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.

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1 Introduction

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% of Black Americans expressing confidence in police relative to 56% of white Americans. This differing experience of policing across race groups is strikingly described by Bratton and Anderson (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 over-representation 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 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 et al., 2014; MacDonald et al., 2016; Weisburd, 2016) 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

[1] https://news.gallup.com/poll/317135/amid-pandemic-confidence-key-institutions-surges.aspx

[2] https://news.gallup.com/poll/317114/black-white-adults-confidence-diverges-police.aspx

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 et al., 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 Jr, 2020). 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 et al., 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 which have had disproportionate impacts on minority communities (Gelman et al., 2007; Goel et al., 2016; Goncalves and Mello, 2020), including the use of violence in interactions with Black suspects (Fryer Jr, 2019). Indeed, over the life course, about 1 in every 1,000 Black men can expect to be shot by police (Edwards et al., 2019).

To what extent does police hiring lead to racially disparate outcomes? Using national data on police employment for a sample of 242 large U.S. 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 et al., 2020) and the composition and quality of the police force (Donohue III and Levitt, 2001; McCrary, 2007; Miller and Segal, 2019; Harvey and Mattia, 2019) — each of which is 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 U.S. 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 police manpower. On a per capita basis, the decline in index crime arrests that we observe is between 4-6 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.

2 Data

Our analysis focuses on 242 large U.S. cities over the 1981-2018 period. The sample is restricted to cities which have populations greater than 50,000 in 1980 and regularly report data to the U.S.

[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.

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 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 U.S. For each city-year, we aggregate homicides separately by race, focusing on homicides with either a non-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"

[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.

offenses (including disorderly conduct, liquor violations, loitering, loitering, and drug possession), and arrests for any other type of offense (see Appendix Table 12 and 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 of police force size on index crimes. These data are obtained from the UCR's offenses known to law enforcement data series, and unfortunately, are not available by race/ethnicity.

We supplement our analysis with additional data on city demographics and budgets from the U.S. 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.

3 Econometric Methods

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

$$Y_{it}^{j} = \beta S_{it-1} + \gamma^{'} X_{it} + \rho_i + \psi_{st} + \varepsilon_{it} \tag{1}$$

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] S_{it-1} is the number of sworn police

[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 U.S. Census to account for this uncoded category.

[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

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 of β that is attenuated towards 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).

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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.

[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 order to obtain consistent estimates of β, we use two different instrumental variables strategies which 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 U.S. Census Annual Survey of Governments (ASG IV) as an instrument for the FBI measure of police manpower. As we show in 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 non-hiring purposes and indicators for police department decisions to apply for grants over time (Weisburst, 2019b). Critically, controls for non-hiring 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 which 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 1990-2018 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 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).

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 Appendix A1 where we provide additional details and evidence concerning the robustness of these strategies.

4 Results

4.1 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% non-Hispanic Black, 19% Hispanic, and 49% 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 U.S. The COPS grants we utilize in our estimation award funding for 143 officers on average (weighted by population) and there are over 1000 grants in our data.

In an average city-year in our data, there are 244 homicide victims, of which 138 (57%) are non-Hispanic Black and 64 (26%) 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 4 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 3 to 4 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.

4.2 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% bias as computed by Stock et al. (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%) 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, 2017; 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% 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

[10] As we note in 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 Appendix Table 9 and Appendix Table 10 we compute estimates which include more granular race and ethnicity categories where available.

particularly apparent as point estimates imply that the incidence of low-level arrests is 70% 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 which includes 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 Black-white disparities are especially large and significant for arrests for liquor law violations and drug possession, arrest categories which account for over 20% 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.

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

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

and Owens, 2007; Kaplan and Chalfin, 2019; Weisburst, 2019b), we find that each police officer abates approximately 18-24 index crimes, an estimate which implies 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 which 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 re-estimate 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.

5 Discussion

This research reports novel estimates of the race-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 et al., 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 et al., 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 et al., 2016; Leslie and Pope, 2017) or peer effects (Bayer et al., 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 Anderson, 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 et al., 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 Jr, 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 or female police officers (Donohue III and Levitt, 2001; McCrary, 2007; West, 2018; Miller and Segal, 2019; Harvey and Mattia, 2019; Ba and Rivera, 2020; Linos and Riesch, 2020) as well as by the application of a "precision policing" strategy in which police effort is re-allocated towards the most serious offending in a community (Chalfin et al., 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 "de-funding" 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, 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 et al., 2008). There is also evidence that social service-based strategies such as summer jobs for disadvantaged youth (Heller, 2014; Gelber et al., 2016; Davis and Heller, 2017), cognitive behavioral therapy (Blattman et al., 2017; Heller et al., 2017), mental health treatment (Deza et al., 2020; Jácome, 2020) and local non-profits more generally (Sharkey et al., 2017) can have important crime-reducing effects. While social service interventions are often difficult to scale (Mofiitt, 2006; Ludwig et al., 2011), the increasing number of studies which show that there are 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 – an outcome which 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.

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Table 1: Summary Statistics

	Mean	S.D.		Mean	S.D.
Outcomes			**Covariates**		
Homicide Victims	244.26	(397.01)	Population	1593676	(2402359)
Black	138.93	(214.65)	Total Government Expenditure	12781422	(29128508)
White	63.56	(143.18)	Total Government Revenue	12775643	(28812641)
Homicide Clearance Rate	66.11	(22.50)	Total Taxes	5011031	(12037901)
Black	63.51	(24.90)	% Black	24.23	(18.23)
White	70.26	(23.55)	% White	48.56	(19.74)
Quality of Life Arrests	58178	(132128)	% Hispanic	18.98	(16.99)
Black	29896	(71320)	% Male	48.26	(1.29)
White	27605	(61220)	% Age <14	20.37	(2.91)
Index Crime Arrests	16430	(26081)	% Age 15-24	16.00	(2.76)
Black	9142	(15507)	% Age 25-44	31.18	(3.15)
White	7030	(11080)	% Age >45	32.44	(4.29)
Index Crimes	93928	(145967)	% Female Head of Household	16.34	(4.58)
			% Never Married	36.96	(7.09)
			% Education < High School	24.47	(8.94)
			Unemployment Rate	8.68	(3.08)
			Poverty Rate	16.97	(4.87)
			Median Household Income	36314	(7749)

	Mean	S.D.		N
Policing			**Sample Counts**	
UCR Employment	5831	(10288)	Number of Cities	242
ASG Employment IV			N: ASG Models	9438
ASG Employment	6647	(12447)	N: COPS Models	7018
COPS Eligible Hires IV				
Eligible Hires (Per Grant)	143.04	(346.68)		
Number of Hiring Grants	1125			

Note: Summary statistics are weighted by population of each city in 1980. COPS IV Models cover the period 1990-2018, 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

	A. ASG IV					
	Coeff.	S.E.	β/Pop.	S.E.	Mean	N
First Stage						
Police Employment	0.962	(0.041)	-	-	6047.0	8645
(F-Test = 553.38)						
Homicides						
Victims	-0.058	(0.004)	-0.003	(0.000)	249.0	8553
Black	-0.026	(0.003)	-0.006	(0.001)	140.4	8522
White	-0.016	(0.002)	-0.002	(0.000)	65.5	8502
Difference: P-Value	*0.002*		*0.000*			
Clearance Rate	0.001	(0.001)	-	-	65.2	7675
Black	0.001	(0.001)	-	-	62.6	6065
White	-0.001	(0.001)	-	-	69.5	7045
Difference: P-Value	*0.227*					
Arrests						
Quality of Life	7.12	(0.88)	0.53	(0.06)	60244	7804
Black	2.15	(0.51)	0.66	(0.16)	30896	7768
White	5.03	(0.50)	0.55	(0.05)	28827	7779
Difference: P-Value	*0.000*		*0.498*			
Index	-0.97	(0.28)	-0.07	(0.02)	16351	7796
Black	-0.68	(0.20)	-0.21	(0.06)	8930	7753
White	-0.45	(0.09)	-0.05	(0.01)	7214	7770
Difference: P-Value	*0.291*		*0.009*			
Index Crimes	-17.82	(1.40)	-1.07	(0.08)	96892	8645

Note: Table 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 U.S. Census. Standard errors are clustered at the city-level. Models are weighted by population of each city in 1980. The data sample covers 1981-2018. Models have differing observations due to data availability and the outlier cleaning procedure described in 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 does not include sub-categories 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.

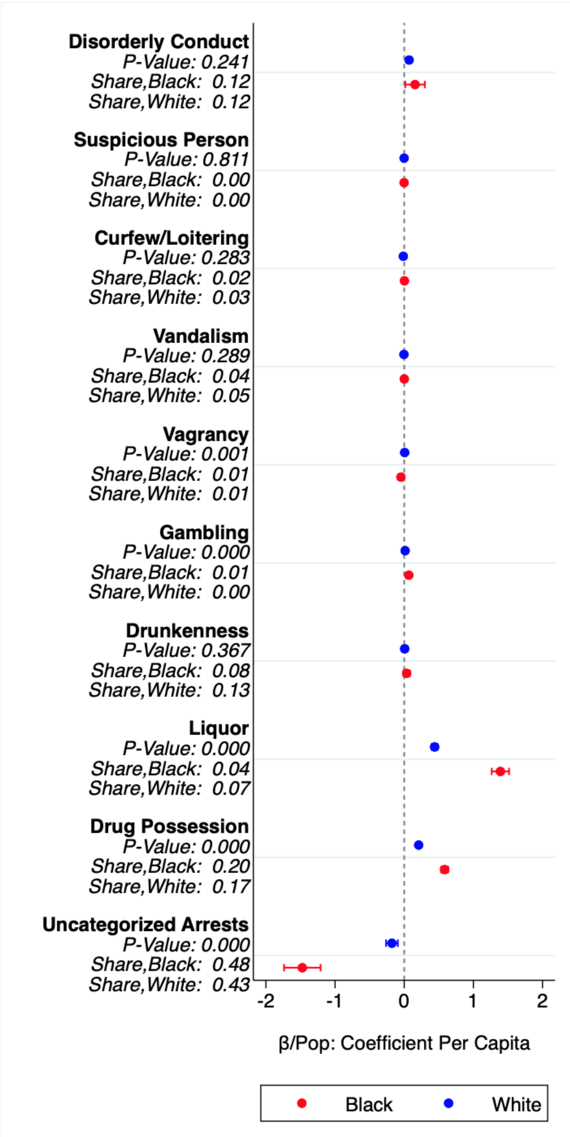
Table 3: Marginal Impact of Police Employment
COPS Eligible Hires IV

	B. COPS IV					
	Coeff.	S.E.	β/Pop.	S.E.	Mean	N
First Stage						
Police Employment	3.196	(0.806)	-	-	6390.7	6623
(F-Test = 15.71)						
Homicides						
Victims	-0.103	(0.010)	-0.006	(0.001)	223.3	6530
Black	-0.050	(0.005)	-0.012	(0.001)	130.0	6501
White	-0.044	(0.001)	-0.008	(0.000)	59.2	6489
Difference: P-Value	*0.205*		*0.000*			
Clearance Rate	0.001	(0.001)	-	-	60.4	5766
Black	0.001	(0.001)	-	-	56.8	4598
White	0.000	(0.002)	-	-	66.4	5223
Difference: P-Value	*0.603*					
Arrests						
Quality of Life	22.01	(5.09)	1.74	(0.40)	49908	5839
Black	8.17	(1.64)	2.80	(0.56)	24807	5831
White	14.01	(3.47)	1.66	(0.41)	24674	5818
Difference: P-Value	*0.128*		*0.102*			
Index	-1.60	(0.37)	-0.13	(0.03)	13366	5833
Black	-1.14	(0.21)	-0.39	(0.07)	7007	5808
White	-0.55	(0.17)	-0.06	(0.02)	6137	5811
Difference: P-Value	*0.032*		*0.000*			
Index Crimes	-23.54	(1.98)	-1.39	(0.12)	83209	6623

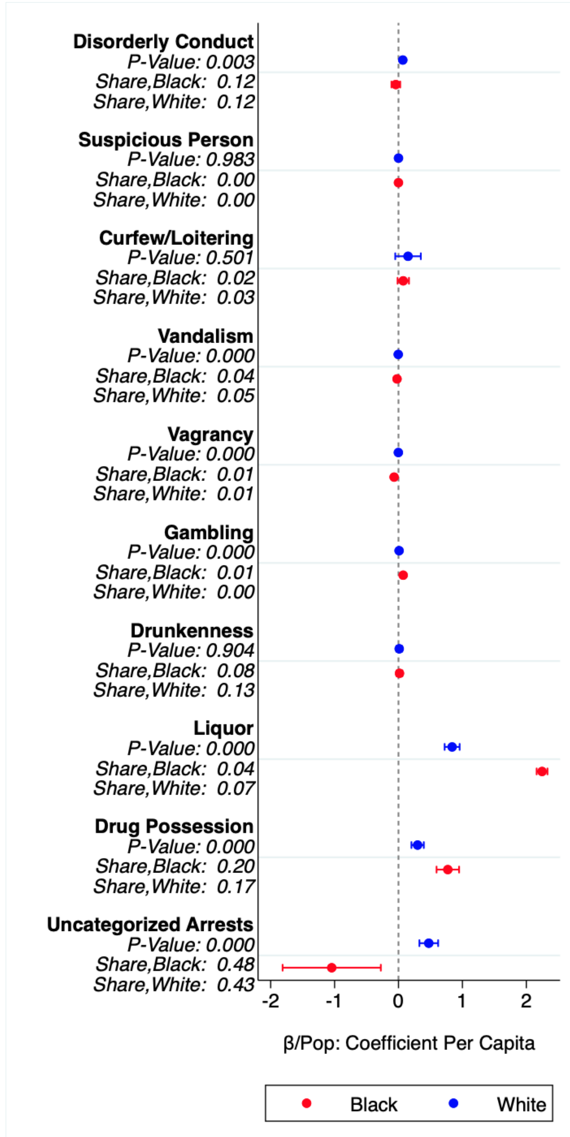
Note: Table reports estimates from equation (1) in which the once-lagged number of sworn police officers in a city derived from the FBI's Uniform Crime Reports is instrumented using the number of eligible hires awarded through a COPS Hiring grant. Standard errors are clustered at the city-level. Models are weighted by population of each city in 1980. The data sample covers 1990-2018. Models have differing observations due to data availability and the outlier cleaning procedure described in 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 non-hiring grant award size and whether a city applied for a hiring or non-hiring grant (lagged). "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests does not include sub-categories 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.

