated with higher counts of police-caused killings of blacks and Latinos. This effect remains whether testing group threat at the metropolitan area level, as well as the county level.

Implications for Public Policy

This dissertation has several implications for public policy. First, like many researchers have previously stated (Nix et al. 2017; Planty et al. 2015; Ross 2015), the US government needs to act swiftly and establish a reliable and timely account of police-caused killings. The severe undercounting of police-caused killings by the government is problematic, and certainly does not help their legitimacy in communities of color which is foundational feature of effective and just policing (Peyton, Sierra-Arévalo, and Rand 2019). In fact, without police-legitimacy, residents of communities are less likely to call the cops (Carr, Napolitano, and Keating 2007) report crimes (Desmond Papachristos, and Kirk 2016) and support their local law enforcement agencies (Sunshine and Tyler 2003). Without legitimacy police-public interactions are more likely to escalate into contests for dominance and result in injury or deaths of police and citizens alike (Peyton et al. 2019). As former FBI director James Comey stated "It is unacceptable that The Washington Post and The Guardian newspaper from the U.K. are becoming the lead source of information about violent encounters between police and civilians. That is not good for anybody (Davis and Lowery 2015: Online)."

For majority-minority group relations to improve, minority presence needs to be normalized, a difficult task normally, and harder still in such polarizing times. However, research has pointed to some promising ways of doing so. For example, Pettigrew and Tropp (2006) find that intergroup contact can reduce prejudice. Moreover, Park and Glaser (2011) find that after police officers performed modified police-shooting simulations they tend to demonstrate less bias on subsequent simulations. Lastly, procedural justice training in police departments could be a fruitful avenue of intervention as researchers have consistently pointed to an increase in legitimacy and trust between police and the public (Wolfe, Nix, Kaminski and Rojek 2016).

Moreover, as this analysis has shown, it is important to focus on the distal causes of police-caused killings that may influence the overall, as well as the race-specific incidence of police-caused killings. Doing so requires us to re-think the role of police in society before proposing policy interventions aimed at reducing state sanctioned violence. Many have demonstrated how the origins and functions of police agencies across the nation have been intrinsically tied to the management and reproduction of racial and class inequalities, leaving the poor, socially marginal and non-white populations at risk of state sanctioned violence (Alexander 2010; Vitale 2017). Thus, policy proposals aimed at increasing legitimacy and procedural justice will do just that, without having a meaningful effect on the overall, as well as race specific killing of US residents. As Neocleous (2000) argues, the central role of police is to "fabricate social order", an order which, relies on systems of exploitation that benefit those in control of political and economic power structures. We must re-imagine what social order looks like and how we achieve it.

Vitale (2017) provides a thorough discussion on how the expansion of police authority in the last forty years is inconsistent with goals of community empowerment, social justice and public safety, and has, in fact exacerbated the problems this increased authority was supposed to address. Thus, it may be time to not only re-imagine police authority, but to curtail police's authority in ways that prevent them from disproportionately targeting communities of color. For example, under the guise of "Broken windows" policing and policies such as "The War on Drugs" and "Stop and Frisk" police have been responsible for the criminalization and over-policing of communities of color, which in turn, has resulted in disproportionate arrests and incarceration rates for people in these communities (Alexander 2010; Tonry 1995; Clear 2007).

Issues of drug addiction, homelessness and joblessness more broadly cannot be managed by the police and should instead be addressed by professionals and social workers. Thus, policy interventions aimed at reducing racial disparities in police-caused killings should focus on limiting activities that often affect minorities disproportionately. As such, decriminalization or de-prioritizing enforcement of offenses that do not threaten the public safety but often fall under quality of life issues (i.e. consumption of alcohol on streets, simple possession of drugs, loitering, spitting, jaywalking, among others) can prove useful in reducing racial disparities in state sanctioned violence and should be considered. However, police officers, administrators, and politicians must realize that in order to reduce disparities in police-caused killings, and improve police-community relations, a multi-faceted approach grounded on re-imagining the role of the police is needed. There are no simple solutions to the racial disparities observed in police-caused killings and the legitimacy crisis that police departments face after controversial killings.

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APPENDIX

Table 3- 1. Total Number of Killings Reported and Used by The Counted in 2015 and 2016

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
Total Reported, 2015	1,146	562	307	195	584
Total Reported, 2016	1,093	519	266	183	574
Total Used, 2015	862	486	250	174	376
Total Used, 2016	803	418	208	162	385
Percent of cases used, 2015	75.22%	86.48%	81.43%	89.23%	64.38%
Percent of cases used, 2016	73.47%	80.54%	78.20%	88.52%	67.07%

Table 3- 2. Total Number of Killings Reported and Used by Fatal Force in 2015, 2016 and 2017

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
Total Reported, 2015	994	497	258	172	497
Total Reported, 2016	962	497	234	160	465
Total Reported, 2017	986	527	223	179	459
Total Used, 2015	975	487	250	170	488
Total Used, 2016	940	483	222	160	457
Total Used, 2017	971	517	214	178	454
Percent of cases used, 2015	98.09%	97.99%	96.90%	98.84%	98.19%
Percent of cases used, 2016	97.71%	97.18%	94.87%	100.00%	98.28%
Percent of cases used, 2017	98.48%	98.10%	95.96%	99.44%	98.91%

Table 3- 3. Total Number of Killings Reported and Used by the SHR in 2015 and 2016

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
Total Reported, 2015	452	273	24	57	179
Total Reported, 2016	439	277	20	56	162
Total Used, 2015	393	231	23	56	162
Total Used, 2016	355	225	18	54	130
Percent of cases used, 2015	86.95%	84.62%	95.83%	98.25%	90.50%
Percent of cases used, 2016	80.87%	81.23%	90.00%	96.43%	80.25%

Table 3- 4. Total Number of Killings Reported and Used by the SHR in 2015, 2016 and 2017

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
Total Reported, 2015	452	273	24	57	179
Total Reported, 2016	439	277	20	56	162
Total Reported, 2017	428	252	20	54	176
Total Used, 2015	393	231	23	56	162
Total Used, 2016	355	225	18	54	130
Total Used, 2017	349	193	16	49	156
Percent of cases used, 2015	86.95%	84.62%	95.83%	98.25%	90.50%
Percent of cases used, 2016	80.87%	81.23%	90.00%	96.43%	80.25%
Percent of cases used, 2017	81.54%	76.59%	80.00%	90.74%	88.64%

Table 3- 5. Reported Deaths by Source

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
The Counted, 2015	1146	562	307	195	584
The Counted, 2016	1093	519	266	183	574
Fatal Force, 2015	994	497	258	172	497
Fatal Force, 2016	962	497	234	160	465
Fatal Force, 2017	986	527	223	179	459
Supplemental Homicide Report, 2015	452	273	24	57	179
Supplemental Homicide Report, 2016	439	277	20	56	162
Supplemental Homicide Report, 2017	428	252	20	54	176

Table 3- 6. Rate of Killings Per-million Residents by Source

	All Deaths	Black Deaths	White Deaths	Total Population	Total Black Population	Total White Population	Total rate per million	Black rate per million	White rate per million	Black-White ratio
The Counted, 2015	1,146	307	584	320,742,673	39,873,263	197,793,554	3.57	7.70	2.95	2.61
The Counted, 2016	1,093	266	574	323,071,342	40,243,218	197,793,943	3.38	6.61	2.90	2.28
Fatal Force, 2015	994	258	497	320,742,673	39,873,263	197,793,554	3.10	6.47	2.51	2.58
Fatal Force, 2016	962	234	465	323,071,342	40,243,218	197,793,943	2.98	5.81	2.35	2.47
Fatal Force, 2017	986	223	459	325,147,121	40,580,268	197,698,793	3.03	5.50	2.32	2.37
Supplemental Homicide Report, 2015	452	24	179	320,742,673	39,873,263	197,793,554	1.41	0.60	0.90	0.67
Supplemental Homicide Report, 2016	439	20	162	323,071,342	40,243,218	197,793,943	1.36	0.50	0.82	0.61
Supplemental Homicide Report, 2017	428	20	176	325,147,121	40,580,268	197,698,793	1.32	0.49	0.89	0.55

Table 3- 7. Part I and Part II Offense Description

Part I Offenses	
Murder and Non-negligent manslaughter	The willful killing of one person by another. Deaths caused by negligence, attempts to kill, assaults to kill, suicides, and accidental deaths are excluded.
Manslaughter	The Killing of another person through gross negligence. Deaths of persons due to their own negligence, accidental deaths and traffic fatalities are not included.
Forcible Rape	The penetration, no matter how slight, of the vagina or anus with any body part or object, or oral penetration by a sex organ of another person, without the consent of the victim.
Robbery	The taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear.
Aggravated Assault	An unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. Simple assaults are excluded.
Burglary	The unlawful entry of a structure to commit a felony or a theft. Attempted forcible entry is included.
Larceny	The unlawful taking, carrying, leading, or riding away of property from the possession or constructive possession of another. Attempted larcenies are included.
Motor Vehicle Theft	The theft or attempted theft of a motor vehicle.
Arson	Any willful or malicious burning or attempt to burn, with or without intent to defraud, a dwelling house, public building, motor-vehicle or aircraft, personal property of another, etc.
Part II Offenses	
Other Assaults (simple)	Assaults and attempted assaults where no weapon were used or no serious or aggravated injury resulted to the victim. Stalking, intimidation, coercion, and hazing are included.
Forgery and Counterfeiting	The altering, copying, or imitating of something, without authority or right, with the intent to deceive or defraud by passing the copy or thing altered or imitated as that which is original or genuine; or the selling, buying, or possession of an altered, copied, or imitated thing with the intent to deceive or defraud. Attempts included.
Fraud	The intentional perversion of the truth for the purpose of inducing another person or other entity in reliance upon it to part with something of value or to surrender a legal right.

