RESEARCH ARTICLE

More cops, fewer prisoners?

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Research Summary: The results reported in a large amount of the criminology literature reveal that hiring police officers leads to reductions in crime and that investments in police are an efficient means of crime control compared with investments in prisons. One concern, however, is that because police officers make arrests in the course of their duties, police hiring, albeit efficient, is an inevitable driver of "mass incarceration." In this article, we consider the dynamics through which police hiring affects downstream incarceration rates.

Policy Implications: Using state-level panel data as well county-level data from California, we uncover novel evidence in favor of a potentially unexpected and yet entirely intuitive result: that investments in law enforcement are unlikely to increase state prison populations markedly and may even lead to a modest decrease in the number of state prisoners. As such, investments in police may, in fact, yield a "double dividend" to society by reducing incarceration rates as well as crime rates.

KEYWORDS
crime, incarceration rates, police, prison

There are two primary mechanisms through which criminal justice policy inputs like police and prisons can reduce crime: deterrence and incapacitation. Deterrence represents a behavioral response of crime to a given crime control strategy and is based on the idea that a rational offender will reduce the amount of crime he or she supplies when the price of crime (which is, in turn, a function of the certainty of apprehension and the severity of the expected sanction) increases. This conception of deterrence is a core idea in Becker's (1968) seminal contribution to the economics of crime and in Cornish and Clarke's (2013) *The Reasoning Criminal* and can be found in early treatises on the subject by Beccaria (1785) and Bentham (1793) and later extensions by Ehrlich (1973), Shavell (1991), and McCrary (2010) among others.[1] Incapacitation, on the other hand, represents a mechanical response of

crime to police and is premised on the idea that by arresting and subsequently incarcerating offenders, some number of crimes will be abated—or incapacitated—away.[2]

Deterrence, it has been noted, is inexpensive relative to incapacitation (Nagin, 2013). Even though preventing crimes via incapacitation requires that municipal and state governments finance the considerable costs of arresting, adjudicating, and confining offenders, deterrence has far fewer costs. Accordingly, the cost-effectiveness of a given crime control strategy will depend on the relative mixture of deterrence and incapacitation effects that the strategy generates. Critically, if deterrence effects are sufficiently large, it is possible for a crime control strategy to reduce both crime *and* incarceration, allowing society to achieve a "double dividend" in which two costly outcomes—crime and resources allocated to crime control—are simultaneously minimized (Durlauf & Nagin, 2011).

A critical task then for scholars of criminal justice policy is to generate evidence not only around the efficacy of a given crime control strategy but also its cost-effectiveness in reducing crime. Naturally, one of the principal crime control strategies employed by governments around the world involves public investments in law enforcement. In a large amount of published literature, scholars have considered the responsiveness of crime to the presence and availability of police and have found that police resources have a modest but, in general, important impact on crime.[3] With respect to police manpower, prevailing estimates indicate that a 10% increase in police hiring will reduce violent crimes by between 4% and 10% and property crimes by between 2% and 5% (Cook, Kapustin, Ludwig, & Miller, 2017; DeAngelo & Hansen, 2014; Evans & Owens, 2007; Levitt, 2002; Marvell & Moody, 1996; McCrary, 2002; Weisburst, 2016; Worrall & Kovandzic, 2010) and that, given these estimates, the benefits of hiring police officers likely exceeds the cost of doing so (Chalfin & McCrary, 2017a).[4]

The extent to which police are a cost-effective crime control strategy is less certain as it is difficult to disentangle deterrence empirically from incapacitation effects (Kessler & Levitt, 1999; Owens, 2013; Webster, Doob, & Zimring, 2006). In summarizing the literature, however, scholars have noted that there is considerable evidence—both theoretical and empirical—to suggest that investments in law enforcement are an efficient means of controlling crime when compared with investments in corrections (Chalfin & McCrary, 2017b; Durlauf & Nagin, 2011; Nagin, 2013). Empirically, this is seen in recent estimates of the elasticity of crime with respect to the prison population, which is, at most, approximately –0.1 to –0.2, considerably smaller in magnitude than the crime–police elasticity (Cullen, Jonson, & Nagin, 2011; Durlauf & Nagin, 2011; Johnson & Raphael, 2012; Liedka, Piehl, & Useem, 2006; Nagin, 2013; Raphael, Lofstrom, & Martin, 2017). With respect to theory, beginning with Bentham (1793), scholars have noted that because apprehension and subsequent incarceration are both uncertain and indeed improbable for most crimes, to the extent that offenders are myopic, offending will be more sensitive to the certainty of punishment that is experienced in the present rather than to the severity of punishment that is experienced in the future (Durlauf & Nagin, 2011; Lee & McCrary, 2017; Nagin, 2013; Paternoster, 2010). Given the modest relationship between crime and state prison populations as well as the concern that the experience of prison could itself be criminogenic, a consensus has developed among criminal justice policy scholars that social planners could maximize the effectiveness of public safety resources by reallocating away from investments in incarceration toward investments in law enforcement (Chalfin & McCrary, 2017b; Durlauf & Nagin, 2011).

In recent years, as crime has declined from its national peak in the early 1990s, policy makers and citizens alike have turned their attention to the collateral harms of incarceration—costs that fall disproportionately on low-income, racially segregated neighborhoods from which the incarcerated population is predominately drawn (Aizer & Doyle, 2015; Clear, 2009; Foster & Hagan, 2009; Genty, 2002; Hagan & Dinovitzer, 1999; Mueller-Smith, 2015; Turanovic, Rodriguez, & Pratt, 2012). As attention has turned to the social harms of "mass incarceration," it has been suggested that spending on both prisons and police has been excessive (Tonry, 2011). After all, the story goes, even if the police affect

crime partially through deterrence, given that police make arrests—approximately 13 per officer per year according to the Bureau of Justice Statistics—they must also incapacitate offenders and therefore contribute directly to the staggering scale of incarceration growth in the United States.[5]

In this article, we note that such analysis is not a foregone conclusion and that the extent to which—and even whether—investments in police lead to greater incarceration rates is an empirical question, one that depends on the precise mixture of deterrence and incapacitation effects through which police reduce crime. To the extent that a sufficiently large share of the impact of police is through deterrence, then it is possible for police to have the attractive quality of reducing both crime and incarceration.[6] Can investments in law enforcement, in fact, generate a double dividend to society? If so, this would be an important revelation as it suggests a rare instance in which, over some range of plausible values, society can have its cake and eat it too. Perhaps as a result of its attractiveness, this idea has gained traction in policy circles and has formed the basis for a recent and widely read article in *The New York Times* in which New York City's dramatic decline in both its crime rate and its jail population has been documented. The idea has also been advanced in a prominent article in the *City Journal* by George Kelling and former LAPD and NYPD Commissioner William Bratton.[7] This is also a core idea in Durlauf and Nagin's (2011) influential and thought-provoking article in *Criminology & Public Policy*—"Imprisonment and Crime: Can Both Be Reduced?"—in which they speculate that policies that affect the certainty of punishment can potentially have the attractive quality of yielding a double dividend.

Surprisingly, despite the fact that the effect of investments in law enforcement on downstream incarceration rates is a key policy estimand with broad implications for how public safety resources are allocated, we have been unable to locate an estimate of this quantity in the literature. The closest research we are aware of is that of Owens (2013), who used a clever identification strategy—the quasi-random rollout of hiring grants from the Department of Justice's COPS Office—to estimate the marginal impact of police hiring on the number of arrests made at the agency level and, intriguingly, found little evidence that arrests increase with police hiring, which narrows the scope for incapacitation effects to be large.