Figure 1: Effects of Police Force Size on Quality of Life Arrests by Race

A. ASG Employment IV



B. COPS Eligible Hires IV



Note: 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 U.S. Census or the number of eligible hires awarded through a COPS Hiring grant. In Figure A, the instrument is police employment from the U.S. Census; in Figure B the instrument is the number of eligible hires awarded through a COPS Hiring grant. Models include covariates in Table 1; Figure B also controls for non-hiring grant award size and whether a city applied for a hiring or non-hiring 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. Figure A covers 1981-2018; Figure B covers 1990-2018. Arrest categories correspond to Appendix Table 12. Models have differing observations due to data availability and the outlier cleaning procedure described in 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 does not include sub-categories 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.

ONLINE APPENDIX

A1 Identification Strategy

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

$$Y_{it} = \beta S_{it-1} + \gamma^{'} X_{it} + \rho_i + \psi_{st} + \varepsilon_{it}$$

In this regression, Y_{it} is a given outcome of interest measured in city i in year t. In keeping with the extant literature, S_{it-1} is the number of sworn police officers measured in the previous year (Levitt, 1996, 2002; Chalfin and McCrary, 2018). We pursue two different instrumental variables strategies in order to obtain a plausibly consistent estimate of β. We describe each of the two strategies in this appendix.

A1.1 Measurement Error Models

As Chalfin and McCrary (2018) show and as has been suggested indirectly by King et al. (2011), police force size in U.S. cities is measured with error in the available administrative data. We demonstrate this empirically using two measures of police manpower which are both available annually in a large number of U.S. cities. The first measure, which can be found in the Law Enforcement Officers Killed or Assaulted (LEOKA) data collected by the FBI Uniform Crime Report (UCR) program is the mainstay of the empirical literature that studies police manpower or uses police manpower as a control variable. These data contain a point-in-time measure of the number of sworn police employees in each year, as of October 31st. A second measure of police manpower is available in the U.S. Census Annual Survey of Government Employment (ASG) which collects data on municipal employees. As with the UCR system, the ASG reports a point-in-time measure of police, reporting the number of sworn officers employed as of March 31st of a given year (for 1997-2018 the reference date is June 30th).

Following the approach of Chalfin and McCrary (2018), we begin by demonstrating that while the two available measures of police align well when plotting the raw data, there are important differences between the two measures once city and state-by-year fixed effects and covariates are netted out. We present this analysis in Appendix Figure A1.

In the figure, Panel A presents a scatterplot of the raw measures; Panel B presents a scatterplot of the two measures, residualized using the covariates and fixed effects described in (1). The fact that the two measures are no longer as well aligned conditional on covariates provides evidence that there may be important errors in the official FBI UCR measure of police. It likewise implies that β, estimated using equation (1), may be biased as a result of measurement error.

In the presence of two potentially independent measures of the same quantity, the standard solution to the measurement error problem is to instrument one measure with the other, retaining variation that is common to both measures. As is shown by Fuller (1987), this IV framework allows for a consistent estimate of the parameter of interest subject to the assumption that the measures are independent. To motivate this property of the classical measurement error model, suppose that the two observed series on police force size (S_{it} and Z_{it}) are related to the true measure as:

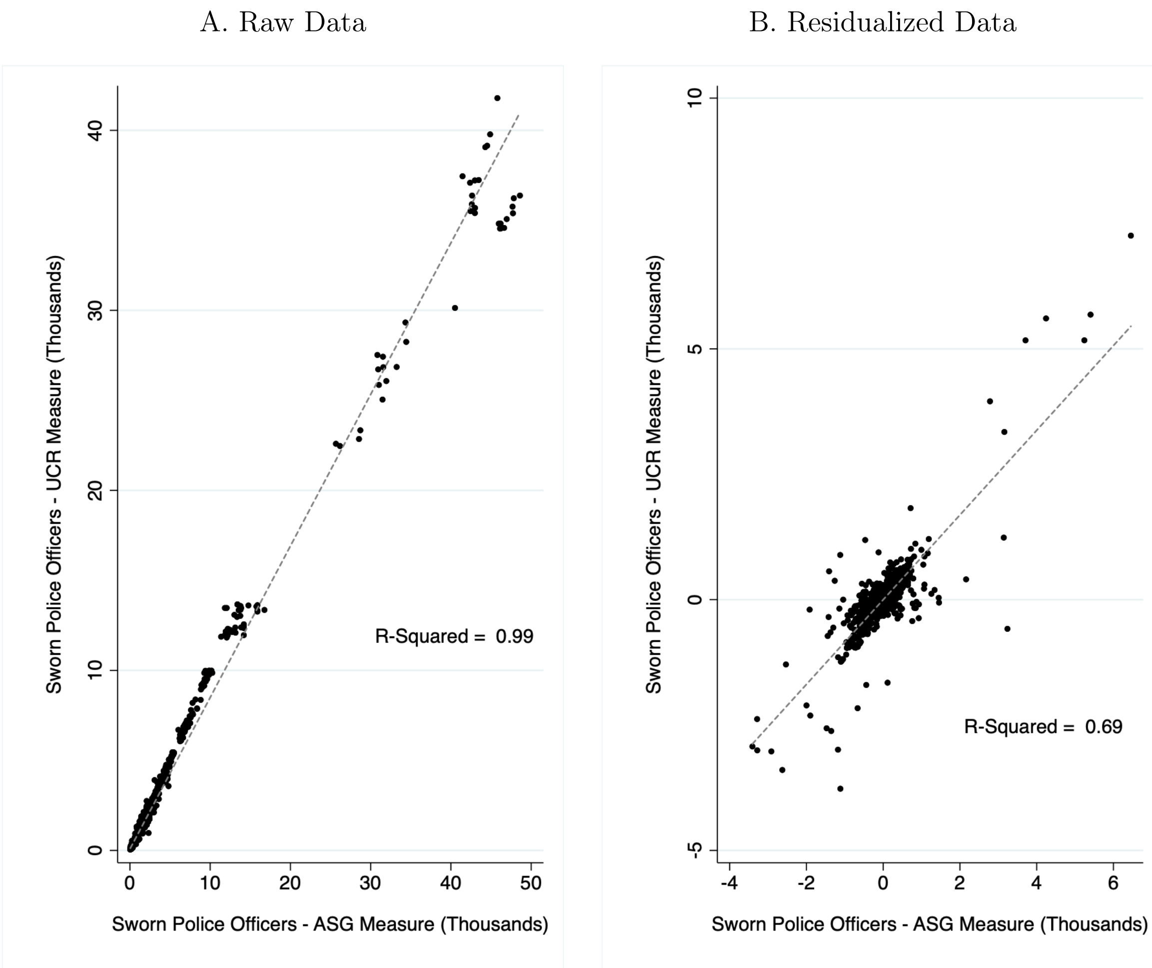
$$S_{it} = S^*_{it} + u_{it} \quad (2)$$

$$Z_{it} = S^*_{it} + v_{it} \quad (3)$$

Further suppose that the outcome of interest, Y_{it}, is related to police force size as:

$$Y_{it} = \beta S^*_{it} + \gamma^{'} X_{it} + \varepsilon_{it} \quad (4)$$

Figure A1: Two Measures of Police Force Size



Note: Panel A plots the UCR measure of police force size (y-axis) against the U.S. Census measure of police force size (x-axis). In Panel B, both measures are residualized to account for city and state-by-year fixed effects and covariates.

Here, S_{it} is the UCR measure of police in a given city and year, Z_{it} is the ASG measure of police, S^*_{it} is the "true" number of sworn police officers or the "signal" and X_{it} are other covariates measured without error. For notational simplicity, we are omitting the fixed effects terms. The error terms, u_{it} and v_{it}, are mean zero measurement errors that are mutually uncorrelated and are likewise uncorrelated with ε_{it}, S^*_{it} and X_{it} and ε_{it}.

A famous result from the econometric literature on measurement errors (see, for example, Wooldridge (2002), Section 4.4.2) relates the probability limit of the least squares regression estimate of β, under the assumptions of the classical measurement error model:

$$plim_{n\to\infty}\ \hat{\beta}_{OLS} = \beta\ \times\ \frac{\sigma^2_*(1-R^2)}{\sigma^2_*(1-R^2)+\sigma^2_u} \tag{5}$$

In (5), σ_u^2 is the variance of the error term in (2), and R^2 is the population R-squared from a regression of the signal, S_{it}^*, on X_{it}. This formula includes two important ideas. First, since $\sigma_u^2 > 0$, a least squares estimate of β will be too small in magnitude. Second, while it is a staple of empirical work to confirm that a regression estimate is robust to the inclusion of various control variables, equation (5) indicates that the cure of additional covariates may be worse than the disease of omitted variables bias. Adding more controls increases the R^2, exacerbating any attenuation bias.

Next, assume that X_{it} is measured without error and that S_{it} and Z_{it} are residualized to remove shared variation with X_{it}. In that case, under the classical measurement error model, the probability limit on the coefficient on Z_{it} in a regression of $\tilde{S}_{it}$ on $\tilde{Z}_{it}$ is given by:

$$\frac{cov(\tilde{S}, \tilde{Z})}{var(\tilde{Z})} = \frac{cov(\tilde{S^*} + \tilde{u}, \tilde{S^*} + \tilde{v})}{var(\tilde{Z})} = \frac{var(\tilde{S^*})}{var(\tilde{Z})} \equiv \pi \tag{6}$$

This implies that the ratio of the least squares estimate of the police elasticity of crime, relative to the estimate of π, is consistent for β, suggesting a role for an instrument.

Table 1: Test of the Equality of Forward and Reflected IV Estimates

	Forward Coeff.	S.E.	Reflected Coeff.	S.E.	Difference: P-Value
Homicide Victims	-0.058	(0.004)	-0.064	(0.005)	0.370
Black	-0.026	(0.003)	-0.027	(0.003)	0.757
White	-0.016	(0.002)	-0.010	(0.001)	0.025
Homicide Clearance Rate	0.001	(0.001)	0.000	(0.001)	0.606
Black	0.001	(0.001)	0.000	(0.001)	0.750
White	-0.001	(0.001)	-0.001	(0.001)	0.666
Quality of Life Arrests	7.12	(0.88)	6.29	(0.75)	0.471
Black	2.15	(0.51)	1.03	(0.57)	0.143
White	5.03	(0.50)	5.51	(0.25)	0.388
Index Arrests	-0.97	(0.28)	-0.87	(0.26)	0.789
Black	-0.68	(0.20)	-0.63	(0.19)	0.860
White	-0.45	(0.09)	-0.40	(0.09)	0.716
Index Crimes	-17.82	(1.40)	-20.40	(1.22)	0.165

Note: Table reports coefficients from the "forward" and "reflected" IV regressions derived from equation (1) in which a given measure of police force size is instrumented using an alternative measure of police force size. In the forward specification, the UCR measure of police is the endogenous regressor and the U.S. Census measure of police is the instrument. The roles are reversed in the reflected specification. In the third column, we report the p-value on a test of the equality of the forward and reflected coefficients.

Finally, we need to consider the extent to which the assumptions of the classical measurement error model hold in practice. As noted by Chalfin and McCrary (2018), the classical measurement error assumes that S and Z are independent and mean zero but does not prescribe a precise role for S

and Z in the instrumental variables setup. That is, under the classical measurement error model, it is *a priori* unclear which measure should play the role of the instrumental variable and which measure should play the role of the endogenous covariate in the IV setup. More formally, $\frac{cov(Z,Y)}{cov(Z,S)}$ will, in expectation, equal $\frac{cov(S,Y)}{cov(S,Z)}$. This insight suggests that an omnibus test of the classical measurement error model is to test the equality of β from an IV regression in which S is instrumented using Z and β from an IV regression in which Z is instrumented using S. To the extent that these estimates are significantly different from one another, at least one of the assumptions of the classical measurement error must fail to hold–see Chalfin and McCrary (2018) for a detailed motivation of this feature of the classical measurement error model. We can test this proposition formally by stacking the IV orthogonality conditions for the "'forward" and "reflected" IV models in a broader set of moments:

$$g_i(\beta) = \begin{pmatrix} Z_{it}(Y_{it} - \beta_1 S_{it} - \gamma_1^* X_{it} \\ X_{it}(Y_{it} - \beta_1 S_{it} - \gamma_1^* X_{it} \\ S_{it}(Y_{it} - \beta_2 Z_{it} - \gamma_2^* X_{it} \\ X_{it}(Y_{it} - \beta_2 Z_{it} - \gamma_2^* X_{it} \end{pmatrix} \tag{7}$$

We then test the pooling restriction that $\beta_1 = \beta_2$. The results of this exercise are available in Appendix Table 1 which, for each of our primary outcomes, reports the forward and reflected IV estimates as well as the p-value on the equality between the coefficients.[13]

With respect to our most central outcome — homicide victimization by race — there is little evidence against the classical measurement error model as the forward and reflected IV estimates are extraordinarily similar. With only a single exception among 16 tests, we fail to reject the null hypothesis that $\beta_1 = \beta_2$. As such, the IV estimates presented in Table 2 in which we instrument for the UCR measure of police manpower using the U.S. Census measure are expected to be consistent subject to selection assumptions.

[13]This test is available as Hansen's J-test of overidentifying restrictions. In practice, this test is also available by stacking the equations and estimating the interaction term between the instrument and the sample.

A1.2 COPS Eligible Hires Instrument

A1.2.1 Background on COPS Grants

The Community Oriented Policing Services (COPS) office of the Department of Justice was established under the Violent Crime Control Act of 1994 with the goal of distributing funding for local police departments to improve operations and increase police hiring. Approximately half of COPS funding has been distributed through hiring grants, which have retained the same basic features over time. These three year grants require that police departments not use this funding to supplant funds for existing officers and that departments match a portion of the funds distributed.[14] Non-hiring grants have supported investments police technology, targeted crime initiatives, and community policing programs.

Appendix Figure A2.A displays the number of hiring and non-hiring grants distributed in each year within our sample of large police departments in the U.S. Hiring grants have not been evenly distributed over time; funding declined in the early 2000s amid concerns that the funds were being used to supplant police department budgets for existing hires. However, following the financial crisis in 2008, funding for this program was increased as a way of providing stimulus funds to local governments and to avoid large cuts to police forces. Appendix Figure A2.B shows that funding for hiring grant programs has exceeded funding for non-hiring grants in each year, with a large $600 million spike in 2009.

Each hiring grant designates a number of "eligible hires." Appendix Figure A2.C shows the total eligible hires granted in each year within our sample of large cities. These grants are capable of providing meaningful shocks to the size of police departments, as the average department in our sample has 740 officers (5830 officers when weighted by population) and the average hiring grant awards funding for 23.5 officers (143 when weighted by population).

