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Police Force Size and Civilian Race†

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Abstract

We report novel empirical estimates of the race-specific effects of larger police forces in the United States. Each additional police officer abates approximately 0.1 homicides. In per capita terms, effects are twice as large for Black versus White victims. Larger police forces also make fewer arrests for serious crimes, with larger reductions for crimes with Black suspects, implying that police force growth does not increase racial disparities among the most serious charges. At the same time, larger police forces make more arrests for low-level "quality-of-life" offenses, with effects that imply a disproportionate impact for Black Americans.

Following increased public attention on police shootings and the growth of social movements like Black Lives Matter, American support for law enforcement is currently at its lowest point in nearly thirty years despite the dramatic decline in crime since the 1990s.[1] The large drop in overall support for law enforcement is compounded by a widening race gap in support for police, with 19 percent of Black Americans expressing confidence in police relative to 56 percent of White Americans. This differing experience of policing across racial groups is strikingly described by Bratton and Murad (2018) as the "great divide in American policing."[2]

For decades, activists, policymakers, and social scientists have debated the role of police presence, particularly in lower income neighborhoods where crime tends to be most prevalent. Given the overrepresentation of Black Americans among both homicide victims and civilians shot by the police, race remains a central fixture of public discourse on policing reform—in particular, reforms that are intended to decrease the exposure of low-income minority communities to the collateral costs of policing. Proposed reforms emerging from

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[1]See https://news.gallup.com/poll/317135/amid-pandemic-confidence-key-institutions-surges.aspx.
[2]See https://news.gallup.com/poll/317114/black-white-adults-confidence-diverges-police.aspx.

recent public discourse include widespread calls for reductions in municipal funding for police departments. While there is now a strong consensus in the academic literature that the number of police officers (McCrary 2002; Evans and Owens 2007; Chalfin and McCrary 2018; Mello 2019; Weisburst 2019b) combined with their presence and visibility (Sherman and Weisburd 1995; Di Tella and Schargrodsky 2004; Klick and Tabarrok 2005; Braga, Papachristos, and Hureau 2014; MacDonald, Klick, and Grunwald 2016; Weisburd 2019) reduces crime, whether the effect of additional law enforcement is heterogeneous across Black and White Americans remains a surprisingly open question.

The available literature offers several possible explanations as to why homicide reductions that are attributable to the expansion of law enforcement could differ across racial groups. First, the intense spatial concentration of street vice and homicide in Black neighborhoods provides more opportunities to address victimization through expanded policing efforts (Sampson and Wilson 1995; Cook et al. 2007; O'Flaherty and Sethi 2010b). To the extent that an expansion of law enforcement successfully reduces the influence of illicit markets or dedicates additional resources to increasing the opportunity costs of offending, these interventions could lead to disproportionate decreases in Black homicide victimization (O'Flaherty and Sethi 2010a; Williams 2018). Second, an increase in police manpower permits the deployment of additional resources to communities with higher homicide rates. If homicide serves as a particularly salient signal of criminal behavior, racial disparities in homicide rates could shape the allocation of policing resources. Finally, potential deterrence effects of more police may differ across Black and White neighborhoods if differences also exist in social norms or community perceptions of law enforcement legitimacy (Tyler 2003; Gau and Brunson 2010; Lovett and Xue 2018).

Expanded law enforcement presence also raises concerns that policing strategies involving the use of directed patrol may create collateral costs for disadvantaged communities (Weitzer, Tuch, and Skogan 2008; Bandes et al. 2019). Research finds that while concentrating police at crime hot spots improves public safety, such a strategy has not been effective in making community members feel safer or in improving perceptions of police legitimacy (Ratcliffe et al. 2015; Kochel and Weisburd 2017). There is likewise evidence that mass enforcement policies have served to widen the net of the criminal justice system (Hagan and Dinovitzer 1999; Kohler-Hausmann 2018), leading to an increase in discriminatory practices that have had disproportionate impacts on minority communities (Gelman, Fagan, and Kiss 2007; Goel, Rao, and Shroff 2016; Goncalves and Mello 2020), including the use of violence in interactions with Black suspects (Fryer 2019). Indeed, over the life course, about 1 out of 1,000 Black men can expect to be shot by police (Edwards, Lee, and Esposito 2019).

To what extent does police hiring lead to racially disparate outcomes? Using national data on police employment for a sample of 242 large US cities over a 38-year period, this research provides novel evidence on the racial differences in public safety returns to law enforcement expansion in the United States. We focus on two primary outcomes: homicide victimization and enforcement activity as proxied by various types of arrests. By focusing on the size of a city's police force, we provide *historical* evidence on a critical policy estimand that is implicated by the "defund" movement and which, for many years,

has been the primary means by which municipal policymakers have invested in public safety. In focusing on police manpower, we note that we are implicitly holding fixed many additional sources of variation in police effectiveness—including police management styles and training (Mummolo 2018; Owens et al. 2018; Ba and Rivera 2019; Nagin and Telep 2020; Wood, Tyler, and Papachristos 2020) and the composition and quality of the police force (Donohue and Levitt 2001; McCrary 2007; Miller and Segal 2019; Harvey and Mattia 2020)—all of which are worthy of independent study.