Table 3-7 (Continued)

	Part II Offenses
Embezzlement	The unlawful misappropriation or misapplication by an offender to his/her own use or purpose of money, property, or some other thing of value entrusted to his/her care, custody, or control.
Stolen Property	Buying, receiving, possessing, selling, concealing, or transporting any property with the knowledge that it has been unlawfully taken. Attempts are included.
Vandalism	To willfully or maliciously destroy, injure, disfigure, or deface any public or private property, real or personal, without the consent of the owner or person having custody or control.
Weapons Violations	The violations of laws or ordinances prohibiting the manufacture, sale, purchase, transportation, possession, concealment, or use of firearms, cutting instruments, explosives, incendiary devices, or other deadly weapons.
Prostitution and Commercialized Vice	The unlawful promotion of or participation in sexual activities for profit, including attempts.
Sex Offenses (except for forcible rape)	Offenses against chastity, common decency, morals, and the like. Incest, indecent exposure, and statutory rape are included. Attempts are also included.
Drug Abuse violations	The violation of laws prohibiting the production, distribution, and/ or use of certain controlled substances. The unlawful cultivation, manufacture, distribution, sale, purchase, use, possession, transportation, or importation of any controlled drug or narcotic substance.
Drug Sale	
Drug Possession	
Gambling	To unlawfully bet or wager money or something else of value; assist, promote, or operate a game of chance for money or some other stake.
Offenses against the Family and Children	Unlawful nonviolent acts by a family member (or legal guardian) that threaten the physical, mental, or economic well-being or morals of another family member and that are not classified as other offenses.
Driving Under the Influence (DUI)	Driving or operating a motor vehicle or common carrier while mentally or physically impaired as the result of consuming an alcoholic beverage or using a drug or narcotic.
Liquor Law violations	The violation of state or local laws or ordinances prohibiting the manufacture, sale, purchase, transportation, possession, or use of alcoholic beverages, not including DUI or drunkenness.

Table 3-7 (Continued)

	Part II Offenses
Drunkenness	To drink alcoholic beverages to the extent that one's mental faculties and physical coordination are substantially impaired. DUI excluded.
Disorderly Conduct	Any behavior that tends to disturb the public peace or decorum, scandalize the community, or shock the public sense of morality.
Vagrancy	The violation of a court order, regulation, ordinance, or law requiring the withdrawal of persons from the streets or other specified areas; prohibiting persons from remaining in an area or place in an idle or aimless manner; or prohibiting persons from going from place to place without visible means of support.
All Other Offenses	All violations of state or local laws not specifically identified as Part I or Part II offenses, except traffic violations.
Suspicion	Arrested for no specific offense and released without formal charges being placed.
Curfew and Loitering laws	Violations by juveniles of local curfew or loitering ordinances.
Runaways	Limited to juveniles taken into protective custody under the provisions of local statutes.

Table 3- 8. Correlation Matrix for 2015 and 2016 Deaths at the Metropolitan Level, using The Counted Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) All deaths, 2015	1.00									
(2) Minority deaths, 2015	0.95	1.00								
(3) Black deaths, 2015	0.77	0.82	1.00							
(4) Latino deaths, 2015	0.82	0.86	0.44	1.00						
(5) White deaths, 2015	0.82	0.61	0.47	0.55	1.00					
(6) All deaths, 2016	0.91	0.89	0.68	0.80	0.70	1.00				
(7) Minority deaths, 2016	0.86	0.88	0.64	0.83	0.60	0.95	1.00			
(8) Black deaths, 2016	0.70	0.72	0.80	0.46	0.46	0.74	0.76	1.00		
(9) Latino deaths, 2016	0.74	0.78	0.41	0.88	0.48	0.84	0.91	0.44	1.00	
(10) White deaths, 2016	0.72	0.64	0.54	0.49	0.69	0.81	0.58	0.49	0.45	1.00

Table 3- 9. Correlation Matrix for 2015, 2016 and 2017 Deaths at the County Level, using Fatal Force Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
(1) All deaths, 2015	1.00														
(2) Minority deaths, 2015	0.93	1.00													
(3) Black deaths, 2015	0.73	0.80	1.00												
(4) Latino deaths, 2015	0.83	0.88	0.46	1.00											
(5) White deaths, 2015	0.77	0.48	0.34	0.45	1.00										
(6) All deaths, 2016	0.82	0.81	0.60	0.76	0.56	1.00									
(7) Minority deaths, 2016	0.81	0.83	0.63	0.78	0.48	0.94	1.00								
(8) Black deaths, 2016	0.58	0.62	0.69	0.42	0.29	0.70	0.76	1.00							
(9) Latino deaths, 2016	0.75	0.77	0.43	0.84	0.45	0.84	0.88	0.42	1.00						
(10) White deaths, 2016	0.54	0.45	0.32	0.43	0.50	0.73	0.47	0.31	0.43	1.00					
(11) All deaths, 2017	0.83	0.81	0.60	0.75	0.58	0.87	0.85	0.60	0.78	0.57	1.00				
(12) Minority deaths, 2017	0.82	0.82	0.60	0.78	0.54	0.87	0.87	0.62	0.81	0.54	0.94	1.00			
(13) Black deaths, 2017	0.55	0.54	0.58	0.38	0.38	0.59	0.57	0.63	0.37	0.40	0.69	0.74	1.00		
(14) Latino deaths, 2017	0.76	0.76	0.44	0.82	0.49	0.81	0.83	0.46	0.87	0.46	0.82	0.86	0.37	1.00	
(15) White deaths, 2017	0.58	0.52	0.41	0.46	0.47	0.59	0.54	0.37	0.49	0.45	0.79	0.54	0.40	0.49	1.00

Table 3- 10. Correlation Matrix for 2015, 2016 and 2017 Deaths at the County Level, using SHR Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
(1) All deaths, 2015	1.00														
(2) Minority deaths, 2015	0.91	1.00													
(3) Black deaths, 2015	0.55	0.59	1.00												
(4) Latino deaths, 2015	0.80	0.78	0.46	1.00											
(5) White deaths, 2015	0.89	0.61	0.39	0.66	1.00										
(6) All deaths, 2016	0.85	0.79	0.52	0.73	0.72	1.00									
(7) Minority deaths, 2016	0.75	0.79	0.48	0.71	0.55	0.93	1.00								
(8) Black deaths, 2016	0.55	0.59	0.38	0.56	0.38	0.58	0.62	1.00							
(9) Latino deaths, 2016	0.76	0.73	0.52	0.85	0.62	0.83	0.80	0.53	1.00						
(10) White deaths, 2016	0.79	0.64	0.46	0.61	0.79	0.88	0.64	0.41	0.71	1.00					
(11) All deaths, 2017	0.87	0.80	0.49	0.77	0.76	0.88	0.79	0.54	0.78	0.81	1.00				
(12) Minority deaths, 2017	0.74	0.77	0.45	0.76	0.55	0.81	0.80	0.54	0.80	0.65	0.90	1.00			
(13) Black deaths, 2017	0.40	0.43	0.23	0.38	0.28	0.46	0.39	0.48	0.38	0.45	0.52	0.50	1.00		
(14) Latino deaths, 2017	0.73	0.74	0.50	0.87	0.57	0.80	0.76	0.54	0.93	0.68	0.80	0.85	0.41	1.00	
(15) White deaths, 2017	0.78	0.61	0.40	0.57	0.80	0.73	0.55	0.38	0.55	0.79	0.84	0.53	0.39	0.51	1.00