Law enforcement agencies apply for grants by submitting short narrative applications that outline plans for using funds. Applications are then reviewed by the COPS office and awarded according to fiscal need, application narrative and other office funding constraints. In later years of the grant program, COPS scored applications and weighted scores based on fiscal need (30-75%), local crime conditions (20-35%), and community policing objectives (15-50%). The COPS office faces the additional allocation constraint that at least 0.5% of funds must go to each state and 50% of funding must go to departments serving cities with fewer than 150,000 residents during each grant cycle. While local crime conditions are a small factor in the allocation process, prior work has shown that conditional on fixed effects and city-level covariates, grant awards do not appear to be endogenous to changes in crime rates (Evans and Owens, 2007; Weisburst, 2019b).

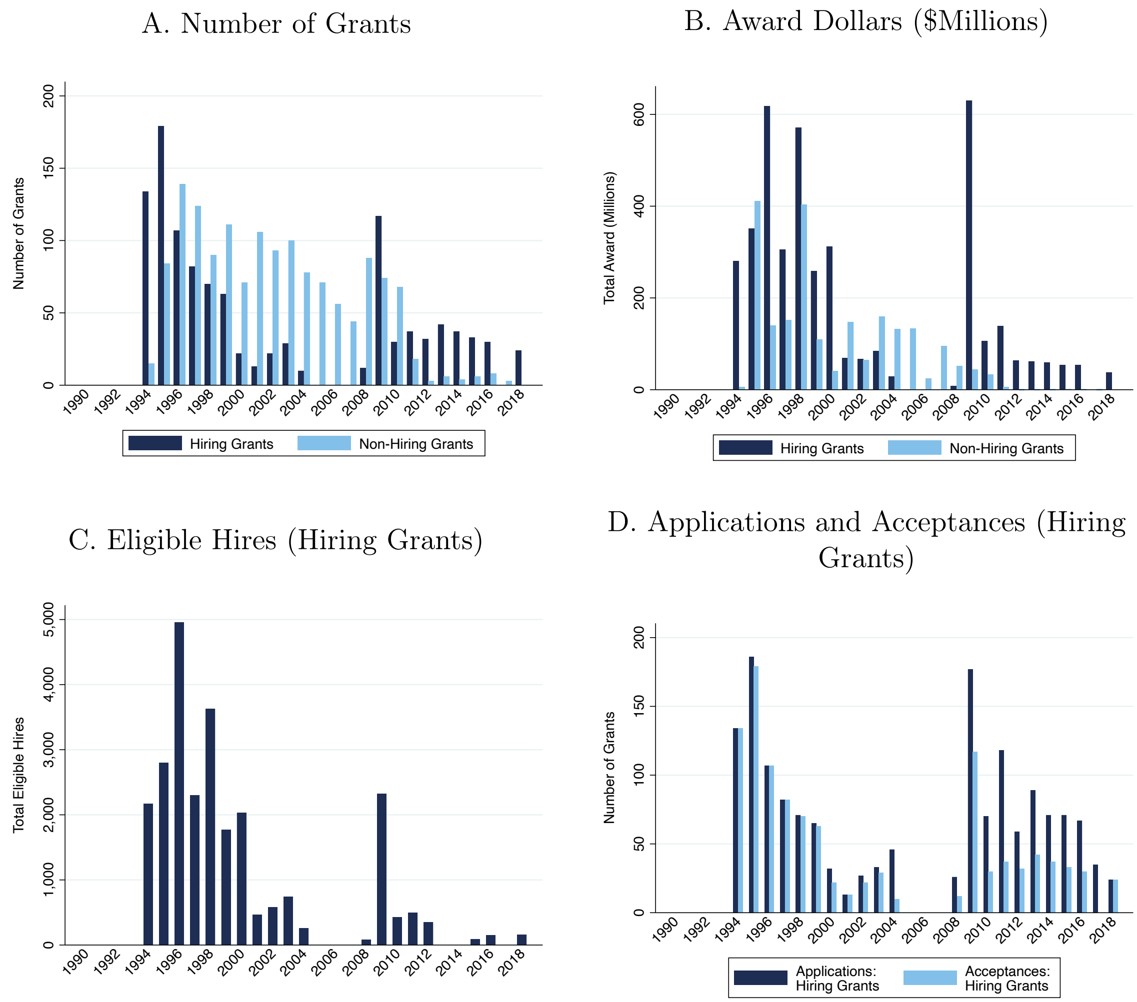
This paper is also able to exploit variation in grant applications that are rejected in the estimation model. Appendix Figure A2.D shows the number of grant applications and acceptances in each year of the COPS program within our sample. Prior to 2000, nearly all applications for hiring grants were awarded. However, after 2000, these grants became more competitive and demand for hiring grants exceeded the number of grants awarded.

A1.2.2 Discussion of Model

The main features of the model are provided in Section 1; this section provides additional detail on the model specification and robustness. The general model used in this paper is:

[14] Prior to 2009, hiring grants provided up to 75% funding per officer or a max of $75,000 per officer over 3 years. In 2009, funding rules were changed to provide up to 100% funding per officer or a max of $125,000 per officer over 3 years.

Figure A2: COPS Grants Over Time



Note: The figures above summarize the DOJ COPS grant variation between 1990-2018 for this sample of cities. Panel A plots the number of hiring grants and other non-hiring COPS grants distributed in each year. Panel B plots the award dollars distributed each year under these two types of grants. Panel C plots the number of eligible hires designated by hiring grants in each year. Panel D plots the number of grant applications and acceptances in each year of the sample.

$$
\begin{aligned}
Y_{it} &= \beta S_{it-1} + \gamma' X_{it} + \rho_i + \psi_{st} + \varepsilon_{it} \\
S_{it-1} &= \pi Z_{it-1} + \phi' X_{it} + \rho_i + \psi_{st} + \mu_{it}
\end{aligned}
$$

where Y_{it} is the outcome of interest, S_{it-1} is the UCR measure of police employment, and Z_{it-1} is the COPS instrument. This model includes U.S. Census covariates in X_{it} (included in Table 1), police department fixed effects ρ_i, and state by year fixed effects ψ_{st}. More specifically, the COPS Eligible Hires IV specification is as follows:

$$
\begin{aligned}
Y_{it} =& \beta Police_{it-1} + \gamma_1 AwardNonHiring_{it-1} \\
& + \gamma_2 ApplyHiring_{it-1} + \gamma_3 ApplyNonHiring_{it-1} \\
& + \gamma' X_{it} + \rho_i + \psi_{st} + \varepsilon_{it} \\
Police_{it-1} =& \pi COPSEligible_{it-1} + \phi_1 AwardNonHiring_{it-1} \\
& + \phi_2 ApplyHiring_{it-1} + \phi_3 ApplyNonHiring_{it-1} \\
& + \phi_x' X_{it} + \rho_i + \psi_{st} + \mu_{it}
\end{aligned}
$$

There are three additional grant controls in these models. First, the model controls for the size of any non-hiring grant awards, which may fund technology improvements or targeted crime initiatives.[15] Second, the model includes indicators for whether an agency applied for hiring or non-hiring grants in a particular year. This variable captures changes in police employment and crime outcomes associated with grant applications, rather than awards, and controls for the possible outcome that departments increase (or decrease) hiring when they are interested in obtaining COPS grant funds but these funds are not awarded. The resulting model has the identification assumption that conditional on the decision to apply for a hiring grant, the number of officers designated by an awarded COPS hiring grant does not depend on changes in crime within a city. These application controls increase precision, though as discussed below, the models are robust to excluding them.

The model draws heavily on the existing literature on the COPS program. The models used in Evans and Owens (2007); Owens (2013) are identical to the model above, when the application controls are not included. Weisburst (2019b) explicitly controls for grant applications and uses an excluded instrument of indicators for grant awards, where both application and award variables are defined over a grant award period, rather than in the first year the grant was distributed (lagged), as in the above model.

We include several variants of this model as robustness checks in Appendix Table 2. In specification (2), we assign grant eligible hires, awards, and applications according to the full time period of a grant from the first year of the award to the year when the funding ends, a feature of the design in Weisburst (2019b). The estimates using this approach are larger in magnitude but qualitatively consistent with the preferred estimates. In specifications (3)-(5), we consider different sub-groups of the sample defined by police department participation in the COPS grant programs. The results are robust to restricting to cities that applied for a hiring grant (3), received a hiring grant (4), or cities that both had grant applications that were accepted and rejected (5) at different points in the study sample period. Lastly, in specification (6), the results are robust to excluding controls for time-varying grant applications.

[15] The dollar value of hiring grants is excluded as this quantity is nearly perfectly collinear with the number of officers eligible for hiring for a grant, or $COPSEligible$.

Table 2: Additional Robustness Specifications, COPS IV

B. COPS Eligible Hires IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(1) Baseline Model	-0.1030	-0.0503	-0.0442	22.013	8.169	14.015
(First Stage F-Test = 15.71)	(0.0104)	(0.0047)	(0.0010)	(5.087)	(1.642)	(3.473)
Race Difference: P-Value			*0.205*			*0.128*
β/Pop	-0.006	-0.012	-0.008	1.74	2.80	1.66
Race Difference: P-Value			*0.000*			*0.102*
Y-Mean	223.35	130.03	59.17	49908.0	24807.0	24674.4
N	6530	6501	6489	5839	5831	5818
(2) Grants Split Across Grant Years	-0.1871	-0.0837	-0.0725	45.777	16.668	29.100
(First Stage F-Test = 60.07)	(0.0188)	(0.0103)	(0.0105)	(5.498)	(1.421)	(5.093)
Race Difference: P-Value			*0.445*			*0.019*
β/Pop	-0.011	-0.021	-0.012	3.62	5.71	3.44
Race Difference: P-Value			*0.003*			*0.004*
Y-Mean	223.35	130.03	59.17	49908.0	24807.0	24674.4
N	6530	6501	6489	5839	5831	5818
(3) Cities that Applied for Grants	-0.1030	-0.0503	-0.0442	22.016	8.171	14.016
(First Stage F-Test = 15.70)	(0.0104)	(0.0047)	(0.0010)	(5.088)	(1.642)	(3.474)
Race Difference: P-Value			*0.204*			*0.128*
β/Pop	-0.006	-0.012	-0.008	1.74	2.79	1.65
Race Difference: P-Value			*0.000*			*0.102*
Y-Mean	223.65	130.20	59.25	50006.3	24856.8	24722.6
N	6502	6473	6461	5805	5797	5784

Table 2: Additional Robustness Specifications, COPS IV (Continued)

B. COPS Eligible Hires IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(4) Cities with Accepted Grant	-0.1028	-0.0503	-0.0442	21.901	8.123	13.948
(First Stage F-Test = 16.19)	(0.0101)	(0.0046)	(0.0010)	(5.018)	(1.619)	(3.430)
Race Difference: P-Value			*0.198*			*0.125*
β/Pop	-0.006	-0.012	-0.007	1.71	2.74	1.63
Race Difference: P-Value			*0.000*			*0.100*
Y-Mean	225.81	131.46	59.82	50571.0	25142.8	24998.4
N	6330	6302	6292	5636	5628	5615
(5) Cities with Accepted & Rejected Grants	-0.1219	-0.0599	-0.0460	27.969	9.890	18.225
(First Stage F-Test = 4.90)	(0.0271)	(0.0128)	(0.0019)	(7.187)	(1.917)	(5.313)
Race Difference: P-Value			*0.281*			*0.140*
β/Pop	-0.006	-0.013	-0.007	1.94	3.08	1.87
Race Difference: P-Value			*0.015*			*0.138*
Y-Mean	237.52	130.49	65.78	60307.1	29672.2	30154.9
N	4711	4688	4684	4269	4263	4253
(6) Drop Application Controls	-0.1039	-0.0508	-0.0452	21.807	8.031	13.917
(First Stage F-Test = 14.49)	(0.0104)	(0.0049)	(0.0012)	(4.983)	(1.596)	(3.417)
Race Difference: P-Value			*0.270*			*0.119*
β/Pop	-0.006	-0.012	-0.008	1.73	2.75	1.65
Race Difference: P-Value			*0.000*			*0.104*
Y-Mean	223.35	130.03	59.17	49908.0	24807.0	24674.4
N	6530	6501	6489	5839	5831	5818

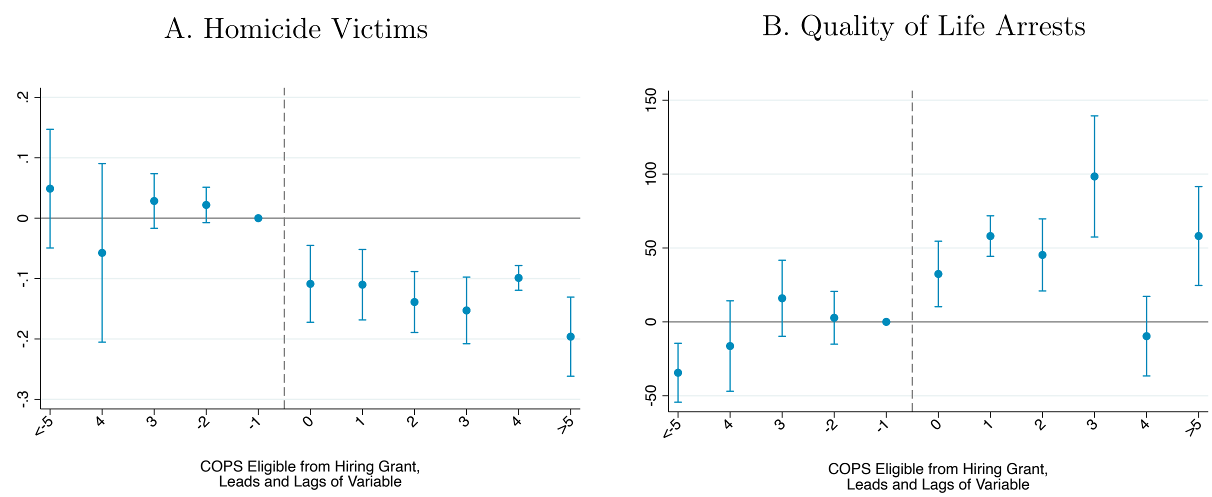
Note: Table reports estimates from equation (1) in which the once-lagged number of sworn police officers in a city derived from the FBI's Uniform Crime Reports is instrumented using the number of eligible hires awarded through a COPS Hiring grant. Baseline specifications correspond to models in Table 3. "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests does not include sub-categories for Hispanic residents; as a result, white population share includes Hispanic residents for these outcomes in calculating the "β/Pop." measure. Standard errors are clustered at the city-level.

A1.2.3 Reduced Form Results Over Time

As an additional check of the COPS instrument, we present the reduced form results of the model over time. This exercise directly relates the number of COPS eligible hires to our primary outcomes in the years preceding a grant award. To do this, we construct lead variables of the Cops Eligible Hires IV for the four preceding periods (t=-4,-3,-2, -1 omitted) and lag variables of the IV (t=0,1,2,3,4) as well as bookend variables that sum the leads and lags for periods -5 and before and +5 and later. Note that this framework uses the IV of Eligible Hires which is not an indicator for a grant but the number of officers designated by a grant. This structure flexibly permits multiple treatments over time, as a department that has two grant awards separated by a period of years may have positive values for both leads and lags in the same observation that reflect these multiple treatments.

