Action or Distraction? Assessing the Impact of Post-2020 Police Use of Force Reforms in American Cities

Vineet Chovatia

Professor Michelle P. Connolly, Faculty Advisor

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Abstract

Between 2013 and 2024, police killed 13,468 people in the United States. Low-income communities of color, who are disproportionately targeted, bear the brunt of this violence. This reality reflects a legacy rooted in a deeply racist history that continues to shape American policing today. In the wake of regular, highly-publicized killings of unarmed Black and Brown Americans and large-scale social movements advocating for police reform, police departments in many American cities implemented a range of reforms over the course of the 21st century. We use data on the adoption of seven of these reforms along with police shootings and killings data from 94 of America's largest cities to construct fixed effects difference in differences models that estimate the effect of these policies individually and in combination on police shootings and killings. Our findings suggest that chokehold bans, de-escalation policies, and comprehensive reporting reforms are associated with reductions in police shootings when implemented together while findings with regards to police killings are more mixed, but indicate that combinations of these policies are associated with reductions in killings as well.

JEL classification: C23, K42, K14

Keywords: Police Use of Force; Fixed Effect Difference in Differences; Post-2020 Police Reforms

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Last but certainly not least, my parents are the reason I have had the privilege of attending Duke these past four years. Everything that Duke has given me is because of all that my parents sacrificed to give me and my brother the lives that we have today. Varun, my twin brother and best friend, always pushes me to be a better student, friend, brother, and person. During my lowest points and most stressful moments in college, my brother was there for me without fail. This thesis would not have been possible without him.

Introduction

In May 2020, George Floyd was murdered by Minneapolis police officer Derek Chauvin, as Chauvin kneeled on Floyd's neck for eight horrific minutes that were captured on camera (Hill et al., 2020). George Floyd's murder was neither an isolated incident nor the sole catalyst for the urgent calls for reform that followed. His death became a flashpoint in a long history of police violence disproportionately targeting unarmed Black Americans—a history marked by the tragic killings of Tamir Rice, Breonna Taylor, Ahmaud Arbery, Jamarion Robinson, Ronald Greene, and countless others (NAACP, n.d.). American policing is rooted in a racist history that dates back to slave patrols, a legacy that continues to manifest in the disproportionate use of force against Black and Brown Americans (NAACP, n.d.). While police violence is not a new issue, the repeated and highly publicized murders of unarmed Black Americans have sparked renewed urgency for systemic reform, with grassroots movements like Black Lives Matter and Campaign Zero amplifying calls for accountability and change (Subramanian and Arzy, 2021). These movements galvanized public outcry, prompting policymakers and police departments in many American cities to enact a wave of reforms including bans on chokeholds, increased officer oversight, and duty to intervene standards to reduce circumstances in which force is used and severely limit deadly force as an option for officers (Subramanian and Arzy, 2021). Despite this slew of reforms, studies of their measurable impacts have been limited. Using police shootings and killings data from Mapping Police Violence, this paper evaluates the effect of department-level post-2020 reform policies on police shootings and killings in 94 large American cities.

The National Institute of Justice defines police use of force as the "amount of effort required by police to compel compliance by an unwilling subject."[1] Use of force can manifest itself through the employment of physical force, restraint techniques, invasive searches, firearms, police dogs, or the use of devices like tasers (Stainbrook 2023). Since 2015 there have been more than 10,000 fatal shootings by police and between 2017 and 2022, there were 1,064,671 use of force incidents overall across 3,000 jurisdictions in the United States (The Washington Post, 2024; Mapping Police Violence, 2022). Police use of force disproportionately affects low-income and racial or ethnic minority communities (DeAngelis, 2024; Stainbrook, 2023). Those impacted are more likely to experience worsened physical and mental health, increased distrust of law enforcement, and, in the most severe cases, fatal outcomes (Stainbrook, 2023). To minimize use of force in civilian interactions, reforms including neck restraint bans and comprehensive reporting requirements have been enacted at the state and police department levels (Subramanian and Arzy, 2021). Between 2020 and 2021, 30 states passed laws reforming police use of force policies and 21 of the country's 100 largest police departments adopted duty to intervene standards (Subramanian & Arzy, 2021). These reforms aim to address systemic failures in policing practices by increasing accountability, limiting permissible actions, and encouraging alternatives to physical force.

To clearly organize our discussion of the policies, we construct four groups for the seven reforms that we will study with each group corresponding to a policy or a set of policies that are related in purpose. Although our policy data tracks the adoption of eight policies, we do not study the effect of use of force continuums because there was no variance in its adoption in our sample cities after 2020. The "process" category includes requiring warning before using deadly

[1]National Institute of Justice. (2020). *Overview of police use of force.* Retrieved January 22, 2025, from https://nij.ojp.gov/topics/articles/overview-police-use-force

force, requiring de-escalation, and exhausting all alternatives before deadly force. All of these policies are linked to the decision-making process that governs how an officer reacts in a situation that may require force and encourage alternatives. The "ban" category focuses on explicit bans on chokeholds and strangleholds as well as bans on shooting at moving vehicles. These two policies prohibit an action altogether. The "bystander" category only has duty to intervene policies in it because these policies are unique in that they compel officers who are witnessing other officers use excessive force to stop the action. Lastly, the "reporting" category only has comprehensive use of force reporting requirements in it. In this paper, we estimate the effect of each of these policies and various combinations of them.

Previous research indicates that bans on neck restraints, de-escalation requirements, and comprehensive reporting policies have demonstrated positive effects in reducing police killings (Kelsay, Silver, and Butler, 2023). However, findings related to duty to intervene policies are mixed, with some studies reporting contradictory outcomes (Kelsay, Silver, and Butler, 2023; Dawson, Blount-Hill, and Hodge, 2022). Additionally, there is limited peer-reviewed research on the causal impacts of mandates to exhaust all alternatives, bans on shooting at moving vehicles, and requirements to issue warnings before using force, leaving these areas less well understood.

Our contribution to this body of literature will be threefold. First, unlike other papers that have studied groups of policies, we look at the effects of reforms implemented after 2020. As a result, our findings add to the literature by extending existing research on earlier adoption. Second, we go beyond only looking at police killings by analyzing police shootings data, increasing the number of observations and allowing for more of the effect of different policy combinations to be estimated. Third, we evaluate the impact of combinations of policies through

both two-way and three-way interactions that have been largely overlooked in studies that evaluate policies individually (Kelsay, Silver, and Butler, 2023).

Literature Review

High incidences of police use of force are associated with a variety of factors related to both officers and civilians. These factors include lower education and training experience among police officers (Terrill and Mastrofski, 2002; Worden, 1995) and civilians who are more likely to be racial or ethnic minorities as well as poorer and younger individuals (Alpert and Dunham, 2004; Garner, Maxwell, and Heraux 2002; Worden, 1995; Terrill and Mastrofski, 2002). Another body of literature finds that perception impairments (ability to process and respond to civilian actions) faced by officers during fight or flight scenarios contribute to poor use of force decision-making by delaying reactions and diminishing the accuracy of actions for even the most experienced officers (Olson 1989; Westmoreland and Haddock 1989).

A significant body of literature shows that a portion of the policies within the four categories we define have positive effects in reducing police killings and misconduct. Cassino and Demir (2024) analyze a suite of reforms including a ban on chokeholds, restrictions on lethal force, mandatory misconduct reporting standards, and the establishment of an officer certification commission that were implemented through Massachusetts' Police Reform Bill of 2020. They find that the reforms collectively reduced cases of officer misconduct involving the use of force (Cassino and Demir, 2024) . However, their analysis only covers the two-year period (January 2022–December 2023) following the bill's enactment, which may be insufficient to observe the full or long-term effects of the reforms (Cassino and Demir, 2024). Moreover, the study only looks at trends in police misconduct involving use of force before and after the policy

implementation (Cassino and Demir, 2024). Our study uses interaction terms to more rigorously test the joint effects of policies and employs policy variation from 94 cities rather than one.

Kelsay, Silver, and Butler (2023) expand the analysis of police reform by examining the effects of the same seven policies we study, tracked by Campaign Zero, across 66 police departments using police killings data from 2013 to 2019. They employ a Bayesian random intercept model to analyze each policy individually, finding that two "process" policies—de-escalation and exhausting all alternatives—significantly reduce officer-involved deaths (Kelsay, Silver, and Butler, 2023). Additionally, they find that duty to intervene, bans on chokeholds and comprehensive reporting policies individually reduce officer-involved deaths (Kelsay, Silver, and Butler, 2023). However, the timeframe of their analysis does not capture the impact of post-2020 reforms implemented by police departments, which is the focus of our study (Kelsay, Silver, and Butler, 2023). We extend their work by using a different empirical framework to evaluate the impact of the same policies, including interactions between policies in our fixed effects difference in differences regressions, and using police shootings as the outcome variable which provides us with additional outcome observations that could capture more of an effect of different policies.

In the “process” category, evidence from randomized control trials supports Kelsay, Silver, and Butler’s (2023) findings related to de-escalation policies, showing that such policies reduce use of force and improve officer conduct. Engel et al. (2022) and Andersen and Gustafsberg (2016) demonstrate that officers who undergo de-escalation training show improved decision-making and experience reductions in use of force incidents, as well as fewer officer and citizen injuries. However, these trials are limited to individual departments, and the types of de-escalation training tested vary widely, making generalization challenging (Engel et al., 2022;

Andersen and Gustafsberg, 2016). By interacting de-escalation policies with other policies including chokehold bans and comprehensive reporting, we provide a more complete assessment of de-escalation policies' impacts, building on and extending Kelsay, Silver, and Butler's (2023) findings.

For "reporting" policies, Kelsay, Silver, and Butler's (2023) findings align with prior studies showing that increased oversight reduces excessive use of force (Cheng and Long, 2018; Mummolo, 2018; Shjarback, White, and Bishhopp, 2021). For instance, Ba and Rivera (2023) find similar outcomes in Chicago, where more stringent oversight measures resulted in significant reductions in officer misconduct.

In the "ban" category, Beck, Antonelli, and LaScala-Gruenewald (2024) analyze 2,180 U.S. municipalities from 2009 to 2021 and find that neck restraint bans reduce fatal officer-civilian interactions, aligning with Kelsay, Silver, and Butler's (2023) findings. They hypothesize that neck restraint bans signal a preference for minimal uses of force in all circumstances and that the bans have larger impacts because they are likely to be part of larger packages of reforms (Beck, Antonelli, and LaScala-Gruenewald, 2024). The group of control variables we use in our analysis is informed by the control variables used by Beck, Antonelli, and LaScala-Gruenewald (2024) and we further their analysis by evaluating neck restraint bans within a larger group of "ban" policies to mitigate collinearity between individual policies.