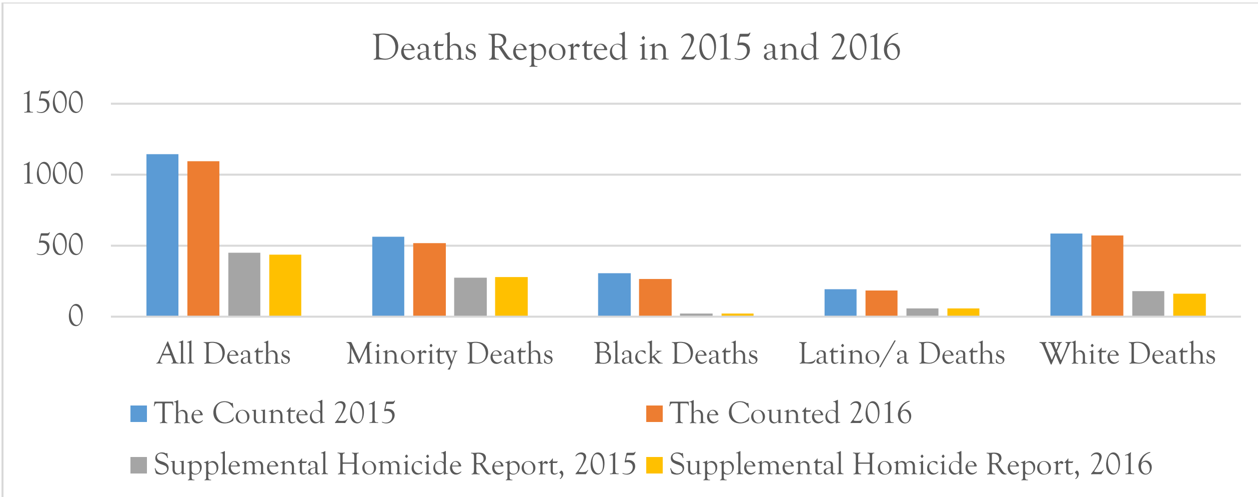

Figure 3- 1. Deaths Reported in 2015 and 2016

Table 4- 1. Mean and Variances for Killings at the Metro-Level

	Mean	Variance	Percent of cases without deaths
All deaths	5.15	116.48	21.05%
Minority deaths	2.79	60.03	46.13%
Black deaths	1.42	13.29	62.85%
Latino deaths	1.04	20.88	74.92%
White deaths	2.36	15.34	33.75%

Table 4- 2. Count Models Comparison

	AIC	Δ AIC	AIC weight
Poisson	1950	520	0.00
Negative Binomial	1433	3	0.18
Zero-Inflated Poisson	1807	377	0.00
Zero-Inflated Negative Binomial	1430	0	0.82

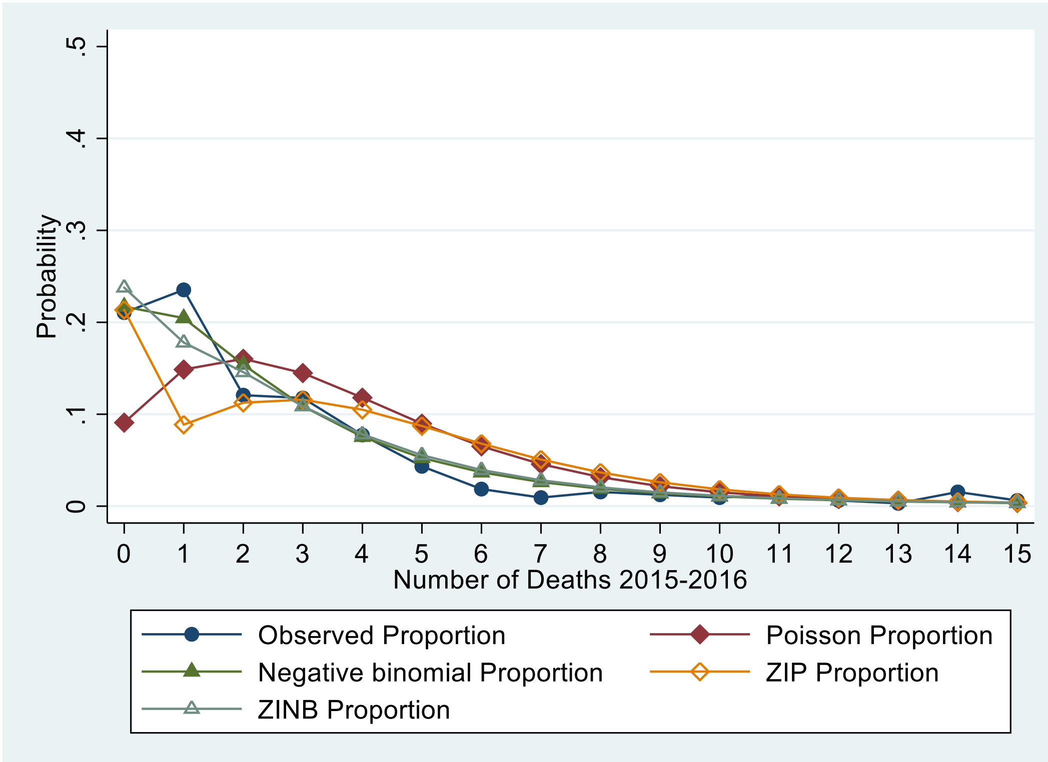

Figure 4- 1. Observed vs Predicted Probabilities by Model

Table 4- 3. Number of Deaths and Percent 0s by Source

Variables	The Counted 2015-2016	SHR 2015-2016	The Counted % 0's	SHR % 0's	% total deaths captured by the SHR
All deaths	1665	748	21%	57%	44.92
Black deaths	458	41	63%	94%	8.95
Latino Deaths	336	110	75%	92%	32.74
White deaths	761	292	34%	83%	38.37
Minority Deaths	904	456	46%	63%	50.44

Table 4- 4. Descriptive Statistics for Metropolitan-Level Analyses

	N	Mean	St.Dev	Min	Max
All Deaths, The Counted	323	5.15	10.79	0	124
Minority Deaths, The Counted	323	2.8	7.75	0	104
Black Deaths, The Counted	323	1.42	3.65	0	25
Latino Deaths, The Counted	323	1.04	4.57	0	70
White Deaths, The Counted	323	2.36	3.92	0	31
All Deaths, SHR	323	2.32	6.95	0	87
Minority Deaths, SHR	323	1.41	3.95	0	51
Black Deaths, SHR	323	.13	.62	0	6
Latino Deaths, SHR	323	.34	2.21	0	34
White Deaths, SHR	323	.9	3.6	0	36
Black/White population	323	.17	.21	0	1.24
Latino/White population	323	.42	1.76	.01	26.02
Black/White income	323	.56	.13	.24	1.17
Latino/White income	323	.5	.1	.29	1.05
Black-White interaction	323	57.36	19.84	4.74	94.39
Latino-White interaction	323	63.12	19.35	3.26	95.35
5-year population growth	323	2.23	2.35	-4.13	10.5
5-year Black population growth	323	5.62	12.42	-63.37	60.69
5-year Latino population growth	323	10.92	6.09	-2.91	62.47
15-year population growth	323	14.05	12.23	-10.45	64.07
15-year Black population growth	323	33.6	55.86	-46.62	392.47
15-year Latino population growth	323	102.81	50.49	12.7	300.87
Population, 2015	323	693.37	1355.06	54.48	13154.46
Violent crime, 2015	323	375.22	179.41	61.6	1160
Police per capita, 2015	323	2.3	1.78	.92	16.75
South	323	.36	.48	0	1
West	323	.24	.42	0	1
Northeast	323	.11	.31	0	1
Midwest	323	.23	.42	0	1
Southwest (Reference)	323	.18	.39	0	1
Northwest	323	.13	.34	0	1

