

# The Material of Policing: Budgets, Personnel and the United States' Misdemeanour Arrest Decline

Brenden Beck, Eaven Holder, Abigail Novak and Jacob Kaplan\*

\*Brenden Beck, Department of Sociology, University of Colorado, Denver, CO, USA; [brenden.beck@ucdenver.edu](mailto:brenden.beck@ucdenver.edu); Eaven Holder, Department of Sociology and Criminology & Law, University of Florida, Gainesville, FL, USA; Abigail Novak, Department of Criminal Justice and Legal Studies, University of Mississippi, Oxford, MS, USA; Jacob Kaplan, Research on Policing Reform and Accountability, Princeton School of Public and International Affairs, Princeton University, Princeton, NJ, USA

What accounts for the steady decline in misdemeanour arrest rates in the United States following their peak in the mid-1990s? This article links the fluctuation in low-level law enforcement to changes in the budget and staffing resources cities devoted to policing. This materialist explanation contrasts with accounts that emphasize policy changes like the adoption of community policing. Dynamic panel regression analyses of 940 municipalities indicate low-level arrest rates declined most in places that reduced their police expenditure and personnel, net of crime and other controls. The adoption of community policing was unrelated to misdemeanour arrests. Findings suggest lawmakers should consider how increasing police budgets or police force sizes will likely be accompanied by increases in misdemeanour arrests and their attendant harms.

**Key Words:** misdemeanour arrests, police budgets, police force size, community policing

## INTRODUCTION

Police departments in many United States cities sharply increased their use of low-level arrests in the 1980s and early 1990s as they adopted misdemeanour-focused strategies like broken windows and zero tolerance policing. This change diffused widely, with police in countries as diverse as New Zealand, Mexico, Ukraine, and Brazil adopting similar approaches (Diniz and Stafford 2021; Marat 2018; Martin 2018; Swanson 2013). The expansion of low-level law enforcement in the 1990s was well documented by scholars, but subsequent changes to misdemeanour arrest rates have received less attention. Some researchers have suggested U.S. misdemeanour arrest rates remained high, writing that 'since the mid-1990s, police departments across the country have adopted tactics that intentionally increase the volume of citations and arrests for low-level offences' (Kohler-Hausmann 2014, p. 613), and that 'during the past three decades, a reduction in adult arrest rates has not occurred' (Engel *et al.* 2017, p. 375; see, also, Kubrin *et al.* 2010; Natapoff 2018). These statements, however, were asides in studies not explicitly focused on aggregate arrest patterns. Whether prior misdemeanour arrest rates endured through the 2000s

and 2010s was not directly investigated until 2018 when the first analyses revealed that, contrary to these assumptions, misdemeanour arrests in the U.S. had 'been falling steadily since 1997' (Stevenson and Mayson 2018: 120; Beck 2019).

Uniform Crime Report (UCR) data demonstrates this. Police in the U.S. made 45% fewer misdemeanour arrests per capita in 2018 than they did in 1997, as shown in Figure 1. Quality-of-life arrests, a subset of misdemeanour arrests that includes only very minor offences like loitering and disorderly conduct, peaked in the early 1980s and declined a remarkable 77% between then and 2018. By the mid-2010s, the level of misdemeanour arrests was well below any year since national record-keeping began in 1974, and the drop was widespread. More than 80% of U.S. cities decreased their low-level arrest rate during these years (Authors' analysis of UCR data).

What explains this rise and fall in low-level arrest rates? One cause might be that police departments moved away from broken windows policing policies and toward community policing policies. With its focus on community engagement, not arrests, community policing might have reoriented police priorities away from discretionary arrests. The widespread uptake of broken windows policing during the 1980s and 1990s occurred alongside the surge of misdemeanour arrests and the popularity of community policing occurred alongside the decline, so this explanation is temporally plausible (Zhao *et al.* 1999). Community policing originated in the U.K. and U.S., and, like broken windows policing, it travelled far (Willis 2014). The strategy has been practised on all six inhabited continents and has seen particular uptake in the global south (Blair *et al.* 2021; Ordu and Nnam 2017). Because U.S. policing policies are often exported to other countries, understanding how policies interact with outcomes like low-level arrests in the U.S. could have far-reaching implications.

City officials often turn to policy reforms like community policing to change policing in their cities. The Black Lives Matter protests against police brutality in 2015 and 2020, however, called for a different kind of change: the reinvestment of police funding into social services (Movement for Black Lives 2016; Kim *et al.* 2021; Reclaim the Block 2021). This demand echoed theorists of the carceral state who have identified the importance of funding and staffing resources

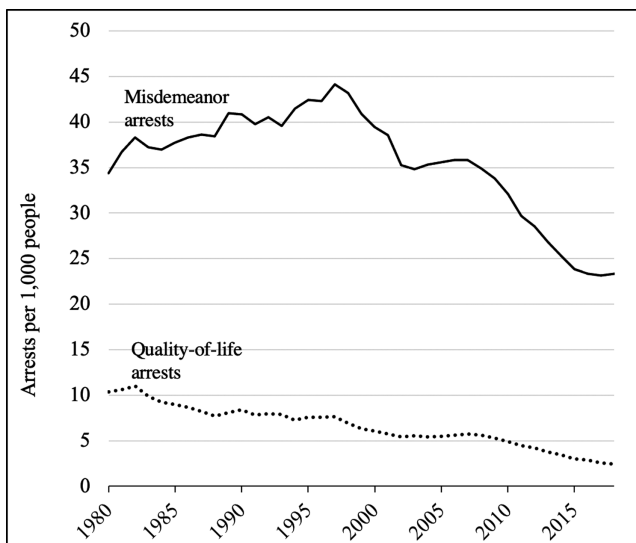


Figure 1. Low-level arrest rates across 1,695 United States cities. Data Source: FBI Uniform Crime Reports.

in determining criminal justice outcomes like incarceration rates (Gilmore 2007; Schoenfeld 2018). We build on this work by social movements and theorists to argue that the number of officers on a department's force and the amount of money in its budget—what we call the *material of policing*—is more consequential for policing outcomes than policy. We suspect these material resources are more powerful because they create a durable, institutional presence that outlasts individual policy decisions. We apply our theory to the United States between 1990 and 2018 by hypothesizing that changes in police funding and staffing were more strongly connected to the declines in low-level arrest rates than was the shift toward community policing.