Although this evidence indicates that the scope for police to generate large incapacitation effects is modest, and therefore that police reduce crime primarily through deterrence, we argue that an analysis of arrests is not sufficient to draw a conclusion about the effect of police on *new prison commitments*. To support this claim, we note that the empirical relationship between arrests and prison admissions is far from perfect. Indeed, for a given state, the number of arrests made is a modest predictor of the number of subsequent prison spells. How can this be? As it turns out, there is substantial slippage in the criminal justice system between arrests and subsequent prison spells. Indeed, for most serious crimes, the conditional probability of a prison spell given a criminal charge is ~40% and many arrests are not converted into sustained criminal charges in the first place. The rate at which arrests convert into prison spells depends on many factors including the type of arrest, the strength of the available evidence, the dynamics of plea bargaining, and the *de jure* and *de facto* sentencing regimes in a given jurisdiction. This is not merely an academic point as several scholars have noted the importance of prosecutorial discretion in generating exponential growth in incarceration (Pfaff, 2011, 2014).

Through this research, we address the important but understudied question of whether more intensive investments in law enforcement lead to an increase in the size of a state's prison population. Using state-level panel data as well as more detailed county-level panel data from California, we estimate the effect of increases in police expenditures on actual and *expected* new prison commitments. To address the traditional concern that annual changes in police expenditures may be endogenous, we appeal to an instrumental variables strategy that is a mainstay of the literature on police manpower and crime—one in which we leverage the empirical regularity that investments in law enforcement tend to move in tandem with investments in fire safety (Kovandzic, Schaffer, Vieraitis, Orrick, & Piquero, 2016; Levitt,

TABLE 1 Criminal justice funnel

Crime	% Convicted Felons Sentenced to Prison
Murder	93%
Rape	72%
Robbery	71%
Aggravated Assault	43%
Burglary	49%
Larceny	34%
Motor Vehicle Theft	42%
Drug Possession	33%
Drug Sale	41%

Note. Data come from the Bureau of Justice Statistics' 2006 Felony Sentences in State Courts report, the last year for which the report is available.

2002).[8] Empirically, we demonstrate that this is the case both among states at the national level and among counties in California.[9]

Although estimates are not sufficiently precise to rule out a positive relationship between investments in law enforcement and the growth of a state's prison population completely, the available data indicate that the effect of police spending on new prison commitments is more likely than not to be negative and, critically, is highly unlikely to be large and positive. Even using an extraordinarily pessimistic estimate of the effect of police spending on prison commitments (arising from the upper bound of the 95% confidence interval of our preferred estimate), the premise that an increase in spending on law enforcement has been a driver of mass incarceration is unlikely to be true.

The remainder of the article is organized as follows. In the next section, a simple mathematical model of the criminal justice system is motivated that facilitates a set of expectations against which the empirical analysis may be compared. Then we present the article's empirical strategy. We also discuss the data used in the research. Finally, we present the results and conclusion.

1 | POLICE AND PRISON SPELLS: A SIMPLE MODEL

In this section, we motivate a simple model of police hiring and its downstream effects. The purpose of this discussion is to elucidate the conditions under which police hiring will either increase or decrease the rate of new prison admissions. The model is simple but realistic and generates predictions that provide context for the empirical estimates we present later in the article.

We begin by considering a society that, at time t, employs N_t police officers and experiences Y_t crimes where the share of crimes that are cleared by arrest is P_t.[10] In year t, this society will record $A_t = Y_t P_t$ arrests in year t. Some share of arrests will lead to a prison sentence and, thus, to a new prison admission. This conversion rate of an arrest into a prison spell, q_t, is a function of several different inputs: the rate at which charges are accepted by a prosecuting attorney, a jurisdiction's sentencing policy along the intensive and extensive margin, trial dynamics, and the relative efficiency of plea bargaining. Table 1 is a replication of descriptive research conducted by the U.S. Bureau of Justice Statistics that reports the share of individuals processed in state courts who receive a custodial prison sentence for several serious crimes. The data suggest that q is probably a number like 0.25 for a typical index crime and possibly much lower given that it is not uncommon for an arrest to be dismissed prior

to a sentence.[11] Given that q_t is the share of arrests that result in prison time, then the number of new prison commitments is given by $C_t = q_t Y_t P_t$.

Now assume that between years t and $t + 1$ our hypothetical society allocates more resources to police and hires some additional number of police officers.[12] In year $t + 1$, the society will employ N_{t+1} officers, will experience Y_t+1 crimes, and will record $A_{t+1} = Y_{t+1} P_{t+1}$ arrests that will lead to $q_{t+1} Y_{t+1} P_{t+1}$ new prison commitments. Our task is to identify the underlying dynamics of how the rate of new prison commitments responds to police—specifically we will be interested in the elasticity of new prison commitments with respect to police, which we denote using ε_c. To derive this, first note that:

$$\varepsilon_c = \frac{\%\Delta C}{\%\Delta N} = \frac{\frac{q_{t+1}}{q_t} \frac{Y_{t+1}}{Y_t} \frac{P_{t+1}}{P_t} - 1}{\frac{N_{t+1}}{N_t} - 1} \tag{1}$$

The numerator of Equation (1) is the percentage change in the number of prison commitments and the $\frac{q_{t+1}}{q_t} = 1$ (that is, the denominator is the percentage change in the number of police officers). When q_t when police do not affect the rate at which arrests are converted into prison sentences—as they could through better resourced investigations, the change in prison commitments will be proportional to the change in arrests.

Equation (1) can be rewritten more compactly using elasticity notation. Let ε_p, ε_c, and ε_y be the elasticities of the probability of apprehension, the probability of a prison commitment, and the number of crimes with respect to the number of police, respectively. In that case, the elasticity of new prison commitments with respect to police can be written as:

$$\varepsilon_c = \frac{[1 + \varepsilon_q \%\Delta N][1 + \varepsilon_y \%\Delta N][1 + \varepsilon_p \%\Delta N] - 1}{\%\Delta N} \tag{2}$$

Setting Equation (2) equal to zero allows us to characterize the trade-off among ε_q, ε_p, and ε_y. If we fix $\Delta N = 0.01$ (e.g., assuming that we observe a 1% increase in police) and rearrange, then Equation (2) can be re-written as:

$$\left[1 + \frac{\varepsilon_q}{100}\right]\left[1 + \frac{\varepsilon_p}{100}\right] = \frac{1}{\left[1 + \frac{\varepsilon_y}{100}\right]} \tag{3}$$

When the left-hand side of Equation (3) is greater than the right-hand side of Equation (3), $\varepsilon_c < 0$, meaning that police hiring leads to a decrease in prison commitments. If $\varepsilon_q = 0$, then for police hiring to lead to an increase in prison commitments, $1 + \frac{\varepsilon_p}{100} > \frac{1}{1 + \frac{\varepsilon_y}{100}}$. Practically speaking, this means that whether police hiring increases or decreases the flow of prisoners depends on the relative magnitudes of ε_p and ε_y.[13]

Next, we note that the results reported in the prior literature indicate that $\varepsilon_y < 0$ and theory would suggest that ε_p and ε_q should both be non-negative. Critically, whether police lead to larger or smaller flows of prisoners depends on the magnitudes of ε_p and ε_q as well as on the interaction of these two quantities. Thus, for a given sum of the two elasticities, there is greater upward pressure on the prison population when ε_p and ε_q are equal in magnitude. Conversely, upward pressure on the prison population is minimized when one of the elasticities is zero. Provided that ε_y is approximately –0.5 to –1, which is consistent with results in the prior literature, either ε_p or ε_q will need to be quite large for police hiring to increase the size of a state's prison population. In other words, for police to increase the incarceration rate, more police resources will need to increase crime clearance rates and

the efficacy of investigations considerably. For instance, if expanding the size of a city's police force by 10% increased both the clearance rate and the rate at which arrests lead to prison spells by 2%, it is unlikely that this would be sufficient to lead to an increase in the number of prison spells.