While there are positive findings for some policies in the four categories we construct, there is a portion of the literature that is either limited in scope or reports insignificant results (Dawson, Blount-Hill, and Hodge, 2022). In the "bystander" category, Dawson, Blount-Hill, and Hodge (2022) analyze the New York Police Department's duty to intervene standards using a non-equivalent dependent variable design and find only moderate initial reductions in

officer-involved deaths which they largely attribute to the impact of social movements and public pressure at the time of the policy's enactment. This finding mitigates the Kelsay, Silver, and Butler (2023) finding that duty to intervene policies lessen police killings. Our analysis contributes to this literature on duty to intervene policies by providing new evidence that focuses on post-2020 reforms and incorporates interaction terms as well as a comprehensive set of controls to estimate the impact of these policies more robustly.

In the "process" category, Kelsay, Silver, and Butler (2023) find that providing warnings and implementing use of force continuums have no significant effect on reducing police killings. Additionally, our review does not find other peer-reviewed research that examines the causal effects of exhausting alternatives or issuing warnings outside of the Kelsay, Silver, and Butler (2023) study. Notably, no studies interact these policies with other policies as we do. With respect to de-escalation within this category, Todak and James (2018) find, through their observation of 131 police-civilian interactions in Spokane, Washington, that many de-escalation tactics—such as honesty, respect, and humanizing strategies—are already regularly practiced by officers, even without formal de-escalation training. This implies that such training may not introduce new methods to reduce use of force. However, as with previous de-escalation training literature, the kind of training examined and the small sample size limit the ability to generalize these findings (Todak and James, 2018). We contribute to the "process" literature significantly by extending the Kelsay, Silver, and Butler (2023) analysis post-2020 and using fixed effects difference in differences with policy interaction terms.

In the "ban" policy category, there is little peer-reviewed research investigating the causal effects of bans on shooting at moving vehicles. Kelsay, Silver, and Butler (2023) find no

statistically significant effect of such bans on reducing police killings in their analysis of the eight policies evaluated in their study.

Empirical Specification

In our analysis, we estimate the effect of the seven use of force policies on police shootings and killings. As opposed to many previous studies that focus solely on police killings, police shootings were chosen as a key outcome variable because, while their reporting may be less reliable than police killings, they capture a broader range of use of force behaviors that policymakers intend to affect through the policies and still represent a deadly act (Mapping Police Violence, 2024).

To estimate the effects of policies on use of force outcomes, we employ a fixed effects difference in differences model. This design is well-suited for the post-2020 policy reform environment, where cities faced pressure to adopt a wide range of policing reforms in response to nationwide protests. While exact policy implementation dates are not available, we leverage the sharp timing of the 2020 policy wave by treating 2020 as a cutoff and examining whether cities that adopted policies after this turning point show differential changes in police violence outcomes. The difference-in-differences interaction terms allow us to estimate causal effects by comparing each city to itself before and after 2020, and to other cities that did not implement the same policy. The inclusion of city fixed effects controls for unobserved, time-invariant characteristics of cities (e.g., political orientation, policing culture), while year fixed effects capture shocks common to all cities in a given year (e.g., national protests, economic disruptions). This approach assumes that, in the absence of policy reform, treated and untreated cities would have followed parallel trends in police shootings and killings. Though this

assumption is not directly testable, it is supported by the fixed effects structure and robustness checks presented in the appendix.

All standard errors are clustered at the city level to account for potential serial correlation and heteroskedasticity within cities over time. Because cities are the unit of policy adoption and analysis, observations within each city may be correlated across years, which violates the independence assumption of ordinary least squares. Clustering at the city level ensures that inference is robust to arbitrary correlation in the error structure within each city, which is particularly important in panel data settings where treatment is implemented at the city level. In addition, we use heteroskedasticity-robust standard errors to address the possibility that the variance of the error term may differ across cities or years. This dual adjustment improves the validity of hypothesis testing and confidence interval construction in the presence of both within-cluster dependence and non-constant error variance and ensures that the estimated standard errors accurately reflect the uncertainty in our estimates. Thus we can avoid underestimation of standard errors that could lead to overstated statistical significance.

In our model, we select eight controls that are informed by controls used in similar studies of police departments (Kelsay, Silver, and Butler, 2023; Beck, Antonelli, and LaScala-Gruenewald, 2024). Population size scales the number of police shootings by ensuring that differences in outcomes are not simply due to population differences. Crime rates provide a measure of the contextual demand for police interventions, which directly influences the likelihood and frequency of force being used (Beck, Antonelli, and LaScala-Gruenewald, 2024) . Police department size serves as a proxy for law enforcement presence and capacity, which could independently affect the frequency of incidents (Kelsay, Silver, and Butler, 2023). City demographics capture variations in the racial, age, and socioeconomic composition of

communities, factors that have historically been correlated with disparities in policing outcomes (Kelsay, Silver, and Butler, 2023). City economic advantage provides insight into the broader social and economic conditions that shape crime and police interactions, such as poverty, employment, and education levels (Beck, Antonelli, and LaScala-Gruenewald, 2024). Together, these controls ensure that the analysis more closely isolates the true effect of each policy or combination of policies by accounting for other likely influential factors.

Our empirical specifications are as follows:

Baseline DiD Model:

$$Y_{ct} = \sum_{p=1}^{P} \beta_p \cdot (\text{Policy}_{pc} \times \text{Post}_t) + \sum_{k=1}^{K} \gamma_k \cdot X_{kct} + \alpha_c + \lambda_t + \varepsilon_{ct} \quad \textbf{(1)}$$

Interaction Model:

$$Y_{ct} = \sum_{p=1}^{P} \beta_p \cdot (\text{Policy}_{pc} \times \text{Post}_t) + \sum_{q=1}^{Q} \theta_q \cdot (\text{Interaction}_{qc} \times \text{Post}_t) + \sum_{k=1}^{K} \gamma_k \cdot X_{kct} + \alpha_c + \lambda_t + \varepsilon_{ct}$$

(2)

Racial Heterogeneity Model:

$$Y_{ct} = \sum_{p=1}^{P} \beta_p \cdot (\text{Policy}_{pc} \times \text{Post}_t) + \sum_{q=1}^{Q} \theta_q \cdot (\text{Interaction}_{qc} \times \text{Post}_t)$$

$$+ \sum_{p=1}^{P} \delta_p \cdot (\text{Policy}_{pc} \times \text{Post}_t \times pct\_black_{ct}) + \sum_{q=1}^{Q} \psi_q \cdot (\text{Interaction}_{qc} \times \text{Post}_t \times pct\_black_{ct})$$

$$+\sum_{k=1}^{K}\gamma_k \cdot X_{kct} + \alpha_c + \lambda_t + \varepsilon_{ct} \quad \textbf{(3)}$$

Where Y_{ct} is the outcome of interest (police shootings or killings) in city *c* at year *t*, $Policy_{pc}$ is a binary indicator for whether city *c* ever adopts policy *p*, $Post_t$ equals 1 for years ≥ 2020, $Policy_{pc} \times Post_t$ is the DiD treatment term for each policy with β_p being the policy coefficient, $Interaction_{qc}$ represents key combinations of policies, $Interaction_{qc} \times Post_t$ represents the DiD term for interactions of multiple policies with the coefficient being θ_q, X_{kct} are time-varying city-level covariates (e.g., crime rate, unemployment, population) with coefficient γ_k, pct_black_{ct} is the percent of city *c*'s population that is Black in year *t* with δ_p being the coefficient of the pct_black and individual policy interaction and φ_q being the coefficient of the pct_black and multiple policy interaction, α_c represents city fixed effects, λ_t represents year fixed effects, and ϵ_{ct} is the error term.

The models above enable us to measure the effect of policies individually (model 1), in different combinations (model 2), and across racial groups (model 3). This will provide a fuller understanding of the true impact of reforms on police shootings and killings.

Data Description

Outcomes Data

To measure the main outcome of police shootings, we use data from Mapping Police Violence's policedata.org dataset that includes 1,064,671 incidences between 2017 and 2022 of

both fatal and non-fatal use of force including the employment of firearms, tasers, batons, neck restraints, K-9 units, and chemical sprays (Mapping Police Violence, 2024). We further merge this data with data from 2013 to 2017 provided by Campaign Zero's police scorecard. Each observation represents one instance of a specific type of force employed by an officer and is attributed to a state, police department, and year. The data were collected through public records requests to police and sheriff's departments in all 50 states, as well as statewide databases in California, Connecticut, New Jersey, New York, Ohio, and Virginia. It includes data on the types and frequency of force used, along with demographic details of those involved (Mapping Police Violence, 2024). The data covers 3,000 police jurisdictions that cover 60% of the U.S. population (Mapping Police Violence, 2024). The key outcome variable that we use from the dataset is police shootings per city and year. We also use police killings data from Mapping Police Violence's police killings database that aggregates yearly killings data by city with data from 2013 to 2023.

Policy Data

The basis of my department-level policy data will be the 8cantwait dataset of policies that the 100 largest police departments in the United States changed following George Floyd's murder in 2020 (Campaign Zero, 2021). 8cantwait tracks the implementation of eight reforms that they identified as critical to reducing use of force: requiring de-escalation, requiring a use of force continuum, banning or restricting neck restraints, requiring warnings before shooting, banning shooting at moving vehicles, exhausting all alternatives, instituting duty to intervene, and requiring comprehensive reporting (Campaign Zero, 2021). The data on dates of policy implementation are limited given restrictions in the dataset provided by 8cantwait. The given dataset only contains information on whether police departments implemented each of the eight

policies before or after 2020, but does not contain exact dates of policy changes. After matching cities present in the outcomes and policy data, the sample is narrowed to 94 cities with police killings as the outcome. Given additional missing data in the policedata.org database, the sample is narrowed to 86 cities with police shootings as the outcome.