In addition to policy and resource changes, another, not-mutually-exclusive factor in the misdemeanor arrest decline might have been the crime decline. We suspect the large and widespread drop in crime in the 2000s and 2010s in the United States reduced the amount of misdemeanor crimes for which police could make an arrest. Hypothesizing that crime impacts arrests inverts a more frequently tested idea: that low-level arrests reduce crime. Some early studies evaluating this foundational theory of broken windows policing found misdemeanor enforcement reduced crime, especially robberies (Wilson and Boland 1978; Sampson and Cohen 1988). However, the balance of recent evidence suggests the 'generalized aggressive use of increased misdemeanor arrests ... generates small to null impacts on crime' (Committee on Proactive Policing 2018: 4–44; see also Bowling 1999; Braga *et al.* 2015). While the question of whether misdemeanor arrests affect crime is an important one, the present study sets it aside and reverses the causal arrow to instead investigate whether crime and other social forces drive misdemeanor arrests.

Determining correlates of low-level arrest rates is important because the consequences of such arrests can be severe for the people detained, for their communities, and for police. Being arrested threatens one's employment, finances, family connections, health, school performance, likelihood of voting, and immigration status (Kirk and Sampson 2013; Geller *et al.* 2014; Lerman and Weaver 2014; Kohler-Hausmann 2019; Legewie and Fagan 2019; Baćak and Apel 2020). Misdemeanor arrests can cascade into fines and fees, incarceration, and, in the most extreme cases like those of George Floyd and Eric Garner, deaths in custody. For young people, especially Black adolescents, a single arrest increases the likelihood of subsequent re-offence (Lieberman *et al.* 2014; McGlynn-Wright *et al.* 2020). Even among those not arrested, proximity to pervasive, low-level enforcement delegitimizes police, making solving violent crimes less likely (Tyler 2005; Geller and Fagan 2019). Intense low-level enforcement also degrades equity. Poor, Black, and Latino people are disproportionately arrested (Fagan and Davies 2000; Soss and Weaver 2017). This study will assess which element, police resources or police policy, elected officials and police chiefs ought to adjust if they are looking to reduce the harms that attend low-level arrests.

## PRIOR EXPLANATIONS FOR LOW-LEVEL ARREST TRENDS

Researchers examining misdemeanor arrest rate fluctuations have frequently studied their relationship to crime, racial threat, and policing policy.

The question of whether crime increases arrests has received extensive attention, but a consensus in the literature remains elusive. There is a weak relationship at the individual level between people committing low-level crimes and their getting arrested, in part because police officers have wide discretion when making such arrests (Blumstein and Cohen 1979; Smith and Visser 1981; Crank and Langworthy 1992). This discretion comes from the subjectivity inherent in determining what constitutes, for instance, 'loitering' or 'disorderly conduct,' and it comes from misdemeanors rarely being reported to the police (Sykes 1986; Bratton and Kelling 2015). Lack of reporting by the public means most misdemeanor arrests stem from

police searches and observations. Police discretion is further enshrined by U.S. Supreme Court rulings giving street-level officers choice in whether to enforce even ‘apparently mandatory arrest statutes’ (*Castle Rock v. Gonzales* 2005, p. 11).

Moving from the individual-level up to the neighbourhood-level, there is also mixed evidence of a crime-arrest connection. Like street-level officers, police managers exercise discretion when making deployment and job-promotion decisions. This can incentivize or discourage police from making arrests in certain areas, further delinking crime and arrests at this scale (Lundman 1979; Kochel *et al.* 2011; Beck 2020). Some studies of cities have found arrest rates are associated with cities’ crime rates (Chappell *et al.* 2006; Beck 2019) and others that they are not (Beckett *et al.* 2006; Lum and Vovak 2018). On balance, extant research suggests that while misdemeanour arrest rates most directly reflect police enforcement priorities, those priorities can be influenced by crime rates at the city level.

Racial bias is another prominent explanation for misdemeanour arrest patterns. Studies have routinely found the number of Black people in a place to be a strong predictor of its low-level arrest rate, even after controlling for differences in group offending rates (Liska *et al.* 1981; Liska 1992; Jacobs and O’Brien 1998; Eitle *et al.* 2002; Eitle and Monahan 2009; Kane *et al.* 2013; Beck 2019; Gaston 2019; Gaston *et al.* 2020). Two studies diverged from this consensus, however, and found mixed or negative associations between arrests and the number of Black people (Parker *et al.* 2005; Ousey and Lee 2008). Some scholars attribute the Black population-arrest relationship to perceived racial threat and whites’ fears over demographic change (Weitzer 2014). A similar threat effect of Latino people or immigrants has not been observed (Holmes *et al.* 2008; Zhao *et al.* 2010; Feldmeyer *et al.* 2015).

Though elected officials cannot easily change their cities’ crime rates or racial demographics, they can change their cities’ policies, and this is often the lever they pull when they want to reform policing. Between 1990 and 2018, many cities adopted computerized crime tracking software, hot spots policing strategies, and civilian complaint review boards, but by far the two most prominent law enforcement policy changes during this time, and therefore the best candidates for having precipitated the misdemeanour arrest decline, were broken windows policing and community policing (Willis 2014).