Although empirical estimates of these elasticities are difficult to come by, we note that scholars have been skeptical that faster police response times (a leading way in which police resources would affect clearance rates) yield higher crime clearance rates (Bayley, 1996; Siegel & Worrall, 2013; Skogan & Frydl, 2004; Walker & Katz, 2012).[14] In a smaller amount of literature, other scholars have attempted to study the effect of police on clearance rates directly and have generally found modest effects at most (Bennett, 1982; Puckett & Lundman, 2003). This exercise thus casts some doubt on whether ε_p and ε_q are sufficiently large to lead to a positive prison police elasticity. Indeed, empirical estimates reported later in this article indicate that this elasticity is more likely to be negative than positive.

2 | EMPIRICAL STRATEGY

We seek to generate a national estimate of the effect of investments in law enforcement on new commitments to state prisons. Although we sometimes express results in this article in terms of the *number* of police officers employed, recognizing that overtime hours are a key law enforcement input, we model the relationship between new prison commitments and *expenditures* on law enforcement salaries as this better captures the magnitude of society's investment in police manpower. Following Owens (2013), the model we would ideally like to estimate is:

$$\text{COMMIT}_{\text{it}} = \alpha + \tau \text{POLICE}_{\text{it}-1} + X'\beta + \phi_{\text{i}} + \gamma_{\text{t}} + \varepsilon_{\text{it}} \tag{4}$$

In Equation (4), $COMMIT_{it}$ is the number of new prison commitments in state i in year t, $POLICE_{it-1}$ are expenditures (in tens of thousands of dollars) on law enforcement in the previous year, X_{it} is a vector of time-varying control variables, and ϕ_{i} and γ_{t} are state and year fixed effects, respectively.[15] In keeping with the standard model employed by scholars in the literature, we lag police spending by 1 year so as to minimize simultaneity bias (Chalfin & McCrary, 2017a; Evans & Owens, 2007; Marvell & Moody, 1996; Worrall & Kovandzic, 2010).

There are two primary challenges in estimating this equation. The first is aggregation bias, a concern that arises because police spending varies across cities within a given state, whereas prison admissions are measured statewide in our data. The specific concern is that, by aggregating city-level law enforcement data to the state level, we face a classic ecological inference problem—that of deducing the nature of individuals from inferences for the group to which those individuals belong. Although this issue is commonly found in the applied literature, to address this concern thoughtfully, we augment our national analysis with an analysis of data on county-level prison admissions in California, reestimating Equation (4) at the county level within California.[16] The resulting estimates are remarkably similar to national estimates that aggregate to the state level, providing confidence that the level of aggregation is not a first-order issue for estimation.[17]

A second estimation challenge is that police spending may be endogenous, and thus, estimating Equation (4) via ordinary least squares (OLS) could return an inconsistent estimate of τ. In particular, the concern that has been articulated in the literature is that cities might have the ability to manipulate police spending in anticipation of a future change in crime, a prospect that has been raised by many scholars in this area, beginning with Marvell and Moody (1996) and Levitt (2002). To the extent that this is true, there will be a mechanical correlation between police on the one hand and crime or incarceration rates on the other, thus leading to an upward bias in estimating τ.

Economists, in particular, have devoted careful attention to addressing endogeneity problems in the literature but is year-over-year police spending endogenous? Our reading of the political science and public administration literatures is that the realities of city constraints and politics make strategic police hiring extremely difficult, dampening the scope for endogeneity of this type. In particular, cities labor under state- and city-level statutory and constitutional requirements that they balance their budgets annually,[18] they face tax and expenditure limitations,[19] they may suffer from inattention regarding staffing or may use staffing reductions as bargaining chips (e.g., bailout-seeking),[20] and they may be hamstrung by unilateral changes to state and federal revenue-sharing funds that are difficult to anticipate.[21] In addition, state and local civil service ordinances necessitate a lengthy hiring process, making it difficult to adjust policing levels quickly or in great numbers. Finally, cities may suffer from important principal–agent problems with elected officials having potentially much different objectives from those of the median voter. In short, cities face binding liquidity constraints, limited information, inattention, and perhaps even self-commitment problems in hiring more police officers in anticipation of a crime wave, a prospect that is discussed in considerable detail in Chalfin and McCrary (2017a).[22]

Nevertheless, the scope for endogeneity to affect parameter estimates remains viable and, therefore, should be addressed. To deal with the potential endogeneity of investments in law enforcement, researchers generally turn to an instrumental variable—a variable that is correlated with the regressor of interest (law enforcement spending) but that is not related to the outcome of interest (in our case, new prison commitments) except through the potentially endogenous regressor. The idea behind such an identification strategy is intuitive: To the extent that the instrument captures some portion of the variation in law enforcement spending that is exogenous, this variable can be used to separate endogenous from exogenous variation to generate a consistent estimate of the effect of law enforcement spending on downstream incarceration rates. Instruments that have been used in this literature include mayoral and gubernatorial election cycles (Levitt, 2002), COPS hiring grants (Cook et al., 2017; Evans & Owens, 2007; Weisburst, 2016; Worrall & Kovandzic, 2007) and investments in fire safety (Levitt, 2002). Our mandate is to select an instrumental variable that predicts investments in law enforcement and that is ideally unrelated to prison admissions except through its correlation with investments in law enforcement.

A challenge is that this instrument must be valid (that is, it must predict law enforcement spending) as well as be relevant enough to generate a sufficiently precise estimate of the quantity of interest. This is easier said than done—even though scholars often point to an F-statistic of 10 (or 14 with respect to 10% maximal bias according to simulations by Stock and Yogo [2005]) as a sufficient rule of thumb for an unbiased second stage estimate, we note that this is not a sufficient statistic for a precise estimate of the second stage parameter. What we will need is an instrument that is not only predictive enough to generate a consistent estimate of the effect of police, but the instrument must also explain a sufficiently large share of the variation in police expenditures to be useful. This discussion is not merely academic—as noted by Kovandzic et al. (2016), identifying strong instruments has been a key challenge for this literature that has been plagued, to a large degree, by sizable standard errors and therefore by considerable parameter uncertainty (Chalfin & McCrary, 2017a).