Appendix Figure A3.A and A3.B shows these result for homicides and quality of life arrests. Prior to a COPS hiring grant, there is no trend in homicides, suggesting that the distribution of grants is exogenous to these outcomes. Coinciding with the grant awards there is a negative shift in the number of homicide victims that is persistent over time. Similarly, these outcomes do not show a pre-trend and show a consistent increase in this arrest category after the grant receipt.

Figure A3: Reduced Form Estimates Over Time, COPS Eligible Hires



Note: Standard errors are clustered at the city-level. The sample covers treatment variation from 1990-2018. Each graph plots the reduced form relationship between the number of eligible hires designated by COPS hiring grants and an outcome over time (IV). The graphs plot lags and leads of the IV, where the -5 and +5 categories are summed values of remaining periods, and the first lead (t=-1) is omitted. Controls include corresponding lags and leads of other grant variables: whether a city applied for a hiring or non-hiring grant, and the award size of non-hiring grant awards.

A2 Supplementary Results

In this appendix we present a series of supplementary results which compliment the analyses presented in the main body of the paper.

A2.1 Ordinary Least Squares Estimates

We begin by presenting least squares estimates of the effect of police manpower on each of our main outcomes estimated using equation (1). The results are presented in Table 3. The least squares estimates are negative but are smaller in magnitude than IV estimates using the COPS hiring instrument. With respect to the measurement error models, given the that the first stage coefficient is not far from 1, the OLS estimates are fairly similar, but remain smaller in magnitude.[16]

A2.2 Robustness

A2.2.1 Common Sample Results

We begin by re-estimating our results on the common sample of city-years for which all dependent variables are non-missing in the data. These estimates are presented in Table 4 and Table 5. In all cases, estimates are substantively similar to those reported in the main analysis. We utilize a sample that does not fully overlap in dependent variables for our baseline specification in order to use the maximal amount of information and increase power.

A2.2.2 Alternative Specifications

Next, in Tables 6 and 7 we further test the robustness of the results. First, we re-estimate our models without using population weights (2). These estimates conform closely with the baseline estimates that are weighted by each city's 1980 population. Next, we present "reflected" estimates in which we switch the role of the UCR and the U.S. Census measures of police manpower or, in the case of the COPS instrument, substitute the U.S. Census ASG measure of police for the UCR measure (3). These coefficients provide an alternative estimate of the effect of police manpower given that the role of each variable is ambiguous under the assumptions of the classical measurement error model. In the case of the COPS instrument, the estimates also provide assurance that the estimates reported in the main body of the paper are robust to this alternative employment measure.

In our baseline model, we estimate the effect of police manpower on race-specific homicide victimization using the first lag of the police variable. In model (4), we re-specify the model using a contemporaneous measure of police manpower. Once again, estimates are very similar. In models (5) and (6), instead of conditioning on interacted state-by-year fixed effects we condition instead on either on population group-by-year fixed effects, dividing our cities into the following population groups 50-100k, 100-200k, >250k residents in 1980 (5) or homicide group-by-year fixed effects which use quartiles of the homicide rate in 1980 (6). In model (7), we estimate the model with additional controls for municipal education spending to adjust for spending allocation decisions in cities; the results show that the returns to police manpower are similar when holding total municipal spending and education spending fixed. In each case, estimates are nearly identical to those reported in Tables 2 and 3.

In model (8) we present estimates in which we do not condition on covariates. For the measurement error models, these estimates are larger in magnitude which is consistent with the idea that the

[16] We note that as our models are estimated in levels, the strength of the first stage coefficient is closer to 1 than in Chalfin and McCrary (2018) which estimates models using growth rates.

inclusion of covariates helps to capture time-varying omitted factors which are correlated with police hiring and outcomes. For the models which use the COPS instrument, the homicide estimates are smaller, though the sign of the estimates is consistent with that in our baseline models.

Next, we consider a log-log specification which yields a direct estimate of the elasticity of each outcome with respect to police force size, where outcomes are defined as $\log(y+1)$ to account for zeros (9). Because there are sometimes zero homicides in a given year for a given subgroup of victims, we utilize the inverse hyperbolic sine transformation (Ramirez et al., 1994) in (10). For the ASG IV models, the elasticity of overall homicides with respect to police manpower is approximately -0.5, which is smaller than the elasticity calculated from our levels models of -1.4-3. Our log-log models show estimates are substantively similar to those reported in most of the prior literature including Evans and Owens (2007) and Chalfin and McCrary (2018).[17] Using the COPS IV, there is no first stage when the model is specified in log-log form in this set of cities. This lack of a first stage is likely due to the small set of cities in this sample, as we are restricted to using large cities to merge to Census police employment and expenditure data which defines our baseline set of covariates. This sample differs from prior work on COPS that typically uses a larger set of cities with a lower population threshold (Evans and Owens, 2007; Mello, 2019; Weisburst, 2019b).

Next, we present estimates in which we do not remove outliers (11) and in which we use a balanced panel retaining only panels with complete data (12); estimates are not sensitive to either of these choices. Also, for the ASG models, we present estimates for the 1990-2018 sample period which corresponds with the sample period in the COPS models (13). Estimates for homicides are very similar between the two IV strategies when the models are executed using the same data. For "quality of life" arrests, the estimates are considerably larger in the COPS models indicating either that there is some remaining simultaneity bias in the measurement error corrected models or that the instruments identify different local average treatment effects.

A2.2.3 Police Force Size and Reporting of Arrests

Next we explicitly consider whether larger police forces could change patterns of reporting crime and arrests, particularly in terms of the large increases we observe in low-level or "quality of life" arrests. There are generally four reasons reported arrests could increase when police force size increases:

1. There is an increase in criminality when the police force expands.
2. There is an increase officer enforcement of offenses when the police force expands.
3. The UCR reports only the "highest offense" for any incident. This means that a reduction in serious charges that had previously been coupled with less serious charges could result in a mechanical increase in lower level offenses.
4. There is a change in police reporting of crime when the police force expands.

The first point is not consistent with the large decreases in homicides we observe. The second point is our leading primary hypothesis. The third hypothesis is unlikely because the increases in

[17] It is worth noting that our levels models yield incredibly similar estimates for population weighted and unweighted models implying that the number of lives saved is a constant function of the change in police employment in a city. Because these constant changes in homicide occur relative to very different base rates of homicide (and police employment), we do not expect a percentage change in police employment to produce a uniform percentage decrease in homicide in our sample. It is therefore unsurprising that the elasticities from the log-log models differ from the elasticities that are implied by our baseline models.

low-level (and other non-index arrests) dwarf the magnitude of the decreases in index arrests and homicides. We provide more evidence against the fourth hypothesis in Table 8.

While our primary estimates provide robust evidence *reported* arrests for low-level crimes increase, these are based on police reports. Thus a natural questions is whether police reporting changes as a police force expands. In particular, we are concerned with the possibility that increased manpower allows departments to record minor offenses to the FBI that it previously did not disclose.

We do not believe that this hypothesis is driving the results for several reasons. First, it is worth noting in all models we control for state-by-year FE, so any policy which varies within state across years (but is shared with departments) that is related to reporting protocols is accounted for with that control. Second, we focus on large departments which generally have most consistent reporting regimes. Moreover, we include "uncategorized arrests" in our definition of low-level arrests, so our approach accounts for any reduction in this category that could be offset by better categorization of other arrests.

However, it could still be that as resources (and officers) become more plentiful, departments record better records. To address this, in Table 8, we re-estimate the main models for low-level arrests. In panel (1) we provide our main estimates for comparison. In the next panel (2), we present estimates for the same models, expect now dropping all observations in which there were zero observations in the low-level or "quality of life" category. Essentially the results are unchanged, suggesting that the extensive margin of reporting particular sub-offenses is not driving the results. In the final two panels we explore whether the extensive margin crime reporting changes for departments for arrests subgroups. In Panel (3), we measure the outcome of whether a police department reported any low-level arrests in a particular year. In Panel (4), we measure the outcome of whether a police department reported at least one arrest in all sub-categories of the low-level arrest group. We find generally the estimated relationships are small, suggestive there are not large increases in reporting due to increases in police reporting.

A2.2.4 Sensitivity of Results

Finally, we consider the sensitivity of our estimates to highly leveraged cities. Given that estimates are similar with and without the use of population weights, highly leveraged cities are unlikely. We confirm this empirically in Appendix Figure A4 which re-estimates our primary outcomes removing one city at a time and plots the distribution of estimated treatment effects for homicide (Panels A and B) and "quality of life" arrests (Panels C and D).

A2.3 Treatment Effect Heterogeneity

A2.3.1 Disaggregated Race Categories

Our main analyses consider the impact of police force size on homicides with non-Hispanic white and non-Hispanic Black victims. In this section, we consider an alternative groupings for these outcomes in which individuals of Hispanic ethnicity are folded into the Black and white categories. We also separately estimate the effect of police force size on homicides with Hispanic victims. Estimates are presented in Appendix Table 9 and Appendix Table 10. There is not a large difference between estimates for non-Hispanic Black victims and overall Black victims since there are relatively few Black victims of Hispanic origin in the data. With respect to Hispanic victims, each police officer abates between 0.006 and 0.015 homicides with Hispanic victims depending on which IV estimate is used. Arrest outcomes cannot be split by Hispanic/Latinx ethnicity and these individuals are classified as white in this data set.

A2.3.2 Arrest Outcomes by Offense Type

Index Crime and Index Arrests We provide additional detail on the effect of police manpower on index crimes known to law enforcement and index crime arrests in Table 11. The most common index crimes are theft and burglary. Overall, violent crimes (homicide, rape, robbery and aggravated assault) constitute just over 20 percent of index crimes. Index crime arrests follow a similar pattern.

For both of our identification strategies, there is strong evidence that a larger police force leads to a reduction in index crimes. On an annual basis, each police officer hired is estimated to abate between approximately 0.07-0.1 homicides, 0.05-0.1 rapes, 0.6-0.8 aggravated assaults, 3-4 robberies, 4-5 burglaries, 5-7 thefts and 4-6 motor vehicle thefts. With respect to arrests, larger police forces lead to significantly fewer arrests for robbery and motor vehicle theft. In the COPS model, there is also evidence that large police forces make fewer arrests for homicide and burglary. Since a larger police force leads to reductions in both crime and arrests, this suggests that the primary driver of manpower-led crime reductions is deterrence rather than incapacitation (Owens, 2013).

Appendix Figure A5 explores heterogeneity in the arrest estimates by race, using the per capita estimates. Our aggregate finding that per capita declines in index arrests are larger for Black vs. white arrestees is driven by disparate race group effects for robbery, theft and motor vehicle theft arrests.

"Quality of Life" Arrests We provide additional detail on the effect of police manpower on "quality of life" arrests focusing on specific arrest types in Appendix Table 12. Aside from "uncategorized arrests," the most common quality of life arrests are drug possession, disorderly conduct and liquor law violations. Using both of our identification strategies, we see that the marginal "quality of life" arrests that are made when a city expands the size of its police force are predominantly for liquor law violations and drug possession and, to a lesser extent, disorderly conduct. The coefficients on liquor violations imply that such arrests are incredibly sensitive to police force size with increases of 0.3-0.5 arrests per 100,000 residents (or 6 to 8 total arrests) for every additional officer hired.

Appendix Figure 1 displays the race heterogeneity for each sub-offense using the per capita estimates. There are large and highly significant race disparities in liquor violation and drug possession arrests for both strategies, whereby arrests of Black individuals disproportionately increase in per capita terms. The opposite pattern is present for "uncategorized arrests," leading to total race differences in this aggregate category that are marginally significant using the COPS strategy and not significant for the ASG strategy.

Other Arrests We also present results for other arrests which are classified as neither index nor "quality of life" crimes. Such crimes include simple assaults, the sale of illegal drugs, driving under the influence (DUI), fraud and weapons charges among other offense types. Here we report evidence that larger police forces make more arrests for simple assault, fraud, forgery and sex offenses (other than rape) and fewer arrests for weapons possession and stolen property, though the effects vary by strategy. Likewise, the race differences shown in Appendix Figure A6 also differ across strategies.

A2.4 Deaths and Injuries of Police Officers

We also estimate the effect of police force size on violence against police officers. These results are presented in Appendix Table 14. We observe that each officer hired leads to between 0.14 and 0.23

fewer officer injuries. This result is counter-intuitive in the sense that, other things equal, the risk of adverse events rises with the size of a city's police force. Instead, the evidence suggests that this mechanical "exposure" effect is dominated by the protective effect of greater manpower and may increase the share of officers who patrol in teams or the speed which officers are able to assist a fellow officer in distress. We do not find any robust effects of law enforcement on officer deaths but these are difficult to study given that they are rare events.

Table 3: OLS Model Results

	Coeff.	S.E.	β/Pop.	S.E.	Mean	N
Homicides						
Victims	-0.051	(0.004)	-0.003	(0.000)	249.0	8581
Black	-0.022	(0.002)	-0.005	(0.001)	140.5	8550
White	-0.008	(0.001)	-0.001	(0.000)	65.5	8530
Difference: P-Value	*0.000*		*0.000*			
Clearance Rate	0.000	(0.001)	-	-	65.2	7698
Black	0.000	(0.001)	-	-	62.5	6087
White	-0.001	(0.001)	-	-	69.4	7069
Difference: P-Value	*0.264*					
Arrests						
Quality of Life	5.85	(0.70)	0.43	(0.05)	60121	7824
Black	0.96	(0.53)	0.30	(0.16)	30843	7788
White	5.12	(0.24)	0.56	(0.03)	28758	7799
Difference: P-Value	*0.000*		*0.113*			
Index	-0.80	(0.24)	-0.06	(0.02)	16342	7816
Black	-0.59	(0.18)	-0.18	(0.05)	8933	7773
White	-0.37	(0.08)	-0.04	(0.01)	7202	7790
Difference: P-Value	*0.276*		*0.012*			
Index Crimes	-16.33	(0.85)	-0.98	(0.05)	96791	8675

Note: Table reports estimates from equation (1) in which each outcome is regressed on the once-lagged number of sworn police officers in a city derived from the FBI's Uniform Crime Reports conditional on fixed effects and covariates. Standard errors are clustered at the city-level. All models are weighted by population of each city in 1980 and cover the period 1981-2018. Models have differing numbers of observations due to data availability and the outlier cleaning procedure for outcomes described in Appendix A3. OLS models directly relate UCR police employment to outcomes. All models include covariates in Table 1. "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests does not include sub-categories 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, except for the coefficient on "Quality of Life Arrests, Black."