Controls

Our analysis incorporates a comprehensive set of control variables that is informed by prior research on police use of force reforms across large cities (Kelsay, Silver, and Butler, 2023; Beck, Antonelli, and LaScala-Gruenewald, 2024). Data on city traits including population size, and demographics (racial and age), and economic advantage (employment rates, education levels, and median income) are sourced from the American Community Survey (ACS), providing a detailed view of city-level (central city) characteristics. Crime rate data is obtained from the FBI Uniform Crime Reporting Program (UCR), while information on police department size comes from the Law Enforcement Management and Administrative Statistics (LEMAS). We linearly interpolate to fill in missing values of police department size and drop crime rate values that fell two standard deviations outside the mean of each city's crime rate to exclude extreme deviations. These controls allow us to better isolate the effect of each reform and improve the precision of our estimates.

Summary Statistics

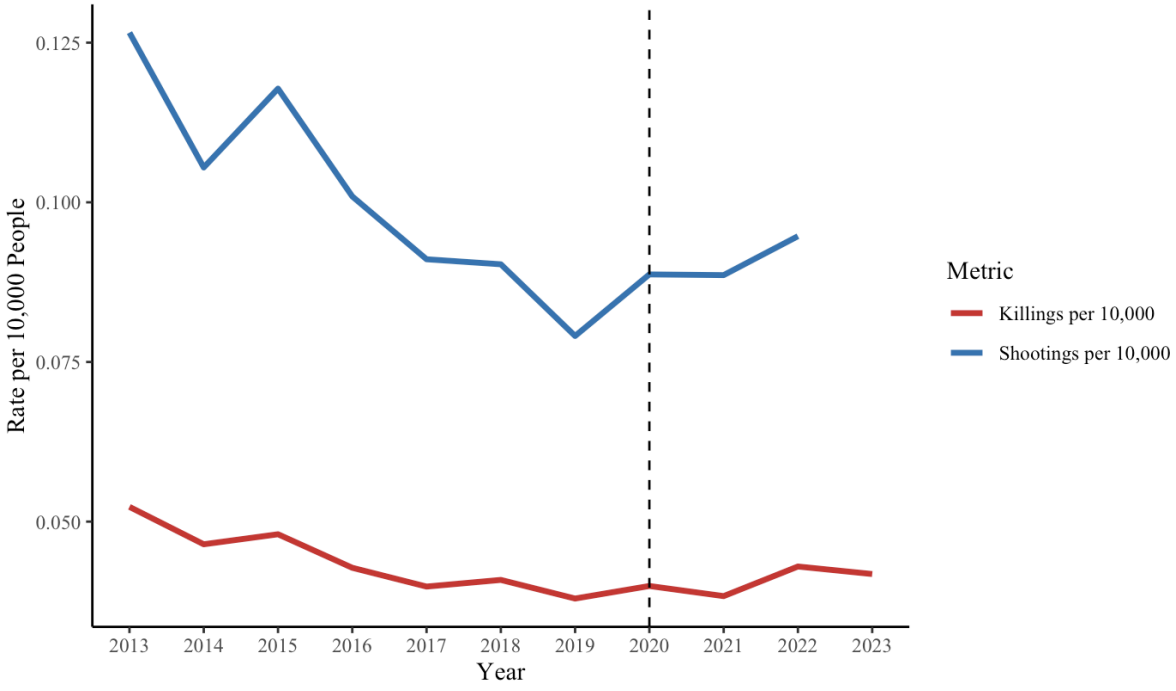
Table 1: Descriptive statistics of the means of outcome, policy, and control variables by city (years 2013-2023)

City Mean (2013-2023)	Mean	Std. dev.	Min.	25th	Median	75th	Max.
Outcomes							
Police killings	2.85	2.81	0.00	1.09	2.00	3.55	16.82
Police shootings	6.59	7.32	0.00	2.50	4.12	8.20	41.80
Policies							
Total policies	1.93	1.61	0.00	0.00	2.00	3.00	6.00
Deescalation	0.38	0.49	0.00	0.00	0.00	1.00	1.00
Chokehold ban	0.64	0.48	0.00	0.00	1.00	1.00	1.00
Duty to intervene	0.37	0.49	0.00	0.00	0.00	1.00	1.00
Warning	0.16	0.37	0.00	0.00	0.00	0.00	1.00
Shooting at moving vehicles ban	0.11	0.31	0.00	0.00	0.00	0.00	1.00
Reporting	0.15	0.36	0.00	0.00	0.00	0.00	1.00
Exhaust alternatives	0.12	0.32	0.00	0.00	0.00	0.00	1.00
Controls							
Total population	664963.55	978808.49	220637.18	284658.27	398820.09	659801.55	8457015.36
Median age	34.99	2.73	29.26	33.49	34.63	35.98	47.82
Black pop (%)	20.09	16.68	0.45	6.26	15.33	28.19	78.16
Non-White Other pop (%)	25.27	14.88	8.41	13.51	22.11	31.64	80.57
Median HH income	69644.78	19663.44	35424.36	57358.73	66992.86	75988.91	153984.82
Bachelors educ (%)	36.44	11.72	15.31	29.36	34.56	43.76	68.45
Unemployed (%)	6.35	2.01	3.28	5.17	5.92	7.17	16.29
Violent crimes per 10,000	75.26	41.85	6.18	44.81	70.10	98.61	208.25
Total officers per 10,000	21.87	10.55	7.51	14.73	18.31	26.23	56.60
Observations	94						

The summary statistics in Table 1 were calculated with the within-city means of each variable across the time period we study (2013-2023). In the outcome statistics, it is apparent that there is significantly more variance in the number of shootings compared to the number of killings. While the difference between the 25th and 75th percentiles for killings is less than three, the same difference for shootings is greater than five for police shootings. This suggests that there is likely more variance in police shootings from year to year that we can exploit by using it as an additional outcome variable. The total policies mean of 1.93 and median of 2 indicates that most cities adopted more than one of the seven policies we studied following 2020. This supports our use of interaction terms to understand the joint effect of policies given that they are often implemented in combinations rather than alone. Appendix Table C.7 contains the same

descriptive statistics calculated using the 1,034 city-year observations as opposed to the means we use in Table 1.

Figure 1: Trends in police shootings and killings per 10,000 people in the 94 city sample (years 2013-2023)



In Figure 1, we observe trends in police shootings and killings per 10,000 people in our combined sample of 94 cities. For both outcomes, we observe a steady decline from 2013 to 2019. Shootings decline from .127 in 2013 to .079 in 2019 while killings drop from .052 to .038. In the following years, which include our treatment period, both outcomes increase in frequency.

Figure 2: Number of departments implementing each policy before or after 2020

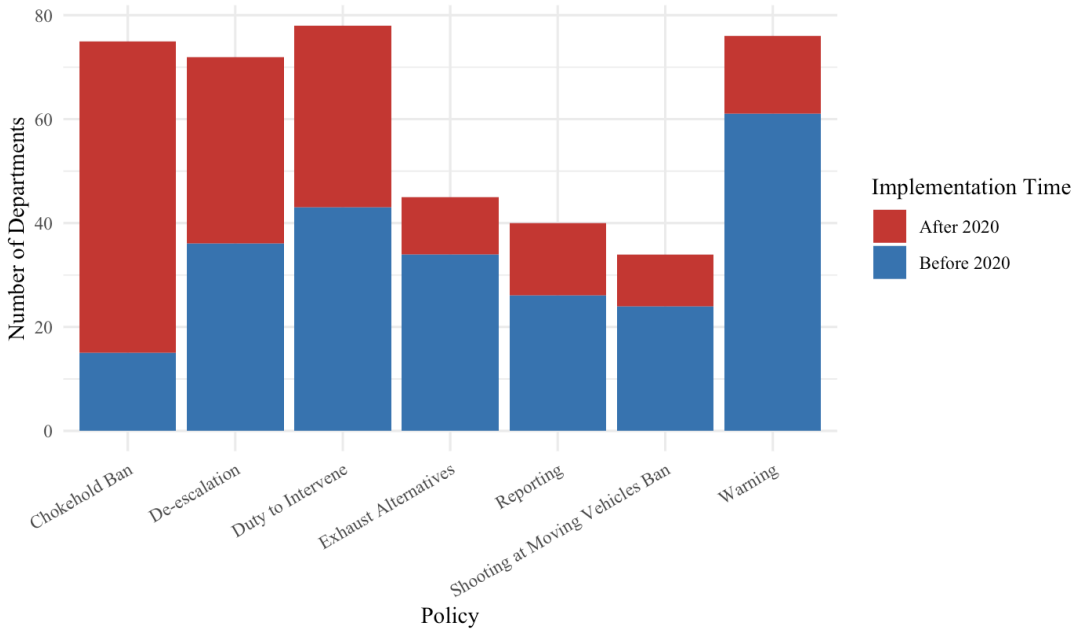
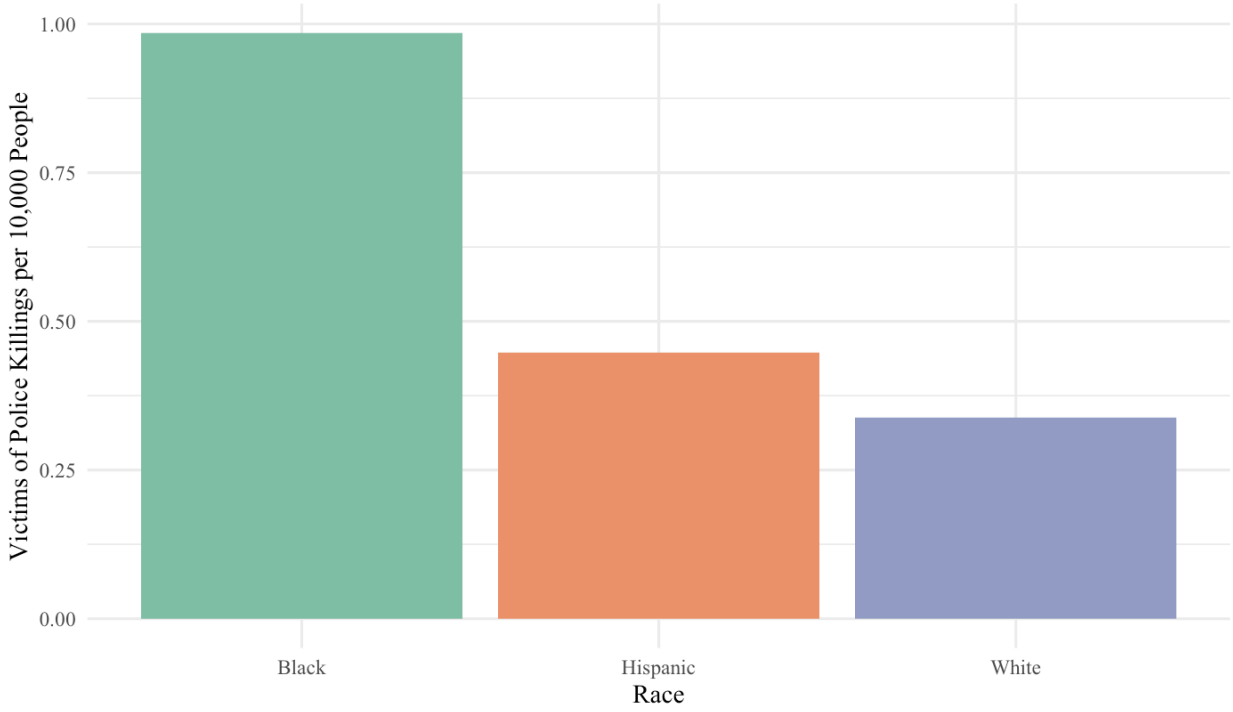


Figure 2 shows policy implementation by sample departments before and after 2020, revealing that chokehold bans are by far the most widely adopted post-2020 policy. In our 94 city sample, 60 departments implemented chokehold bans after 2020, followed by 36 departments implementing de-escalation policies and 35 adopting duty to intervene standards. The widespread implementation of chokehold bans results in the policy featuring prominently in the two-way policy interactions that we test.