As has been pointed out by McCrary (2002) and Kovandzic et al. (2016), mayoral and gubernatorial election cycles generate a weak first stage and therefore are not useful in studying the downstream effects of increases in police manpower. Our two remaining choices are to use police hiring grants or investments in fire safety. In keeping with recent scholarship, we obtained data on police hiring grants from the U.S. Department of Justice's COPS office. Although COPS grants predict agency-level hiring well, aggregating these data to the state level yields only limited predictive power and an

insufficient first stage relationship. Hence, we turn to investments in fire safety. Investments in fire safety, specifically the hiring of firefighters, was first proposed as an instrumental variable by Levitt (2002). Levitt noted that, for a variety of institutional reasons, police and firefighter hiring will tend to move together. In particular, it has been noted that the power of public-sector unions, citizen tastes for government services, and the predilection of politicians to "provide spoils" to their electorates suggest that police and fire safety budgets might co-vary. Empirically, we verify that this is the case at the state level. The instrument is both relevant and sufficiently strong to draw reasonable inferences about the relationship between police hiring and prison growth.

With respect to the exclusion restriction, the primary argument in favor of the validity of the identification strategy is simple: Firefighters do not have sworn arrest powers, and so they do not directly affect crime or subsequent prison admissions. Indeed, as firefighters do not go on routine patrols as do police officers, it is difficult to see how firefighters would have a first-order effect on crime. In selecting an identification strategy in which expenditures on fire safety are used, we are asserting that we believe this statement to be reasonable and, to first order, true, a prospect that has received support from Levitt (2002) and subsequently from Kovandzic et al. (2016), among others.[23] In support of this assertion, we note that, holding police spending constant, spending on firefighters is uncorrelated with new prison commitments and is uncorrelated with changes in a city's demographic composition and its crime rates.

We need not be completely blind to potential violations of the exclusion restriction, however. In particular, investments in police and fire safety could be part and parcel of a "safety first" approach to governing that also involves harsher sentencing or further investments in prison infrastructure. To the extent that this is true, the instrument will be positively correlated with prison admissions, other than through its correlation with police. Critically, though, this bias would be *in the direction of the least-squares estimate* and is therefore conservative given that the 2SLS estimates that we present later will point in the negative direction and will be larger in magnitude than their least-squares counterparts. Thus, in using these estimates to rule out a large positive relationship between law enforcement spending and prison growth, a critic would have to believe that spending on fire safety is *negatively* related to policy choices that lead to higher incarceration rates even though firefighter spending is positively related to police spending.[24] Ultimately, we note that OLS and 2SLS estimates are, in practice, similar, which indicates that, at the state level, police hiring may not be nearly as endogenous as has been supposed.

Using investments in fire safety as an instrumental variable motivates the following simultaneous equation framework, which is solved straightforwardly using 2SLS:

$$\text{POLICE}_{it-1} = \alpha + \eta \text{FIRE}_{it-1} + X'\beta + \phi_i + \gamma_t + \varepsilon_{it} \tag{5}$$

$$\text{COMMIT}_{it} = \alpha + \delta \text{FIRE}_{it-1} + X'\beta + \phi_i + \gamma_t + \varepsilon_{it} \tag{6}$$

Equation (5) is the first stage equation in which we regress police expenditures on fire expenditures, net of covariates and fixed effects—in practice, following Owens (2013), we use state and interacted region-year fixed effects. Equation (6) is the reduced form that provides an estimate of the effect of expenditures on fire safety on prison admissions. Dividing δ in Equation (6) by η in Equation (5) yields the 2SLS estimate of the effect of police spending on prison admissions. Models are weighted by the state population in 1997, the initial year in our data—however, results are not sensitive to the population weights. In all models, to account for arbitrary serial correlation in the residuals, standard errors are clustered at the state level (Bertrand, Duflo, & Mullainathan, 2004).[25]

3 | DATA

The goal of this research is to estimate the effect of investments in law enforcement on the number of new commitments to state prisons. To do so, we use data from six different sources and build a state-year panel data set that spans the time period from 1997 to 2015, a period in which police and prison populations increased at roughly equal rates. In this section, we provide detail on the source of our data and how our analytic data set is constructed.

3.1 | Prison admissions

The Bureau of Justice Statistics National Prisoner Statistics (NPS) Program provides an annual accounting of the number of new admissions to state prisons for each U.S. state. Data are available as far back as 1978. In addition to the total number of annual admissions, the NPS provides a detailed accounting of new admissions by the cause of their commitment. There are seven primary commitment causes: (1) new court commitments—those prisoners whose prison commitment is the result of a conviction for a new crime and subsequent court sentence, (2) parole violators who receive a new sentence, (3) parole violators who are returned to prison for a parole violation and without a new sentence, (4) other conditional release violators with a new sentence, (5) other conditional release violators without a new sentence, (6) transfers from other jurisdictions, and (7) returned escapees. In this research, we are interested in those inmates who are admitted to prison on a new sentence, meaning that the source of the admission to prison was a new crime that could have potentially been deterred by a police officer. This group is composed of those who are admitted to prison on a new court commitment, those who are admitted to prison on a parole violation with a new sentence, and those who are other conditional release violators with a new sentence.[26]

3.2 | State and local expenditures

State and local payroll for police officers and firefighters come from the Annual Survey of Public Employment and Payroll (ASPEP), which has been collected each year by the U.S. Census Bureau since 1957.[27] The ASPEP contains data on state and local government resource outlays by sector (e.g., police, fire safety, education, and sewerage) using information sourced from each jurisdiction's March payroll report.[28] This information includes the number of individuals employed in each jurisdiction-sector as of March 30th of each year as well as salary outlays that when multiplied by 12 is a reasonable approximation for annual salary expenditures. As a result of the high costs of data collection, the information is collected from a strategic sample of municipalities. The Census Bureau surveys large cities (> 250,000 population) annually and randomly samples smaller cities from year to year.[29] Given that our outcome (new prison commitments) is measured at the state level, we aggregate the March payroll information to the state level, adjusting for inflation. Although the aggregated series will contain some noise resulting from the sampling strategy, random sampling means that the series should not be biased and the fact that large cities are always represented in the data means that year-to-year noise is thankfully minimal. As we show, state-level information on police and fire expenditures are closely related despite any noise in the data.

We also use total government expenditure for payroll (for all sectors combined) to compute total government expenditures, which is a key control variable in our regression models.

3.3 | Crimes and arrests

Data on the number of crimes and arrests come from the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) Program, which collects monthly data from almost every law enforcement

agency in the United States. These are mostly municipal law enforcement agencies but also include county and state police organizations as well as special police forces (e.g., university police departments). The data contain information on the number of index crimes, by type, that is known to law enforcement and is the dominant source of national and local crime data in the United States.[30] The data also contain information on the number of arrests made by each law enforcement agency, which we use to study the relationship between the number of index crime arrests and new prison commitments.