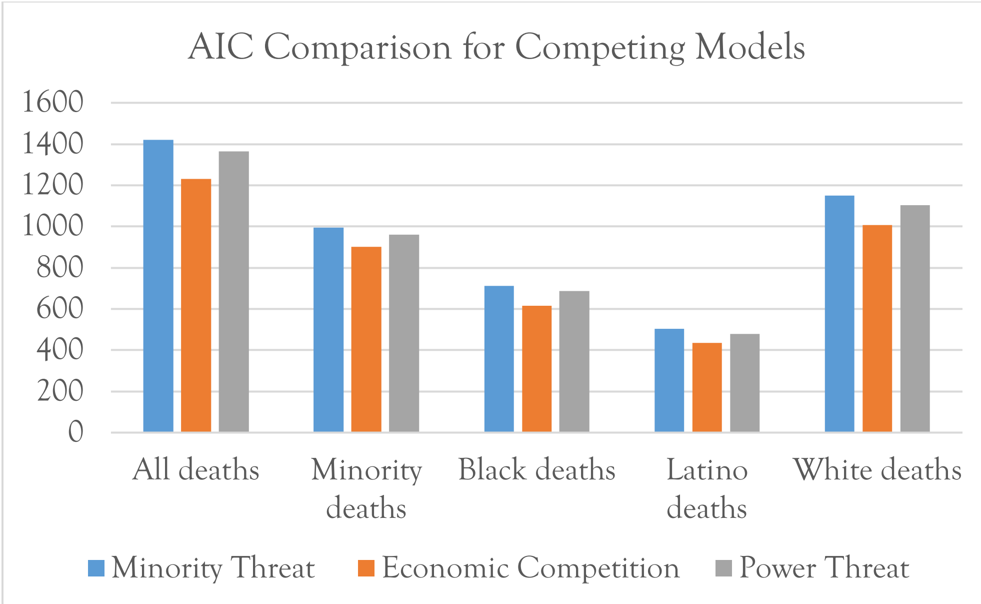

Figure 4- 2. AIC Comparison for Competing Models

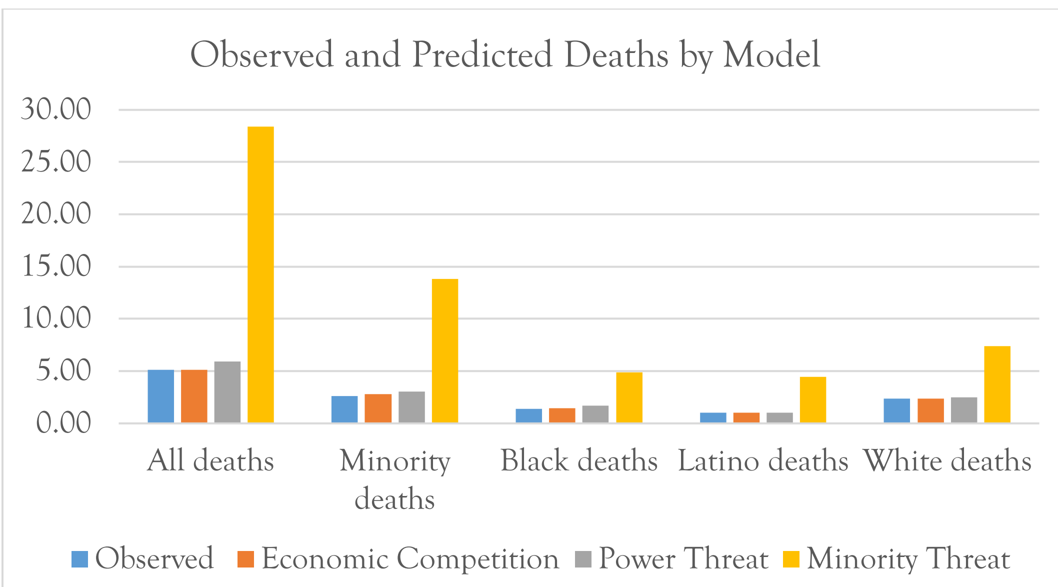

Figure 4- 3. Overall and Predicted Deaths by Model

Table 4- 5. Negative Binomial Models Predicting All, Minority, Black, Latino and White Deaths 2015-2016

	All Deaths b/se	Minority Deaths b/se	Black Deaths b/se	Latino Deaths b/se	White Deaths b/se
Black/White population	-0.090	0.181	0.679***	-0.339	-0.122
	(0.08)	(0.12)	(0.17)	(0.20)	(0.11)
Latino/White population	0.171	0.254	-0.090	1.510***	0.205
	(0.09)	(0.13)	(0.16)	(0.29)	(0.13)
Black/White income	0.050	0.009	-0.103	-0.009	0.083
	(0.05)	(0.08)	(0.10)	(0.12)	(0.06)
Black/White income	0.046	0.113	0.067	-0.110	-0.045
	(0.05)	(0.07)	(0.08)	(0.13)	(0.06)
Black-White exposure	0.124	-0.004	0.003	0.305	0.399**
	(0.10)	(0.13)	(0.16)	(0.23)	(0.14)
Latino-White exposure	0.014	-0.007	0.048	0.381	0.160
	(0.11)	(0.15)	(0.21)	(0.27)	(0.16)
Population, 2015	1.076***	1.082***	1.179***	1.245***	1.082***
	(0.04)	(0.05)	(0.07)	(0.10)	(0.05)
Violent crime, 2015	0.200***	0.195**	0.008	0.530***	0.226***
	(0.05)	(0.07)	(0.09)	(0.13)	(0.06)
Police per capita, 2015	0.071	0.076	0.119	0.089	0.055
	(0.04)	(0.06)	(0.07)	(0.12)	(0.06)
Southwest (reference)					
South	-0.007	-0.239	0.051	-0.347	0.049
	(0.13)	(0.18)	(0.21)	(0.33)	(0.18)
Northeast	-0.738***	-0.675**	-0.261	-0.688	-0.815***
	(0.17)	(0.24)	(0.27)	(0.53)	(0.24)
Midwest	-0.277*	-0.294	0.126	-0.415	-0.329
	(0.14)	(0.20)	(0.23)	(0.42)	(0.19)
Northwest	0.021	0.179	0.086	0.164	-0.145
	(0.13)	(0.19)	(0.29)	(0.27)	(0.17)

Table 4-5 (Continued)

	All Deaths b/se	Minority Deaths b/se	Black Deaths b/se	Latino Deaths b/se	White Deaths b/se
Constant	0.934***	0.036	-1.084***	-2.028***	0.385**
	(0.10)	(0.15)	(0.18)	(0.33)	(0.14)
lnalpha	-3.008***	-2.489***	-3.932	-2.239***	-2.529***
	(0.42)	(0.47)	(2.17)	(0.57)	(0.50)
Pseudo_R-squared	0.298	0.313	0.364	0.427	0.250
AIC	1230	902	614	435	1008
N	323.0	323.0	323.0	323.0	323.0

$p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4- 6. Negative Binomial Models Predicting All, Minority, Black, Latino and White Deaths 2015-2016

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
	b/se	b/se	b/se	b/se	b/se
Black/White population	-0.130	0.155	0.647***	-0.333	-0.196
	(0.08)	(0.12)	(0.18)	(0.21)	(0.11)
Latino/White population	0.120	0.265	-0.149	1.396***	0.079
	(0.10)	(0.14)	(0.17)	(0.31)	(0.14)
Black/White income	0.061	0.013	-0.101	-0.008	0.107
	(0.05)	(0.08)	(0.10)	(0.12)	(0.06)
Black/White income	0.057	0.118	0.065	-0.024	-0.016
	(0.05)	(0.07)	(0.08)	(0.14)	(0.07)
Black-White exposure	0.091	0.029	0.030	0.286	0.306*
	(0.10)	(0.13)	(0.17)	(0.23)	(0.14)
Latino-White exposure	-0.014	-0.068	-0.061	0.340	0.124
	(0.11)	(0.16)	(0.19)	(0.29)	(0.16)
Population, 2015	1.052***	1.093***	1.158***	1.225***	1.022***
	(0.04)	(0.06)	(0.08)	(0.10)	(0.06)
Violent crime, 2015	0.207***	0.176*	0.023	0.493***	0.256***
	(0.05)	(0.07)	(0.09)	(0.14)	(0.06)
Police per capita, 2015	0.067	0.082	0.118	0.053	0.040
	(0.04)	(0.06)	(0.07)	(0.12)	(0.06)
Southwest (reference)					
South	0.005	-0.172	0.096	-0.279	0.033
	(0.13)	(0.18)	(0.19)	(0.34)	(0.17)
Northeast	-0.643***	-0.687**	-0.196	-0.507	-0.617*
	(0.18)	(0.26)	(0.27)	(0.59)	(0.25)
Midwest	-0.237	-0.262	0.209	-0.462	-0.274
	(0.14)	(0.20)	(0.22)	(0.44)	(0.19)
Northwest	0.021	0.245	0.175	0.196	-0.199
	(0.13)	(0.19)	(0.28)	(0.27)	(0.17)

Table 4-6 (Continued)

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
	b/se	b/se	b/se	b/se	b/se
5-year Population growth	0.086	-0.038	0.090	0.088	0.191*
	(0.06)	(0.09)	(0.11)	(0.16)	(0.08)
5-year Black growth	-0.066	-0.114	-0.269	-0.084	-0.048
	(0.06)	(0.09)	(0.14)	(0.15)	(0.07)
5-year Latino growth	-0.021	0.018	0.059	-0.307	-0.073
	(0.06)	(0.09)	(0.10)	(0.25)	(0.07)
Constant	0.909***	0.002	-1.159***	-2.087***	0.358**
	(0.10)	(0.15)	(0.17)	(0.34)	(0.14)
lnalpha	-3.023***	-2.698***	-14.679	-2.381***	-2.691***
	(0.42)	(0.56)	(481.01)	(0.68)	(0.55)
Pseudo_R-squared	0.300	0.315	0.368	0.430	0.254
AIC	1233	905	616	439	1008
N	323.0	323.0	323.0	323.0	323.0