Figure 3 displays the police killings per 10,000 people split by race over our sample period. The figure makes abundantly clear that police killings disproportionately impact the Black community. Both in raw count and as a proportion of each race's population, Black individuals are overrepresented in the victims of police killings. Over the sample period (2013–2023), 0.985

Black individuals per 10,000 were killed by police, more than twice the rate of 0.339 White individuals per 10,000. Both the coefficients on our race controls and the interaction terms involving the share of the population that is Black provide additional insight into the disproportionate impact of police violence on Black communities.

Figure 3: Police killings per 10,000 by race in 94 sample cities combined (years 2013-2023)



Results

In this section, I begin by discussing the three analytical strategies I considered in addition to the final interaction-based approach. I then discuss the process by which I selected interactions to test along with the results of models with potential explanations split by police shootings and killings outcomes. Before proceeding with any model, I conducted a Hausman test to determine the appropriate structure for the data. The test, found in Appendix Tables A.1 and A.2 supported

the use of fixed effects over random effects, confirming the importance of controlling for unobserved heterogeneity across cities and years.

Before measuring the joint effect of policies through interaction terms, I estimated models that tested policies in an index, through principal components analysis, and individually through my baseline regression.

First, I tested an index-based approach, combining the binary policy indicators into a simple count of how many reforms a city had implemented. However, this strategy failed to yield significant results. One likely explanation is that different policies have opposing effects—some may reduce police violence while others have no effect or even increase reported police violence. When these are aggregated into a single index, their effects are combined, making the index an unreliable measure of reform strength. Furthermore, not all policies are equally important; weighting them equally in an index may obscure the impact of more influential combinations. I then applied principal components analysis (PCA) in an attempt to reduce the dimensionality of the policy space and capture underlying reform patterns. However, PCA identifies patterns of variation among predictor variables without regard to the outcome variable and generates components that explain variance in the policy inputs, not in the dependent variable. Moreover, the binary nature of the policy indicators and their relatively weak correlations further undermined PCA's utility. The resulting components were not substantively meaningful or statistically significant in explaining police violence. These limitations made PCA an ineffective approach, and pointed to the need for a more theory-driven, interaction-based modeling strategy.

Next, in my baseline regression, I test the effects of each policy individually on police shootings and killings (Tables 2 and 3, column 1). As expected, none of the policy indicators

were statistically significant on their own. This makes conceptual sense for two reasons. First, many of these policies were likely designed to work in conjunction with others. For instance, de-escalation policies may only change officer behavior when paired with stricter use-of-force restrictions, and reporting requirements may only be meaningful if departments are also banned from using high-risk tactics. Second, most departments implemented more than one policy after 2020, making measurement of individual effects difficult. Of the 70 departments that implemented one of the seven policies after 2020, 51 of them implemented more than one policy and the average number of policies implemented across departments is 1.93 with a median of 2. This suggests that the effects of policies should be studied together rather than independently.

Interaction modeling was therefore a natural next step because many of the policies studied are likely interdependent as indicated by their concurrent implementation, and their effects are likely to be conditional on one another. With the seven policies we analyze, there are 21 possible two-way interactions, and we select only those that are present in at least 10% of post-2020 city-year observations to mitigate the possibility of spurious findings. Although seven such interactions met this threshold, two were excluded because, in each of those cases, one policy (warning before shooting and shooting at moving vehicles ban) was only implemented in cities that also adopted the other policy (chokehold bans). As a result, their estimated individual effects actually capture only the additional impact of these policies beyond that of chokehold bans, rather than their independent effects.

Our interaction analysis process began with Appendix Tables B.1 and B.3, where we individually tested the five two-way interactions that met our threshold and narrowed our focus to the two significant two-way interactions (Chokehold ban X De-escalation and Chokehold ban X Reporting). Then we tested the robustness of our narrowed findings by putting both significant

interaction terms in the same regression (Tables 2 and 3, column 2) and putting all 21 possible two-way interactions in the same regression (Appendix Table B.5). Lastly, we estimated a model with a three-way interaction (Chokehold ban X Reporting X De-escalation) that includes the policies from the significant two-way interactions. Overall, after narrowing, testing robustness, and including a three-way interaction, the results of our investigation of various policy interactions reported below finds that de-escalation, comprehensive reporting, and chokehold bans are associated with significant reductions in police shootings and killings.

Appendix Tables B.1 and B.3 report the findings of regressions with each of the five interactions that meet our threshold. In columns 1 and 2 of both tables, the interactions between chokehold bans and reporting and between chokehold bans and de-escalation are highly significant ($p < 0.01$) and negative, while the other three interactions are insignificant. Although the individual de-escalation term is significant and positive in column 1 and the reporting term is significant and positive in column 2, the combined effect with the negative interaction term (Chokehold ban X De-escalation and Chokehold ban X Reporting) is significantly negative for both interaction terms on police shootings and killings. The individual positive terms indicate that the respective policies without chokehold bans are associated with higher police shootings and killings. The significant and positive reporting term is likely because comprehensive reporting policies result in higher reported incidences of police shootings and killings rather than actual increases in the two outcomes. Overall, this set of regressions indicated that a combination of chokehold bans, de-escalation, and comprehensive reporting policies appeared to be linked to a significant reduction in both police shootings and killings. Next, we split our discussion of the three policies' joint effects by outcome with police shootings first and killings second.

Table 2: Policy effects on police shootings by city in fixed effects models (years 2013-2022)

	(1) Base	(2) Two-way Interactions	(3) Chokehold ban X Reporting X De-escalation	(4) Differential Effects by Race
Policies				
De-escalation	-0.460	2.333*	2.348*	14.435***
	(0.855)	(1.200)	(1.216)	(2.796)
Duty to intervene	0.959	0.605	0.462	0.220
	(0.697)	(0.631)	(0.624)	(0.655)
Chokehold ban	-0.011	1.080	0.761	2.070**
	(1.136)	(1.075)	(1.125)	(0.979)
Warning	-0.189	0.250	0.209	0.094
	(0.823)	(0.734)	(0.708)	(0.654)
Exhaust alternatives	-0.050	-0.736	-0.781	-1.010
	(1.309)	(1.144)	(1.103)	(1.096)
Reporting	-0.844	5.814***	-	-119.655***
	(1.329)	(1.652)	-	(12.722)
Shooting at moving vehicles ban	-0.645	-0.587	-0.407	-0.170
	(1.052)	(0.987)	(0.944)	(0.702)
Chokehold ban X De-escalation		-3.709***	-3.171**	-17.136***
		(1.260)	(1.347)	(2.948)
Chokehold ban X Reporting		-7.598***	-0.710	119.843***
		(2.023)	(1.492)	(12.361)
De-escalation X Reporting			5.972***	
			(1.662)	
Chokehold ban X Reporting X De-escalation			-8.331***	
			(2.617)	
Black X De-escalation				-2.868***
				(0.602)
Black X Chokehold ban				-0.058
				(0.072)
Black X Reporting X Chokehold ban				-5.434***
				(0.454)
Black X Reporting				5.315***
				(0.459)
Black X De-escalation X Chokehold ban				2.963***
				(0.620)
Controls				
Total population	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Median age	-0.297	-0.313	-0.293	-0.277
	(0.238)	(0.238)	(0.242)	(0.232)
Black pop (%)	0.359**	0.423**	0.428**	0.375**
	(0.166)	(0.169)	(0.170)	(0.173)
Non-White Other pop (%)	0.047	0.050	0.047	0.032
	(0.032)	(0.033)	(0.034)	(0.034)
Median HH income	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Bachelors educ (%)	0.033	0.027	0.019	0.032
	(0.139)	(0.138)	(0.135)	(0.126)
Unemployed (%)	0.208	0.216*	0.221*	0.199
	(0.131)	(0.128)	(0.128)	(0.135)
Violent crimes per capita	97.431	132.636	128.813	94.017
	(137.303)	(127.097)	(127.954)	(129.747)
Total officers per capita	762.071	988.511	905.485	837.531
	(870.063)	(811.819)	(816.936)	(757.817)
Observations	829	829	829	829
Departments	86	86	86	86
Adjusted R-squared	0.778	0.782	0.782	0.784

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As a robustness check, in column 2 of Table 2, we put both significant two-way interactions (Chokehold ban X De-escalation and Chokehold ban X Reporting) into the same regression and found that with police shootings as the outcome, both terms remain significant and negative. For completeness, Appendix Table B.5 presents results including all 21 possible two-way interactions between the seven policies in a single regression and finds that both interaction terms (Chokehold ban X De-escalation and Chokehold ban X Reporting) remain significant and negative with police shootings as the outcome. These checks strengthen our confidence that chokehold bans combined with either de-escalation or reporting policies are associated with reductions in police shootings.