Given the potential endogeneity of police force size, we use two different instrumental variable strategies employed in the policing literature. First, we predict police force size using variation in the timing of federal block grants provided by the US Department of Justice's Community Oriented Policing Services (COPS) office (Evans and Owens 2007; Mello 2019; Weisburst 2019b). Second, recognizing that cities operate under numerous constraints that make it difficult to get out ahead of crime waves, we follow an approach utilized in Chalfin and McCrary (2018), which argues that a primary driver of endogeneity bias in regressions of crime on police manpower is measurement error in police employment data. Using two distinct measures of police force size from different data sources, we derive estimates of the effect of police manpower on homicide victimization that are robust to measurement error.

We find that each additional police officer hired abates between 0.06 and 0.1 homicides with estimates that are strikingly similar across the two estimation strategies. The estimates suggest that investments in police manpower can save a life at a cost of between $1.6 and $2.7 million, far lower than common for accepted estimates of the value of a statistical life, which typically exceed $7 million (Viscusi and Aldy 2003; Chalfin and McCrary 2018).[3] Although the total reduction in homicide is roughly equal across Black and White victims, the decline in homicide is twice as large for Black victims in per capita terms.

Next, we consider the extent to which investments in police manpower expand civilian interactions with the criminal justice system, or create "net widening" effects, focusing on differences by race in police enforcement activity. Here, we find that investments in police manpower lead to larger total numbers of low-level "quality-of-life" arrests, with each additional officer making 7–22 new arrests. These increases are driven by an increase in arrests for liquor violation and drug possession, with effects that imply that increases in these types of arrests are 2.5 –3 times larger for Black civilians. At the same time, we find that arrests for the most serious offenses (so-called "index crimes") fall with investments in police manpower. On a per capita basis, the decline in index crime arrests that we observe is between 4 and 6 times greater for arrests involving Black suspects. This finding is consistent with the idea that police hiring has the potential to create a "double dividend" (Bratton 2011; Cook and Ludwig 2011; Durlauf and Nagin 2011) for both Black and White Americans by generating reductions in both crime and arrests for serious offenses.

[3]We draw on estimates of the cost of a fully-loaded police officer ($130,000) noted in Chalfin and McCrary 2018. After adjusting for inflation, this figure is $158,000 in 2021 constant dollars. As we estimate that one life is saved per 10–17 police officers hired, we multiply this figure by either 10 or 17 depending upon whether the ASG or the COPS IV model is used.

I. Data

Our analysis focuses on 242 large US cities over the 1 981–2018 period. The sample is restricted to cities that have populations greater than 50,000 in 1980 and regularly report data to the US Census' Annual Surveys of Governments (ASG), which includes public employment information from the Annual Survey of Public Employment and Payroll and budget information from the Annual Survey of State and Local Government Finances. We focus on municipal police departments serving these cities and on full-time sworn police employment. A detailed explanation of data sources and cleaning can be found in online Appendix A3.

Our principal treatment variable is a measure of annual police employment collected as part of the Federal Bureau of Investigation's (FBI) Law Enforcement Officers Killed and Assaulted (LEOKA) series. Our first instrumental variables strategy uses a secondary measure of police employment collected independently by the ASG. A second instrumental variables strategy leverages federal grants for hiring police officers administered by the DOJ COPS office. Given that these grants began in 1994 as part of the Violent Crime Control Act, our analysis using COPS grants covers the period of 1990–2018.

Data on homicides come from the FBI's Supplementary Homicide Reports (SHR) dataset, which assembles records of homicides reported from each police agency in the United States. For each city-year, we aggregate homicides separately by race, focusing on homicides with either a n on-Hispanic Black or a non-Hispanic White victim. We exclude homicides committed in prisons or jails as well as felons killed in the commission of a crime as these are likely to fall under the legal definition of justifiable homicide. We also use the SHR data to calculate a homicide clearance rate—the proportion of homicides in which a suspect or perpetrator is identified.[4]

To assess the extent to which a larger police force widens the net of the criminal justice system, we use data on arrests collected by the FBI's Uniform Crime Reports (UCR). For much of the analysis, we group arrests into the FBI's definition of seven major "index crimes" (murder, rape, robbery, aggravated assault, burglary, grand larceny, and motor vehicle theft), lower-level "quality-of-life" offenses (including disorderly conduct, liquor violations, loitering, and drug possession), and arrests for any other type of offense (see online Appendix Table 12 and online Appendix Table 13 for a full list of the components of these groups).[5] For each category, we track total arrests as well as race-specific arrests; though here data limitations restrict our attention to Black and White race groups, where White includes Hispanic/Latinx individuals.[6] To provide a useful benchmark against which to compare estimates of index crime arrests, we also include an analysis of the effect

[4]This measure focuses on preliminary reports and will differ from clearance rates reported directly by police departments, which include cases cleared in subsequent years.

[5]Notably included are "uncategorized" arrests. This means our estimates account for any potential improvements in reporting that could shift arrests recorded without a category into another of the arrest categories.