At its inception, intense enforcement of misdemeanour laws was only one of broken windows policing’s elements, but it would become the main, if not sole, technique in practice. George Kelling, one of broken windows policing’s progenitors, lamented how arrest intensity eclipsed other aspects of the strategy, writing, ‘many critics and unfortunately supporters as well, “understand,” for example, that broken windows policing is based on police making numerous arrests’ (Kelling 2015: p. 628). Might cities have abandoned broken windows policing starting in the late 1990s, contributing to the decline in low-level arrests? Answering this question is, unfortunately, hindered by a lack of national data on the policy’s implementation, but a survey of 80 police departments conducted in 2013 found 79% of them reported using broken windows policing (Mastrofski and Fridell 2019). This means the strategy was being widely practised well into the misdemeanour arrest decline and suggests it was neither abandoned nor responsible for that decline.

The second potential policy driver of the misdemeanour arrest decline was the adoption of community policing. Community policing lets police ‘cut loose from an obsessive focus with enforcing law’ and brings them ‘back in touch with community concerns’ (Moore 1992: p. 144). The policy does this by encouraging officers to solve problems preemptively, interact with the community, and accept community input on problems and tactics (Cordner 1997; Weisburd and Eck 2004). Community policing was widely adopted in the United States during the study years, with 87 percent of large departments reporting having community policing personnel in 2018 (Hyland and Davis 2019). The U.S. federal government encouraged the policy starting

in 1994 when it began funding the practice among local agencies. One by-product of this federal support was reliable, longitudinal, national data on the practice. Community policing's deemphasis on arrests and the quality of data on its uptake provide a good test for whether a policy change was related to misdemeanour arrest rate fluctuations.

That misdemeanour arrests declined in a wide range of cities pursuing a wide range of policies suggests cities' policy differences were not salient. Low-level arrests fell in cities like New York that vociferously practised broken windows policing and they fell in cities like San Diego that were national leaders in community policing (Greene 1999; Chauhan *et al.* 2018). Two separate studies, each of about 200 U.S. cities, found no relationship between community policing and arrests (Chappell *et al.* 2006; Eitle and Monahan 2009). However, at least one city diverged from this trend, as Cincinnati made fewer arrests after a court ordered that they adopt community policing (Ridgeway *et al.* 2009). The present study expects community policing will not be associated with aggregate low-level arrest rate changes, and we test this hypothesis using data from wider geographic and temporal scales than have been used in past work.

### The material of policing

Municipal officials have been reluctant to reduce their cities' police budgets or police forces as a means of reform. As a result, most cities increase the percentage of their budgets devoted to policing in most years (Beck and Goldstein 2018). Public demonstrations against police brutality in 2015 and 2020, however, ushered in an unprecedented reevaluation of police expenditures. Black Lives Matter protestors called for the reinvestment of police funding into social services. Advocates suggested that reducing budget and personnel resources would be the most effective way to reform policing (Movement for Black Lives 2016; Reclaim the Block 2021; Kim *et al.* 2021). While few cities took up these demands and decreased their police expenditure, the calls renewed attention to the importance of budgets.

These social movement demands echoed theorists of the carceral state. Ruth Wilson Gilmore describes government spending and staffing as components of *state capacity*, the institutions that also include the state's bureaucratic structures, tax powers, office buildings, computer systems, and laws (Gilmore 2007: 78). Heather Schoenfeld (2018) further defines the state's *carceral capacity* as those components devoted to the criminal legal process, including prison buildings, police officers, and court expenditures (p. 4). These scholars posit that a state's capacity will be largely determinative of its practices and that changing any state practice—for instance, reducing misdemeanour arrests—requires changing the 'material means' the state has 'to transform its intervention from an ad hoc to a durable presence in society' (Gilmore 2007: 78). Changes in carceral capacity will be durable and fundamental, and, therefore, the most likely to change criminal legal outcomes like incarceration and arrest rates.

Gilmore (2007) and Schoenfeld (2018) do not see a tension between policy and resources, but rather see both as components of the state's capacity. While we agree with them that changing police resources and changing police policy are not mutually exclusive, we are interested in weighing the relative power of each. Which lever should a lawmaker pull if they want to reduce low-level arrests? To address this question, we isolate police policy from police resources, and analyse which component of a city's law enforcement capacity is most strongly related to arrest output. We argue that the *material of policing*—police forces and funding—play a larger role in shaping a police department's structure and outputs than its policies, leadership, or even how it allocates its resources. City officials might adopt a policy to try and decrease low-level arrests, but if they do not decrease *arresters*, apprehension rates will change little. Applying our theory to the United States between 1990 and 2018, we hypothesize that police funding and staffing changes were more strongly associated with low-level arrest rate changes than community policing policies were.

Previous empirical investigations into the resource-arrest relationship have arrived at mixed results. Two studies analysing the impact of federal grants made to cities as part of the 1994 crime bill found cities that hired more officers after receiving grant money made more low-level arrests (Zhao *et al.* 2003; Chalfin *et al.* 2020). A separate study found no statistically significant relationship between a city's number of officers and its number of drug arrests (Eitle and Monahan 2009). Two other research papers each found an *inverse* relationship: departments with more officers made fewer total arrests, fewer trespassing arrests, and fewer cannabis possession arrests per capita, a finding they attributed to smaller departments focusing more on arrests and less on newer policing strategies like targeted enforcement and community engagement (Crank 1990; Chappell *et al.* 2006).

The bulk of any city's police spending goes to personnel, so a department's budget and its number of officers will be closely related. Studies of police force size, therefore, might be sufficient to understand how budgets will relate to arrest rates. Still, police expenditure can indicate, over and above the staffing it funds, how much a department puts into overtime pay, technology, and equipment, three factors that might enable or encourage more arrests. As such, this study will examine personnel and budgeting separately and will be the first, to our knowledge, to quantitatively analyse police spending's relationship to misdemeanour apprehensions.

Drawing on theories of carceral capacity and with past empirical evidence in mind, we emphasize the material of policing and contend it is the amount of policing, not the type of policing, that will be most strongly associated with low-level arrest rates.