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4- 7. Negative Binomial Models Predicting All, Minority, Black, Latino and White Deaths 2015-2016

	All Deaths b/se	Minority Deaths b/se	Black Deaths b/se	Latino Deaths b/se	White Deaths b/se
Black/White population	-0.098	0.206	0.802***	-0.278	-0.193
	(0.08)	(0.12)	(0.19)	(0.21)	(0.11)
Latino/White population	0.206	0.385*	0.200	1.430***	0.128
	(0.11)	(0.17)	(0.20)	(0.37)	(0.15)
Black/White income	0.043	0.013	-0.148	0.025	0.070
	(0.05)	(0.08)	(0.10)	(0.12)	(0.07)
Black/White income	0.052	0.118	0.071	-0.126	-0.036
	(0.05)	(0.07)	(0.07)	(0.13)	(0.06)
Black-White exposure	0.103	0.044	0.133	0.291	0.303*
	(0.10)	(0.13)	(0.18)	(0.23)	(0.14)
Latino-White exposure	0.020	0.006	0.099	0.370	0.129
	(0.11)	(0.15)	(0.19)	(0.27)	(0.16)
Population, 2015	1.075***	1.102***	1.213***	1.210***	1.052***
	(0.04)	(0.05)	(0.07)	(0.10)	(0.06)
Violent crime, 2015	0.205***	0.189**	-0.007	0.539***	0.241***
	(0.05)	(0.07)	(0.08)	(0.13)	(0.06)
Police per capita, 2015	0.071	0.084	0.116	0.084	0.045
	(0.04)	(0.06)	(0.07)	(0.12)	(0.06)
Southwest (reference)					
South	-0.092	-0.288	-0.159	-0.249	-0.064
	(0.14)	(0.19)	(0.21)	(0.38)	(0.18)
Northeast	-0.721***	-0.728**	-0.359	-0.743	-0.709**
	(0.17)	(0.24)	(0.26)	(0.54)	(0.23)
Midwest	-0.288*	-0.310	0.072	-0.447	-0.302
	(0.14)	(0.19)	(0.21)	(0.42)	(0.18)
Northwest	0.006	0.214	0.135	0.164	-0.213
	(0.13)	(0.19)	(0.27)	(0.27)	(0.16)

Table 4-7 (Continued)

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
	b/se	b/se	b/se	b/se	b/se
15-year Population growth	-0.005	-0.129	-0.203	-0.019	0.129
	(0.07)	(0.10)	(0.12)	(0.19)	(0.09)
15-year Black growth	0.018	0.039	-0.028	0.161	-0.048
	(0.07)	(0.12)	(0.18)	(0.22)	(0.08)
15-year Latino growth	0.088	0.092	0.247*	-0.190	0.089
	(0.07)	(0.10)	(0.11)	(0.27)	(0.09)
Constant	0.961***	0.051	-1.042***	-2.033***	0.410**
	(0.10)	(0.15)	(0.17)	(0.33)	(0.13)
lnalpha	-3.046***	-2.659***	-15.415	-2.261***	-2.825***
	(0.43)	(0.54)	(502.45)	(0.59)	(0.64)
Pseudo_R-squared	0.300	0.314	0.371	0.429	0.255
AIC	1233	906	613	439	1007
N	323.0	323.0	323.0	323.0	323.0

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4- 8. Negative Binomial Models Predicting All, Minority, Black, Latino and White Deaths 2015-2016

	All Deaths b/se	Minority Deaths b/se	Black Deaths b/se	Latino Deaths b/se	White Deaths b/se
Black/White population	-0.250	-0.148	1.755*	0.149	-0.303
	(0.18)	(0.20)	(0.78)	(0.33)	(0.36)
Latino/White population	0.045	0.050	0.030	1.764	-0.127
	(0.22)	(0.25)	(0.72)	(0.91)	(0.44)
Black/White income	0.009	0.037	0.864	-0.022	-0.064
	(0.11)	(0.12)	(0.48)	(0.35)	(0.24)
Black/White income	-0.103	-0.152	0.133	-0.083	-0.031
	(0.11)	(0.12)	(0.34)	(0.37)	(0.21)
Black-White exposure	0.159	0.216	-0.043	1.239**	0.447
	(0.21)	(0.23)	(0.68)	(0.44)	(0.41)
Latino-White exposure	-0.077	-0.271	0.035	-0.401	-0.084
	(0.24)	(0.28)	(0.84)	(0.55)	(0.45)
Population, 2015	1.238***	1.155***	1.565***	1.899***	1.497***
	(0.09)	(0.10)	(0.32)	(0.24)	(0.18)
Violent crime, 2015	0.251*	0.138	0.442	1.210***	0.866***
	(0.10)	(0.11)	(0.39)	(0.25)	(0.21)
Police per capita, 2015	0.170	0.179	-0.054	-0.359	0.022
	(0.09)	(0.10)	(0.35)	(0.40)	(0.20)
Southwest (reference)					
South	-0.544	0.376	-0.804	-19.477	-2.038***
	(0.33)	(0.40)	(0.85)	(4367.68)	(0.57)
Northeast	-0.843*	-0.244	-0.035	-20.064	-1.515*
	(0.39)	(0.45)	(1.23)	(20461.16)	(0.70)
Midwest	-0.303	0.694	0.200	0.343	-2.053**
	(0.33)	(0.39)	(0.96)	(0.98)	(0.63)
Northwest	-0.051	1.023**	-0.535	-1.445*	-2.408***
	(0.28)	(0.34)	(1.25)	(0.67)	(0.60)

Table 4-8 (Continued)

	All Deaths b/se	Minority Deaths b/se	Black Deaths b/se	Latino Deaths b/se	White Deaths b/se
Constant	0.025	-1.055***	-5.129***	-5.459***	-0.454
	(0.25)	(0.31)	(0.92)	(1.13)	(0.43)
lnalpha	-0.527*	-0.459	-0.693	-17.172	0.455
	(0.24)	(0.27)	(0.95)	(2295.22)	(0.29)
Pseudo_R-squared	0.216	0.206	0.422	0.620	0.245
AIC	900	765	152	139	459
N	323.0	323.0	323.0	323.0	323.0

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

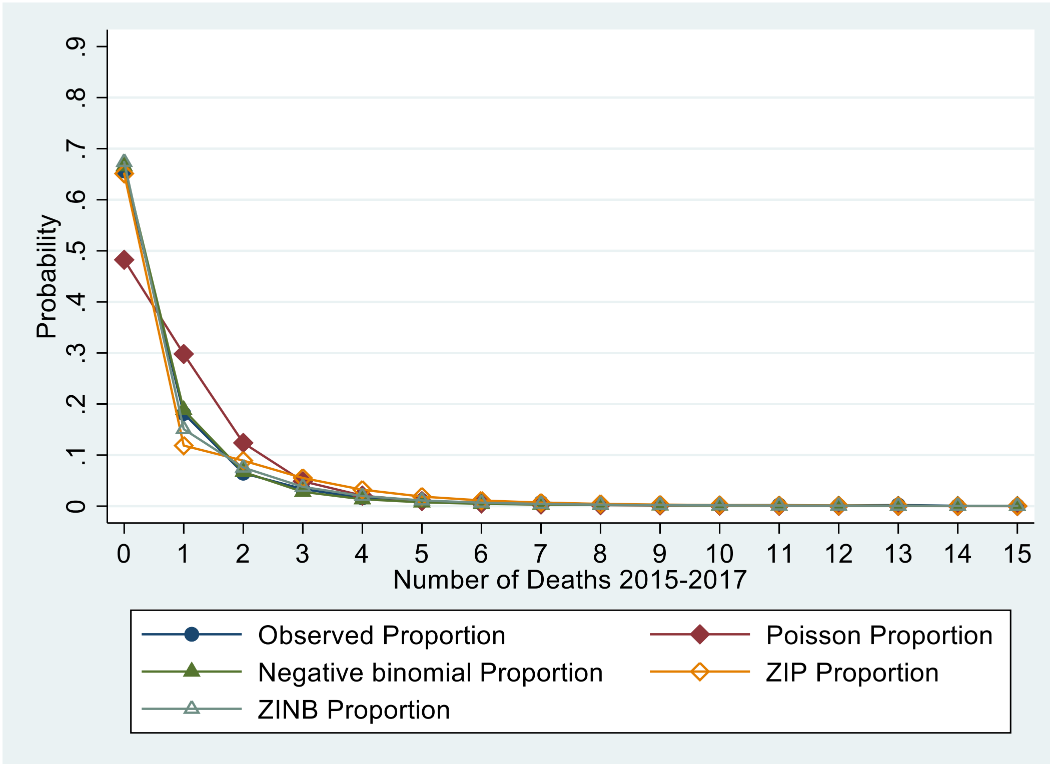

Figure 5- 1. Observed vs Predicted Probabilities by Model

Table 5- 1. Mean and Variances for Killings at the County-Level

	Mean	Variance	Percent of cases without deaths
All deaths	1.06	17.07	65.63%
Minority deaths	0.55	9.46	81.9%
Black deaths	0.25	1.63	89.45%
Latino deaths	0.19	3	93.62%
White deaths	0.52	1.93	73.53%