[6]While Hispanic/Latinx victims have their own category in the FBI's Supplementary Homicide Reports data, the FBI does not report arrests by the Hispanic/Latinx ethnicity of the suspect. Instead, these victims are classified either as White, Black, Asian, or Other. The "β/Pop." benchmarks we include for the arrest outcomes adjust White estimates for the combined Non-Hispanic White and Hispanic population in the US census to account for this uncoded category.

of police force size on index crimes. These data are obtained from the UCR's offenses known to law enforcement data series, and unfortunately, the data are not available by race/ethnicity.

We supplement our analysis with additional data on city demographics and budgets from the US Census to construct control variables. Demographic data for each analysis include population, resident race, gender, age shares, educational attainment, marital status, and income. Our budget data includes city expenditures, revenue, and tax receipts.

II. Econometric Methods

Our empirical strategy is motivated by the following least squares regression:

$$Y_{it}^{j} = \beta S_{it-1} + \gamma' X_{it} + \rho_i + \psi_{st} + \epsilon_{it}. \quad (1)$$

In (1), Y_{it}^{j} is a given outcome of interest measured in city i for individuals of race j in year t. Given our central research question regarding the public safety returns to an increase in police manpower, we specify each of our models in levels with β reflecting the marginal returns to employment of an additional officer within the policing production function.[7] The variable S_{it-1} is the number of sworn police officers measured in the previous year, a convention that is used in order to minimize endogeneity bias (Levitt 1996, 2002; Chalfin and McCrary 2018).[8]

The model conditions on city (ρ_i) and interacted state-by-year (ψ_{st}) fixed effects. The latter term accounts for annual variation in state-level policies including changes in incarceration levels and sentencing practices, as well as aggregate changes in policing technology. State-by-year fixed effects also account for changes in crime and arrest recording practices, which could influence counts in the SHR and UCR data we use to the extent that these change in states over time. We control for a vector of time-varying covariates, X_{it}, which includes a quadratic function of population and detailed demographic data including a city's race, gender, and age composition, median household income, the poverty rate, and the unemployment rate. Our models also account for each city's tax receipts, revenue, and expenditures in order to directly study the effects of law enforcement expansion holding municipal spending constant. Accordingly, β represents the effect of hiring one additional police officer relative to the *historical* opportunity cost of using the funds for an alternative purpose. Our baseline specification weights the data by each city's 1980 population. Standard errors are clustered at the city-level.

There are primarily two challenges to identifying a causal estimate of β, the impact of police employment. First, as shown by Chalfin and McCrary (2018), police employment is measured with error. If measurement errors are classical, equation (1) will yield an estimate

[7]Focusing on levels models presents several advantages. First, per capita models and other functional form assumptions do not directly address our main research question concerning the marginal public safety returns associated with hiring an additional officer. Second, the levels model permits greater flexibility in controlling for the relationship between population and homicide or other key outcomes. Lastly, per capita models at the city-level are not easily translated to race-specific outcomes as covariates like city budget expenditures do not make sense when scaled by race-specific population.

[8]Estimates are extremely similar when a contemporaneous measure is used.

of β that is attenuated toward zero—a problem that is likely made worse by the inclusion of covariates and fixed effects.[9] A second concern is that β may be biased due to the omission of covariates or simultaneity bias between police hiring and crime (Levitt 1996; Evans and Owens 2007).

In order to obtain consistent estimates of β, we use two different instrumental variables strategies that have been employed in the prior literature. First, following Chalfin and McCrary (2018), we explicitly correct for measurement error bias in police force size using a second potentially independent measure of police manpower from the US Census Annual Survey of Governments (ASG IV) as an instrument for the FBI measure of police manpower. As we show in online Appendix A1, switching the role that each police measure plays in the IV framework leads to statistically identical estimates, consistent with the proposition that measurement errors are classical. Second, following Evans and Owens (2007); Mello (2019); and Weisburst (2019b), we instrument for police manpower with variation in federal COPS grants that were awarded to cities to facilitate police hiring. Previous work demonstrates that the likelihood of an agency receiving a grant in a given year remains plausibly exogenous conditional on covariates and fixed effects. Similar to Evans and Owens (2007), our specification uses the number of police officers eligible for hiring under an awarded grant as the instrumental variable. The model includes additional controls for the size of grant awards for nonhiring purposes and indicators for police department decisions to apply for grants over time (Weisburst 2019b). Critically, controls for n onhiring grant awards and applications proxy for police department interest in and funding for other types of investments in police operations, including technology improvements.

In addition to estimating different local average treatment effects, each IV strategy has costs and benefits that can be characterized as a trade-off between bias and variance. While models using COPS grants as an instrument credibly addresses both sources of inconsistency in OLS estimates (i.e., endogeneity *and* measurement error), these models retain only a small amount of the variation in police hiring and are less precisely estimated. Moreover, the commencement of the COPS program in 1994 restricts the study period for these analyses to the 1 990–2018 period. On the other hand, while our measurement error instrument generates an extremely strong first stage and uses the full sample of data, the cost is that these models do not leverage a natural experiment to address endogeneity concerns. With respect to the latter point, we note that while concerns about simultaneity bias dominate the literature, similar to a famous result—that measurement error bias may be more important than ability bias in estimating a Mincer equation (Ashenfelter and Krueger 1994)—simultaneity bias concerns may be less important than measurement error bias in our context. As discussed in online Appendix A1, the political science and public administration literatures have detailed a variety of constraints faced by municipal leaders that make strategic police hiring difficult, at least over a one-year time period (Lewis 1994; Joyce and Mullins 1991; Poterba and Rueben 1995; Shadbegian 1998; Shavell 1991; Koper 2004; Rubin 2016).