## DATA AND METHODS

To analyse the correlates of misdemeanour arrest rates, we gathered data from five sources, each described below along with the variables they provided. Our unit of analysis is the city-year, and our sample includes municipalities with more than 25,000 people in 1990. Our main sample includes data from 940 municipalities measured annually from 1990 to 2016 (the last year with data available for all variables). While we do not weight our sample to make it representative of all U.S. cities, the 940 sample cities are home to 100 million people, just under a third of the country's population. City-years missing data on any covariate and cities missing more than two-thirds of their dependent variable data were omitted. Models with alternate missing data procedures produced substantively identical results and are discussed in the sensitivity analysis section below. This approach created a sample of 24,046 city-years. Data on community policing's adoption is more geographically and temporally limited, so we create a secondary sample to analyse just the subset of cities and years for which these data are available. This sample includes 696 cities over 14 years for a total of 9,556 city-years with non-missing data.

Table 1 summarizes the sources of and dates covered by each variable. Our outcome measures are available through 2018, and our covariates are available through 2016, so our descriptive graphs include more years than our multivariate models. We describe each variable below.

### Outcome measures

Though there is no universal definition of 'misdemeanour,' we use a common one, the FBI's 'part-two' offence type, which includes 19 crimes, the most frequent of which were drug possession or sale, driving under the influence, disorderly conduct, drunkenness and 'not elsewhere classified.' Our primary outcome, the *misdemeanour arrest rate*, is the number of arrests per 1,000 residents in which the most severe charge was for a part-two crime. We tested four alternate constructions the misdemeanour arrest rate. Those results were largely identical, and we discuss them further in the sensitivity analysis section below. Our second measure of low-level enforcement, the *quality-of-life arrest rate*, is a subset of the five most minor and discretionary

**Table 1.** Data coverage

Data source	Variable(s)	Years covered
Federal Bureau of Investigation (FBI) Uniform Crime Reports 'Arrests by Age, Sex, and Race' and 'Offences Known and Clearances by Arrest' (Kaplan, 2020a)	Quality-of-life arrests, misdemeanor arrests, violent crime rate, property crime rate	1980–2018
FBI 'Law Enforcement Officers Killed and Assaulted' (LEOKA; Kaplan 2021)	Number of officers	1990–2016
Census Bureau 'Census of Governments: Annual Survey of State and Local Government Finances'	Police expenditure	1990–2016
Bureau of Justice Statistics 'Law Enforcement Management and Administration Survey' (LEMAS)	Community policing score	1997–2016
Census Bureau decennial census and 'American Community Survey'	Percent non-Hispanic Black, percent Latino, index of economic advantage, percent young men, percent foreign born, percent vacant housing units, population	1990–2016

misdemeanour arrest types: those for disorderly conduct, public drunkenness, vandalism, curfew/loitering, and vagrancy. We use the phrase 'low-level arrests' as an umbrella term to refer to both misdemeanor and quality-of-life arrests.

The third outcome variable, *misdemeanours as a percent of all arrests*, is the misdemeanor arrest count divided by all arrests a department made multiplied by 100. Since the only other arrest type is 'felony', this metric is a rescaled version of the misdemeanor-to-felony arrest ratio used in some research (e.g. Lum and Vovak 2018) and reflects changes in felony arrests in addition to changes in misdemeanor arrests. Each dependent variable is transformed to its natural logarithm to reduce skewness and outliers.

Uniform Crime Reports (UCR), the source of our arrest data, have some limitations. We take steps to mitigate each shortcoming. First, some police departments classify offence categories differently than others. This makes between-city comparisons difficult, but it is less of a concern in the panel models we use here because they analyse trends within units over time. This controls for time-invariant characteristics of police agencies like their idiosyncratic reporting practices. As long as the agency is consistent with its own reporting practices over time, their data will consistently contribute to the models. A second shortcoming is that UCR counts can be inaccurate due to poor imputation methods that have been designed to handle missing monthly data at the county-level (Maltz and Targonski 2002). This is less of a concern with annual, agency-level data, the kind used here (Lynch and Jarvis 2008). Data after 1993 are also more accurate than prior years, though missing data is a problem throughout all UCR data types (Lauritsen *et al.* 2016). To be cautious, we only include places that have two-thirds or more of their arrest data non-missing. We ran sensitivity analyses to see if this decision was driving the results, and it was not. Finally, not all police agencies report data to the UCR (Neusteter and O'Toole 2019). While this prevents our study from being nationally comprehensive, our primary sample covers cities home to one-third of the U.S.'s population. Furthermore, excessive missingness biases results to the null when the missingness is random, as we find here (Hutcheon *et al.* 2010), so if the UCR's missingness is affecting our results, it will be in a conservative direction.

### Explanatory variables

Our measure of personnel capacity is the number of *police officers per 1,000 residents*. Like UCR data, Law Enforcement Officers Killed and Assaulted (LEOKA) data suffer from missingness due to agency non-reporting (Kuhns *et al.* 2016). The methods used to account for UCR missingness, described above, also help mitigate missing data here.

The variable *police expenditure* is each city's spending on police in inflation-adjusted thousands of dollars per resident and includes inter-governmental transfers from states and the federal government. Because the federal 1994 crime bill created several grant programs that funded municipal police hiring, accounting for these inter-governmental transfers is important. Municipal budgets are usually created a year in advance, so we lag police expenditure one year to align each city's budget allocation with the year in which the money was spent.

The source of our community policing data, Law Enforcement Management and Administrative Statistics (LEMAS), is a survey conducted every three to seven years, and while it is not designed to be longitudinal, we identified five community policing survey questions asked with identical wording each year. These survey items indicate police departments that (1) trained current officers on community policing, (2) trained new recruits on the practice, (3) encouraged SARA-type techniques that help officers assess and solve problems, (4) conducted community surveys, and (5) had officers patrol consistent beats, a technique aimed at building relationships. These five variables were summed into a *community policing score* from 0 to 5 for each year.