Table 5- 2. Descriptive Statistics for County-Level Analyses

	N	Mean	St.Dev	Min	Max
All Deaths, The Post	2712	1.06	4.13	0	138
Minority Deaths, The Post	2712	.55	3.08	0	116
Black Deaths, The Post	2712	.25	1.28	0	27
Latino Deaths, The Post	2712	.19	1.73	0	72
White Deaths, The Post	2712	.52	1.39	0	31
All Deaths, SHR	2712	.48	3.25	0	118
Minority Deaths, SHR	2712	.29	1.9	0	77
Black Deaths, SHR	2712	.02	.25	0	8
Latino Deaths, SHR	2712	.06	1.13	0	53
White Deaths, SHR	2712	.19	1.63	0	46
Black/White population	2712	.19	.43	0	6.18
Latino/White population	2712	.26	2.25	0	104.29
Black/White income	2712	.57	.41	.01	11.11
Latino/White income	2712	.53	.21	.03	2.44
Black-White exposure	2712	.7	.22	0	.98
Latino-White exposure	2712	.73	.2	.01	.98
Population, 2015	2712	112.4	337.19	.97	10038.39
Violent crime, 2014	2712	1.09	.92	0	8.31
Police per capita, 2015	2712	2.06	2.05	0	71.69
Trigger	2712	.03	.18	0	1
Metropolitan County	2712	.41	.49	0	1
Southwest	2712	.12	.32	0	1
South	2712	.4	.49	0	1
Northeast	2712	.08	.27	0	1
Midwest	2712	.32	.47	0	1
Northwest	2712	.08	.27	0	1

Table 5- 3. Negative Binomial Models Predicting All, Minority, Black, Latino, and White Deaths 2015-2017

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
	b/se	b/se	b/se	b/se	b/se
Black/White population	-0.273***	-0.133	0.443***	-0.130	-0.260**
	(0.06)	(0.08)	(0.13)	(0.16)	(0.08)
Latino/White population	-0.156**	-0.054	-0.273**	0.833***	-0.234***
	(0.05)	(0.07)	(0.10)	(0.13)	(0.07)
Black/White income	0.038	0.032	-0.075	-0.031	0.028
	(0.04)	(0.07)	(0.12)	(0.12)	(0.05)
Latino/White income	0.037	0.035	-0.037	-0.090	0.040
	(0.04)	(0.07)	(0.09)	(0.15)	(0.05)
Black-White interaction	-0.427***	-0.674***	-0.302	0.744**	0.023
	(0.10)	(0.13)	(0.21)	(0.27)	(0.13)
Population, 2015	1.352***	1.374***	1.657***	1.596***	1.361***
	(0.04)	(0.06)	(0.09)	(0.11)	(0.05)
Violent crime, 2014	0.139***	0.172***	0.109*	0.134	0.115**
	(0.03)	(0.04)	(0.05)	(0.08)	(0.04)
Police per capita, 2015	0.097**	0.164***	0.121*	0.242**	0.059
	(0.03)	(0.05)	(0.06)	(0.08)	(0.04)
Southwest (Reference)	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)
South	-0.176	-0.322*	0.203	-0.764**	-0.220
	(0.11)	(0.16)	(0.24)	(0.27)	(0.14)
Northeast	-1.180***	-1.212***	-0.519*	-1.216***	-1.327***
	(0.13)	(0.18)	(0.25)	(0.30)	(0.16)
Midwest	-0.654***	-0.654***	0.149	-1.093***	-0.778***
	(0.12)	(0.17)	(0.24)	(0.31)	(0.14)
Northwest	-0.007	0.185	0.109	-0.091	-0.158
	(0.11)	(0.15)	(0.27)	(0.21)	(0.13)
Metropolitan County	0.071	0.266*	0.279	0.241	0.005

Table 5-3 (Continued)

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
	b/se	b/se	b/se	b/se	b/se
	(0.08)	(0.13)	(0.20)	(0.26)	(0.09)
Constant	-0.894***	-2.159***	-3.852***	-3.873***	-1.246***
	(0.11)	(0.15)	(0.25)	(0.28)	(0.13)
lnalpha	-1.363***	-1.258***	-1.088***	-1.146***	-1.446***
	(0.14)	(0.19)	(0.23)	(0.35)	(0.23)
Pseudo_R-squared	0.278	0.344	0.365	0.413	0.228
AIC	4980	2822	1741	1125	3911
N	2712.0	2712.0	2712.0	2712.0	2712.0

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5- 4. Negative Binomial Models Predicting All, Minority, Black, Latino, and White Deaths 2015-2017

	All Deaths	Minority Deaths	Black Deaths	Latino Deaths	White Deaths
	b/se	b/se	b/se	b/se	b/se
Black/White population	-0.279***	-0.140	0.436**	-0.138	-0.265**
	(0.06)	(0.08)	(0.13)	(0.16)	(0.08)
Latino/White population	-0.161**	-0.058	-0.278**	0.830***	-0.239***
	(0.05)	(0.07)	(0.10)	(0.13)	(0.07)
Black/White income	0.037	0.030	-0.079	-0.032	0.027
	(0.04)	(0.07)	(0.12)	(0.12)	(0.05)
Latino/White income	0.037	0.034	-0.039	-0.091	0.040
	(0.04)	(0.07)	(0.09)	(0.15)	(0.05)
Black-White exposure	-0.431***	-0.680***	-0.308	0.734**	0.020
	(0.10)	(0.13)	(0.21)	(0.27)	(0.13)
Population, 2015	1.349***	1.370***	1.652***	1.589***	1.359***
	(0.04)	(0.06)	(0.09)	(0.11)	(0.05)
Violent crime, 2014	0.138***	0.171***	0.108*	0.132	0.115**
	(0.03)	(0.04)	(0.05)	(0.08)	(0.04)
Police per capita, 2015	0.096**	0.163***	0.119*	0.241**	0.058
	(0.03)	(0.05)	(0.06)	(0.08)	(0.04)
Southwest (Reference)	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)
South	-0.170	-0.316*	0.212	-0.759**	-0.214
	(0.11)	(0.16)	(0.24)	(0.27)	(0.14)
Northeast	-1.176***	-1.209***	-0.512*	-1.211***	-1.323***
	(0.13)	(0.18)	(0.25)	(0.30)	(0.16)
Midwest	-0.647***	-0.646***	0.163	-1.084***	-0.773***
	(0.12)	(0.17)	(0.24)	(0.31)	(0.15)
Northwest	-0.002	0.190	0.117	-0.086	-0.152
	(0.11)	(0.15)	(0.27)	(0.21)	(0.14)
Metropolitan County	0.073	0.269*	0.282	0.248	0.007
	(0.08)	(0.13)	(0.20)	(0.26)	(0.09)
Trigger	0.091	0.084	0.087	0.074	0.081

Table 5-4 (Continued)

	All Deaths b/se	Minority Deaths b/se	Black Deaths b/se	Latino Deaths b/se	White Deaths b/se
	(0.11)	(0.13)	(0.17)	(0.19)	(0.14)
Constant	-0.901***	-2.165***	-3.860***	-3.881***	-1.253***
	(0.11)	(0.16)	(0.25)	(0.28)	(0.13)
lnalpha	-1.358***	-1.255***	-1.094***	-1.127**	-1.440***
	(0.14)	(0.19)	(0.24)	(0.34)	(0.23)
Pseudo_R-squared	0.278	0.344	0.365	0.413	0.228
AIC	4981	2823	1743	1127	3912
N	2712.0	2712.0	2712.0	2712.0	2712.0

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5- 5. Descriptive Statistics for Logistic Regression Models