[9]Conditioning on fixed effects removes some of the true signal in S_{it} with the remaining variation left to include a larger share of error.

In practice, both the measurement error IV model and the COPS IV model lead to substantively similar outcomes which strengthens our confidence in the resulting estimates. Given the support for both identification strategies in the previous literature, we omit further discussion from the main body of the paper and refer readers to online Appendix A1, where we provide additional details and evidence concerning the robustness of these strategies.

III. Results

A. Descriptive Statistics

Table 1 reports summary statistics for each of our key outcomes and control variables, weighted by 1980 population. The unit of analysis is the city-year. On average, individuals living in the cities in our sample are 24 percent n on-Hispanic Black, 19 percent Hispanic, and 49 percent non-Hispanic White.

The average city in our sample employs approximately 400 police officers per 100,000 residents, with estimates that are quite close for the endogenous UCR employment measure (365) and the ASG employment IV (417). This is higher than the national average, approximately 250 per 100,000 residents, but unsurprising given that our sample includes the largest cities in the United States. The COPS grants we utilize in our estimation award funding for 143 officers on average (weighted by population) and there are over 1,000 grants in our data.

In an average city-year in our data, there are 244 homicide victims, of which 138 (57 percent) are non-Hispanic Black and 64 (26 percent) are non-Hispanic White. Nationally, approximately half of homicide victims are Black—the proportion in our sample is slightly higher as we focus on large cities. In per capita terms, Black residents are approximately four times as likely to be the victim of a homicide compared to White residents.

Black Americans are also disproportionately arrested for both serious index crimes and low-level “quality-of-life” offenses. Black civilians make up over half of each of these types of arrests, and in per capita terms are arrested at three to four times the rate of their White counterparts. To provide an instructive benchmark for understanding subsequent analyses of index crime arrests, we also measure the number of total index crimes. In our sample, there are approximately 5.7 index crimes per index crime arrest.

B. Main Estimates

Our primary results are presented in Table 2, which reports estimates for the measurement error correction instrument, and Table 3, which reports estimates for the COPS instrument. For the measurement error model, the *F*-statistic on the excluded instrument is over 500, indicating a very strong first-stage relationship between the measures. For the COPS IV, the *F*-statistic on the excluded instrument is 16, which, while smaller, exceeds the critical value for maximal 10 percent bias as computed by Stock, Wright, and Yogo (2002).

For each outcome, we estimate the effect of a change in police force size separately for Black and White civilians. Because Black civilians make up a comparatively small share (24 percent) of the population in our sample, we also present the estimate as a change

per 100,000 residents of a given race group, allowing us to comment more directly on the differential effects of police force growth in proportional terms. Finally, for each outcome we present a *p*-value from a test of the equality of a given Black versus White estimate. This test forms the basis for making formal inferences about racially disparate treatment effects.

Our first result is that an increase in police manpower reduces homicide victimization, in total and for each racial group. The marginal police officer abates between 0.06 and 0.1 homicides indicating that, on average, there is one life saved per 10–17 police officers hired.[10] On a per capita basis, police force expansion has a larger effect on homicide victimization for Black civilians (0.006–0.012 homicides per 100,000 population) than for White civilians (0.002–0.008 homicides per 100,000 population). The per capita racial disparity in the effect of police force size on homicide victimization is significant at conventional levels for both IV estimators ($p < 0.001$).[11]

Next, given that police officers typically have broad discretion over whether or not to make arrests (Goldstein 1963; Linn 2009; Weisburst 2020; Chalfin and Goncalves 2020), we consider different types of arrests as markers of police activity. This analysis, which is new to the literature, shows that investments in police manpower lead to important increases in police activity as proxied by low-level arrests, which include nonviolent misdemeanor offenses such as drug possession, disorderly conduct, and liquor violations. Using the ASG IV (COPS IV), we estimate that the marginal police officer makes approximately 7.1 (22) arrests for low-level "quality-of-life" offenses. While approximately 60 percent of the marginal arrests accrue to White civilians, on a per capita basis, the additional low-level arrests are disproportionately experienced by Black civilians. Using the COPS IV, this contrast is particularly apparent as point estimates imply that the incidence of low-level arrests is 7 0 percent greater among Black civilians than White civilians, a difference in per capita terms that is not significant at conventional levels but is nevertheless marginally significant ($p = 0.1$).[12]

This result is subject to two important clarifications. First, while the racial disparity that we estimate is not significant at conventional levels, this test is likely conservative since, due to arrest data limitations, Hispanic arrestees are overwhelmingly classified as White for this outcome. As research indicates important Hispanic-White disparities with respect to policing outcomes (Sanga 2009), the White estimate that includes the Hispanic arrestees estimate is likely to be larger than the non-Hispanic White estimate. Second, referring to Figure 1, which disaggregates our estimates by arrest type, we note that B lack-White disparities are especially large and significant for arrests for liquor law violations and drug possession, arrest categories that account for over 20 percent of "quality-of-life" arrests and over which police officers have especially considerable discretion (Goldstein 1963). In these categories, the per capita results imply arrest increases that are 2.5–3 times larger for Black civilians.