### Control variables

Crime rates were declining sharply during our study period. Ideally, we would account for each city's misdemeanor crime rate to test whether the misdemeanor arrest decline was a by-product of there being fewer crimes for which to arrest. Unfortunately, an accurate, city-level measure of misdemeanor offences does not exist and is arguably not possible. Many misdemeanor offences are identified subjectively or through police searches, making them difficult to count objectively. Lacking a measure of misdemeanor crimes, we approximate the level of crime in a city by using its *violent crime rate*, a measure of the murders, non-negligent manslaughters, rapes, robberies and aggravated assaults per 1,000 residents gathered from the UCR. Violent crimes are less susceptible than property or misdemeanor crimes to underreporting by the public or misclassification by the police (Morgan and Oudekerk 2019). As such, they are often used as proxies for the actual crime rate (e.g. in Chappell *et al.* 2006; Lum and Vovak 2018), and we use them as such here. A sensitivity analysis that instead used the *property crime rate* found substantively identical results.

We include demographic controls including each city's *percent non-Hispanic Black* and *percent Latino* to capture racial and ethnic threat. To capture economic characteristics, we used factor analysis to create an *index of economic advantage* that combines each city's employment rate, non-poverty rate, percent with a B.A. degree, and median income. The factor had an eigenvalue of 2.95 and each variable loaded onto it with a score of at least 0.6. Since men 18–34 years old commit a disproportionate share of crime and because police disproportionately target them for arrest, we control for the *percent young men* in each city. We control for the *percent foreign-born* because immigrants have been shown to suppress crime (Stowell *et al.* 2009). We include a measure of *percent vacant housing units* as a proxy for disorder, and we control for each city's *population* (logged because of intense skewness). Because our models analyse change within cities over time and city boundaries rarely change, the population variable also indicates population density. We linearly interpolated independent variable data for those years missing data, a robustness check with non-imputed data is discussed in the sensitivity analysis section.



### Analytic strategy

Estimating the correlates of low-level arrests in a time-series context presents several methodological challenges and opportunities. The first challenge is that police department arrest rates in one year are likely influenced by their arrest rates in previous years. Police practices are path-dependent, guided by a persistent departmental structure, culture and set of habits. To ignore such an ‘inertial effect’ risks bias due to omitting the past values of both independent and dependent variables (Bailey 2016: 463). To account for such historical factors, we create ‘dynamic’ models that include multiple temporal lags of the dependent variable on the right-side of the equation to purge past associations (Wooldridge 2009, p. 310).

Including lagged values in the equation also helps address a second challenge: endogeneity. Crime and arrests likely influence one another in a feedback loop. As crime increases, police might respond with more arrests, which might affect crime, which might affect arrests, and so on. This common problem in policing research is often addressed by using an instrumental variable, a variable correlated with the endogenous independent variable but not the dependent variable or the error term (see, e.g. Rosenfeld and Fornango 2017; Chalfin and McCrary 2018). This instrumental variable approach isolates the direction of the two variables’ relationship (Murray 2006). We instrument the *violent crime rate* by including multiple lags of it, adapting a method developed by Arellano and Bond (1991) and used by Rosenfeld and Fornango (2017) in a similar context. Previous values of the *violent crime rate* are highly correlated with its future values ( $r = 0.73 - 0.90$ ), but weakly correlated with the outcome variables and their lags ( $r = 0.18 - 0.20$ ), making the lags effective instruments.

A limitation of dynamic models with an instrumented endogenous covariate like ours is that they risk compounding serial autocorrelation by adding highly correlated variables and by introducing a new independent variable (the lagged dependent variable) that is correlated with the error (Bailey 2016). We use two techniques to reduce this autocorrelation. We difference the variables by subtracting each value from the previous year’s value, a process that reduces serial autocorrelation by emphasizing year-to-year change (Nickell 1981). We also include additional past lags ( $t-2$  to  $t-4$ ) of the dependent variable to act as instruments that are related to the  $t-1$  lag, but not the error term (Arellano and Bond 1991). Because first-order autocorrelation is expected (indeed required to instrument the past values) we test for residual, second-order autocorrelation using Arellano-Bond tests and present those in the results table. In one of our models, problematic autocorrelation remained even after our corrections. Such second-order correlation bias obscures the lagged dependent variable’s coefficient more than those of other independent variables (Keele and Kelly 2006; Wilson and Butler 2007). Since it is the latter of interest here, this residual correlation does not present a large concern. Nevertheless, to insure it is not driving our results, we ran sensitivity analyses with a standard fixed-effects model without a lagged dependent variable nor an instrumented *violent crime rate* and found very similar results.

Per Roodman (2009), we present each model’s number of instruments and its Sargan  $J$ -test statistic’s  $p$ -value in the results table. All  $J$ -test  $p$ -values are above 0.05, so we do not reject the null hypothesis that the models’ overidentifying restrictions are valid.

In addition to reducing autocorrelation, differencing purges any time-invariant city characteristics like urban-rural status, region of the country, weather, political partisanship, and governmental structure (i.e. mayor-council vs. district council constitutions). In a practice akin to including dummy variables for each city, differencing accounts for these city-specific fixed effects (Allison 2009; Velez et al. 2012). Differencing also helps model non-linear time trends. As Figure 1 shows, the trend in misdemeanour arrests over time is not linear. By differencing the variable values, we eliminate the time trend, and this approach achieves

greater efficiency when modelling trends than other techniques like two-stage least squares (Baum *et al.* 2003).

To incorporate these modelling specifications, we use an Arellano and Bond (1991) first-difference, generalized method of moments (GMM) estimator to calculate models of the following form:

$$y_{it} = \alpha_i + \beta_1 y_{i,t-1} + \beta_2 x_{it} + \beta_3 z_{it} + \varepsilon_{it}$$

where  $y_{it}$  is the differenced dependent variable in city  $i$  at time  $t$ ,  $\beta_1$  is the coefficient of  $y_{i,t-1}$ , the lagged dependent variable,  $\beta_2$  is the coefficient of  $x_{it}$ , exogenous explanatory variables,  $z_{it}$  is a vector of the instrumented endogenous covariate (the *violent crime rate*), and  $\varepsilon_{it}$  is the idiosyncratic error.