Table 4: Common Outcome Sample: Marginal Impact of Police Employment
ASG Employment IV

	A. ASG IV					
	Coeff.	S.E.	β/Pop.	S.E.	Mean	N
First Stage						
Police Employment	0.925	(0.029)	-	-	4600.4	7511
(F-Test = 1047.27)						
Homicides						
Victims	-0.079	(0.009)	-0.006	(0.001)	245.3	7511
Black	-0.039	(0.005)	-0.012	(0.002)	132.1	7511
White	-0.042	(0.006)	-0.008	(0.001)	66.9	7511
Difference: P-Value	*0.695*		*0.016*			
Arrests						
Quality of Life	6.83	(0.85)	0.50	(0.06)	61548	7511
Black	2.05	(0.50)	0.63	(0.15)	31431	7511
White	4.98	(0.49)	0.54	(0.05)	29401	7511
Difference: P-Value	*0.000*		*0.566*			
Index	-1.02	(0.26)	-0.07	(0.02)	16554	7511
Black	-0.71	(0.19)	-0.22	(0.06)	8970	7511
White	-0.47	(0.09)	-0.05	(0.01)	7339	7511
Difference: P-Value	*0.243*		*0.004*			
Index Crimes	-19.87	(2.52)	-1.45	(0.18)	95511	7511

Note: Table 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 an alternative measure of sworn officers from the U.S. Census. Standard errors are clustered at the city-level. This table replicates Table 2 on a common sample that includes data on all outcomes. Clearance outcomes are excluded from this exercise as data is more restricted for this outcome. "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests does not include sub-categories for Hispanic residents; as a result, white population share includes Hispanic residents for these outcomes in calculating the "β/Pop." measure. Standard errors are clustered at the city-level. All estimates pass a Bonferroni multiple hypothesis correction.

Table 5: Common Outcome Sample: Marginal Impact of Police Employment
COPS Eligible Hires IV

	B. COPS IV					
	Coeff.	S.E.	β/Pop.	S.E.	Mean	N
First Stage						
Police Employment	2.452	(0.526)	-	-	4412.3	5608
(F-Test = 21.76)						
Homicides						
Victims	-0.117	(0.007)	-0.009	(0.001)	208.3	5608
Black	-0.058	(0.003)	-0.020	(0.001)	114.2	5608
White	-0.057	(0.003)	-0.012	(0.001)	58.8	5608
Difference: P-Value	*0.818*		*0.000*			
Arrests						
Quality of Life	22.01	(5.21)	1.72	(0.41)	51016	5608
Black	8.17	(1.70)	2.80	(0.58)	25243	5608
White	14.01	(3.50)	1.64	(0.41)	25201	5608
Difference: P-Value	*0.134*		*0.103*			
Index	-1.63	(0.38)	-0.13	(0.03)	13460	5608
Black	-1.15	(0.21)	-0.39	(0.07)	6964	5608
White	-0.56	(0.18)	-0.07	(0.02)	6243	5608
Difference: P-Value	*0.038*		*0.000*			
Index Crimes	-26.31	(1.81)	-2.06	(0.14)	76620	5608

Note: Table 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. This table replicates Table 3 on a common sample that includes data on all outcomes. Clearance outcomes are excluded from this exercise as data is more restricted for this outcome. "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests does not include sub-categories for Hispanic residents; as a result, white population share includes Hispanic residents for these outcomes in calculating the "β/Pop." measure. Standard errors are clustered at the city-level. All estimates pass a Bonferroni multiple hypothesis correction.

Table 6: Robustness Specifications, ASG Employment IV

A. ASG Employment IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(1) Baseline Model	-0.0580	-0.0258	-0.0156	7.120	2.147	5.031
(First Stage F-Test = 553.38)	(0.0043)	(0.0027)	(0.0018)	(0.876)	(0.511)	(0.500)
Race Difference: P-Value			*0.002*			*0.000*
β/Pop	-0.003	-0.006	-0.002	0.53	0.66	0.55
Race Difference: P-Value			*0.000*			*0.498*
Y-Mean	249.05	140.36	65.55	60243.6	30896.3	28827.3
N	8553	8522	8502	7804	7768	7779
(2) Not Weighted by Population	-0.0504	-0.0228	-0.0110	8.735	3.133	5.686
(First Stage F-Test = 45.41)	(0.0118)	(0.0069)	(0.0041)	(1.743)	(1.412)	(0.526)
Race Difference: P-Value			*0.141*			*0.090*
β/Pop	-0.018	-0.036	-0.008	3.25	5.39	3.00
Race Difference: P-Value			*0.013*			*0.325*
Y-Mean	39.21	22.92	9.94	8483.7	3796.1	4565.0
N	8553	8522	8502	7804	7768	7779
(3) ASG as Endogenous X, UCR as IV	-0.0636	-0.0271	-0.0105	6.290	1.027	5.514
(First Stage F-Test = 4997.01)	(0.0046)	(0.0029)	(0.0014)	(0.749)	(0.568)	(0.254)
Race Difference: P-Value			*0.000*			*0.000*
β/Pop	-0.004	-0.007	-0.002	0.47	0.32	0.60
Race Difference: P-Value			*0.000*			*0.185*
Y-Mean	249.05	140.36	65.55	60243.6	30896.3	28827.3
N	8553	8522	8502	7804	7768	7779

Table 6: Robustness Specifications, ASG Employment IV (Continued)

A. ASG Employment IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(4) Police Employment not Lagged	-0.0631	-0.0299	-0.0203	10.472	3.816	6.780
(First Stage F-Test = 550.26)	(0.0049)	(0.0029)	(0.0034)	(0.825)	(0.595)	(0.312)
Race Difference: P-Value			*0.035*			*0.000*
β/Pop	-0.004	-0.007	-0.003	0.77	1.16	0.73
Race Difference: P-Value			*0.000*			*0.021*
Y-Mean	255.96	143.55	67.95	61510.7	31364.9	29596.9
N	8567	8530	8521	7831	7789	7809
(5) Population Group by Year FE	-0.0562	-0.0246	-0.0154	6.732	1.990	4.818
(First Stage F-Test = 509.97)	(0.0043)	(0.0028)	(0.0017)	(0.914)	(0.515)	(0.554)
Race Difference: P-Value			*0.005*			*0.000*
β/Pop	-0.003	-0.006	-0.002	0.50	0.62	0.53
Race Difference: P-Value			*0.000*			*0.606*
Y-Mean	249.05	140.36	65.55	60243.6	30896.3	28827.3
N	8553	8522	8502	7804	7768	7779
(6) Homicide Group by Year FE	-0.0532	-0.0229	-0.0148	6.887	2.101	4.855
(First Stage F-Test = 551.91)	(0.0043)	(0.0028)	(0.0018)	(0.931)	(0.498)	(0.578)
Race Difference: P-Value			*0.015*			*0.000*
β/Pop	-0.003	-0.006	-0.002	0.51	0.65	0.53
Race Difference: P-Value			*0.000*			*0.478*
Y-Mean	249.05	140.36	65.55	60243.6	30896.3	28827.3
N	8553	8522	8502	7804	7768	7779

Table 6: Robustness Specifications, ASG Employment IV (Continued)

A. ASG Employment IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(7) Control for Education Spending	-0.0547	-0.0250	-0.0160	6.879	2.096	4.853
(First Stage F-Test = 488.81)	(0.0043)	(0.0028)	(0.0018)	(0.945)	(0.502)	(0.585)
Race Difference: P-Value			*0.000*			*0.000*
β/Pop	-0.003	-0.006	-0.003	0.51	0.65	0.53
Race Difference: P-Value			*0.000*			*0.513*
Y-Mean	249.90	140.84	65.77	60472.4	31016.4	28935.2
N	8447	8417	8396	7704	7668	7679
(8) Excluding Covariates	-0.1084	-0.0492	-0.0317	0.638	-0.534	1.213
(First Stage F-Test = 4258.64)	(0.0039)	(0.0019)	(0.0006)	(0.253)	(0.267)	(0.116)
Race Difference: P-Value			*0.000*			*0.000*
β/Pop	-0.006	-0.012	-0.005	0.05	-0.17	0.13
Race Difference: P-Value			*0.000*			*0.000*
Y-Mean	248.48	140.05	65.39	60099.8	30823.4	28758.3
N	8602	8571	8551	7848	7812	7822
(9) Log Model (Variable+1)	-0.5349	-0.7687	-0.3793	0.386	0.480	0.373
(First Stage F-Test = 180.94)	(0.2530)	(0.2896)	(0.2294)	(0.209)	(0.228)	(0.222)
Race Difference: P-Value			*0.292*			*0.737*
Y-Mean	4.14	3.46	2.87	9.5	8.5	8.8
N	8551	8520	8500	7802	7766	7777
(10) Inverse Hyperbolic Sine	-0.5204	-0.7649	-0.3329	0.378	0.447	0.370
(First Stage F-Test = 180.86)	(0.2737)	(0.3265)	(0.2422)	(0.207)	(0.230)	(0.222)
Race Difference: P-Value			*0.288*			*0.810*
Y-Mean	4.75	4.00	3.41	10.2	9.1	9.5
N	8553	8522	8502	7804	7768	7779

Table 6: Robustness Specifications, ASG Employment IV (Continued)

A. ASG Employment IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(11) Raw Data	-0.0543	-0.0237	-0.0141	8.495	3.089	5.422
(First Stage F-Test = 501.92)	(0.0046)	(0.0028)	(0.0018)	(1.104)	(0.588)	(0.639)
Race Difference: P-Value			*0.004*			*0.007*
β/Pop	-0.003	-0.006	-0.002	0.62	0.93	0.58
Race Difference: P-Value			*0.000*			*0.065*
Y-Mean	256.60	144.47	67.25	61581.2	31682.5	29375.7
N	7789	7759	7742	7139	7103	7113
(12) Balanced Panel	-0.0597	-0.0266	-0.0193	8.005	4.265	-0.491
(First Stage F-Test = 1833.49)	(0.0039)	(0.0025)	(0.0058)	(4.763)	(2.519)	(1.660)
Race Difference: P-Value			*0.370*			*0.750*
β/Pop	-0.003	-0.006	-0.006	0.87	2.27	-0.08
Race Difference: P-Value			*0.123*			*0.828*
Y-Mean	257.33	142.45	26.18	21708.0	9051.5	11912.2
N	6951	6386	6157	4687	4345	4536
(13) COPS Timeframe, 1990-2018	-0.0893	-0.0453	-0.0409	5.296	0.876	4.645
(First Stage F-Test = 797.67)	(0.0044)	(0.0022)	(0.0016)	(1.438)	(0.769)	(0.625)
Race Difference: P-Value			*0.105*			*0.000*
β/Pop	-0.005	-0.011	-0.007	0.42	0.30	0.55
Race Difference: P-Value			*0.000*			*0.365*
Y-Mean	223.29	129.82	59.18	50034.1	24854.8	24751.9
N	6503	6474	6462	5819	5811	5798

Note: Table 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 an alternative measure of sworn officers from the U.S. Census. Baseline specifications correspond to models in Table 2. Model (2) removes population weights, (3) replaces the endogenous X as the U.S. Census police employment record, (4) estimates the model using a police employment measure (and IV) that are not lagged, (5) includes Population Bin (50K-100K, 100K-250K, >250K in 1980) by Year by Month Fixed Effects, (6) includes City Homicide Quartile (1980) by Year by Month Fixed Effects, (7) controls for Education Spending at the city-year level, (8) removes covariates in Table 1 from the model, (9) transforms the model to a log-log specifications where variables are transformed as $y' = log(y+1)$, (10) uses an inverse hyperbolic sine transformation $y = log(y + \sqrt{y^2+1})$, (11) does not remove outlier observations identified in data cleaning, (12) restricts the sample to the balanced panel, and (13) restricts to the sample period of the COPS IV specification, 1990-2018. Standard errors are clustered at the city-level.

Table 7: Robustness Specifications, COPS Eligible Hires IV

B. COPS Eligible Hires IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(1) Baseline Model	-0.1030	-0.0503	-0.0442	22.013	8.169	14.015
(First Stage F-Test = 15.71)	(0.0104)	(0.0047)	(0.0010)	(5.087)	(1.642)	(3.473)
Race Difference: P-Value			*0.205*			*0.128*
β/Pop	-0.006	-0.012	-0.008	1.74	2.80	1.66
Race Difference: P-Value			*0.000*			*0.102*
Y-Mean	223.35	130.03	59.17	49908.0	24807.0	24674.4
N	6530	6501	6489	5839	5831	5818
(2) Not Weighted by Population	-0.0941	-0.0462	-0.0424	21.180	8.383	12.924
(First Stage F-Test = 10.14)	(0.0058)	(0.0031)	(0.0023)	(5.535)	(2.068)	(3.505)
Race Difference: P-Value			*0.322*			*0.265*
β/Pop	-0.032	-0.072	-0.033	7.80	14.61	6.81
Race Difference: P-Value			*0.000*			*0.054*
Y-Mean	37.48	22.55	8.87	8119.6	3644.0	4337.2
N	6530	6501	6489	5839	5831	5818
(3) ASG as Endogenous X, UCR as IV	-0.1097	-0.0536	-0.0458	20.615	7.656	13.120
(First Stage F-Test = 16.58)	(0.0104)	(0.0047)	(0.0009)	(5.148)	(1.678)	(3.498)
Race Difference: P-Value			*0.101*			*0.159*
β/Pop	-0.006	-0.013	-0.008	1.63	2.63	1.55
Race Difference: P-Value			*0.000*			*0.129*
Y-Mean	223.22	129.78	59.16	50017.6	24846.4	24745.9
N	6509	6480	6468	5824	5816	5802

Table 7: Robustness Specifications, COPS Eligible Hires IV (Continued)

B. COPS Eligible Hires IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(4) Police Employment not Lagged	-0.1046	-0.0573	-0.0517	11.656	3.934	7.448
(First Stage F-Test = 16.97)	(0.0044)	(0.0040)	(0.0016)	(4.711)	(2.354)	(2.340)
Race Difference: P-Value			*0.189*			*0.290*
β/Pop	-0.006	-0.014	-0.009	0.91	1.33	0.87
Race Difference: P-Value			*0.000*			*0.588*
Y-Mean	226.39	131.45	60.73	51136.5	25433.3	25235.6
N	6317	6290	6281	5662	5657	5644
(5) Population Group by Year FE	-0.0970	-0.0467	-0.0443	21.983	8.079	14.078
(First Stage F-Test = 15.64)	(0.0079)	(0.0036)	(0.0010)	(5.556)	(1.860)	(3.722)
Race Difference: P-Value			*0.520*			*0.150*
β/Pop	-0.006	-0.011	-0.008	1.74	2.77	1.66
Race Difference: P-Value			*0.000*			*0.155*
Y-Mean	223.35	130.03	59.17	49908.0	24807.0	24674.4
N	6530	6501	6489	5839	5831	5818
(6) Homicide Group by Year FE	-0.0911	-0.0436	-0.0441	23.979	9.146	15.007
(First Stage F-Test = 17.08)	(0.0053)	(0.0026)	(0.0012)	(5.737)	(2.010)	(3.746)
Race Difference: P-Value			*0.856*			*0.168*
β/Pop	-0.005	-0.011	-0.008	1.90	3.13	1.77
Race Difference: P-Value			*0.000*			*0.098*
Y-Mean	223.35	130.03	59.17	49908.0	24807.0	24674.4
N	6530	6501	6489	5839	5831	5818