Not all UCR agencies consistently submit their crime data as reporting to the UCR is voluntary. This feature of the data creates a potential problem for aggregating the agency-level data to the state level. To the extent that some agencies drop in and out of the data, state-level counts will be measured with error and possibly with errors that are nonrandom. We address this problem in two ways. First, we use state-level data compiled directly by the FBI that audits individual agencies and interpolates crime data when missing. These data have been widely used in the prior literature. Second, we reestimate our models including only agencies that report every year from 1997 to 2015. There are 6,793 agencies from 49 states that are consistent submitters for the Offenses Known and Clearances by Arrest data.[31] There are 6,820 agencies from 41 states that meet our consistent submitting criteria for the arrest data. These consistent agencies contain a significant portion of the U.S. population within their jurisdiction. Agencies from the ASR contain 74.6% of the U.S. population in 2015. Agencies from the Offenses Known and Clearances by Arrest contain 71.9% of the U.S. population in 2015. We note that estimates are substantively similar using both sources of state-level UCR data.[32]

3.4 | Other national data

Finally, we tabulate auxiliary demographic and economic data using the annual American Community Survey (2000–2015), and the 1990 and 2000 U.S. Census to control for demographic and economic differences between states and over time. The included demographics variables are the percentage of the population that are male, non-Hispanic Black, Hispanic, foreign born, and who are between 15 and 24 years old. To control for differences in education, we include the percentage of people 25 years or older who have less than a high-school diploma or equivalent. We use three measures of the state's economic well-being: the percentage of the population who are employed, the unemployment rate, and the percentage of people who are living in poverty. For years 2000 to 2015, we use data directly from the American Community Survey. The American Community Survey is not available for years prior to 2000. For years 1997 to 1999, we linearly interpolate yearly values based on the decennial censuses of 1990 and 2000.

State governor data were downloaded from the National Governors Association website and contain the political party for each governor and their years in office. We convert this data to a binary variable with a value of 1 if the governorship was held by Republicans that year and a 0 otherwise. In years when multiple parties held the governorship (i.e., when a new governor is inaugurated), we use the party that held the office latest in the year. As most inaugurations are early in the year, this measures which party held power during that year.

3.5 | Auxiliary data from California

As the National Prisoner Statistic data set contains only state-level prisoner admission counts, we use data from California's Prisoners and Parolees report to obtain counts of county-level prison admissions data within California. The report is produced annually by the California Department of Corrections and Rehabilitation and includes the number of new felons and parole violators who return to prison with a new sentence for each county in the state. As with the national data, we combine these two

categories into a single new prison admissions count. Data are available for 1997 through 2010 with the exception of 2002.[33]

4 | RESULTS

Before presenting our main findings, it is worth providing a descriptive examination of the data, which are summarized in Table 2. In this table, we report the mean, standard deviation, and minimum and maximum values of variables used in subsequent regression models. Given the panel structure of the data, we also report overall ("O"), within ("W"), and between ("B") state standard deviations to provide a sense for where the identifying variation comes from. Data on arrests are drawn from the pool of agencies that consistently report to the UCR, and consequently, these figures should not be construed as state-year totals. We limit our analytic sample to state-years for which there are no nonmissing data. In our data, an average state spends approximately $970 million in salaries annually on approximately 13,400 police officers for an average of $72,000 per officer. We also explore trends in several key variables—Part I crimes, arrests, sworn police officers, and new prison commitments—in Figure 1.

To provide a sense for the "productivity" of average officer, we appeal to national data from both the UCR and the Bureau of Justice Statistics. These data are summarized in Table 3. Using data for the 2014–2015 period, the United States employs approximately 800,000 sworn police officers for a population of 320 million people, which is a rate of 250 officers per 100,000 residents. Each year there are approximately 11 million arrests in the United States of which slightly more than 2 million are for index crimes and another 1.7 million are for drug or weapons violations. The remaining 7 million arrests are generally for minor offenses that are unlikely to lead to a custodial prison sentence. It is easy to see that this is true when considering the annual number of new prison commitments, which number approximately 600,000.

The descriptive data suggest three main points. First, even though there is likely to be a great deal of heterogeneity among police officers depending on their assigned duties, an "average" officer makes few arrests for serious crimes during the course of a year—by our calculations, an average officer will make approximately 13.8 arrests, of which 2.6 are for index crimes and another 2.1 are for drug or weapons violations. Hence, although police officers may do a great deal to reduce crime by doing things like maintaining visibility, conducting street stops, and undertaking preventive patrol, a typical officer makes few "serious" arrests, perhaps around 4 or 5 per year. Second, arrests, even index crime arrests, are unlikely to lead to a prison spell. Nationally, the probability that an arrest will lead to a prison spell is slightly more than 5%. Even when we consider arrests for crimes that potentially carry long sentences such as index crimes and drug and weapons violations, there are six of these arrests each year for every new prison spell. Finally, putting all of the data together, given that there are 800,000 sworn officers and 600,000 annual prison spells in the United States, an average officer incapacitates fewer than one individual per year in state prison. Thus, unless this individual is highly productive and would have committed a very large number of crimes had he or she not been incarcerated, the scope for officers to generate sizable incapacitation effects may be smaller than is sometimes supposed. This is underscored by the results of recent research that indicate that, in recent years, the marginal offender is, in fact, unlikely to be all that productive (Cullen, Johnson, & Nagin, 2011; Durlauf & Nagin, 2011; Johnson & Raphael, 2012; Liedka et al., 2006; Nagin, 2013; Raphael et al., 2017).

The main results are contained in Table 4 and discussed here as follows. In Table 4, for each dependent variable—crimes, arrests, expected new prison spells (defined later), and actual new court commitments to prison—we present estimates of the effect of an increase in $10,000 of spending on law enforcement personnel. We present least-squares estimates of the effect of law enforcement spending

TABLE 2 Summary statistics

Variable		Mean	SD	Min.	Max.
Annual Spending for Police Payroll (in $10,000)	Overall (O)	96,633.6	143,307.7	4,737.76	801,001.1
	Between (B)		141,853.1		
	Within (W)		19,255.1		
Annual Spending for Firefighter Payroll (in $10,000)	O	41,815.0	60,043.5	1,046.3	393,389.5
	B		59,246.1		
	W		9,222.3		
New Court Commitment Prison Admissions	O	8,958.6	11,609.2	419.0	69,390.0
	B		11,344.7		
	W		2,464.2		
Crimes	O	170,303.7	239,446.2	9,113.0	1,527,750.0
	B		237,516.2		
	W		30,340.2		
Arrests	O	38048.5	52,622.2	1,191.0	394,500.0
	B		52,175.0		
	W		6,846.4		
Expected Prison Admissions	O	22,694.0	34,785.4	801	251,059.7
	B		34,520.4		
	W	4,285.9			
Total Budget (in Millions)	O	1,461.5	1,858.1	127.2	11,048.4
	B		1,846.1		
	W		211.1		
Population (in Millions)	O	6.1	6.7	0.5	39.1
	B		6.7		
	W		0.6		
% Hispanic	O	10.0	10.2	.5	48.1
	B		10.0		
	W		1.7		
% Aged 15–24	O	14.0	1.1	11.0	20.2
	B		0.9		
	W		0.6		
% Non-Hispanic Black	O	11.3	10.1	.2	39.0
	B		10.1		
	W		0.7		
% Male	O	49.2	.8	47.0	52.0
	B		0.7		
	W		0.3		

(Continues)

T A B L E 2 (Continued)

Variable		Mean	SD	Min.	Max.
% Employed	O	61.1	4.1	49.3	70.7
	B		3.6		
	W		1.8		
Unemployment Rate	O	6.8	2.0	2.3	14.9
	B		1.3		
	W		1.6		
% Foreign Born	O	8.4	6.0	.8	27.4
	B		5.9		
	W		1.0		
% No High-School Degree	O	13.0	5.0	4.2	26.5
	B		3.0		
	W		3.9		
Republican Governor	O	0.6	.5	0	1
	B		0.3		
	W		0.4		

Notes. Max. = maximum; Min. = minimum; SD = standard deviation.