## RESULTS

Table 2 presents each variable's mean and standard deviation. Figure 2 graphs the explanatory variables over time. The left graph shows police force sizes were largely stable during this time, hovering around two officers per 1,000 residents. Yet, this figure provides some descriptive evidence that the fluctuations in officers, though small, corresponded to the misdemeanor arrest trend. Aligning this graph with the national misdemeanor arrest rate graph in Figure 1 (note

**Table 2.** Variable means and (standard deviations),  $N = 24,046$  city-years

	1990	2016	% change 1990 to 2016	All years <sup>a</sup>
<i>Outcomes</i>				
Misdemeanour arrest rate per 1,000 people	48.06 (27.54)	31.17 (19.56)	-35.14%	41.14 (18.40)
Percent of arrests for misdemeanours	74.16 (9.59)	77.51 (9.67)	4.53%	77.70 (6.61)
Quality-of-life arrest rate per 1,000	12.31 (10.53)	4.67 (5.37)	-62.06%	7.87 (4.87)
<i>Explanatory Variables</i>				
Number of officers per 1,000	1.76 (0.69)	1.91 (1.13)	8.52%	1.73 (0.24)
Police expenditure (\$1,000s per capita)	0.21 (0.10)	0.30 (0.14)	42.86%	0.25 (0.06)
Community policing score	3.75 (1.21) <sup>b</sup>	3.61 (1.13)	-3.73%	2.48 (0.67)
<i>Controls</i>				
Violent crime rate per 1,000	7.26 (6.53)	4.76 (3.92)	-34.44%	5.47 (2.52)
Percent Black	10.86 (14.75)	13.67 (16.16)	25.87%	11.44 (2.13)
Percent Latino	10.88 (16.11)	15.97 (17.40)	46.78%	16.36 (3.86)
Index of economic advantage	0.05 (0.81)	-0.17 (0.88)	-440.00%	0.08 (0.21)
Percent young men	17.45 (3.89)	15.45 (4.16)	-11.46%	15.71 (1.08)
Percent foreign born	8.85 (9.68)	12.15 (10.37)	37.29%	12.76 (2.63)
Percent vacant housing units	6.95 (3.59)	9.72 (5.81)	39.86%	7.59 (1.83)
Population	95,303 (201,009)	124,978 (258,486)	31.14%	101,684 (24,552)

<sup>a</sup> Single-year standard deviations are between-city, all-year standard deviations are within-city.

<sup>b</sup> The community policing score is measured in 1997, not 1990, reflecting the start of that data.

the different years each covers) reveals similar trends. Both had peaks in 1997 and 2007 with a dip in between. **Figure 2**'s centre graph reveals that police expenditures in the 940 sample cities rose steadily until the 2008 recession, after which they dipped, then began rising again around 2013. **Figure 2**'s right graph shows community policing was most widely practiced in 1999, declined in the early 2000s, and saw a small resurgence after 2006.

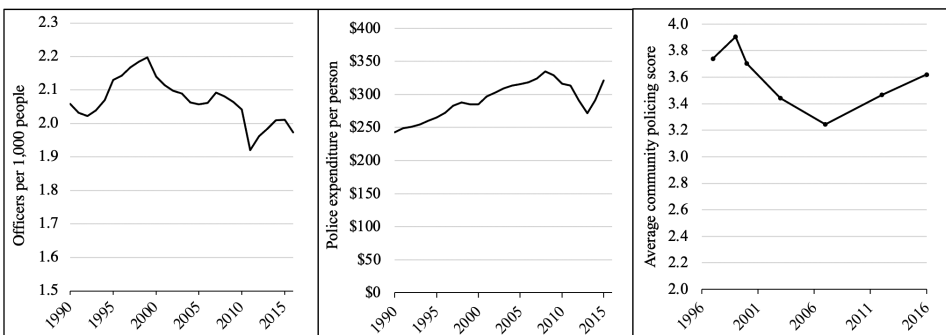
**Table 3** presents results from our multivariate analyses. Model 1 regresses the misdemeanor arrest rate on the explanatory and control variables. Models 2 and 3 repeat this analysis for the two other outcomes: the percent of arrests for misdemeanours and the quality-of-life arrest rate. Because the community policing score is available for only a subset of municipalities and years, it is included separately in models 4–6, which duplicate models 1–3 on the smaller sample.

We expected the amount of staffing and funding resources a city invested in policing to correspond to their low-level arrest rates. Model 1 shows that cities where police force size decreased by one standard deviation (0.24 officers per 1,000 people), saw a corresponding 2.6% decrease in their misdemeanor arrest rate ( $((e^{0.104}-1)*0.24*100 = 2.6)$ ). Cities that decreased their police expenditures by one standard deviation (\$58 per resident) experienced a 1% decline in their misdemeanor arrest rate ( $((e^{0.161}-1)*0.058*100 = 1)$ ). Decreases in police capacity were associated with decreases in low-level arrests.

Model 2 estimates misdemeanor arrests as a percent of all arrests. The only two arrest categories are 'misdemeanour' and 'felony,' so this model also reflects changes in felony arrest practices. An increase in this outcome represents not only a shift toward misdemeanor arrests, but also a proportional shift away from felony arrests. Here, we see the same pattern as in model 1: officers and expenditures are positively and statistically significantly related to the percent of arrests for misdemeanours. A one standard deviation decrease in the number of officers corresponded to an associated 0.4% decrease in the proportion of arrests for misdemeanours ( $((e^{0.018}-1)*0.23*100 = 0.4)$ ). When cities hired fewer officers, their misdemeanor arrests decreased more than their felony arrests, and when they hired more offices, their misdemeanor arrests increased more than felony ones. Police expenditure displayed the same trend. Cities that decreased their police spending by one standard deviation saw a 0.9% decrease in the proportion of arrests for misdemeanours ( $((e^{0.145}-1)*0.058*100 = 0.9)$ ).