[10]As we note in online Appendix Table 6, the fact that the COPS IV estimates are approximately twice as large as those obtained using the ASG IV model is largely an artifact of the restricted sample period for the COPS estimation strategy. Estimating the ASG model using the 1990–2018 period yields a point estimate for homicide (−0.09) that is very close to the estimate using the COPS instrument.

[11]In online Appendix Table 9 and online Appendix Table 10 we compute estimates that include more granular race and ethnicity categories where available.

[12]Using the ASG instrument, the per capita difference is not significant at conventional levels.

We also consider the effects of police manpower on enforcement of more serious crimes. First, we examine whether a larger police force is able to clear more homicides—a metric of police productivity. Neither IV strategy produces any meaningful evidence on homicide clearance rates for victims of either race, nor is there evidence of racial disparities. Next, we consider the effects of police manpower on index crime arrests. Consistent with recent findings (Owens 2013), we do not observe an increase in index crime arrests as a function of police manpower. Indeed, the evidence suggests that index crime arrests fall (by between − 0.97 and − 1.6) with each additional police officer employed. Given that reductions in arrests are a function of both police and offender behavior, we also estimate the effect of police force size on index crimes for reference. Consistent with the prior literature (Evans and Owens 2007; Kaplan and Chalfin 2019; Weisburst 2019b), we find that each police officer abates approximately 18–24 index crimes, an estimate that implies that an elasticity of index crimes with respect to police is approximately −1.1. Since larger police forces lead to reductions in index crimes, the decline in index crime arrests that we observe suggests that larger police forces reduce serious crime primarily through deterrence rather than by arresting and incapacitating additional offenders (Nagin 2013; Chalfin and McCrary 2017; Kaplan and Chalfin 2019).

With respect to the racial incidence of index crime arrests, we observe that, relative to population, a larger police force leads to a reduction in index crime arrests that is between 4–6 times larger for Black arrestees than for White arrestees, a difference that is significant at conventional levels for both IV strategies ($p < 0.001$). This result suggests that increased investments in police manpower, in fact, *decrease* the racial disparity in arrests for the most serious offenses that are most likely to result in prison sentences.

In online Appendix A2, we subject each of the results reported in our main tables to greater scrutiny. We r eestimate the models using a number of different robustness specifications, including using a common sample that includes data on all outcomes, estimating the model without population weights, conditioning on a number of more granular fixed effects, and employing alternative functional forms. We also consider the concern that police hiring could affect the reporting of crimes and arrests to the FBI, by estimating changes in the extensive margin of reporting.

IV. Discussion

This research reports novel estimates of the r ace-specific impacts of a larger police force. We find that larger police forces disproportionately abate homicides with Black victims. With respect to the prospect for police hiring to “widen the net” of the criminal justice system, we report mixed conclusions. On the one hand, we find evidence that larger police forces lead to large and meaningful increases in low-level “quality-of-life” arrests, in particular for Black civilians, where the per capita arrest increase for liquor violations and drug possession is 2. 5–3 times higher for Black civilians. On the other hand, our finding that index crime arrests fall with police manpower, and disproportionately fall for Black civilians, is consistent with the idea that police hiring has the potential to create a “double dividend” for society (Bratton 2011; Cook and Ludwig 2011; Durlauf and Nagin 2011) by generating reductions in both crime and potentially incarceration for serious offenses. While

arrests for “quality-of-life” offenses have the potential to accumulate, the results suggest that larger police forces may not be an important driver of lengthy prison sentences or incarceration, for both Black and White civilians.

Additional research is needed to better understand the net impact of “quality-of-life” arrests on urban life. While recent research suggests that the intensive use of field interrogations and arrests for quality-of-life crimes has only a modest effect on major crimes (Braga and Bond 2008; MacDonald, Klick, and Grunwald 2016), there continues to be considerable debate about whether there are public safety benefits of “broken-windows-style” policing (Corman and Mocan 2005; Harcourt and Ludwig 2006; Sullivan and O’Keeffe 2017). With respect to the costs of quality-of-life arrests for disadvantaged communities, research suggests that these arrests could subject large numbers of people to unnecessary human capital disruptions (Leslie and Pope 2017; Dobbie, Goldin, and Yang 2018), adverse labor market outcomes (Pager 2003; Agan and Starr 2018; Doleac and Hansen 2020), and may have criminogenic effects either though jail sentences (Gupta, Hansman, and Frenchman 2016; Leslie and Pope 2017) or peer effects (Bayer, Hjalmarsson, and Pozen 2009; Stevenson 2017).