Table 7: Robustness Specifications, COPS Eligible Hires IV (Continued)

B. COPS Eligible Hires IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(7) Control for Education Spending	-0.0924	-0.0438	-0.0450	23.975	9.141	15.009
(First Stage F-Test = 17.14)	(0.0052)	(0.0025)	(0.0014)	(5.781)	(2.029)	(3.771)
Race Difference: P-Value			*0.234*			*0.128*
β/Pop	-0.005	-0.011	-0.008	1.89	3.11	1.77
Race Difference: P-Value			*0.000*			*0.102*
Y-Mean	224.35	130.61	59.44	50157.7	24934.7	24795.7
N	6424	6396	6383	5740	5732	5719
(8) Excluding Covariates	-0.0174	-0.0124	-0.0088	24.688	9.913	14.830
(First Stage F-Test = 10.27)	(0.0066)	(0.0014)	(0.0011)	(6.637)	(2.186)	(4.428)
Race Difference: P-Value			*0.047*			*0.319*
β/Pop	-0.001	-0.003	-0.001	1.96	3.39	1.75
Race Difference: P-Value			*0.000*			*0.073*
Y-Mean	222.82	129.73	59.02	49779.7	24744.6	24610.5
N	6570	6541	6529	5875	5867	5853
(9) Log Model (Variable+1)	2.9409	1.5147	4.5989	7.920	7.893	9.297
(First Stage F-Test = 4.15)	(2.0710)	(1.9435)	(2.8371)	(5.659)	(5.658)	(6.485)
Race Difference: P-Value			*0.385*			*0.974*
Y-Mean	4.09	3.44	2.75	9.4	8.4	8.7
N	6528	6499	6487	5837	5829	5816
(10) Inverse Hyperbolic Sine	3.4858	1.8241	4.6335	7.347	7.440	8.653
(First Stage F-Test = 4.89)	(2.2836)	(2.1561)	(2.9028)	(4.895)	(4.983)	(5.640)
Race Difference: P-Value			*0.476*			*0.977*
Y-Mean	4.70	3.99	3.29	10.1	9.1	9.4
N	6530	6501	6489	5839	5831	5818

Table 7: Robustness Specifications, COPS Eligible Hires IV (Continued)

B. COPS Eligible Hires IV	(1) Homicide Victims	(2) Black Homicide Victims	(3) White Homicide Victims	(4) Quality of Life Arrests	(5) Black Quality of Life Arrests	(6) White Quality of Life Arrests
(11) Raw Data	-0.0984	-0.0481	-0.0421	22.020	8.172	14.018
(First Stage F-Test = 15.51)	(0.0100)	(0.0046)	(0.0010)	(5.095)	(1.645)	(3.478)
Race Difference: P-Value			*0.197*			*0.129*
β/Pop	-0.006	-0.012	-0.007	1.74	2.80	1.66
Race Difference: P-Value			*0.000*			*0.102*
Y-Mean	223.28	129.99	59.15	49891.6	24798.6	24668.4
N	6536	6507	6495	5844	5836	5822
(12) Balanced Panel	-0.1049	-0.0510	-0.0313	47.698	9.482	38.787
(First Stage F-Test = 14.59)	(0.0112)	(0.0049)	(0.0059)	(3.226)	(1.224)	(2.488)
Race Difference: P-Value			*0.000*			*0.000*
β/Pop	-0.006	-0.012	-0.010	5.13	5.32	5.98
Race Difference: P-Value			*0.000*			*0.102*
Y-Mean	224.69	127.22	21.36	20994.3	8693.0	11261.9
N	5321	4918	4887	3755	3726	3583

Note: Table 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. Baseline specifications correspond to models in Table 3. Model (2) removes population weights, (3) replaces the endogenous X as the U.S. Census police employment record, (4) estimates the model using a police employment measure (and IV) that are not lagged, (5) includes Population Bin (50K-100K, 100K-250K, >250K in 1980) by Year by Month Fixed Effects, (6) includes City Homicide Quartile (1980) by Year by Month Fixed Effects, (7) controls for Education Spending at the city-year level, (8) removes covariates in Table 1 from the model, (9) transforms the model to a log-log specifications where variables are transformed as $y' = log(y+1)$, (10) uses an inverse hyperbolic sine transformation $y = log(y + \sqrt{y^2+1})$, (11) does not remove outlier observations identified in data cleaning, and (12) restricts the sample to the balanced panel. Standard errors are clustered at the city-level.

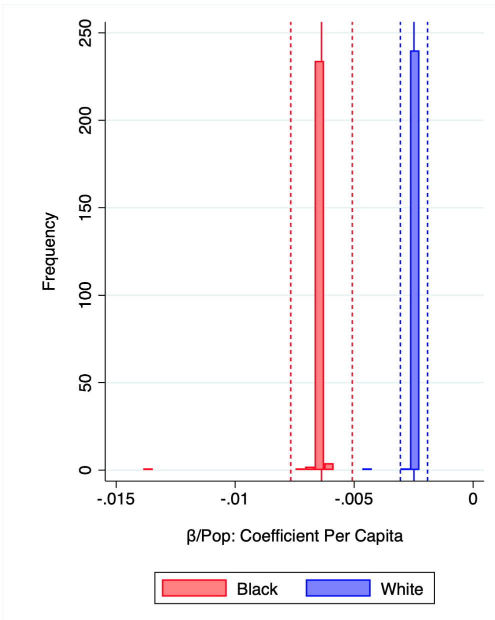
Table 8: Reporting of Quality of Life Arrests

	A. ASG IV			B. COPS IV		
	(1)	(2)	(3)	(4)	(5)	(6)
		Black	White		Black	White
	Quality of Life Arrests	Quality of Life Arrests	Quality of Life Arrests	Quality of Life Arrests	Quality of Life Arrests	Quality of Life Arrests
(1) Baseline Model	7.120	2.147	5.031	22.013	8.169	14.015
	(0.876)	(0.511)	(0.500)	(5.087)	(1.642)	(3.473)
Race Difference: P-Value			*0.000*			*0.128*
β/Pop	0.53	0.66	0.55	1.74	2.80	1.66
Race Difference: P-Value			*0.498*			*0.102*
Y-Mean	60243.6	30896.3	28827.3	49908.0	24807.0	24674.4
N	7804	7768	7779	5839	5831	5818
(2) Drop Zero Values	7.116	2.144	5.029	22.008	8.165	14.013
	(0.875)	(0.509)	(0.500)	(5.091)	(1.645)	(3.474)
Race Difference: P-Value			*0.000*			*0.128*
β/Pop	0.53	0.66	0.55	1.74	2.79	1.65
Race Difference: P-Value			*0.500*			*0.103*
Y-Mean	60315.9	30996.6	28871.8	49989.7	24885.0	24721.5
N	7793	7715	7765	5828	5797	5805
(3) Any Reporting (Total)	0.000000	0.000000	0.000000	0.000000	0.000000	0.000000
	(0.000000)	(0.000001)	(0.000000)	(0.000000)	(0.000001)	(0.000000)
Race Difference: P-Value			*0.896*			*0.860*
Y-Mean	0.999	0.997	0.998	0.998	0.997	0.998
N	7804	7768	7779	5839	5831	5818
(4) Report Arrests in All Sub-Categories	-0.000044	-0.000050	-0.000042	-0.000031	-0.000011	-0.000016
	(0.000014)	(0.000012)	(0.000013)	(0.000021)	(0.000019)	(0.000023)
Race Difference: P-Value			*0.649*			*0.847*
Y-Mean	0.284	0.226	0.238	0.276	0.219	0.223
N	7804	7768	7779	5839	5831	5818

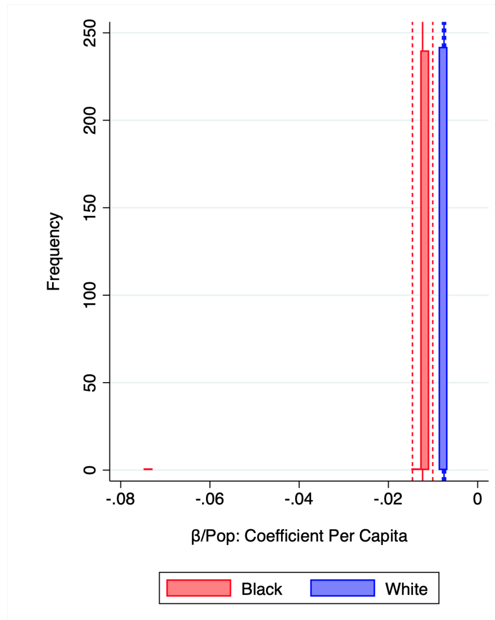
Note: Table reports estimates from equation (1) in which the once-lagged number of sworn police officers in a city derived from the FBI's Uniform Crime Reports is instrumented using either an alternative measure of sworn officers from the U.S. Census or the number of eligible hires awarded through a COPS Hiring grant. Baseline specifications correspond to models in Table 2 and Table 3. Model (2) replaces all zero values for aggregated Quality of Life arrests as missing. Model (3) test the binary outcome of reporting any positive value for aggregated Quality of Life arrests. Model (4) tests the binary outcome of whether all sub-categories of Quality of Life arrests have positive (non-zero) values, excluding "Uncategorized Arrests," which may serve as a residual category, and "Suspicious Person Arrests" which has zero values for a majority of city-years in the data. Standard errors are clustered at the city-level.

Figure A4: Distribution of Estimates Excluding One City at a Time

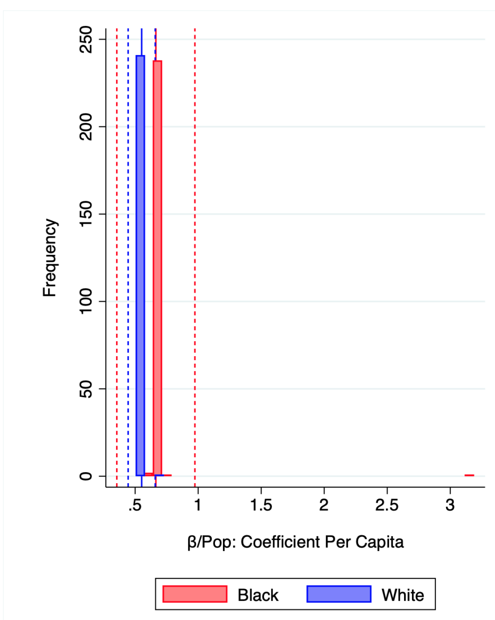
A. Homicide, ASG IV



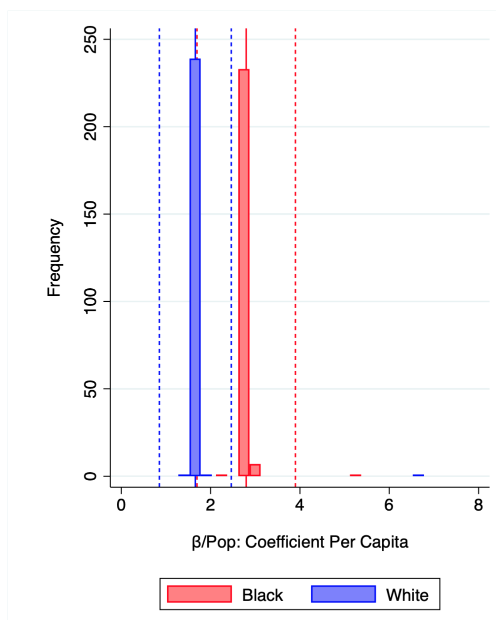
B. Homicide, COPS IV



C. Quality of Life Arrests, ASG IV



D. Quality of Life Arrests, COPS IV



Note: 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 officers from the U.S. Census or the number of eligible hires awarded through a COPS Hiring grant. Figures present histograms of the per capita effect estimates from the primary specifications (with identical controls and sample periods) where each estimate drops a different single city from the sample. All models are weighted by population in 1980. "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests does not include sub-categories for Hispanic residents; as a result, white population share includes Hispanic residents for these outcomes in calculating the "β/Pop." measure. Standard errors are clustered at the city-level.

Table 9: Results Dis-aggregated by Race Subgroups, ASG Employment IV

	(1)	(2)	(3)	(4)	(5)
		Non-Hispanic		Non-Hispanic	
A. ASG Employment IV	Black	Black	White	White	Hispanic
(1) Homicide Victims	-0.0269	-0.0258	-0.0291	-0.0156	-0.0152
	(0.0027)	(0.0027)	(0.0017)	(0.0018)	(0.0008)
β/Pop	-0.007	-0.006	-0.003	-0.003	-0.003
Y-Mean	142.19	140.36	100.74	65.55	37.58
N	8521	8522	8512	8502	8469
(2) Clearance Rates	0.0008	0.0008	-0.0003	-0.0005	0.0013
	(0.0007)	(0.0007)	(0.0009)	(0.0008)	(0.0024)
Y-Mean	62.61	62.56	67.74	69.47	65.73
N	6070	6065	7314	7045	2393
(3) Index Arrests	-0.682		-0.451		
	(0.198)		(0.093)		
β/Pop	-0.21		-0.05		
Y-Mean	8929.7		7214.3		
N	7753		7770		
(4) Quality of Life Arrests	2.147		5.031		
	(0.511)		(0.500)		
β/Pop	0.66		0.55		
Y-Mean	30896.3		28827.3		
N	7768		7779		

Note: Table 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 an alternative measure of sworn officers from the U.S. Census. Results show outcomes by race group, using the most granular categories available for each outcome data source. FBI UCR arrest records do not include information on Hispanic/Latinx ethnicity, and here the white subgroup includes Hispanic/Latinx individuals. Baseline specifications correspond to models in Table 2. Standard errors are clustered at the city-level.