Model 3 reveals staff and budget resources devoted to policing are also positively and statistically significantly related to quality-of-life arrest rates, at similar magnitudes to misdemeanor arrest rates.

Models 4–6 indicate that the adoption of community policing was not statistically significantly related to any of the three low-level arrest measures and controlling for community policing's adoption did not change the resource-arrest association, which remained positive and statistically significant in each model.



**Figure 2.** Explanatory variables, 940 sample municipalities.

Table 3. Regression coefficients (and standard errors), Arellano-Bond GMM models

Variables	1	2	3	4	5	6
	Misd. arrest rate <sup>a</sup>	Misd. arrest percent	Quality-of-life arrest rate	Misd. arrest rate	Misd. arrest percent	Quality-of-life arrest rate
Lagged outcome	0.600*** (0.012)	0.262*** (0.008)	0.629*** (0.012)	0.446*** (0.042)	0.513*** (0.042)	0.487*** (0.049)
Officers per 1,000 residents	0.104*** (0.014)	0.024*** (0.003)	0.100*** (0.015)	0.288*** (0.077)	0.072* (0.036)	0.238** (0.076)
Police expenditure	0.161** (0.058)	0.058** (0.022)	0.150* (0.061)	1.221*** (0.185)	2.177*** (0.129)	0.410* (0.181)
Community policing score				-0.012 (0.014)	0.003 (0.007)	-0.006 (0.014)
Violent crime rate	0.021*** (0.005)	-0.002 (0.002)	0.019*** (0.005)	0.047** (0.016)	0.009 (0.007)	-0.002 (0.015)
Temporally lagged violent crime rate	-0.008 (0.004)	-0.000 (0.001)	-0.008 (0.005)	-0.010 (0.013)	0.016** (0.006)	0.009 (0.013)
% Black	0.002 (0.004)	0.003* (0.001)	0.001 (0.004)	0.012 (0.016)	0.006 (0.008)	0.007 (0.016)
% Latino	-0.007* (0.004)	-0.001 (0.001)	-0.011** (0.004)	-0.016 (0.015)	0.000 (0.007)	-0.005 (0.014)
Economic advantage index	0.167*** (0.021)	0.082*** (0.006)	0.096*** (0.023)	0.264* (0.117)	0.041 (0.057)	0.186 (0.118)
% young men	-0.018** (0.006)	-0.010*** (0.002)	-0.005 (0.006)	-0.007 (0.027)	-0.019 (0.013)	0.004 (0.027)
% foreign born	0.015*** (0.004)	0.006*** (0.001)	0.022*** (0.004)	0.025 (0.019)	-0.006 (0.009)	-0.002 (0.019)
% vacant housing units	0.008** (0.003)	-0.001 (0.001)	0.022*** (0.003)	0.011 (0.014)	-0.006 (0.007)	0.024 (0.014)
Population (logged)	-0.374*** (0.058)	0.012 (0.018)	-0.241*** (0.062)	-0.289 (0.230)	0.140 (0.111)	-0.484* (0.228)
Year	-0.004** (0.001)	-0.001* (0.000)	-0.011*** (0.002)	-0.006 (0.007)	-0.003 (0.004)	-0.007 (0.007)
Constant	5.408*** (0.665)	3.099*** (0.209)	3.019*** (0.702)	4.070 (2.714)	0.060 (1.319)	5.692* (2.678)
Instruments	187	187	187	145	145	145
AB AR(2) test <sup>b</sup>	$p = 0.534$	$p = 0.724$	$p = 0.790$	$p = 0.488$	$p = 0.014$	$p = 0.130$
Sargan $J$ -test <sup>c</sup>	$p = 0.193$	$p = 0.559$	$p = 0.389$	$p = 0.290$	$p = 0.277$	$p = 0.435$
N (city-years)	24,046	24,046	24,046	9,556	9,556	9,556

<sup>a</sup>Dependent variables are logged.

<sup>b</sup>Arellano-Bond test for second-order autocorrelation.

<sup>c</sup>Sargan test of over-identification restrictions.

\* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$ .

The control variables in models 1–3 shed light on other covariates of the misdemeanor arrest decline. Cities that experienced larger violent crime declines saw correspondingly larger drops in their misdemeanor and quality-of-life arrests. There was no association between crime and the percent of arrests for misdemeanours, however, suggesting the crime-arrest re-

relationship was the same for both misdemeanour and felony arrests. The growth or decline in the share of cities that were Latino or Black did not demonstrate any consistent relationship with low-level arrests. Past research has also found no ethnic threat effect for Latinos. The null finding regarding Black residents could be an artefact of stable Black populations during the study period, as our methods look at within-city change. Places undergoing economic decline experienced fewer low-level arrests. The positive coefficients for percent foreign-born residents in three of the four models might suggest an immigrant threat effect. None of the other control variables indicated consistent relationships with the outcomes.

### Sensitivity analyses

To ensure our findings were not sensitive to choices we made in variable construction or modelling specification, we ran 15 robustness checks each for model 1 and model 4 for a total of 30 checks. We included alternate constructions of the misdemeanour arrest measure, different population thresholds, alternate logarithm adjustments, different missing data procedures, the exclusion of interpolated data, the use of property crime instead of violent crime, count instead of rate outcomes, and panel models without lagged variables or differencing. In all 30 checks, the coefficient for the number of officers was statistically significant and positive, and in none of the alternate models was community policing statistically significant, mirroring the models presented above. Police expenditure was positive in all sensitivity analyses but dropped out of statistical significance in six of the 30, including those that changed the logarithm adjustments and one of the two that omitted interpolated data. These largely consistent results give us more confidence in our findings. Full results of all sensitivity analyses are available upon request.