(a) Crimes − Part 1

(b) Total Arrests − Part I + Drug Sales

(c) Spending on Officers
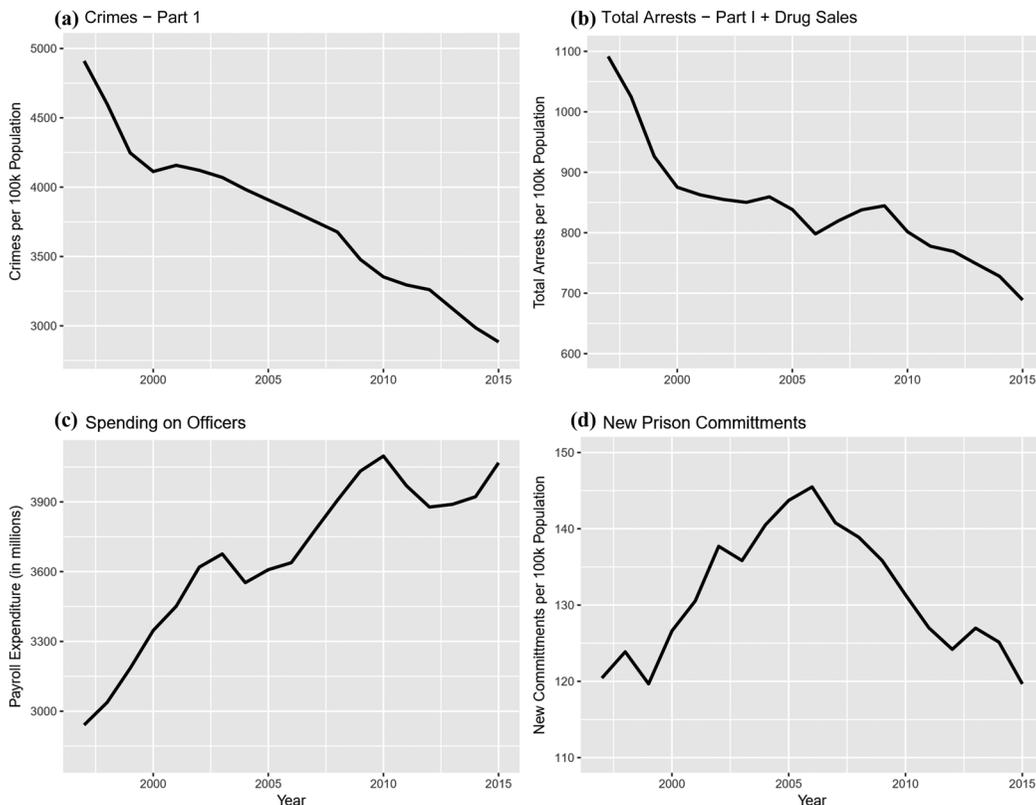
(d) New Prison Committments

F I G U R E 1 Time trends in key variables

Notes. Crime in Panel A uses data from ucrdatatool.gov. Panels A and B do not include arson. Panel D only includes prisoners that are committed to a state prison.

TABLE 3 U.S. criminal justice system

U.S. Population	320,137,939
# of Sworn Officers	774,740
# of Arrests	11,001,461
# of Total Index Crime Arrests	2,010,771
# of Violent Index Arrests	502,174
# of Property Index Arrests	1,508,597
# of Drug Arrests	1,524,969
# of Prison Commitments	572,292

Notes. All data in this table are for the years of 2014–2015. U.S. population comes from the American Community Survey. The number of sworn officers comes from the Uniform Crime Reporting (UCR) Program Law Enforcement Officers Killed and Assaulted (LEOKA) data sets. All arrest data come from the UCR's Arrests by Age, Sex, and Race data sets. Prison Commitment data are from the National Prisoner Statistics 1978–2015 data set produced by the Bureau of Justice Statistics.

on key outcomes, as well as present 2SLS models in which spending on law enforcement is instrumented using spending on firefighters. We also provide robust standard errors, clustered at the state level, and the resulting 95% confidence interval.[34] All models are estimated using 722 observations, covering 41 states over the 1997–2015 sample period.

Throughout the discussion of results, we interpret the 2SLS estimates rather than the least-squares estimates. We note that these coefficients, however, are close in magnitude to the least-squares estimates. This is consistent with the idea that, as a result of political and operational constraints, police hiring, in practice, is less endogenous than has been supposed and that measurement errors in police data are less severe at the state rather than at the local level (Chalfin and McCrary, 2017b).

4.1 | First stage

We begin by documenting the first-stage relationship between expenditures on police personnel and expenditures on firefighters at the state level. Although Levitt (2002) showed that this relationship is strong at the city level, this relationship need not automatically hold at the state level. Visually, the relationship between police and firefighter spending is presented in Figure 2. In the scatterplot, both police and firefighter spending are regressed on state- and region-year fixed effects as well as covariates and the residuals are taken and plotted against one another. A large percentage of the variation in police spending is explained by spending on fire safety. Conditional on control variables (including a state's total budget) and state and interacted region-year fixed effects, each $10,000 spent on fire safety predicts approximately $16,000 in spending on law enforcement. This estimate corresponds with η in Equation (5). The t-ratio on the η is 3.7, and hence, the first stage F-statistic is 13.7, which compares favorably to the Stock–Yogo threshold for the effective sample size of an instrumental variables model with a single instrument and a single endogenous regressor (Stock & Yogo, 2005).[35]

4.2 | Does an increase in police spending lead to less crime?

Any investigation of the effect of law enforcement spending on new prison admissions must necessarily begin by examining the effect of police spending on crime. Given that the effect of police on arrests is expected to be positive, holding crime constant, for the effect of police spending on new prison commitments to be negative, the effect of police spending on crime should also be negative.

Although similar models have been estimated before on similar data, for completeness, we estimate the effect of police expenditures on crime via the 2SLS framework laid out in Equations (5)

TABLE 4 Main results: National data

Variables	Index Crimes	Index Crime Arrests	Expected Prison Spells	Actual Prison Spells
Panel A: OLS Results				
$\hat{\beta}$	−0.598	−0.116	−0.090	−0.034
Se($\hat{\beta}$)	0.189	0.072	0.053	0.013
[CI]	[−0.979, −0.217]	[−0.262, 0.030]	[−0.198, 0.018]	[−0.060, −0.008]
N	722	722	722	722
Marginal Effects per Officer	−7.8	−1.2	−1.0	−0.4
[CI]	[−12.73, −2.82]	[−3.41, 0.39]	[−2.67, 0.23]	[−0.78, −0.10]
Panel B: 2SLS Results				
$\hat{\beta}$	−0.555	−0.100	−0.073	−0.010
Se($\hat{\beta}$)	0.210	0.082	0.053	0.019
[CI]	[−0.996, −0.144]	[−0.262, 0.061]	[−0.178, 0.031]	[−0.048, 0.028]
N	722	722	722	722
Marginal Effects per Officer	−7.2	−1.3	−0.95	−0.13
[CI]	[−12.95, −1.87]	[−3.41, 0.79]	[−2.31, 0.40]	[−0.62, 0.36]