Table 10: Results Dis-aggregated by Race Subgroups, COPS Employment IV

	(1)	(2)	(3)	(4)	(5)
		Non-Hispanic		Non-Hispanic	
B. COPS Eligible Hires IV	Black	Black	White	White	Hispanic
(1) Homicide Victims	-0.0507	-0.0503	-0.0480	-0.0442	-0.0057
	(0.0047)	(0.0047)	(0.0054)	(0.0010)	(0.0045)
β/Pop	-0.012	-0.012	-0.004	-0.007	-0.001
Y-Mean	131.69	130.03	85.70	59.17	28.71
N	6501	6501	6494	6489	6475
(2) Clearance Rates	0.0014	0.0013	0.0006	0.0002	0.0212
	(0.0012)	(0.0012)	(0.0016)	(0.0019)	(0.0085)
Y-Mean	56.81	56.77	64.06	66.42	60.02
N	4600	4598	5455	5223	1734
(3) Index Arrests	-1.137		-0.547		
	(0.212)		(0.174)		
β/Pop	-0.39		-0.07		
Y-Mean	7007.2		6137.3		
N	5808		5811		
(4) Quality of Life Arrests	8.169		14.015		
	(1.642)		(3.473)		
β/Pop	2.80		1.66		
Y-Mean	24807.0		24674.4		
N	5831		5818		

Note: Table 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. Results show outcomes by race group, using the most granular categories available for each outcome data source. FBI UCR arrest records do not include information on Hispanic/Latinx ethnicity, and here the white subgroup includes Hispanic/Latinx individuals. Baseline specifications correspond to models in Table 3. Standard errors are clustered at the city-level.

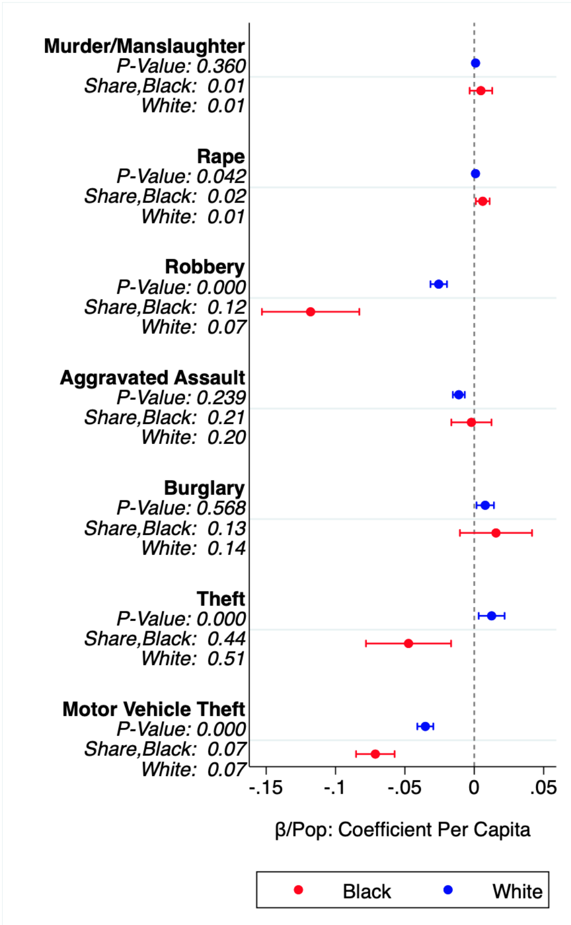
Table 11: Results for Index Crimes and Arrests by Sub-Type

A. ASG Employment IV	Coeff.	S.E.	β/Population	S.E.	Mean	N
Index Crimes						
Murder/Manslaughter	-0.068	(0.004)	-0.004	(0.000)	254.1	8558
Rape	-0.052	(0.016)	-0.003	(0.001)	633.8	8561
Robbery	-3.123	(0.153)	-0.187	(0.009)	10018.6	8565
Aggravated Assault	-0.594	(0.094)	-0.036	(0.006)	9997.1	8595
Burglary	-4.501	(0.460)	-0.270	(0.028)	17299.9	8560
Theft	-5.463	(0.594)	-0.327	(0.036)	45487.9	8552
Motor Vehicle Theft	-3.977	(0.366)	-0.259	(0.024)	14138.6	8592
Index Crime Arrests						
Murder/Manslaughter	0.027	(0.018)	0.002	(0.001)	205.3	7797
Rape	0.030	(0.009)	0.002	(0.001)	232.4	7801
Robbery	-0.605	(0.086)	-0.045	(0.006)	2639.1	7797
Aggravated Assault	-0.055	(0.035)	-0.004	(0.003)	3528.2	7827
Burglary	0.140	(0.072)	0.010	(0.005)	1967.5	7792
Theft	0.032	(0.080)	0.002	(0.006)	6293.0	7794
Motor Vehicle Theft	-0.546	(0.037)	-0.041	(0.003)	1478.8	7807
B. COPS Eligible Hires IV	Coeff.	S.E.	β/Population	S.E.	Mean	N
Index Crimes						
Murder/Manslaughter	-0.106	(0.010)	-0.006	(0.001)	221.2	6546
Rape	-0.094	(0.023)	-0.006	(0.001)	559.9	6554
Robbery	-4.168	(0.347)	-0.244	(0.020)	8305.6	6560
Aggravated Assault	-0.872	(0.265)	-0.051	(0.016)	9627.5	6585
Burglary	-4.914	(0.520)	-0.288	(0.030)	12899.2	6553
Theft	-7.227	(0.689)	-0.423	(0.040)	40592.1	6541
Motor Vehicle Theft	-6.479	(0.609)	-0.424	(0.040)	11801.9	6577
Index Crime Arrests						
Murder/Manslaughter	-0.044	(0.011)	-0.003	(0.001)	158.1	5840
Rape	-0.007	(0.009)	-0.001	(0.001)	177.3	5840
Robbery	-0.994	(0.061)	-0.078	(0.005)	2140.4	5837
Aggravated Assault	0.499	(0.230)	0.040	(0.018)	3308.7	5879
Burglary	-0.416	(0.144)	-0.033	(0.011)	1393.4	5826
Theft	-0.013	(0.168)	-0.001	(0.013)	5023.4	5826
Motor Vehicle Theft	-0.622	(0.179)	-0.049	(0.014)	1146.3	5848

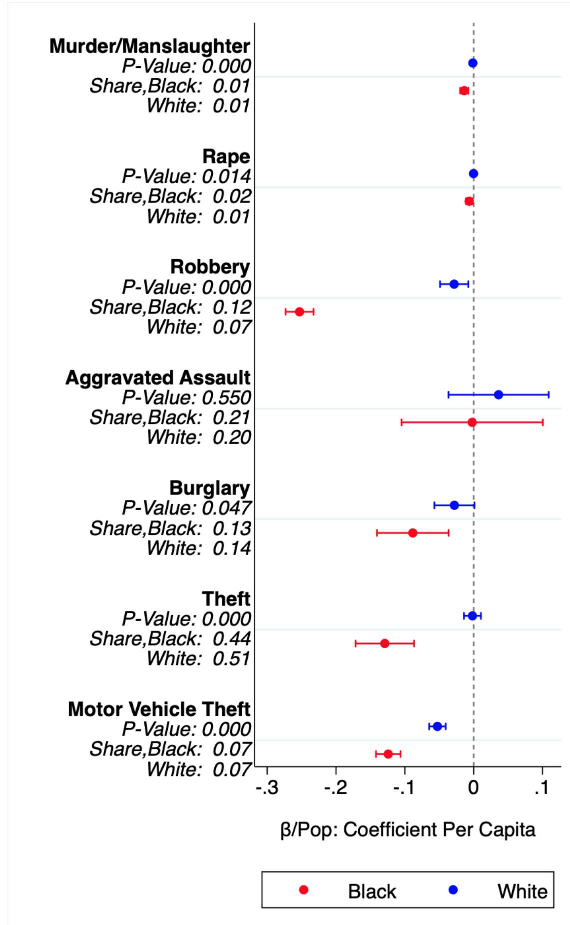
Note: Table reports estimates from equation (1) in which the once-lagged number of sworn police officers in a city derived from the FBI's Uniform Crime Reports is instrumented using either an alternative measure of sworn officers from the U.S. Census or the number of eligible hires awarded through a COPS Hiring grant.Standard errors are clustered at the city-level. Models correspond to primary specifications for both strategies and are weighted by population of each city in 1980. Panel A covers 1981-2018; Panel B covers 1990-2018. Models have differing observations due to data availability and the outlier cleaning procedure described in Appendix A3. The endogenous measure of police employment is recorded in the UCR LEOKA files. The instrument is police employment recorded in the U.S. Census. Models include covariates in Table 1. "β/Pop." divides the coefficient by population (units of 100,000 residents). Standard errors are clustered at the city-level.

Figure A5: Effects of Police Force Size on Index Arrests by Race

A. ASG Employment IV



B. COPS Eligible Hires IV



Note: 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 officers from the U.S. Census or the number of eligible hires awarded through a COPS Hiring grant. Results correspond to per capita estimates. Models are weighted by population of each city in 1980. Figure A covers 1981-2018; Figure B covers 1990-2018. Arrest categories correspond to Appendix Table 11. Models have differing observations due to data availability and the outlier cleaning procedure described in Appendix A3. The endogenous measure of police employment is recorded in the UCR LEOKA files. In Figure A, the instrument is police employment from the U.S. Census; in Figure B the instrument is the number of eligible hires awarded through a COPS Hiring grant. Models include covariates in Table 1; Figure B also controls for non-hiring grant award size and whether a city applied for a hiring or non-hiring grant (lagged). "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests does not include sub-categories for Hispanic residents; as a result, white population share includes Hispanic residents for these outcomes in calculating the "β/Pop." measure. Standard errors are clustered at the city-level. "Share, Black" and "Share, White" display the share of that arrest category within all index crime arrests.

Table 12: Results by Quality of Life Arrest Sub-Type

A. ASG Employment IV	Coeff.	S.E.	β/Population	S.E.	Mean	N
Quality of Life Arrests						
Disorderly Conduct	1.178	(0.343)	0.087	(0.025)	6588.9	7788
Suspicious Person	-0.011	(0.015)	-0.001	(0.001)	28.3	7801
Curfew/Loitering	-0.103	(0.103)	-0.008	(0.008)	1052.5	7790
Vandalism	-0.013	(0.029)	-0.001	(0.002)	1452.9	7801
Vagrancy	-0.082	(0.098)	-0.006	(0.007)	616.0	7798
Gambling	0.334	(0.028)	0.025	(0.002)	630.9	7791
Drunkenness	0.184	(0.257)	0.014	(0.019)	1869.4	7793
Liquor	8.339	(0.437)	0.619	(0.032)	4822.9	7790
Drug Possession	3.829	(0.154)	0.284	(0.011)	7294.4	7811
Uncategorized Arrests	-6.601	(0.741)	-0.489	(0.055)	35887.3	7818
B. COPS Eligible Hires IV	**Coeff.**	**S.E.**	**β/Population**	**S.E.**	**Mean**	**N**
Quality of Life Arrests						
Disorderly Conduct	1.182	(0.152)	0.093	(0.012)	4390.6	5831
Suspicious Person	-0.014	(0.023)	-0.001	(0.002)	23.8	5839
Curfew/Loitering	1.775	(0.914)	0.140	(0.072)	1115.8	5843
Vandalism	-0.111	(0.065)	-0.009	(0.005)	1260.9	5840
Vagrancy	-0.285	(0.085)	-0.023	(0.007)	448.7	5843
Gambling	0.278	(0.016)	0.022	(0.001)	455.1	5825
Drunkenness	0.136	(0.245)	0.011	(0.019)	1480.2	5830
Liquor	14.243	(0.785)	1.134	(0.063)	5231.4	5833
Drug Possession	5.934	(0.853)	0.470	(0.068)	7259.1	5880
Uncategorized Arrests	-1.052	(2.764)	-0.083	(0.219)	28131.5	5872

Note: Table 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 officers from the U.S. Census or the number of eligible hires awarded through a COPS Hiring grant. Models correspond to primary specifications for both strategies and are weighted by population of each city in 1980. Panel A covers 1981-2018; Panel B covers 1990-2018. Models have differing observations due to data availability and the outlier cleaning procedure described in Appendix A3. The endogenous measure of police employment is recorded in the UCR LEOKA files. The instrument is police employment recorded in the U.S. Census. Models include covariates in Table 1. "β/Pop." divides the coefficient by population (units of 100,000 residents). Standard errors are clustered at the city-level.

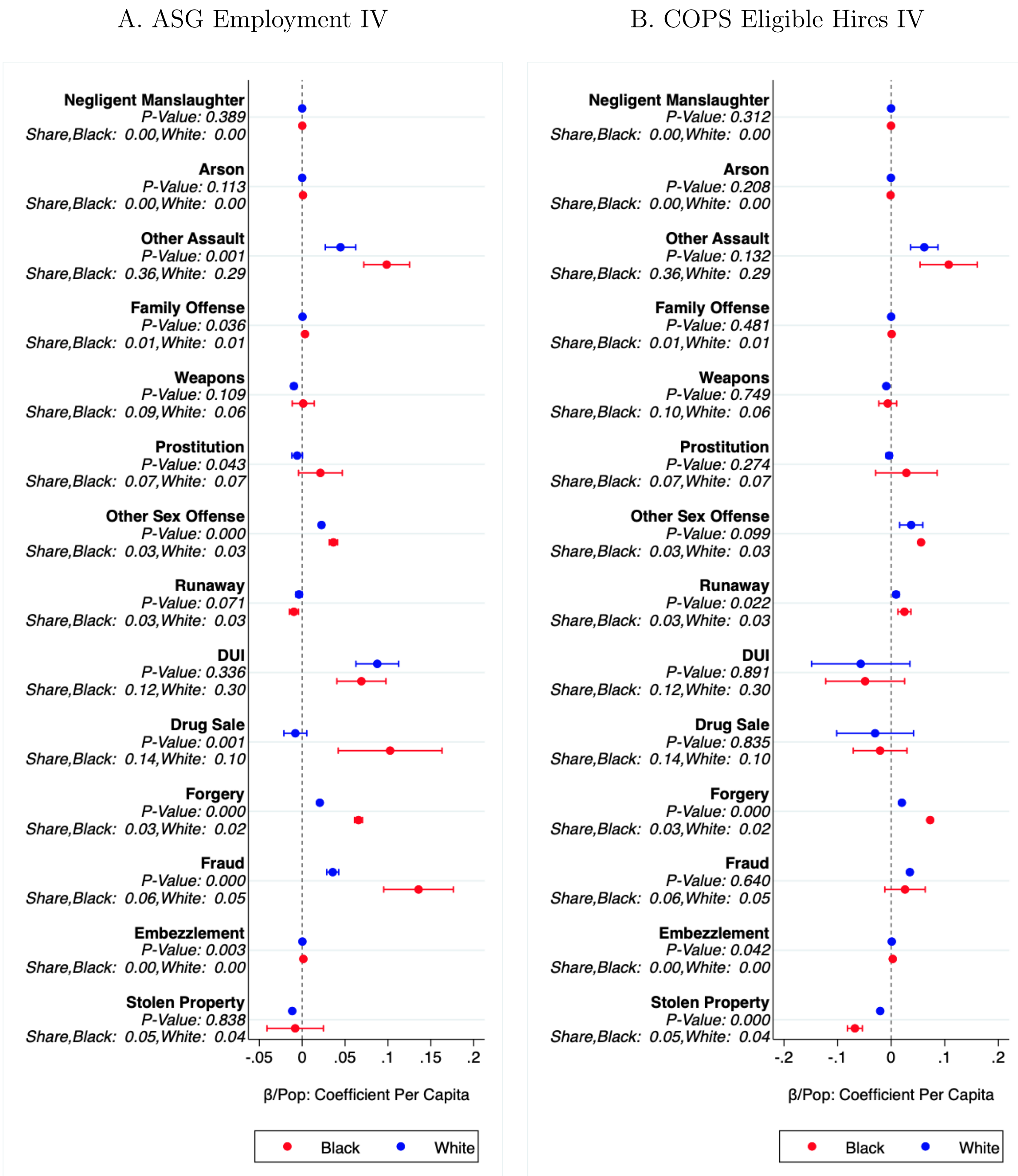
Table 13: Results by Non-Index Arrest Sub-Type