After identifying that chokehold bans, de-escalation, and comprehensive reporting policies appear to be associated with significant reductions in police shootings, we then tested the joint effect of all three policies in a three-way interaction term. The results are reported in column 3 of Table 2. Notably, the variable for comprehensive reporting was omitted in these regressions because of multicollinearity given that comprehensive reporting was always implemented with either de-escalation or chokehold bans. The multicollinearity supports the idea that these policies are adopted together in many jurisdictions and further justifies the use of interaction terms. With police shootings as the outcome, the three-way interaction term is highly significant ($p<0.01$) and negative while the interaction between chokehold bans and de-escalation remains significant and negative. Even with the significant and positive coefficient of the interaction between de-escalation and comprehensive reporting policies, the overall effect of the three policies together remains significantly negative. In Table 3, we consider the same regressions, but with police killings as the outcome.

Table 3: Policy effects on police killings by city in fixed effects models (years 2013-2023)

	(1) Base	(2) Two-way Interactions	(3) Chokehold ban X Reporting X De-escalation	(4) Differential Effects by Race
Policies				
De-escalation	0.206	2.310***	2.299***	3.611***
	(0.351)	(0.527)	(0.529)	(1.080)
Duty to intervene	0.494	0.398	0.349	0.124
	(0.344)	(0.329)	(0.329)	(0.320)
Chokehold ban	0.226	0.599	0.419	0.716
	(0.382)	(0.365)	(0.352)	(0.507)
Warning	-0.614	-0.483	-0.539	-0.433
	(0.471)	(0.461)	(0.466)	(0.404)
Exhaust alternatives	0.569	0.373	0.340	0.353
	(0.683)	(0.671)	(0.641)	(0.580)
Reporting	-0.072	0.639	-	2.228
	(0.545)	(0.820)	-	(4.091)
Shooting at moving vehicles ban	-0.316	-0.267	-0.112	-0.187
	(0.414)	(0.420)	(0.451)	(0.378)
Chokehold ban X De-escalation		-2.436***	-2.109***	-4.492***
		(0.604)	(0.655)	(1.107)
Chokehold ban X Reporting		-0.951	0.339	-1.760
		(0.943)	(0.841)	(4.019)
De-escalation X Reporting			0.731	
			(0.837)	
Chokehold ban X Reporting X De-escalation			-2.079	
			(1.516)	
Black X De-escalation				-0.299
				(0.260)
Black X Chokehold ban				-0.006
				(0.017)
Black X Reporting X Chokehold ban				-0.291
				(0.213)
Black X Reporting				0.238
				(0.214)
Black X De-escalation X Chokehold ban				0.349
				(0.257)
Controls				
Total population	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Median age	-0.080	-0.094	-0.085	-0.052
	(0.104)	(0.104)	(0.105)	(0.102)
Black pop (%)	0.064	0.080	0.078	0.090*
	(0.052)	(0.051)	(0.052)	(0.052)
Non-White Other pop (%)	0.007	0.004	0.003	0.000
	(0.013)	(0.013)	(0.013)	(0.012)
Median HH income	-0.000*	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Bachelors educ (%)	0.012	0.009	0.003	0.008
	(0.041)	(0.042)	(0.041)	(0.043)
Unemployed (%)	0.031	0.034	0.039	0.051
	(0.055)	(0.054)	(0.054)	(0.053)
Violent crimes per capita	11.978	16.881	13.070	-8.834
	(42.108)	(40.809)	(43.013)	(42.077)
Total officers per capita	248.728	291.208	252.474	208.992
	(404.922)	(397.607)	(398.393)	(398.578)
Observations	1006	1006	1006	1006
Departments	94	94	94	94
Adjusted R-squared	0.695	0.697	0.698	0.700

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In column 2 of Table 3, we test the robustness of the two significant interactions by putting them in the same regression with police killings as the outcome. Here, we find that while Chokehold ban X De-escalation is significant and negative, Chokehold ban X Reporting is insignificant. Although this might indicate unclear findings for the Chokehold ban X Reporting interaction term, our second robustness check (Appendix Table B.5) suggests that the term may still remain significant and negative. In Appendix Table B.5, with all 21 possible two-way interactions in the regression, both the Chokehold ban X De-escalation and Chokehold ban X reporting interactions are significant and negative. This indicates that the effect of chokehold bans and reporting is conditional not only on each other, but also on the presence of other interacting policies like de-escalation. In other words, the full context of reform appears to matter: chokehold bans and reporting may only reduce police shootings when embedded in a broader environment of reform, particularly one that also includes de-escalation strategies. Statistically, this also reflects how interaction terms can absorb overlapping variance and suggests that including all relevant interactions could help more precisely isolate the independent effect of each combination. At minimum, these checks provide additional support for our finding that chokehold bans implemented with de-escalation policies are associated with declines in police killings. They also suggest that when a broader policy context is accounted for, chokehold bans and reporting policies may be associated with reductions in police killings.

In column 3 of Table 3, we test the joint effect of chokehold bans, de-escalation, and comprehensive reporting with police killings as the outcome. Again, the variable for comprehensive reporting was omitted in these regressions because of multicollinearity. In this model, only the interaction between chokehold bans and de-escalation remains highly significant and negative while the other terms are insignificant. This either could indicate that the joint

effect of the three policies is weaker on police killings or be the result of other factors including the smaller variation in police killings across our sample compared to police shootings.

Next, we provide a few potential explanations for the consistent and significant negative effect of various combinations of chokehold bans, de-escalation, and comprehensive reporting policies on police shootings and killings. We also explain why findings appear to be more mixed and uncertain with police killings as the outcome.

The significant and negative effect of combining comprehensive reporting, de-escalation requirements, and chokehold bans suggests that these policies are most effective when implemented as a coordinated reform package. As Beck, Antonelli, and LaScala-Gruenewald (2024) argue, neck restraint bans may function as symbolic signals that minimal force is expected from officers, especially when accompanied by other measures that reinforce a shift in departmental norms. When paired with mandated de-escalation, this signal becomes more powerful, jointly conveying a departmental commitment to reducing use of force. While de-escalation alone may have heterogeneous effects, potentially due to wide variation in training content and policy strength, its pairing with force-restricting reforms like chokehold bans appears to clarify expectations and change behavior. Engel et al. (2022) and Andersen and Gustafsberg (2016) provide the possible explanation that de-escalation training improves officer decision-making and reduces both use-of-force incidents and injuries, suggesting that skill-based reforms can work synergistically with policy constraints. Comprehensive reporting requirements, meanwhile, may increase reported use-of-force incidents when implemented in isolation due to heightened transparency and documentation. However, when layered onto chokehold bans and de-escalation requirements, reporting appears to help institutionalize oversight and accountability. This aligns with a growing body of research showing that institutional oversight

reduces excessive force (Cheng and Long, 2018; Mummolo, 2018; Shjarback, White, and Bishhopp, 2021). Our findings are consistent with those of Kelsay, Silver, and Butler (2023), who find that chokehold bans, comprehensive reporting, and de-escalation policies reduce officer-involved deaths and extends their work by analyzing post-2020 reforms and employing police shootings as an outcome variable. Together, this suggests that while individual reforms may have limited or inconsistent effects, their combined implementation can produce meaningful reductions in police violence. It is also important to note that chokehold bans and de-escalation policies were the two most commonly implemented reforms by police departments following 2020. As a result, they were present in a larger number of city-year observations, providing greater statistical power to detect potential treatment effects. In contrast, policies such as requiring officers to exhaust all other alternatives or banning shooting at moving vehicles appeared in fewer than one-third as many city-year observations, limiting the ability to assess their effects with the same level of precision.

The findings related to police killings are less consistent and statistically robust than those for police shootings likely due to fundamental differences in the nature of the two outcomes. While police shootings occur more frequently and thus provide a larger and more variable sample for statistical analysis, police killings represent a smaller subset of those incidents. This lower frequency results in less variation across city-year observations and reduces the statistical power to detect significant effects. Additionally, whether a shooting results in death may depend on factors beyond the scope of the policy reforms studied, such as the proximity of emergency medical care, the type of weapon used, or the location of injury. These sources of random variation introduce noise into the fatality data that may obscure the underlying impact of reform

policies. As a result, although the direction of the effects for killings often aligns with those for shootings, the estimates are generally less precise and should be interpreted with greater caution.

Finally, we study the potential differential impact of policies depending on race by interacting the policies we found to have significant effects on both outcomes (Chokehold ban X De-escalation and Chokehold ban X Reporting) with the pct_black variable, which measures the percent of a city's population that is Black in a given year.[2] Before discussing the results, it is important to note that in every regression run with police shootings as the outcome in Table 2, along with the full two-way interaction robustness check regressions in Appendix Table B.5, the pct_black variable coefficient is significant and positive. This indicates that in cities with greater proportions of Black residents, there are more police shootings. The finding is not surprising given the extensive literature that finds Black communities disproportionately face incidences of force and police violence (DeAngelis, 2024; Stainbrook, 2023).

While results for pct_black interaction terms in column 4 of Table 3 are insignificant, suggesting no measurable differential policy effects on police killings across racial groups, results in column 4 of Table 2 indicate that the effectiveness of certain police reforms on police shootings does vary in cities with higher proportions of Black residents. Specifically, we examine how the effects of de-escalation policies and reporting requirements—both alone and in combination with chokehold bans—differ based on the share of the Black population. For de-escalation, we find a significant and negative interaction between Black population share and de-escalation (–2.868), suggesting that de-escalation policies are associated with fewer police shootings in cities with higher percentages of Black residents. However, this effect is offset by a

[2] We chose to interact pct_black with the two-way policy interactions because this approach allows us to examine how the effect of key policy combinations varies with racial composition while maintaining greater interpretability. Three-way interactions with pct_black suffer from sparse data issues, as relatively few cities implement all three policies simultaneously and the large number of interactions hinders the interpretability of results.

significant and positive coefficient on the Black X De-escalation X Chokehold Ban three-way interaction (2.963), implying that when chokehold bans are also in place, the marginal benefit of de-escalation in Black communities diminishes. In contrast, for reporting policies, we observe the opposite pattern: the Black X Reporting interaction is significant and positive (5.315), indicating a potential increase in reported shootings associated with reporting alone in cities with a higher Black population percentage, but this is more than offset by a significant and negative three-way interaction (Black X Reporting X Chokehold Ban, –5.434), suggesting that the combination of reporting and chokehold bans is particularly effective in reducing shootings in cities with proportionally larger Black communities. Taken together, these findings highlight that the impact of individual reforms may differ depending on context, and that certain combinations of policies may be more effective at reducing police violence in cities with proportionally larger Black populations.