Notes. For each set of models, the following covariates are included: % population that is aged 15–24, % employed, % foreign born, % Hispanic, % male, population, % Non-Hispanic Black, the unemployment rate, % in poverty, % without a high-school diploma or equivalent, total budget, and an indicator if the governor is Republican. All models are estimated using 1997 population weights.
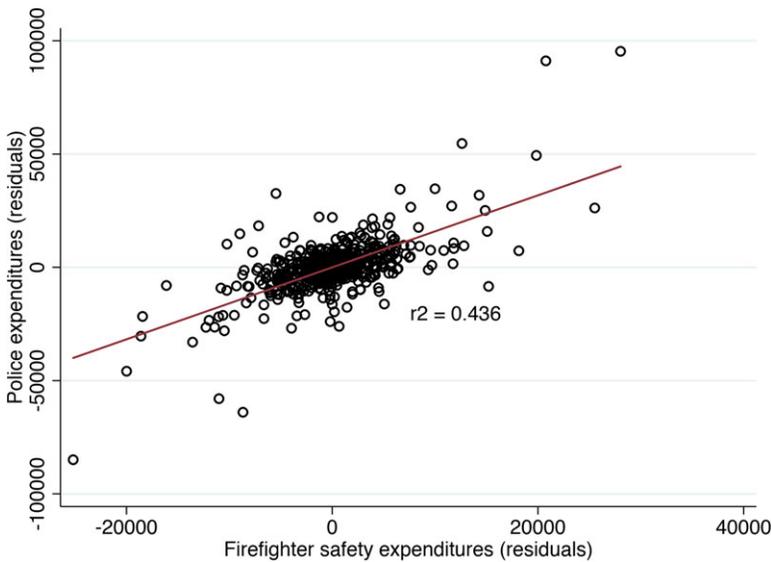
FIGURE 2   First stage relationship between police and firefighter expenditures [Color figure can be viewed at wileyonlinelibrary.com]

and (6). We find that each $10,000 allocated to law enforcement abates approximately 0.6 index crimes ($p < 0.01$).[36] Given that the fully loaded cost of a sworn officer is approximately $130,000 (Chalfin & McCrary, 2017a), each officer hired, through either deterrence or incapacitation, is estimated to abate approximately seven index crimes.[37] Of these, approximately one will be a violent index crime and the remaining six will be property crimes. To verify that these estimates are consistent with those reported in the extant literature, consider that an average U.S. state employs approximately 13,400 sworn officers during the time period we study and the number of index crimes per state is approximately 226,000. According to our estimates, a 10% increase in police strength (1,340 additional officers) would abate approximately 9,600 index crimes. This amounts to a 5.6% reduction from a level of 170,000. These estimates reveal a crime police elasticity of –0.56, which is broadly consistent with the estimates reported in the prior literature.[38]

4.3 | Does an increase in police spending lead to more arrests?

We next consider whether more police spending leads to more arrests as is expected by many to be the case—although in her examination of agency-level data, Owens (2013) found that, if anything, arrests may decline as a function of additional police hiring even though the estimates she reported were not significant at conventional levels. Consistent with Owens (2013), in our data, we find that each $10,000 allocated to law enforcement leads to –0.1 fewer arrests (for either an index crime or a drug crime) although the estimate is imprecise—the 95% confidence interval ranges from a low of –0.26 to a high of 0.06. On a per officer basis, our best guess is that each officer hired leads to 1.3 *fewer* arrests although it is possible that arrests per additional officer could increase arrests by as many as 0.7.

Of course, many—even most—arrests are for crimes that are unlikely to result in a prison sentence. Given that we are interested in the effect of police spending on prison admissions, perhaps a better question to ask is whether arrests that are likely to yield a new prison commitment change as a function of investments in law enforcement. To answer this question, we construct an estimate of the number

of *expected prison commitments* by multiplying the number of each type of arrest (e.g., robbery and burglary) by the probability that an arrest of a given type leads to a subsequent prison spell. Using a national estimate of the conditional probability of prison given a conviction from the State Court Processing Statistics, we compute expected new commitments (EXP_COMMIT_{it}) as:

$$EXP\_COMMIT_{it} = \Sigma^k_{\ i} = 1 P_k \times ARRESTS_{itk} \tag{7}$$

where P_k is the probability of a prison sentence conditional on a conviction for crime k and $ARRESTS_{itk}$ is a state-, crime-, and year-specific estimate of the number of arrests.[39] This is a more reasonable guess as to how changes in law enforcement resources will translate into new prison commitments. We find that each \$10,000 allocated to law enforcement abates 0.073 expected prison commitments (95% CI: –0.178, 0.031). While the result is not precise at $\alpha = 0.05$, 80 percent of the confidence interval lies in the negative direction. Even if we take the upper bound of the confidence interval at face value, our best guess is that each officer hired would lead to an expected increase in new prison commitments of approximately 0.4, meaning that it would take approximately three new hires to generate one additional prison commitment. Given that an average police officer in our data generates an expected 0.7 prison commitments per year, this amounts to a 6% increase. Thus, even though there is a good chance that greater police expenditures lead to a decline in expected prison commitments, even an extreme estimate of the effect of police on expected commitments leads to only a modest increase in new commitments.

4.4 | Do more arrests lead to more new prison spells?

We next ask whether more arrests necessarily lead to more prison commitments. Although we might expect that this relationship would be positive, for several reasons, the relationship may be far from perfect. In particular, not all arrests lead to a criminal conviction, and with the exception of crimes like murder and rape, the majority of convictions, even for index crimes, do not result in a prison sentence. To get a sense for how closely related these two quantities are, we regress new court commitments on arrests net of covariates and state- and region-year fixed effects. When we do so, the coefficient on the number of arrests is 0.25 ($p < 0.01$), indicating that the conversion rate between an arrest and a prison spell is approximately 30%. This is sensible as the conditional probability of a prison sentence given a conviction is between one third and one half for the most common felony crimes. When we regress new court commitments on *expected* prison commitments, the coefficient increases to 0.41 ($p < 0.01$). Thus, even when we adjust for the conditional probability of a sentence, there is still substantial slippage between arrests and actual new court commitments.

4.5 | Does an increase in police spending lead to more prison spells?

Finally, we assess the empirical relationship between police spending and actual new commitments to prison. This relationship is shown visually in Figure 3. We find that each \$10,000 allocated to law enforcement leads to –0.01 fewer expected prison commitments (95% CI: –0.048, 0.028). The coefficient is negative and accords with our prediction using expected prison commitments but is less precisely estimated. The precision of our estimates is not ideal, and so we pause here to consider the practical importance of these findings. Taking the point estimate (–0.01) at face value, we see that hiring a new officer will lead to –0.13 fewer new prison commitments, which is a 19% reduction in commitments per officer. In elasticity terms, this corresponds with an elasticity of approximately –0.02. Later, we will consider what these estimates mean for a state's incarcerated population and conclude that, even using the upper bound of the confidence interval, increases in police hiring are unlikely to
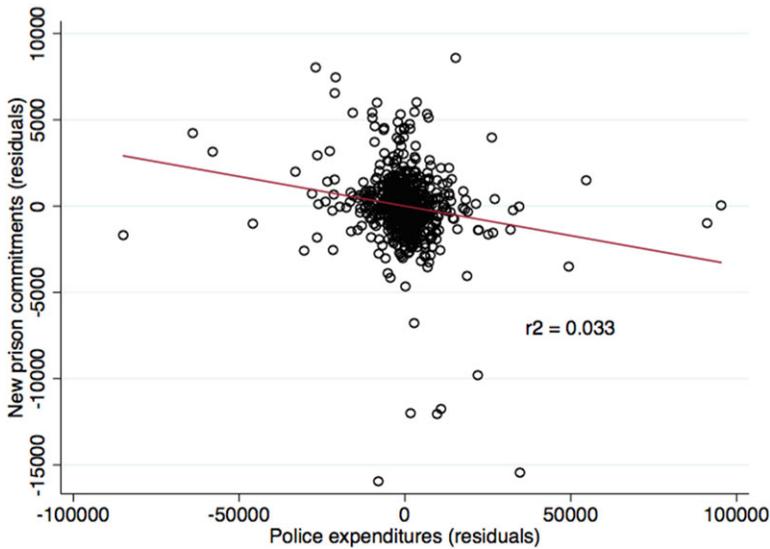
FIGURE 3   Relationship between new prison commitments and police expenditures [Color figure can be viewed at wileyonlinelibrary.com]

lead to a steady state of "mass incarceration." Before exploring this further, we present evidence in favor of the robustness of these findings to alternative specifications.