## DISCUSSION AND CONCLUSION

Police in the United States made half of the misdemeanour arrests in 2018 that they did in 1997 and a third of the quality-of-life arrests. Changes to policing in the U.S. often diffuse globally, so understanding what accounts for this marked decrease has international implications. We drew on theories of carceral state capacity to hypothesize a link between the resources a city devotes to law enforcement—what we call the *material of policing*—and its low-level arrest rates. We suspected police departments' budget and staffing inputs would strongly influence their discretionary arrest outputs. Municipal officials rarely seek reform by reducing these resources, however. They are much more likely to change policing policy or strategy, so we compared our materialist theory with one that emphasizes policy change, focusing on a policy that de-emphasizes arrests and has been adopted globally: community policing.

To test whether resources or policy better-predicted misdemeanour arrest rates, we assembled data from 940 U.S. municipalities across 27 years in Arellano-Bond GMM models. Net of crime and demographic controls, the material of policing demonstrated consistent, positive associations with all three of our low-level arrest measures. When cities shrank their police staff or their police spending, they saw associated declines in low-level arrest rates. The personnel-arrest relationship was robust to all 30 alternate modelling specifications, and the spending-arrest relationship was robust to 24. Notably, the number of police officers per capita did not change dramatically in the U.S. during this time, so our results demonstrate that even small shifts in personnel can correspond to large changes in low-level arrest rates. As then-New York City Police Commissioner Raymond Kelly put it, 'when you hire more officers, they make more arrests' (Robbins 2013).

While our methods cannot reveal the mechanisms connecting personnel to arrests, it is possible that, once hired, police will want to show they are productive, and making discretionary

arrests is a common way to do so. Increased spending might generate more arrests, over and above its contribution to increased staffing, because it can provide more administrative support, equipment and overtime, allowing officers more time and capacity to make and process arrests quickly.

Though our methods cannot indicate causality, the theory that policy—in this case community policing—influences low-level arrest rates was not supported by our findings. Though community policing was the most prominent law enforcement policy to arise in the U.S. during the study years, in no model was its adoption statistically significantly related to the rate or proportion of either misdemeanour or quality-of-life arrests. Still, it is possible that community policing changed arrest rates in some cities or that other policies played a role in the arrest reduction. Overall, our findings are consistent with the theory that the material of policing is more consequential for law enforcement than policy is, at least for low-level arrest rates in the U.S.

Testing the crime-arrest relationship is complicated by endogeneity, so we purged the circular effects and found crime was an important correlate of low-level arrest rates. Cities that saw larger crime reductions experienced larger declines in both misdemeanour and quality-of-life arrest rates. This indicates the steep, secular crime and arrest declines in the U.S. in the 2000s and 2010s were related. Crime was not associated with misdemeanours as a proportion of all arrests, however, indicating crime fluctuations did not impact the allocation of arrests between misdemeanours and felonies.

Despite the steady, 20-year decline in misdemeanour arrests, we estimate municipal police in the U.S. still made 7.6 million such arrests in 2018. This large number of apprehensions likely degraded public opinion of police and, for those arrested, likely compromised employment, damaged family connections, and hurt school performance. Misdemeanour arrests are a common entry point to the rest of the criminal legal system, so they can cascade into incarceration, fines and fees, future arrests, or even, in the most extreme cases, deaths in custody. These consequences underscore the importance of understanding what drives low-level arrest rates.

Police reform efforts have historically centred on changes to policing policy and have steadfastly avoided any changes to policing capacity. Beginning in 2015, however, this shifted as calls to defund the police grew louder in protests from Colombia to the U.S. to South Africa ([Westerman et al. 2020](#)). Demonstrators in the global 2015 and 2020 Black Lives Matter protests against police brutality called for ‘the reallocation of funds from police ... to restorative services’ ([Movement for Black Lives 2016](#)). While such reinvestment has not been widely embraced by mayors or city councils, a few U.S. cities like Seattle and Austin made temporary budgetary moves in this direction ([Kaur 2021](#); [O’Connor 2021](#)).

Regardless of whether municipal lawmakers embrace or eschew demands for police budget reinvestment, they set police budget and staffing levels each year, and when doing so they might consider how increasing either will likely be accompanied by increases in low-level arrests and their attendant harms. If elected officials want to decrease low-level arrests, our results indicate shrinking the size of the police force or the size of its budget would be a more effective approach than adopting a policy like community policing. Municipal budget decisions often receive little attention, but our results suggest they are very consequential for low-level law enforcement.

A common response to any suggestion that cities decrease police spending or staffing is that doing so will increase crime and take resources away from investigating serious crime. This study cannot speak to the effect of staffing or spending changes on crime. Past research on this is mixed (See, e.g. [Lee et al. 2016](#); [Chalfin and McCrary 2018](#)). As to whether resource reductions will inhibit the policing of felony crimes, our results do offer some guidance. When cities decreased their police forces or budgets, both misdemeanour and felony arrests declined, but misdemeanour arrests declined more, meaning police concentrated more on serious crime relative

to low-level offences. Conversely, increasing staffing and spending increased misdemeanour more than felony enforcement.

This study has several limitations. First, we cannot directly measure the adoption of broken windows policing policies, so we cannot say if they were linked either to the increase in misdemeanour arrests leading up to 1997 or to the subsequent decline. Single-city studies or novel methods of measuring the policy's uptake will be required to address this. A second limitation is that we rely on the FBI's UCR data program, to which not all police departments report and to which some departments report only partial data. We took measures to mitigate these shortcomings, as described in the 'Data' section above. Nevertheless, if departments that do not report are systematically different from those that do, our results will not be representative of all U.S. police agencies. Finally, as with any national study, we prioritize breadth over depth, and therefore might miss the idiosyncrasies of specific cities or police agencies.

The large, steady, and enduring decline in misdemeanour arrest rates between 1997 and 2018 in the United States was a significant social change that has yet to receive much attention. Because low-level arrests can have such negative impacts, and U.S. policies often diffuse to other countries, we think more research into the context of this decline is warranted. The present study suggests changes in the material of policing were more closely linked to these arrest rate fluctuations than community policing was, and there is still yet more to know about the causes and consequences of this historic change.

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