However, it is important to view these findings with caution given that many of the coefficients that were consistently significant and negative in earlier models became significant and positive in the model with pct_black interactions. While reporting flipped from being significant and positive to significant and negative with a large coefficient (-119.655), the interaction between reporting and chokehold bans flipped from being significant and negative to significant and positive with a large coefficient (119.843). In addition, the chokehold ban coefficient is significant and positive (2.070) in this regression when it had been insignificant in previous regressions. Due to these large changes in coefficient directions and magnitudes, it is difficult to put significant weight into these regression results. Overall, these findings are suspect, but may suggest that while some policies are less effective in cities with greater Black populations, other policies might be more effective in cities with greater Black populations.

In sum, the results support a view of police reform as a context-sensitive process that often relies on combinations of policies to achieve significant reductions in lethal uses of force. Individual policies appear ineffective in isolation, but we identify combinations of policies involving chokehold bans, de-escalation, and comprehensive reporting are associated with meaningful reductions in police shootings and killings. These effects are most apparent when examining shootings rather than killings, likely due to greater statistical power and the underlying variability of the outcomes.

Our study was limited by a number of factors. First, our policy data only reported whether or not a policy had been implemented after 2020. Without a precise year of implementation, it is more difficult to estimate the true effects of policy adoption with difference in differences modeling. Second, staggered adoption of the policies as well as heterogeneous treatment effects, pose a problem for traditional fixed effects difference in differences models which cannot adjust well for these policy realities. With our model, we had to make the parallel trends assumption, which even despite the fixed effects and controls, is likely not ironclad in the context of police use of force policies. Lastly, our sample only studies large American cities, so our findings are not applicable to rural jurisdictions.

Conclusion

Police departments in American cities adopted combinations of use of force reforms following 2020 with the intention of reducing violent and fatal uses of force. This study sought to better understand the actual effects of these reforms on police shootings and killings. We use

fixed effects difference in differences models as well as interaction terms to measure the effects of policies both individually and in multiple groupings. Our findings suggest that chokehold bans, de-escalation policies, and comprehensive reporting reforms, reduce police shootings when implemented together while findings with regards to police killings are more mixed, but largely indicate that combinations of these policies achieve reductions in killings as well. In line with previous research, we propose that chokehold bans serve as a departmental signal favoring minimal force, de-escalation training reinforces this shift through changes in officer behavior, and comprehensive reporting institutionalizes oversight and accountability. Together, these reforms help establish a clear and enforceable standard for use-of-force conduct. Our regressions testing differential effects based on race were mixed, leaving it difficult to make strong conclusions about whether there are true differential impacts of the policies we analyze. Future studies could take advantage of a longer time period as well as more specific policy adoption data to employ a method like synthetic difference in differences or synthetic control modeling to overcome the limitations of our model and more specifically isolate the effects of reforms.

Overall, we find that individual reforms or isolated policy types are unlikely to produce substantial results. Instead, meaningful change likely comes from reforms that collectively shift departmental culture through enforced constraints, training, and oversight. Often in political contexts, any reform is touted as a victory. But when human life is in the balance, effective policy matters. Our findings suggest that coordinated sets of reforms that challenge deep-seated departmental norms are most likely to produce sustained changes in officer behavior and reductions in police shootings. Policing and violence will always be inextricably linked, as communities of color know all too well, but it is incumbent on police departments to relentlessly implement and enforce policies to reduce unnecessary uses of force in every circumstance.

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Appendix A

Table A.1: Hausman test results of police shootings regressions with individual policies and the five interactions that fit selection criteria

Model Name	Chi-Square	P-Value
7 Policies	17.928	0.083
Deescalation X Chokehold ban	17.366	0.098
Reporting X Chokehold ban	19.535	0.052
Duty to Intervene X Chokehold ban	15.765	0.150
Duty to intervene X Deescalation	18.696	0.096
Duty to intervene X Warning	20.648	0.037

Table A.2: Hausman test results of police shootings regressions with individual policies and the five interactions that fit selection criteria

Model Name	Chi-Square	P-Value
7 Policies	25.278	0.008
Deescalation X Chokehold ban	27.858	0.006
Reporting X Chokehold ban	27.236	0.007
Duty to Intervene X Chokehold ban	27.460	0.007
Duty to intervene X Deescalation	26.378	0.009
Duty to intervene X Warning	45.142	0.000

Appendix B

Table B.1: Joint policy effects of five two-way interactions on police shootings by city in fixed effects models (years 2013-2022)

Policies	(1) 7 Policies	(2) Deescalation X Chokehold ban	(3) Reporting X Chokehold ban	(4) Duty to intervene X Chokehold ban	(5) Duty to intervene X Deescalation	(6) Duty to intervene X Warning
Deescalation	-0.460	5.869**	-1.146	-0.486	-0.583	-0.453
	(0.855)	(2.311)	(0.755)	(0.840)	(1.025)	(0.843)
Duty to intervene	0.959	0.732	0.610	1.506	0.863	1.191*
	(0.697)	(0.647)	(0.628)	(1.355)	(0.813)	(0.716)
Chokehold ban	-0.011	0.985	0.840	0.292	-0.003	-0.003
	(1.136)	(1.076)	(1.042)	(1.291)	(1.135)	(1.129)
Warning	-0.189	0.091	0.245	-0.107	-0.197	0.986
	(0.823)	(0.734)	(0.740)	(0.805)	(0.832)	(1.727)
Exhaust alternatives	-0.050	-0.389	-0.804	-0.048	-0.088	-0.192
	(1.309)	(1.171)	(1.146)	(1.277)	(1.310)	(1.294)
Reporting	-0.844	-1.420	9.401***	-0.893	-0.811	-0.757
	(1.329)	(1.178)	(1.591)	(1.297)	(1.354)	(1.363)
Shooting at moving vehicles ban	-0.645	-0.595	-0.598	-0.707	-0.605	-0.886
	(1.052)	(1.010)	(0.981)	(1.065)	(1.037)	(1.109)
Deescalation X Chokehold ban		-7.203***				
		(2.404)				
Reporting X Chokehold ban			-11.193***			
			(1.864)			
Duty to intervene X Chokehold ban				-0.809		
				(1.557)		
Duty to intervene X Deescalation					0.261	
					(1.249)	
Duty to intervene X Warning						-1.744
						(2.030)
Observations	829	829	829	829	829	829
Departments	86	86	86	86	86	86
Adjusted R-squared	0.778	0.781	0.782	0.778	0.778	0.778

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Controls coefficients from fixed effects regression on police shootings in Table B.1

Controls	(1) 7 Policies	(2) Deescalation X Chokehold ban	(3) Reporting X Chokehold ban	(4) Duty to intervene X Chokehold ban	(5) Duty to intervene X Deescalation	(6) Duty to intervene X Warning
Total population	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Median age	-0.297	-0.324	-0.300	-0.307	-0.291	-0.300
	(0.238)	(0.239)	(0.237)	(0.241)	(0.243)	(0.239)
Black pop (%)	0.359**	0.399**	0.423**	0.356**	0.361**	0.359**
	(0.166)	(0.167)	(0.169)	(0.166)	(0.163)	(0.166)
Non-White Other pop (%)	0.047	0.034	0.061*	0.048	0.048	0.044
	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.033)
Median HH income	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Bachelors educ (%)	0.033	0.037	0.021	0.037	0.032	0.037
	(0.139)	(0.139)	(0.137)	(0.139)	(0.138)	(0.138)
Unemployed (%)	0.208	0.211	0.217*	0.210	0.208	0.209
	(0.131)	(0.128)	(0.129)	(0.131)	(0.131)	(0.132)
Violent crimes per capita	97.431	108.580	140.834	102.144	98.821	91.658
	(137.303)	(126.145)	(127.346)	(134.323)	(135.641)	(136.964)
Total officers per capita	762.071	967.434	939.860	790.092	751.412	835.752
	(870.063)	(809.650)	(816.887)	(846.503)	(876.986)	(857.192)
Observations	829	829	829	829	829	829
Departments	86	86	86	86	86	86
Adjusted R-squared	0.778	0.781	0.782	0.778	0.778	0.778

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Joint policy effects of five two-way interactions on police killings by city in fixed effects models (years 2013-2023)

Policies	(1) 7 Policies	(2) Deescalation X Chokehold ban	(3) Reporting X Chokehold ban	(4) Duty to intervene X Chokehold ban	(5) Duty to intervene X Deescalation	(6) Duty to intervene X Warning
Deescalation	0.206	2.751***	0.039	0.203	0.075	0.213
	(0.351)	(0.519)	(0.346)	(0.350)	(0.474)	(0.355)
Duty to intervene	0.494	0.411	0.404	0.641	0.382	0.360
	(0.344)	(0.332)	(0.328)	(0.550)	(0.381)	(0.319)
Chokehold ban	0.226	0.596	0.434	0.297	0.237	0.212
	(0.382)	(0.367)	(0.368)	(0.446)	(0.386)	(0.382)
Warning	-0.614	-0.505	-0.481	-0.591	-0.617	-1.275
	(0.471)	(0.456)	(0.464)	(0.483)	(0.469)	(1.175)
Exhaust alternatives	0.569	0.419	0.333	0.566	0.531	0.646
	(0.683)	(0.660)	(0.665)	(0.672)	(0.717)	(0.652)
Reporting	-0.072	-0.277	2.976***	-0.078	-0.041	-0.111
	(0.545)	(0.518)	(0.771)	(0.536)	(0.559)	(0.560)
Shooting at moving vehicles ban	-0.316	-0.273	-0.270	-0.329	-0.285	-0.158
	(0.414)	(0.416)	(0.418)	(0.418)	(0.404)	(0.477)
Deescalation X Chokehold ban		-2.880***				
		(0.584)				
Reporting X Chokehold ban			-3.278***			
			(0.806)			
Duty to intervene X Chokehold ban				-0.219		
				(0.688)		
Duty to intervene X Deescalation					0.288	
					(0.654)	
Duty to intervene X Warning						1.001
						(1.386)
Observations	1006	1006	1006	1006	1006	1006
Departments	94	94	94	94	94	94
Adjusted R-squared	0.695	0.697	0.696	0.694	0.694	0.695

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Controls coefficients from fixed effects regression on police killings in Table B.3