Critically, our findings highlight important channels that contribute to the “great divide” in policing across race in America, which has been characterized as the defining generational challenge for large urban law enforcement organizations (Bratton and Murad 2018). While we find that investments in law enforcement disproportionately save Black lives, they also increase racial disparities in the number of arrests for “quality-of-life” crimes like drug possession and liquor law violations. These low-level arrests are often the result of traffic and street stops by police officers, which tend to be higher in communities of color (Goel, Rao, and Shroff 2016) and which have been frequently cited as a source of discord between police officers and minority citizens. Given that the “quality-of-life” arrests mechanically increase police-civilian interactions, they may also be a key driver of differences in the use of force by police against Black versus White civilians (Fryer 2019; Weisburst 2019a), and therefore may carry substantial costs. Though information on the use of force by police officers is not collected nationally, if we use the estimate in Weisburst (2019a)—that 2.5 percent of arrests lead to an incident in which any force was used by a police officer—then the expansion of policing required to abate one homicide would also yield between 7–10 use of force incidents, of which 4–5 incidents would involve a Black civilian.

To the extent that policymakers conclude that the costs of making large numbers of arrests for “quality-of-life” offenses outweigh the potential public safety benefits, we note that a number of different avenues for reform could address racial disparities in the burdens of police enforcement. Consistent with our finding that the racially disparate effects of investments in police manpower are particularly large for drug possession arrests, the decriminalization of the possession of small amounts of drugs may be a particularly promising avenue for reducing racial disparities. Similarly, prior research suggests that racial disparities might be reduced by efforts to recruit a larger number of Black and/or female police officers (Donohue and Levitt 2001; McCrary 2007; West 2018; Miller and Segal 2019; Harvey and Mattia 2020; Ba et al. 2021; Linos and Riesch 2020) as well as by the application of a “precision policing” strategy in which police effort is r eallocated toward the

most serious offending in a community (Chalfin, LaForest, and Kaplan 2021). Finally, given that police officers tend to be highly responsive to managerial directives (Mummolo 2018; Ba and Rivera 2019; Graham and Makowsky 2020), top-down procedural reforms could meaningfully alter officer behavior, even holding officer preferences (Chalfin and Goncalves 2020) and police force size fixed.

Our estimates capture the *historical* opportunity cost of policing, by including controls that hold municipal spending fixed. In this vein, our results suggest that " defunding" the police could result in more homicides, especially among Black victims. Of course, reducing funding for police could allow increased funding for other alternatives. An array of high-quality research suggests that crime can, in certain contexts, be reduced through methods other than policing or its by-product—incarceration. Among the many alternatives to police, for which there is promising evidence, are place-based crime control strategies such as increasing the availability of trees and green space (Branas et al. 2011), restoring vacant lots (Branas et al. 2016; Branas et al. 2018; Moyer et al. 2019), public-private partnerships (Cook and MacDonald 2011), street lighting (Doleac and Sanders 2015; Chalfin et al. 2019), and reducing physical disorder (Sampson and Raudenbush 2001; Keizer, Lindenberg, and Steg 2008). There is also evidence that social service-based strategies such as summer jobs for disadvantaged youth (Heller 2014; Gelber, Isen, and Kessler 2016; Davis and Heller 2020), cognitive behavioral therapy (Blattman, Jamison, and Sheridan 2017; Heller et al. 2017), mental health treatment (Deza, Maclean, and Solomon 2020; Jácome 2020), and local nonprofits more generally (Sharkey, Torrats-Espinosa, and Takyar 2017) can have important crime-reducing effects. While social service interventions are often difficult to scale (Moffitt 2006; Ludwig, Kling, and Mullainathan 2011), the increasing number of studies that show ways to reduce crime outside the deterrence channels of the traditional model of Becker (1968) is encouraging.