	N	Mean	St.Dev	Min	Max
Unarmed	3943	.1	.3	0	1
Not Attacking	3943	.37	.48	0	1
Black	3943	.24	.43	0	1
Latino	3943	.17	.38	0	1
Other	3943	.04	.2	0	1
Suspect was fleeing	3943	.32	.46	0	1
Age	3943	35.6	14.49	0	91
Body-camera in use	3943	.11	.31	0	1
Signs of mental illness	3943	.24	.43	0	1
Suspect was attacking officer or civilian	3943	.63	.48	0	1
Southwest	3943	.31	.46	0	1
Northeast	3943	.07	.26	0	1
South	3943	.32	.47	0	1
Midwest	3943	.17	.37	0	1
Northwest	3943	.13	.34	0	1
Metropolitan County	3943	.84	.36	0	1
Black/White population	3870	.26	.41	0	5.03
Latino/White population	3870	.58	2.14	0	104.29
Black/White income	3870	.56	.18	.03	5.36
Latino/White income	3870	.48	.13	.03	1.91
Black-White exposure	3870	.49	.23	.01	.98
Violent crime, 2014	3870	1.59	1.16	0	8.31
Police per capita, 2015	3870	2.41	2.08	.26	71.69
Population, 2015	3870	1246.38	2190.37	1.2	10038.39

Table 5- 6. Logistic Regression Models Predicting Whether Fatally Shot Civilian was Unarmed

	ML base	Logistic base	ML level1	Logistic level1	ML level2	Logistic level2
	b/se	b/se	b/se	b/se	b/se	b/se
Black			1.387*	1.380*	1.392*	1.385*
			(0.19)	(0.18)	(0.21)	(0.20)
Latino			1.129	1.127	1.141	1.141
			(0.18)	(0.18)	(0.19)	(0.19)
Other			0.835	0.835	0.825	0.826
			(0.25)	(0.25)	(0.25)	(0.25)
Fleeing			1.529***	1.526***	1.566***	1.562***
			(0.18)	(0.18)	(0.18)	(0.18)
Age			0.984***	0.984***	0.984***	0.984***
			(0.00)	(0.00)	(0.00)	(0.00)
Body-camera			1.185	1.186	1.227	1.229
			(0.19)	(0.19)	(0.20)	(0.20)
Mental-Illness			0.664**	0.665**	0.678**	0.678**
			(0.10)	(0.10)	(0.10)	(0.10)
Attacking			0.216***	0.216***	0.217***	0.218***
			(0.03)	(0.02)	(0.03)	(0.03)
Southwest (Reference)						
Northeast			0.829	0.832	0.803	0.800
			(0.21)	(0.20)	(0.22)	(0.22)
South			1.111	1.111	1.098	1.088
			(0.17)	(0.16)	(0.21)	(0.20)
Midwest			1.096	1.089	1.058	1.039
			(0.20)	(0.19)	(0.22)	(0.21)
Northwest			0.809	0.808	0.853	0.844
			(0.16)	(0.16)	(0.19)	(0.18)
Black/White population					0.928	0.928
					(0.08)	(0.08)
Latino/White population					0.993	0.993

Table 5-6 (Continued)

	ML base	Logistic base	ML level1	Logistic level1	ML level2	Logistic level2
	b/se	b/se	b/se	b/se	b/se	b/se
					(0.06)	(0.06)
Black/White income					1.007	1.007
					(0.13)	(0.13)
Latino/White income					1.094	1.094
					(0.12)	(0.12)
Black-White exposure					0.893	0.894
					(0.10)	(0.10)
Violent crime, 2014					0.954	0.955
					(0.05)	(0.05)
Police per capita, 2015					1.084	1.083
					(0.05)	(0.05)
Population, 2015					1.001	1.000
					(0.01)	(0.01)
Metropolitan County					0.727	0.729
					(0.12)	(0.12)
Variance component	1.065		1.017		1.023	
	(0.08)		(0.08)		(0.09)	
Pseudo_R-squared		0.000		0.102		0.105
AIC	2618	2617	2376	2374	2353	2351
N	3943.0	3943.0	3943.0	3943.0	3870.0	3870.0

Exponentiated coefficients
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5- 7. Logistic Regression Models Predicting Whether Fatally Shot Civilian was not Attacking Police or Civilians

	ML base	Logistic base	ML level1	Logistic level1	ML level2	Logistic level2
	b/se	b/se	b/se	b/se	b/se	b/se
Black			0.935	0.939	0.947	0.946
			(0.08)	(0.08)	(0.09)	(0.09)
Latino			1.233*	1.263*	1.234*	1.241*
			(0.12)	(0.12)	(0.13)	(0.13)
Other			1.242	1.271	1.246	1.260
			(0.21)	(0.21)	(0.22)	(0.22)
Fleeing			0.994	0.987	1.011	1.005
			(0.08)	(0.07)	(0.08)	(0.08)
Age			0.994*	0.993**	0.994*	0.993**
			(0.00)	(0.00)	(0.00)	(0.00)
Body-camera			1.434***	1.394**	1.424**	1.389**
			(0.15)	(0.14)	(0.16)	(0.15)
Mental-Illness			1.226*	1.209*	1.227*	1.218*
			(0.10)	(0.10)	(0.10)	(0.10)
Southwest (Reference)						
Northeast			1.015	0.987	1.047	1.092
			(0.16)	(0.14)	(0.18)	(0.17)
South			0.772*	0.767**	0.790	0.842
			(0.08)	(0.07)	(0.10)	(0.10)
Midwest			0.801	0.791*	0.802	0.843
			(0.10)	(0.08)	(0.11)	(0.10)
Northwest			0.950	0.913	0.937	0.962
			(0.13)	(0.10)	(0.14)	(0.12)
Black/White population					0.962	0.956
					(0.06)	(0.05)
Latino/White population					0.976	0.984
					(0.04)	(0.04)

Table 5-7 (Continued)

	ML base	Logistic base	ML level1	Logistic level1	ML level2	Logistic level2
	b/se	b/se	b/se	b/se	b/se	b/se
Black/White income					1.043	1.049
					(0.09)	(0.08)
Latino/White income					0.926	0.923
					(0.07)	(0.06)
Black-White exposure					1.011	1.010
					(0.07)	(0.07)
Violent crime, 2014					0.938	0.947
					(0.03)	(0.03)
Police per capita, 2015					1.048	1.047
					(0.04)	(0.04)
Population, 2015					1.017	1.016*
					(0.01)	(0.01)
Metropolitan County					0.996	0.991
					(0.11)	(0.10)
Variance component	1.169**		1.134*		1.118*	
	(0.06)		(0.06)		(0.06)	
Pseudo_R-squared		0.000		0.011		0.014
AIC	5167	5191	5142	5156	5064	5071
N	3943.0	3943.0	3943.0	3943.0	3870.0	3870.0

Exponentiated coefficients
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

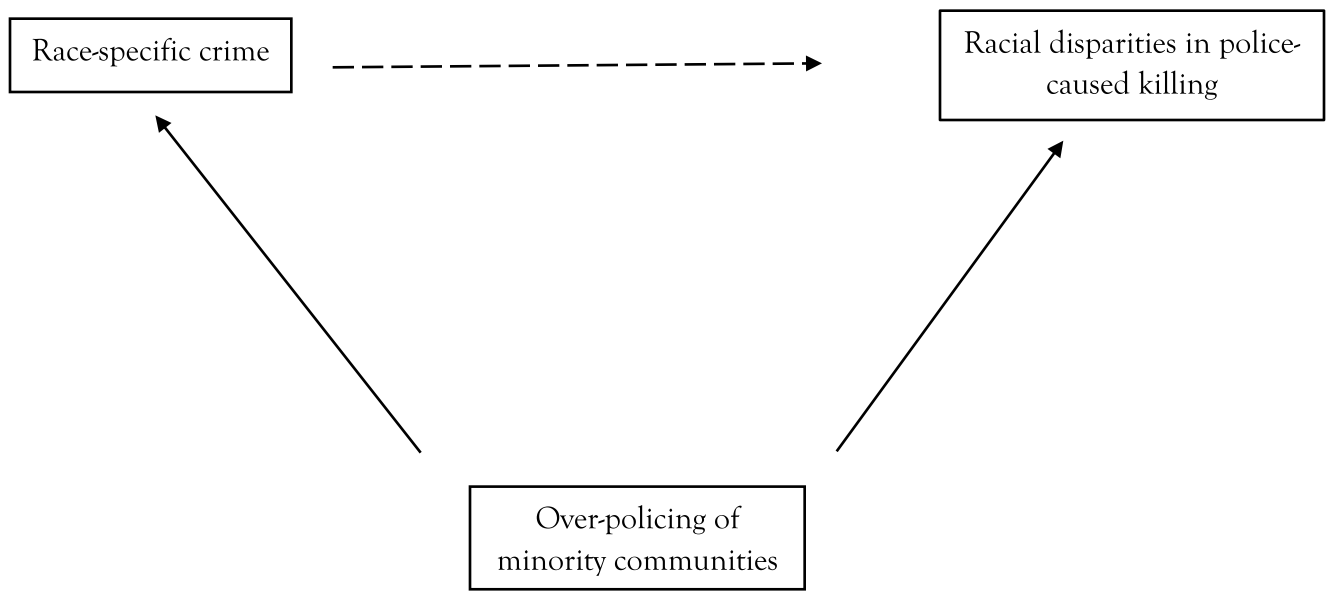

Figure 6- 1. Spurious Relationship between Race-Specific Crime and Police-Caused Killings