Controls	(1) 7 Policies	(2) Deescalation X Chokehold ban	(3) Reporting X Chokehold ban	(4) Duty to intervene X Chokehold ban	(5) Duty to intervene X Deescalation	(6) Duty to intervene X Warning
Total population	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Median age	-0.080	-0.094	-0.086	-0.084	-0.075	-0.079
	(0.104)	(0.104)	(0.103)	(0.105)	(0.104)	(0.102)
Black pop (%)	0.064	0.077	0.081	0.063	0.067	0.065
	(0.052)	(0.051)	(0.051)	(0.052)	(0.052)	(0.051)
Non-White Other pop (%)	0.007	0.002	0.009	0.007	0.008	0.008
	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
Median HH income	-0.000*	-0.000	-0.000*	-0.000*	-0.000*	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Bachelors educ (%)	0.012	0.009	0.009	0.013	0.011	0.010
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
Unemployed (%)	0.031	0.034	0.035	0.032	0.032	0.030
	(0.055)	(0.054)	(0.055)	(0.056)	(0.055)	(0.055)
Violent crimes per capita	11.978	14.649	21.089	12.512	14.591	15.253
	(42.108)	(40.396)	(40.871)	(41.763)	(42.698)	(42.036)
Total officers per capita	248.728	287.983	280.695	250.724	244.210	231.294
	(404.922)	(398.083)	(397.658)	(403.893)	(404.701)	(400.311)
Observations	1006	1006	1006	1006	1006	1006
Departments	94	94	94	94	94	94
Adjusted R-squared	0.695	0.697	0.696	0.694	0.694	0.695

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Robustness check of chokehold ban, de-escalation, and reporting interactions within regressions with all 21 possible two-way policy interactions

	(1) Police Shootings	(2) People Killed
Deescalation	2.695*	2.403***
	(1.445)	(0.512)
Duty to intervene	0.104	0.283
	(1.090)	(0.519)
Chokehold ban	0.629	0.533
	(1.661)	(0.410)
Warning	1.986	-1.748*
	(1.967)	(0.900)
Exhaust alternatives	-5.910***	-0.839*
	(1.403)	(0.488)
Reporting	17.839***	12.079***
	(6.286)	(2.991)
Shooting at moving vehicles ban	-1.026	1.250
	(1.559)	(1.014)
Deescalation X Chokehold ban	-4.185**	-2.518***
	(2.016)	(0.648)
Duty to intervene X Deescalation	1.197	0.986*
	(1.749)	(0.591)
Deescalation X Warning	-0.434	0.129
	(1.848)	(0.706)
Deescalation X Exhaust alternatives	-9.610***	-7.047***
	(3.540)	(1.669)
Deescalation X Reporting	4.830*	1.927**
	(2.576)	(0.810)
Deescalation X Shooting at moving vehicles ban	0.939	-1.429
	(1.832)	(0.931)
Duty to intervene X Chokehold ban	0.077	-0.409
	(1.975)	(0.661)
Chokehold ban X Exhaust alternatives	18.150***	10.416***
	(3.553)	(1.824)
Reporting X Chokehold ban	-23.601***	-14.798***
	(5.627)	(3.054)
Duty to intervene X Exhaust alternatives	-3.402**	-3.735***
	(1.554)	(0.827)
Duty to intervene X Warning	-2.636**	1.306
	(1.197)	(0.820)
Dury to intervene X Reporting	6.144**	2.222***
	(2.404)	(0.755)
Duty to intervene X Shooting at moving vehicles ban	0.055	-2.119**
	(2.530)	(1.008)
Warning X Exhaust alternatives	-1.789	0.353
	(2.444)	(0.928)
Warning X Reporting	0.255	1.314*
	(2.590)	(0.746)
Warning X Shooting at moving vehicles ban	0.202	-0.736
	(1.840)	(1.148)
Exhaust alternatives X Reporting	-6.414*	-4.684***
	(3.404)	(1.613)
Reporting X Shooting at moving vehicles ban	-1.583	0.768
	(2.575)	(0.744)
Total population	0.000	0.000
	(0.000)	(0.000)
Median age	-0.248	-0.066
	(0.246)	(0.108)
Black pop (%)	0.445**	0.089*
	(0.181)	(0.052)
Non-White Other pop (%)	0.026	-0.002
	(0.040)	(0.010)
Median HH income	0.000	-0.000
	(0.000)	(0.000)
Bachelors educ (%)	0.008	0.006
	(0.143)	(0.043)
Unemployed (%)	0.198	0.022
	(0.135)	(0.056)
Violent crimes per capita	79.521	16.593
	(138.868)	(44.739)
Total officers per capita	1191.844	256.874
	(983.145)	(432.769)
Observations	829	1006
Departments	86	94
Adjusted R-squared	0.781	0.706

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix C

Table C.1: Mundlak test results of police shootings regressions with individual policies and the five interactions that fit selection criteria

Model Name	Chi-Square	P-Value
7 Policies	16.619	0.055
Deescalation X Chokehold ban	16.891	0.050
Reporting X Chokehold ban	19.436	0.021
Duty to Intervene X Chokehold ban	16.608	0.055
Duty to intervene X Deescalation	16.771	0.052
Duty to intervene X Warning	15.855	0.070

Table C.2: Mundlak test results of police killings regressions with individual policies and the five interactions that fit selection criteria

Model Name	Chi-Square	P-Value
7 Policies	25.158	0.003
Deescalation X Chokehold ban	24.231	0.004
Reporting X Chokehold ban	27.074	0.001
Duty to Intervene X Chokehold ban	24.983	0.003
Duty to intervene X Deescalation	24.376	0.004
Duty to intervene X Warning	25.388	0.003

Table C.3: Joint policy effects of five two-way interactions on police shootings by city in correlated random effects models (years 2013-2022)

Policies	(1) 7 Policies	(2) Deescalation X Chokehold ban	(3) Reporting X Chokehold ban	(4) Duty to intervene X Chokehold ban	(5) Duty to intervene X Deescalation	(6) Duty to intervene X Warning
Deescalation	-0.161	5.450**	-0.853	-0.212	-0.051	-0.143
	(0.853)	(2.554)	(0.733)	(0.822)	(0.995)	(0.833)
Duty to intervene	1.058	0.835	0.658	1.807	1.136	1.341*
	(0.682)	(0.633)	(0.611)	(1.208)	(0.794)	(0.698)
Chokehold ban	0.146	0.974	0.891	0.493	0.126	0.127
	(0.997)	(0.944)	(0.897)	(1.031)	(0.999)	(0.987)
Warning	0.015	0.276	0.455	0.162	0.027	1.498
	(0.833)	(0.747)	(0.744)	(0.791)	(0.839)	(1.571)
Exhaust alternatives	0.436	0.138	-0.331	0.415	0.472	0.281
	(1.306)	(1.175)	(1.149)	(1.262)	(1.305)	(1.305)
Reporting	-1.162	-1.682	8.874***	-1.209	-1.190	-1.061
	(1.241)	(1.088)	(1.483)	(1.205)	(1.259)	(1.258)
Shooting at moving vehicles ban	-0.553	-0.511	-0.524	-0.660	-0.589	-0.846
	(1.026)	(0.984)	(0.950)	(1.017)	(1.001)	(1.037)
Deescalation X Chokehold ban		-6.404**				
		(2.627)				
Reporting X Chokehold ban			-10.932***			
			(1.676)			
Duty to intervene X Chokehold ban				-1.201		
				(1.377)		
Duty to intervene X Deescalation					-0.230	
					(1.161)	
Duty to intervene X Warning						-2.179
						(1.814)
Observations	829	829	829	829	829	829
Departments	86	86	86	86	86	86
R-squared (Overall)	0.5373	0.5367	0.5384	0.5381	0.5376	0.5396

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: Controls coefficients from police shootings correlated random effects regressions in Table C.3

Controls	(1) 7 Policies	(2) Deescalation X Chokehold ban	(3) Reporting X Chokehold ban	(4) Duty to intervene X Chokehold ban	(5) Duty to intervene X Deescalation	(6) Duty to intervene X Warning
Total population	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Violent crimes per capita	85.190	93.843	127.727	92.660	84.134	77.829
	(129.321)	(118.772)	(119.121)	(125.443)	(128.036)	(128.519)
Total officers per capita	229.217	393.310	437.796	294.444	238.442	306.863
	(878.695)	(822.965)	(821.662)	(844.799)	(884.249)	(870.720)
Black pop (%)	0.320**	0.361**	0.394**	0.326**	0.319**	0.319**
	(0.154)	(0.155)	(0.159)	(0.155)	(0.153)	(0.155)
Non-White Other pop (%)	0.068**	0.056*	0.078***	0.066**	0.066**	0.064**
	(0.030)	(0.031)	(0.030)	(0.031)	(0.031)	(0.032)
Median HH income	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Bachelors educ (%)	0.057	0.060	0.042	0.060	0.057	0.062
	(0.131)	(0.131)	(0.129)	(0.131)	(0.131)	(0.131)
Unemployed (%)	0.163	0.159	0.161	0.156	0.163	0.169
	(0.129)	(0.128)	(0.129)	(0.130)	(0.129)	(0.130)
Median age	-0.273	-0.291	-0.271	-0.283	-0.277	-0.276
	(0.233)	(0.232)	(0.232)	(0.234)	(0.237)	(0.234)
Mean total population	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean violent crimes per capita	157.943	153.399	130.991	153.289	159.476	162.918
	(190.648)	(185.675)	(188.110)	(189.722)	(189.614)	(189.937)
Mean total officers per capita	1780.959	1592.194	1550.799	1704.921	1772.644	1712.844
	(1444.838)	(1412.021)	(1404.365)	(1409.276)	(1443.549)	(1432.748)
Mean Black pop (%)	-0.439**	-0.479***	-0.517***	-0.446**	-0.437**	-0.438**
	(0.178)	(0.180)	(0.183)	(0.181)	(0.177)	(0.179)
Mean Non-White Other pop (%)	-0.122**	-0.109*	-0.134**	-0.120**	-0.122**	-0.118**
	(0.054)	(0.056)	(0.054)	(0.053)	(0.053)	(0.054)
Mean median HH income	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean bachelors educ (%)	-0.096	-0.100	-0.090	-0.097	-0.096	-0.099
	(0.138)	(0.137)	(0.136)	(0.138)	(0.138)	(0.138)
Mean unemployed (%)	0.402	0.418	0.402	0.417	0.404	0.395
	(0.364)	(0.365)	(0.367)	(0.367)	(0.363)	(0.365)
Mean median age	-0.044	-0.024	-0.041	-0.026	-0.039	-0.037
	(0.223)	(0.222)	(0.225)	(0.223)	(0.224)	(0.223)
City year trend	-0.303	-0.316	-0.302	-0.323	-0.302	-0.309
	(0.146)	(0.145)	(0.144)	(0.149)	(0 .147)	(0.147)
Observations	829	829	829	829	829	829
Departments	86	86	86	86	86	86
R-squared (Overall)	0.5373	0.5367	0.5384	0.5381	0.5376	0.5396