Whether communities should invest less in law enforcement and more in alternative strategies remains an open question, as such a material change in our society's approach to public safety has yet to be implemented at scale. Our research focuses on one crucial aspect of this policy debate—the effect of reducing police employment—as an outcome that would likely result if proposals to reduce funding for municipal police departments are adopted in the future. This study provides an estimate of the historical trade-offs of investments in law enforcement and, critically, the resulting implications for communities of color.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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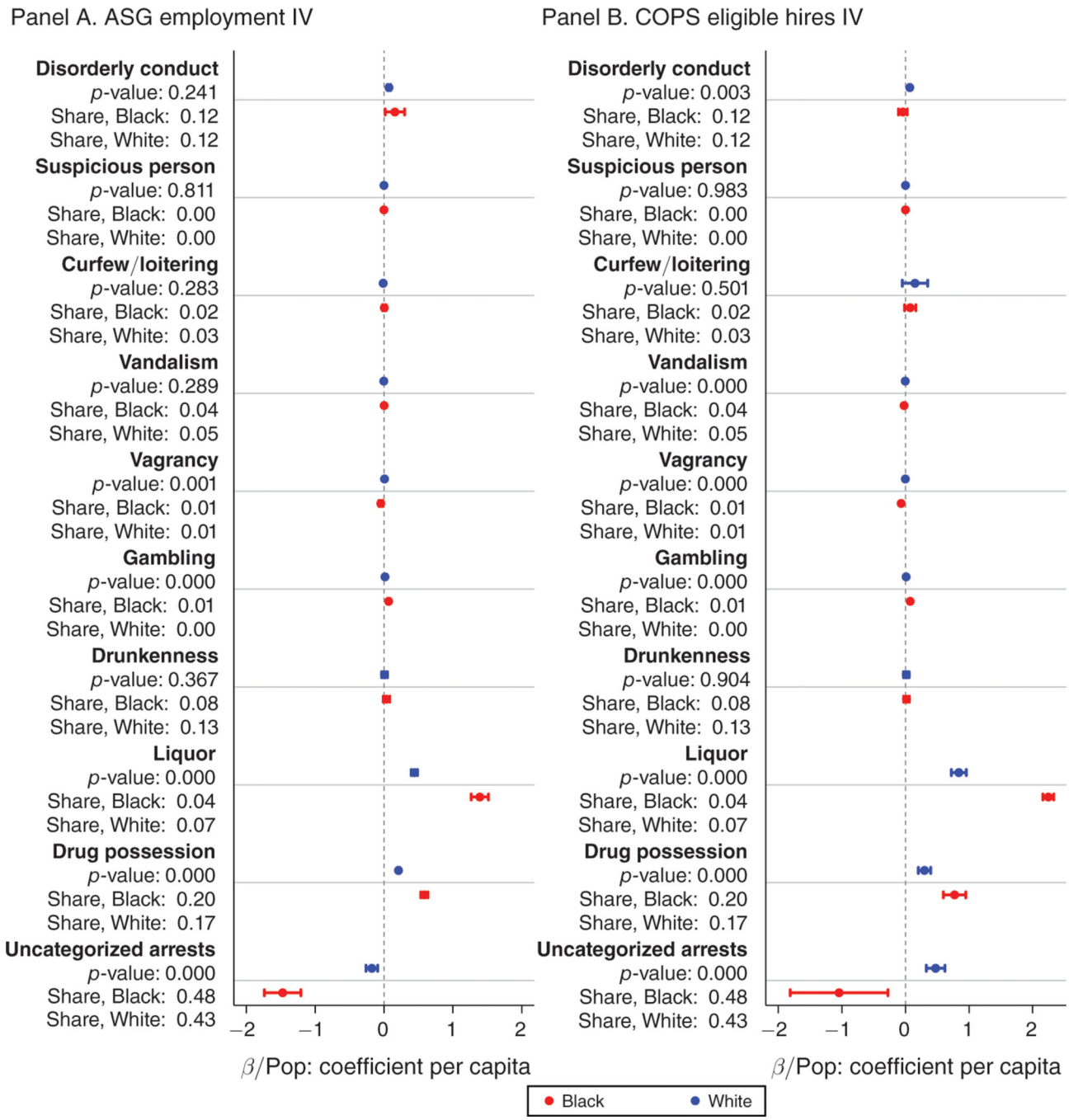

FIGURE 1. EFFECTS OF POLICE FORCE SIZE ON QUALITY OF LIFE ARRESTS BY RACE

Notes: Figure reports estimates from equation (1) in which the once-lagged number of sworn police officers in a city derived from the FBI's Uniform Crime Reports is instrumented using either an alternative measure of sworn police officers from the US census or the number of eligible hires awarded through a COPS hiring grant. In panel A, the instrument is police employment from the US census; in panel B, the instrument is the number of eligible hires awarded through a COPS hiring grant. Models include covariates in Table 1; panel B also controls for nonhiring grant award size and whether a city applied for a

hiring or nonhiring grant (lagged). Standard errors are clustered at the city level. Results correspond to per capita estimates. Models are weighted by population of each city in 1980. Panel A covers 1 981–2018. Panel B covers 1990–2018. Arrest categories correspond to online Appendix Table 12. Models have differing observations due to data availability and the outlier cleaning procedure described in online Appendix A3. The endogenous measure of police employment is recorded in the UCR LEOKA files. "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests do not include subcategories for Hispanic residents; as a result, White population share includes Hispanic residents for these outcomes in calculating the "β/Pop." measure. "Share, Black" and "Share, White" display the share of that arrest category within all low-level or quality-of-life arrests.

Table 1—

SUMMARY STATISTICS

	Mean	SD		Mean	SD
Outcomes			*Covariates*		
Homicide victims	244.26	(397.01)	Population	1,593,676	(2,402,359)
Black	138.93	(214.65)	Total government expenditure	12,781,422	(29,128,508)
White	63.56	(143.18)	Total government revenue	12,775,643	(28,812,641)
Homicide clearance rate	66.11	(22.50)	Total taxes	5,011,031	(12,037,901)
Black	63.51	(24.90)	Percent Black	24.23	(18.23)
White	70.26	(23.55)	Percent White	48.56	(19.74)
Quality of life arrests	58,178	(132,128)	Percent Hispanic	18.98	(16.99)
Black	29,896	(71,320)	Percent male	48.26	(1.29)
White	27,605	(61,220)	Percent age <14	20.37	(2.91)
Index crime arrests	16,430	(26,081)	Percent age 15–24	16.00	(2.76)
Black	9,142	(15,507)	Percent age 25–44	31.18	(3.15)
White	7,030	(11,080)	Percent age >45	32.44	(4.29)
Index crimes	93,928	(145,967)	Percent female head of household	16.34	(4.58)
			Percent never married	36.96	(7.09)
			Percent education < high school	24.47	(8.94)
			Unemployment rate	8.68	(3.08)
			Poverty rate	16.97	(4.87)
			Median household income	36,314	(7,749)
	Mean	SD			*N*
Policing			*Sample counts*		
UCR employment	5,831	(10,288)	Number of cities		242
ASG employment IV			*N*: ASG models		9,438
ASG employment	6,647	(12,447)	*N*: COPS models		7,018
COPS eligible hires IV					
Eligible hires (per grant)	143.04	(346.68)			
Number of hiring grants	1,125				

Notes: Summary statistics are weighted by population of each city in 1980. COPS IV models cover the period 1990–2018, and ASG IV models cover the period of 1981–2018. Additional details on policing variables, including the ASG employment IV measure and COPS eligible hires IV grant variables, can be found in online Appendix A1, which details the instrumental variables strategies used in the paper.