A. ASG Employment IV	Coeff.	S.E.	β/Population	S.E.	Mean	N
Non-Index Arrests						
Negligent Manslaughter	0.001	(0.001)	0.000	(0.000)	7.3	7792
Arson	0.004	(0.003)	0.000	(0.000)	66.7	7794
Other Assault	0.797	(0.114)	0.059	(0.008)	4997.6	7826
Family Offense	0.015	(0.006)	0.001	(0.000)	102.8	7791
Weapons	-0.079	(0.032)	-0.006	(0.002)	1631.7	7805
Prostitution	0.051	(0.068)	0.004	(0.005)	1889.3	7792
Other Sex Offense	0.338	(0.015)	0.025	(0.001)	609.1	7793
Runaway	-0.065	(0.024)	-0.005	(0.002)	323.7	7799
DUI	1.083	(0.161)	0.080	(0.012)	3091.6	7793
Drug Sale	0.269	(0.154)	0.020	(0.011)	4187.5	7809
Forgery	0.434	(0.013)	0.032	(0.001)	501.7	7795
Fraud	0.812	(0.101)	0.060	(0.008)	2448.9	7805
Embezzlement	0.008	(0.003)	0.001	(0.000)	44.2	7790
Stolen Property	-0.123	(0.064)	-0.009	(0.005)	832.9	7802

B. COPS Eligible Hires IV	Coeff.	S.E.	β/Population	S.E.	Mean	N
Non-Index Arrests						
Negligent Manslaughter	0.000	(0.000)	0.000	(0.000)	6.0	5836
Arson	0.001	(0.002)	0.000	(0.000)	49.3	5833
Other Assault	1.079	(0.185)	0.086	(0.015)	4902.3	5887
Family Offense	0.000	(0.008)	0.000	(0.001)	99.5	5854
Weapons	-0.175	(0.038)	-0.014	(0.003)	1410.2	5845
Prostitution	0.092	(0.052)	0.007	(0.004)	1318.8	5842
Other Sex Offense	0.520	(0.090)	0.041	(0.007)	559.0	5823
Runaway	0.064	(0.040)	0.005	(0.003)	227.9	5837
DUI	-0.092	(0.174)	-0.007	(0.014)	2510.0	5826
Drug Sale	0.178	(0.149)	0.014	(0.012)	3988.2	5868
Forgery	0.435	(0.018)	0.034	(0.001)	511.4	5832
Fraud	-0.117	(0.071)	-0.009	(0.006)	2298.9	5852
Embezzlement	0.015	(0.003)	0.001	(0.000)	40.7	5854
Stolen Property	-0.354	(0.040)	-0.028	(0.003)	614.8	5839

Note: Table 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 officers from the U.S. Census or the number of eligible hires awarded through a COPS Hiring grant. Models correspond to primary specifications for both strategies and are weighted by population of each city in 1980. Panel A covers 1981-2018; Panel B covers 1990-2018. Models have differing observations due to data availability and the outlier cleaning procedure described in Appendix A3. The endogenous measure of police employment is recorded in the UCR LEOKA files. The instrument is police employment recorded in the U.S. Census. Models include covariates in Table 1. "β/Pop." divides the coefficient by population (units of 100,000 residents). Standard errors are clustered at the city-level.

Figure A6: Effects of Police Force Size on Non-Index Arrests by Race



Note: 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 officers from the U.S. Census or the number of eligible hires awarded through a COPS Hiring grant. Results correspond to per capita estimates. Models are weighted by population of each city in 1980. Figure A covers 1981-2018; Figure B covers 1990-2018. Arrest categories correspond to Appendix Table 13. Models have differing observations due to data availability and the outlier cleaning procedure described in Appendix A3. The endogenous measure of police employment is recorded in the UCR LEOKA files. In Figure A, the instrument is police employment from the U.S. Census; in Figure B the instrument is the number of eligible hires awarded through a COPS Hiring grant. Models include covariates in Table 1; Figure B also controls for non-hiring grant award size and whether a city applied for a hiring or non-hiring grant (lagged). "β/Pop." divides the coefficient by population (units of 100,000 residents). FBI UCR data on arrests does not include sub-categories for Hispanic residents; as a result, white population share includes Hispanic residents for these outcomes in calculating the "β/Pop." measure. Standard errors are clustered at the city-level. "Share, Black" and "Share, White" display the share of that arrest category within all non-index arrests.

Table 14: Police Force Size and Officer Deaths and Injuries

A. ASG IV	Coeff.	S.E.	Mean	N
Officer Felonious Deaths	0.0000	(0.0000)	0.224	8554
Officers Assault Injuries	-0.1365	(0.0110)	291.4	8563

B. COPS IV	Coeff.	S.E.	Mean	N
Officer Felonious Deaths	-0.0001	(0.0001)	0.158	6566
Officers Assault Injuries	-0.2258	(0.0058)	203.6	6555

Note: Table 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 officers from the U.S. Census or the number of eligible hires awarded through a COPS Hiring grant. Models correspond to primary specifications for both strategies and are weighted by population of each city in 1980. Panel A covers 1981-2018; Panel B covers 1990-2018. Officer deaths includes only felonious deaths of officers; and officer injuries include injuries caused by assaults on the job. Models have differing observations due to data availability and the outlier cleaning procedure described in Appendix A3. The endogenous measure of police employment is recorded in the UCR LEOKA files. Standard errors are clustered at the city-level.

A3 Data Appendix

A3.1 Data and Procedures

This project compiles data from a number of different public data sources. Below is a description of each data set and the procedures used to clean the data.

FBI Uniform Crime Report, Law Enforcement Officers Killed or Assaulted (UCR LEOKA) The principal measure of police manpower used in this paper comes from the FBI's Law Enforcement Officers Killed or Assaulted (LEOKA) series, which has been collected annually since 1960. This data set compiles information on officers that are killed or assaulted in the field as well as total officer employment each year. We access the LEOKA data using Jacob Kaplan's concatenated LEOKA data available from ICPSR (Kaplan, 2019b). These data are used to create the primary police employment measure that is the main focus of the analysis. We define police employment as full time sworn officer employment. We measure officer deaths as deaths that occur as a result of a civilian felony. We measure officer assaults as assaults by civilians that resulted in officer injuries. This dataset covers the period between 1981-2018.

Annual Surveys of Governments, Annual Survey of Public Employment and Payroll (ASG Employment, Census) This U.S. Census survey collects data on employment in local governments and is the source of data for the measurement error instrument, or Annual Survey of Governments (ASG) IV. The ASG is an annual survey of municipal employment and payrolls that has been administered by the Bureau of Labor Statistics and reported to the U.S. Census annually since 1952. The ASG data provide annual payroll data for a large number of municipal functions including elementary and secondary education, judicial functions, public health and hospitals, streets and highways, sewerage and police and fire protection, among others.This data surveys all local governments every 5 years and a sub sample of local governments including large cities (covering our sample of cities) every year. The survey generally provides information on the number of full-time, part-time and full-time equivalent sworn and civilian employees for each function and for each municipal government.

The instrument is a measure of full time sworn police officer employment from this survey. As with the UCR system, the ASG reports a point-in-time measure of police, reporting the number of sworn officers employed as of March 31st of a given year (for 1997-2010 the reference date is June 30th). We linearly interpolate values for years when this data is missing in particular years, including 1996 and 2003, when no survey was collected for any city. This dataset covers the period 1981-2018.

Department of Justice, Community Oriented Policing Services (COPS) Grants Data on grants administered by the Department of Justice COPS office was obtained through a Freedom of Information Act (FOIA) request. These grants were established in 1994 through the Violent Crime Control Act (VCCA). Given the coverage period of the grants, the analysis using COPS grants spans the period of 1990-2018. The COPS data includes records of all grants awarded by the office as well records of all applications that were rejected by the office. Grants are divided into grants whose primary purpose is hiring police officers versus grants for other law enforcement needs (non-hiring grants), including investments in technology and targeted crime control. The dollar size of a grant is available for grants that were awarded and the number of eligible hires designated by a hiring grant is available for hiring grants that were awarded. This data is collapsed to contain records of new hiring and non-hiring grant applications and awards for each city-year in the data. Data covering award amounts are converted into 2018 constant dollars using the consumer price index as an inflator.

FBI Uniform Crime Report, Supplementary Homicide Report (UCR SHR) These data include records of homicides as reported to the FBI by police departments. The SHR has been available since 1976 and is the most comprehensive national source of information on the victims and, when available, the perpetrators of homicide (Loftin et al., 2015). We access the SHR data using Jacob Kaplan's concatenated Supplementary Homicide Reports files available from ICPSR (Kaplan, 2019a). We use these data to construct our primary outcomes of total number of homicides each year, as well as homicides by race, gender and age group. Unlike with the UCR Arrest data (below), the category of Hispanic or Latino is available in this dataset. These outcomes are replaced as zeros when missing (but are subject to the outlier cleaning described below). We exclude homicides where the civilian was killed by a police officer, as well as homicides where the person killed was engaging in a felony and killed by a private civilian and homicides that occur in institutional settings such as prisons. These data are also used to construct our measure of homicide clearance rates. We code a homicide as being "cleared" if demographic information for the suspect of the homicide is available in the SHR, which permits the construction of clearance rates separately by victim race. This data covers the period 1981-2018.

FBI Uniform Crime Report, Arrest Data (UCR Arrest) This data set includes records of arrests for different types of offenses as submitted by city agencies. We access these data using Jacob Kaplan's concatenated offenses known and clearances by arrest files available from ICPSR (Kaplan, 2019c). These data have been collected annually at the agency-level since 1974. The data includes records of total arrests, and arrests by the race of the civilian (e.g. Black or white), where the category of Hispanic or Latino is not available. We extract records of individual crime category arrests, total and by race, as well as construct larger group categories of arrests by type (see Appendix Tables 11, 12, and 13 for groupings). Before constructing these sums, we replace any negative arrest values as missing. In several cases, an individual crime category may be missing for a particular year or city, when this happens we treat this value as a zero in the sum. Our procedure that identifies outliers (see below) helps identify cases when this approach might create large fluctuations in the data over time. This data set covers the period 1981-2018.

Annual Surveys of Governments, Annual Survey of State and Local Government Finances (ASG, Census) This U.S. Census survey collects data on local government finances, tax collection, and spending. With a few exceptions, the Census Bureau has conducted an Annual Survey of Government Finances in every year since 1902. Like the Annual Survey of Public Employment and Payroll, this survey covers all local governments every 5 years and a sub-sample of local governments (including large cities) every year (covering our sample). Like the data on employees and payroll, data on government expenditures are reported separately for a large number of municipal functions, including elementary and secondary education, judicial functions, public health and hospitals, streets and highways, sewerage, police and fire protection among others. For each function, expenditures are divided among three categories of spending: (1) current operations,(2) capital expenditures and (3) expenditures on construction. The data are reported annually in dollars and, as such, we convert all dollar figures into 2018 constant dollars using the consumer price index as an inflator.

We use this resource to gather data on total government expenditures, taxes, and revenue, which we include as controls in our preferred specifications. This data covers the period of 1981-2018. Similar to the Census covariates, we linearly interpolate the expenditure variables when missing.

U.S. Census and American Community Survey (Census) We collect information from the U.S. Census on a vector of time-varying covariates upon which to condition in all subsequent models. The data we collect includes each city's population, the resident share in each age group (<14, 15-24, 25-44, >45), share male, share Black, white and Hispanic, the share of residents never married, the share of female headed households, the poverty rate, median household income, and the unemployment rate. Since 2000, we can obtain annual measures for each of these variables from the American Communities Survey; prior to 2000 we use the decennial Census and, following Levitt (1996) and Chalfin and McCrary (2018) among others, linearly interpolate between Census years.

A3.2 Identifying Outliers

UCR crime data sets are voluntarily reported by police departments and are known for having issues with reporting and measurement. Further, mass homicide events, while rare, can create large volatile swings in homicide outcomes. We follow prior papers using UCR data that clean these outcomes for outliers (Evans and Owens, 2007; Mello, 2019; Weisburst, 2019b). Specifically, we separately regress the set of outcomes on a polynomial cubic time trend for each city and calculate the percent deviation of the actual value from the values predicted by this regression (the outcomes used for this exercise are the raw values plus one, given the large number of zeros in homicide data). The Civilians Shot by Police uses a polynomial squared time trend instead given its shorter panel.We then summarize the absolute value of these percent deviations within city population groups (of 50k-100k, 100k-250k and >250k residents in 1980) and replace the value as missing if it is greater than the 99th percentile of this distribution or 50%, whichever is larger. This procedure is used for all outcomes as well as the UCR measure of police employment, the Census expenditure variables and the Census ASG police employment instrument. We clean sub-groups of outcomes, such as arrest sub-types or race sub-groups using this procedure as a first step, but also replace these sub-groups as missing if the total associated with a sub-group is identified as an outlier.

In addition to using this general algorithm correction, we pay particular attention to correcting outliers in our largest city, New York. We manually impute the UCR police employment measure for 2003, which represents over 2,000 reduction in sworn police officers in New York in that year, that is recovered the following year (identified in Chalfin and McCrary (2018)).

A3.3 Other Cleaning and Sample Restrictions

We merge our data sets together using the UCR police department identifier and the crosswalk to census identifiers. Our data set includes only the 242 large cities that regularly report to the Census Annual Survey of Local Government Finances and Annual Survey of Public Employment and Payroll. These cities all have populations that exceed 50,000 in 1980.

The final panel is not balanced. This can occur because of outliers that are replaced as missing (see above), or impartial panels in the source data sets. We use the imbalanced panel to capture as much information as possible in the estimation and to increase power.