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Joint policy effects of five two-way interactions on police killings by city in correlated random effects models (years 2013-2023)

Policies	(1) 7 Policies	(2) Deescalation X Chokehold ban	(3) Reporting X Chokehold ban	(4) Duty to Intervene X Chokehold ban	(5) Duty to intervene X Deescalation	(6) Duty to intervene X Warning
Deescalation	0.327	2.445***	0.162	0.319	0.288	0.357
	(0.314)	(0.574)	(0.310)	(0.313)	(0.408)	(0.311)
Duty to intervene	0.392	0.321	0.300	0.568	0.357	0.342
	(0.310)	(0.300)	(0.295)	(0.436)	(0.329)	(0.276)
Chokehold ban	0.062	0.355	0.246	0.137	0.065	0.025
	(0.334)	(0.319)	(0.317)	(0.373)	(0.337)	(0.328)
Warning	-0.365	-0.271	-0.242	-0.331	-0.360	-0.434
	(0.408)	(0.396)	(0.399)	(0.416)	(0.407)	(0.979)
Exhaust alternatives	0.801	0.677	0.572	0.795	0.795	0.884
	(0.684)	(0.664)	(0.672)	(0.671)	(0.705)	(0.670)
Reporting	-0.151	-0.326	2.713***	-0.156	-0.143	-0.175
	(0.481)	(0.449)	(0.779)	(0.474)	(0.491)	(0.483)
Shooting at moving vehicles ban	-0.194	-0.158	-0.155	-0.211	-0.181	-0.123
	(0.300)	(0.306)	(0.306)	(0.307)	(0.289)	(0.372)
Deescalation X Chokehold ban		-2.400***				
		(0.621)				
Reporting X Chokehold ban			-3.075***			
			(0.751)			
Duty to intervene X Chokehold ban				-0.276		
				(0.573)		
Duty to intervene X Deescalation					0.092	
					(0.588)	
Duty to intervene X Warning						0.208
						(1.170)
Observations	1006	1006	1006	1006	1006	1006
Departments	94	94	94	94	94	94
R-squared (Overall)	0.430	0.428	0.431	0.431	0.430	0.431

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.6: Controls coefficients from police shootings correlated random effects regressions in Table C.5

Controls	(1) 7 Policies	(2) Deescalation X Chokehold ban	(3) Reporting X Chokehold ban	(4) Duty to Intervene X Chokehold ban	(5) Duty to intervene X Deescalation	(6) Duty to intervene X Warning
Total population	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Violent crimes per capita	6.649	8.335	15.220	7.339	7.460	7.331
	(40.973)	(39.127)	(39.498)	(40.479)	(41.480)	(40.853)
Total officers per capita	180.326	206.446	215.735	186.177	177.918	172.157
	(385.330)	(378.569)	(377.634)	(382.799)	(385.516)	(382.161)
Black pop (%)	0.061	0.072	0.079	0.062	0.061	0.060
	(0.050)	(0.049)	(0.049)	(0.051)	(0.050)	(0.050)
Non-White Other pop (%)	0.007	0.003	0.008	0.006	0.007	0.006
	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)
Median HH income	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Bachelors educ (%)	0.021	0.020	0.018	0.022	0.021	0.020
	(0.041)	(0.041)	(0.041)	(0.040)	(0.041)	(0.040)
Unemployed (%)	0.021	0.020	0.021	0.019	0.021	0.020
	(0.050)	(0.049)	(0.050)	(0.050)	(0.049)	(0.050)
Median age	-0.067	-0.077	-0.072	-0.071	-0.066	-0.066
	(0.100)	(0.100)	(0.099)	(0.100)	(0.100)	(0.099)
Mean total population	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean violent crimes per capita	192.679***	191.932***	187.720***	192.491***	191.699***	191.574***
	(60.197)	(60.121)	(60.107)	(60.187)	(60.859)	(59.458)
Mean total officers per capita	190.819	153.601	151.572	183.172	192.250	197.923
	(433.072)	(424.895)	(425.438)	(429.076)	(432.672)	(430.733)
Mean Black pop (%)	-0.122**	-0.133***	-0.141***	-0.123**	-0.122**	-0.120**
	(0.052)	(0.051)	(0.051)	(0.052)	(0.052)	(0.052)
Mean Non-White Other pop (%)	-0.019	-0.014	-0.021	-0.019	-0.019	-0.019
	(0.016)	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)
Mean median HH income	0.000**	0.000*	0.000**	0.000**	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean bachelors educ (%)	-0.047	-0.046	-0.046	-0.048	-0.047	-0.047
	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)
Mean unemployed (%)	-0.081	-0.074	-0.081	-0.077	-0.082	-0.081
	(0.117)	(0.118)	(0.118)	(0.118)	(0.116)	(0.118)
Mean median age	0.039	0.051	0.046	0.044	0.037	0.038
	(0.104)	(0.104)	(0.104)	(0.105)	(0.105)	(0.103)
City year trend	-0.051	-0.053	-0.048	-0.055	-.0505	-0.049
	(0.055)	(0.054)	(0.054)	(0.056)	(0.055)	(0.054)
Observations	1006	1006	1006	1006	1006	1006
Departments	94	94	94	94	94	94
Adjusted R-squared	0.430	0.428	0.431	0.431	0.430	0.431

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.7: Supplemental summary statistics across all city-year observations

	Mean	Std. dev.	Min.	25th	Median	75th	Max.
Outcomes							
Police killings	2.85	3.29	0.00	1.00	2.00	4.00	23.00
Police shootings	6.66	8.24	0.00	2.00	4.00	8.00	73.00
Policies							
Total policies	1.93	1.60	0.00	0.00	2.00	3.00	6.00
Deescalation	0.38	0.49	0.00	0.00	0.00	1.00	1.00
Chokehold ban	0.64	0.48	0.00	0.00	1.00	1.00	1.00
Duty to intervene	0.37	0.48	0.00	0.00	0.00	1.00	1.00
Warning	0.16	0.37	0.00	0.00	0.00	0.00	1.00
Shooting at moving vehicles ban	0.11	0.31	0.00	0.00	0.00	0.00	1.00
Reporting	0.15	0.36	0.00	0.00	0.00	0.00	1.00
Exhaust alternatives	0.12	0.32	0.00	0.00	0.00	0.00	1.00
Controls							
Total population	664963.55	974323.12	210722	285344	401993.50	655158	8622698
Median age	34.99	2.84	28.20	33.30	34.70	36.10	50.10
Black pop (%)	20.09	16.64	0.22	6.39	15.23	28.38	80.33
Non-White Other pop (%)	25.27	17.33	2.91	12.31	19.52	33.56	85.63
Median HH income	69644.78	20291.91	30601	56789	65775.50	77792	169023
Bachelors educ (%)	36.44	12.02	11.49	28.16	35.05	43.62	71.81
Unemployed (%)	6.35	2.62	1.93	4.60	5.76	7.38	25.33
Violent crimes per 10,000	75.63	44.55	2.38	44.00	66.44	97.71	354.88
Total officers per 10,000	21.87	10.67	6.17	14.75	18.43	26.41	63.81
Observations	1034						

Figure C.8: Number of policies implemented by cities after 2020

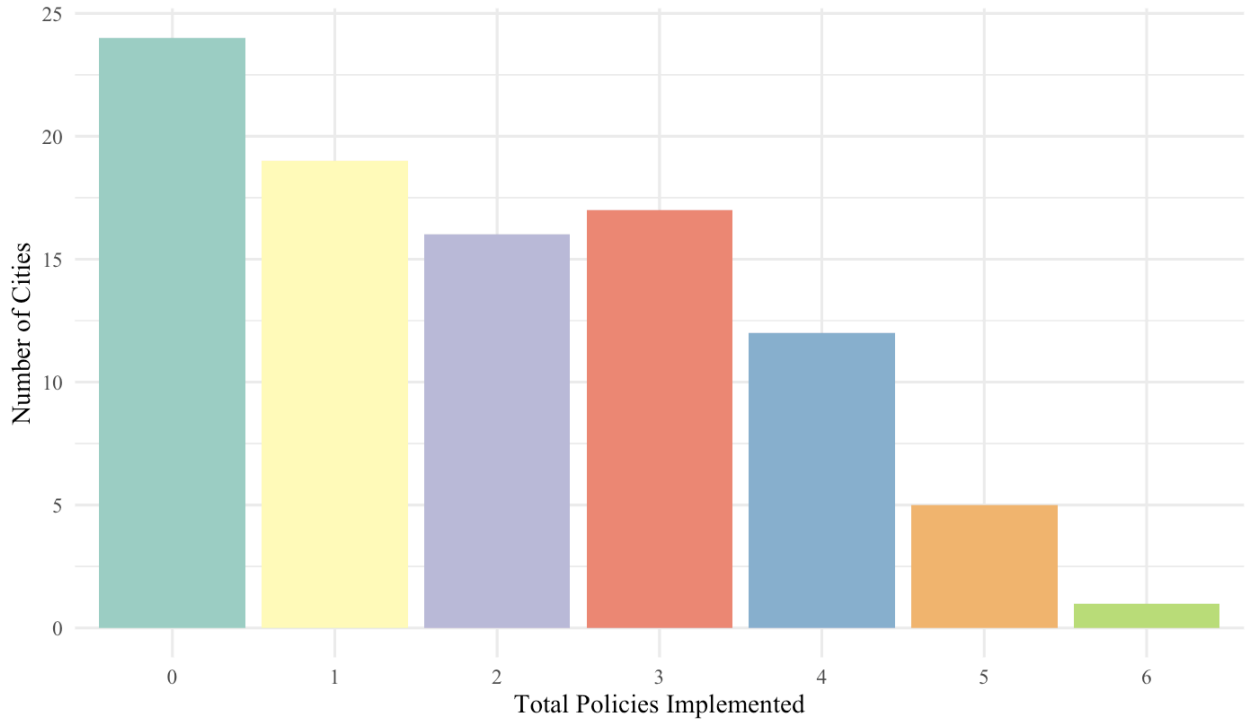

Figure C.9: Rate of police shootings and killings per 10,000 in cities grouped by the total number of policies implemented 2013-2023