Table 2—

MARGINAL IMPACT OF POLICE EMPLOYMENT ASG EMPLOYMENT IV

	ASG IV					
	Coeff.	**SE**	***β*/Pop.**	**SE**	**Mean**	***N***
First stage						
Police employment	0.962	(0.041)	—	—	6,047.0	8,645
(*F-test = 553.38*)						
Homicides						
Victims	−0.058	(0.004)	−0.003	(0.000)	249.0	8,553
Black	−0.026	(0.003)	−0.006	(0.001)	140.4	8,522
White	−0.016	(0.002)	−0.002	(0.000)	65.5	8,502
Difference: p-value	*0.002*		*0.000*			
Clearance rate	0.001	(0.001)	—	—	65.2	7,675
Black	0.001	(0.001)	—	—	62.6	6,065
White	−0.001	(0.001)	—	—	69.5	7,045
Difference: p-value	*0.227*					
Arrests						
Quality of life	7.12	(0.88)	0.53	(0.06)	60,244	7,804
Black	2.15	(0.51)	0.66	(0.16)	30,896	7,768
White	5.03	(0.50)	0.55	(0.05)	28,827	7,779
Difference: p-value	*0.000*		*0.498*			
Index	−0.97	(0.28)	−0.07	(0.02)	16,351	7,796
Black	−0.68	(0.20)	−0.21	(0.06)	8,930	7,753
White	−0.45	(0.09)	−0.05	(0.01)	7,214	7,770
Difference: p-value	*0.291*		*0.009*			
Index crimes	−17.82	(1.40)	−1.07	(0.08)	96,892	8,645

Note: Table 2 reports estimates from equation (1) in which the once-lagged number of sworn police officers in a city derived from the FBI's Uniform Crime Reports is instrumented for using an alternative measure of sworn police officers from the US census. Standard errors are clustered at the city level. Models are weighted by population of each city in 1980. The data sample covers 1981 to 2018. Models have differing observations due to data availability and the outlier cleaning procedure described in online Appendix A3. Models include covariates in Table 1. "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests do not include subcategories for Hispanic residents; as a result, White population share includes Hispanic residents for these outcomes in calculating the "β/Pop." measure. All estimates pass a Bonferroni multiple hypothesis correction.

Table 3—

MARGINAL IMPACT OF POLICE EMPLOYMENT COPS ELIGIBLE HIRES IV

	COPS IV					
	Coeff.	**SE**	**β/ Pop.**	**SE**	**Mean**	***N***
First stage						
Police employment	3.196	(0.806)	—	—	6,390.7	6,623
(*F-test = 15.71*)						
Homicides						
Victims	−0.103	(0.010)	−0.006	(0.001)	223.3	6,530
Black	−0.050	(0.005)	−0.012	(0.001)	130.0	6,501
White	−0.044	(0.001)	−0.008	(0.000)	59.2	6,489
Difference: p-value	*0.205*		*0.000*			
Clearance rate	0.001	(0.001)	—	—	60.4	5,766
Black	0.001	(0.001)	—	—	56.8	4,598
White	0.000	(0.002)	—	—	66.4	5,223
Difference: p-value	*0.603*					
Arrests						
Quality of life	22.01	(5.09)	1.74	(0.40)	49,908	5,839
Black	8.17	(1.64)	2.80	(0.56)	24,807	5,831
White	14.01	(3.47)	1.66	(0.41)	24,674	5,818
Difference: p-value	*0.128*		*0.102*			
Index	−1.60	(0.37)	−0.13	(0.03)	13,366	5,833
Black	−1.14	(0.21)	−0.39	(0.07)	7,007	5,808
White	−0.55	(0.17)	−0.06	(0.02)	6,137	5,811
Difference: p-value	*0.032*		*0.000*			
Index crimes	−23.54	(1.98)	−1.39	(0.12)	83,209	6,623

Notes: Table 3 reports estimates from equation (1) in which the once-lagged number of sworn police officers in a city derived from the FBI's Uniform Crime Reports is instrumented using the number of eligible hires awarded through a COPS hiring grant. Standard errors are clustered at the city-level. Models are weighted by population of each city in 1980. The data sample covers 1990 to 2018. Models have differing observations due to data availability and the outlier cleaning procedure described in online Appendix A3. The endogenous measure of police employment is recorded in the UCR LEOKA files. Models include covariates in Table 1 as well as controls for nonhiring grant award size and whether a city applied for a hiring or nonhiring grant (lagged). "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests do not include subcategories for Hispanic residents; as a result, White population share includes Hispanic residents for these outcomes in calculating the "β/Pop." measure. All estimates pass a Bonferroni multiple hypothesis correction.