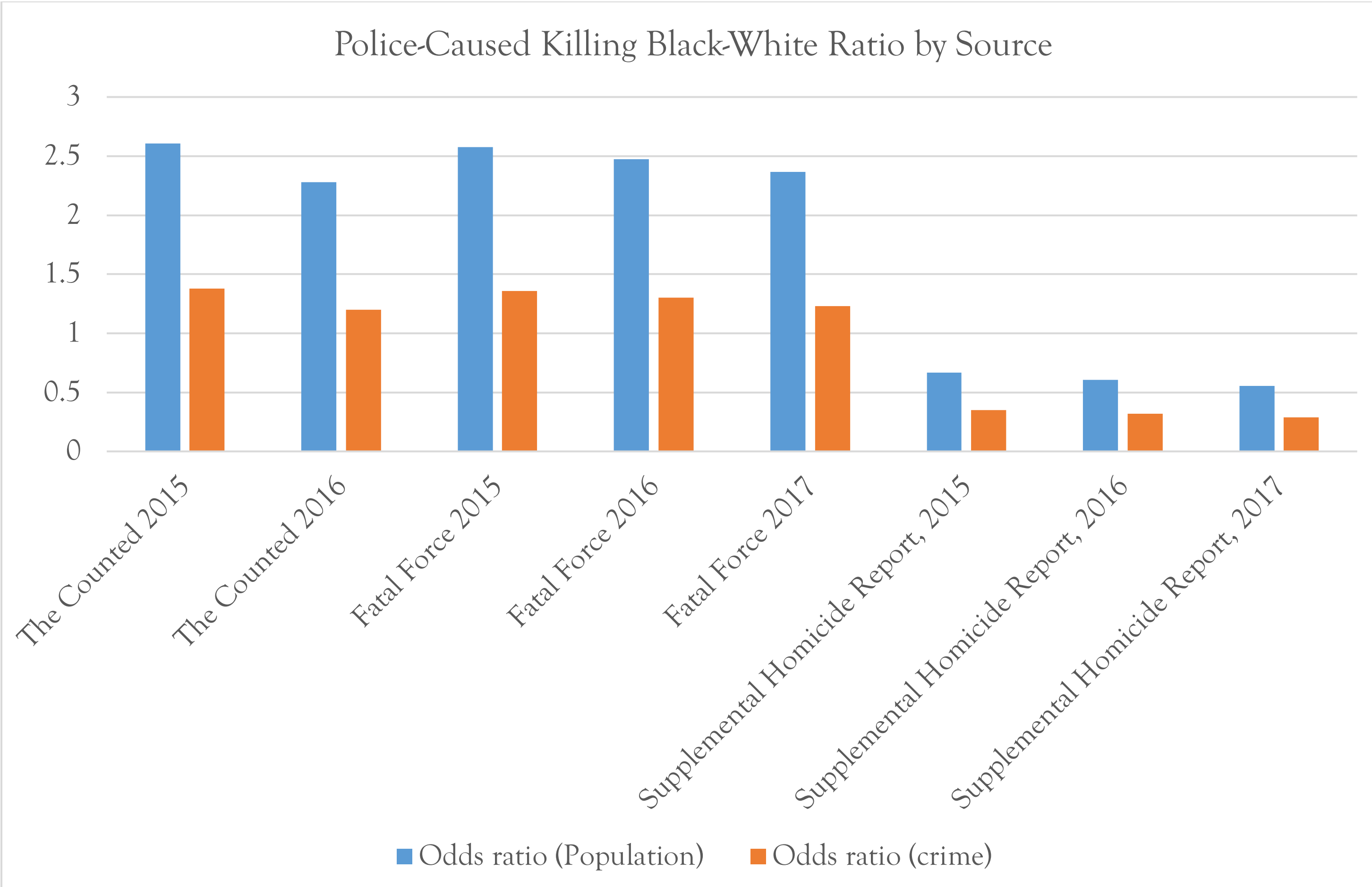

Figure 6- 2. Police-Caused Killings Black-White Ratio Benchmarked by Total Population and Total Crime Arrests

Table 6- 1. Distribution of Arrests by Crime Category in 2015, 2016 and 2017

Arrest Categories	Totals for 2015	Percent of total, 2015	Totals for 2016	Percent of total, 2016	Totals for 2017	Percent of total, 2017
TOTAL	10,797,088	100.00	10,662,252	100.00	10,554,985	100.00
Murder and nonnegligent manslaughter	11,092	0.10	11,788	0.11	12,208	0.12
Rape	22,863	0.21	23,632	0.22	23,436	0.22
Robbery	95,572	0.89	95,754	0.90	94,046	0.89
Aggravated assault	376,154	3.48	383,977	3.60	388,927	3.68
Burglary	216,010	2.00	207,325	1.94	199,266	1.89
Larceny-theft	1,160,390	10.75	1,050,058	9.85	950,357	9.00
Motor vehicle theft	77,979	0.72	86,088	0.81	91,023	0.86
Arson	8,834	0.08	9,812	0.09	9,111	0.09
Violent crime	505,681	4.68	515,151	4.83	518,617	4.91
Property crime	1,463,213	13.55	1,353,283	12.69	1,249,757	11.84
Other assaults	1,081,019	10.01	1,078,808	10.12	1,062,370	10.07
Forgery and counterfeiting	55,333	0.51	56,661	0.53	55,604	0.53
Fraud	133,138	1.23	128,531	1.21	124,232	1.18
Embezzlement	15,909	0.15	15,937	0.15	15,967	0.15
Stolen property; buying, receiving, possessing	88,576	0.82	93,981	0.88	98,660	0.93
Vandalism	191,015	1.77	195,951	1.84	188,350	1.78
Weapons; carrying, possessing, etc.	145,358	1.35	156,777	1.47	164,984	1.56
Prostitution and commercialized vice	41,877	0.39	38,306	0.36	36,247	0.34
Sex offenses (except rape and prostitution)	51,388	0.48	51,063	0.48	48,525	0.46
Drug abuse violations	1,488,707	13.79	1,572,579	14.75	1,632,921	15.47
Gambling	4,825	0.04	3,705	0.03	3,237	0.03

Table 6-1 (Continued)

Arrest Categories	Totals for 2015	Percent of total, 2015	Totals for 2016	Percent of total, 2016	Totals for 2017	Percent of total, 2017
Offenses against the family and children	94,837	0.88	88,748	0.83	94,062	0.89
Driving under the influence	1,089,171	10.09	1,017,808	9.55	990,678	9.39
Liquor laws	266,250	2.47	234,899	2.20	207,332	1.96
Drunkenness	405,880	3.76	376,433	3.53	366,824	3.48
Disorderly conduct	386,078	3.58	369,733	3.47	353,151	3.35
Vagrancy	25,151	0.23	24,851	0.23	23,321	0.22
All other offenses	3,218,880	29.81	3,254,871	30.53	3,290,015	31.17
Suspicion	1,389	0.01	576	0.01	885	0.01
Curfew and loitering law violations	44,802	0.41	34,176	0.32	30,131	0.29

Notes. Because of rounding, the percentages may not add up to 100. Violent crimes are offenses of murder and nonnegligent manslaughter, rape, robbery, and aggravated assault. Property crimes are offenses of burglary, larceny-theft, motor vehicle theft, and arson.

Table 6- 2. Fit Statistics for Lowest AIC Models Predicting Police-Caused Killings in the United States, 2015-2017

	All Deaths (1)	White Deaths (2)	Black Deaths (3)
Lowest AIC Model			
Predictor[a]	Overall Population	White Population	Black Population
NBR coefficient	1.19 ***	0.74***	1.03***
Standard error	0.06	0.04	0.05
AIC	5881	4430	2253
BIC	5998	4441	2264
Second lowest AIC Model			
Predictor	Simple Assault	Larceny	Larceny
NBR coefficient	0.82***	0.62***	0.73 ***
Standard error	0.04	0.04	0.04
AIC	6112	4620	2518
ΔAIC	232	190	277
BIC	6130	4632	2530
Relative likelihood	4.04E-51	7.64E-33	2.61E-58
Third lowest AIC Model			
Predictor	Weapon Violations	Simple Assault	Simple Assault
NBR coefficient	0.96***	0.62***	0.74***
Standard error	0.05	0.04	0.04
AIC	6117	4667	2525
ΔAIC	236	237	272
BIC	6135	4678	2537
Relative likelihood	4.57E-52	4.14E-52	6.03E-60

n=2,712; *p<0.05, **p<0.01, ***p<.001

[a]All predictors are 2015 data (logged) per 1,000 people

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