4.6 | Robustness

In this section, we consider the robustness of the results presented in the previous subsection. In Table 5, we repeat the analysis presented in Table 4 using county-level data for California. That is, instead of estimating the effect of state-level spending on law enforcement on state-level prison spells, we estimate the effect of county-level law enforcement spending on the number of new commitments to California prisons from each county, net of county and year fixed effects. This exercise yields estimates that are remarkably similar to those generated from the national data and provide us with confidence that estimates are not an artifact of the level of aggregation of the data. Since the California data were obtained after our preferred specification was chosen for the national analysis, this is also a crucial check against specification searching on the part of the authors.

Table 6 demonstrates the robustness of results to a set of alternative choices in analyzing the national data. The first two columns report coefficients and robust standard errors from a 2SLS regression of expected prison commitments and new prison commitments—these estimates were reported earlier. Although the estimates differ in magnitude, both are negative. The estimated effect of police spending on expected prison commitments is unlikely to be positive. In column 3, we aggregate the data into 2-year bins to see whether models suffer from temporal aggregation bias, the idea being that arrests and prison spells may not occur in the same year. The point estimate is similar in magnitude to the estimate in column 2. In column 4, we reestimate the model in column 2 without the full set of controls—controlling for fixed effects, population, and the total state spending only. Finally, in columns 5 and 6, we examine whether point estimates change substantially when we change the way that new prison commitments are defined. In column 5, we estimate the effect of police on all new prison admissions rather than simply on new court commitments and parole violators with a new sentence. In column

TABLE 5 Main results: California data

Variables	Index Crimes	Index Crime Arrests	Expected Prison Spells	Actual Prison Spells
Panel A: OLS Results				
$\hat{\beta}$	−0.252	0.003	−0.025	−0.007
Se($\hat{\beta}$)	0.208	0.018	0.020	0.004
[CI]	[−0.675, 0.171]	[−0.033, 0.039]	[−0.065, 0.015]	[−0.016, 0.001]
N	352	352	352	352
Marginal Effects per Officer	−3.3	0.0	−0.3	−0.1
[CI]	[−8.78, 2.22]	[−0.43, 0.51]	[−0.85, 0.20]	[−0.21, 0.01]
Panel B: 2SLS Results				
$\hat{\beta}$	−0.260	−0.133	−0.091	−0.038
Se($\hat{\beta}$)	0.163	0.101	0.071	0.031
[CI]	[−0.581, 0.060]	[−0.331, 0.065]	[−0.229, 0.048]	[−0.098, 0.023]
N	352	352	352	352
Marginal Effects per Officer	−3.4	−1.7	−1.2	−0.5
[CI]	[−7.56, 0.78]	[−4.30, 0.85]	[−2.98, 0.62]	[−1.27, 0.30]

Notes. For each set of models, the following covariates are included: % population that is aged 15–24, % employed, % foreign born, % Hispanic, % male, population, % Non-Hispanic Black, the unemployment rate, % in poverty, % without a high-school diploma or equivalent, total budget, and an indicator if the governor is Republican. All models are estimated using 1997 population weights.

TABLE 6 Robustness checks

| Variables | Expected Prison Spells | Actual Prison Spells | | Population and Budget Controls Only | All Prison Admissions | New Sentences Only |
		One-Year Bins	Two-Year Bins			
$\hat{\beta}$	−0.073	−0.010	−0.005	−0.017	−0.028	−0.016
Se($\hat{\beta}$)	0.053	0.019	0.003	0.019	0.052	0.014
[CI]	[−0.178, 0.031]	[−0.048, 0.028]	[−0.012, 0.002]	[−0.055, 0.020]	[−0.130, 0.073]	[−0.044, 0.012]
N	722	722	403	722	722	722

Notes. For each set of models, the following covariates are included: % population that is aged 15–24, % employed, % foreign born, % Hispanic, % male, population, % Non–Hispanic Black, the unemployment rate, % in poverty, % without a high school diploma or equivalent, total budget, and an indicator if the governor is Republican. All models are estimated using 1997 population weights.

6, we reestimate this model for those with new court sentences only. In all cases, point estimates are negative and are similar in magnitude to the estimate reported earlier.

5 | DISCUSSION

Although 2SLS estimates are slightly imprecise, we argue that most of the available evidence indicates that investments in law enforcement do not result in appreciably larger correctional populations and might even lead to reductions in the number of state prisoners in the United States. Taking the 2SLS point estimates at face value, hiring one additional police officer is estimated to reduce the number of expected prison spells by 0.9 and the number of actual new prison commitments by 0.13. We further note that (a) the OLS estimates are similar to 2SLS estimates and are more precisely estimated, providing additional support for the hypothesis that investments in police do not lead to more incarceration; and (b) the estimated effect of police resources on *expected* prison commitments, which is also negative, provides additional foundation for our main result.

What does this mean for prison populations in practical terms? If an average state were to increase the size of its police force by 10%, thus hiring approximately an additional 1,340 officers, we would expect that this state would commit 174 fewer criminal defendants to state prison in the subsequent year. This represents a 2% decrease in the flow of inmates to state prisons in a typical U.S. state. If instead we were to take the lower and upper bounds of the confidence interval at face value, then we estimate that a 10% increase in police manpower would lead to between a 9.3% reduction and a 5.4% increase in the number of new prison commitments. A 10% increase in police manpower is very large and, at the state level, is unprecedented. Thus, we also note that a more realistic increase in law enforcement resources of 2% would yield a change in the number of new prison commitments that is between –1.9% and +1.1%. Of course, any change in the flow will affect the stock of prisoners more slowly and will only change the prison population by these magnitudes in steady state.

During the last three decades, growth in the number of state prisoners in the United States has quadrupled, whereas growth in the number of sworn law enforcement officers has increased by approximately 50%. Even taking the upper limit of the confidence interval at face value, growth in police manpower would explain less than 10% of the overall increase in the number of state prisoners. Indeed, the data indicate that growth in law enforcement has likely been a moderating factor with respect to the growth of prisons and, as other results have shown, that changes in punitiveness along both the intensive and the extensive margin explain the lion's share of incarceration growth (Raphael & Stoll, 2009). We further suggest that federally induced increases in police hiring during the 1990s have played, at most, a modest role in fueling the growth rate in incarceration during the last 15 years and may have even been a moderating factor.

These findings create room for optimism that society might be able to achieve a "double dividend" by reallocating criminal justice dollars toward law enforcement—that it is certainly feasible that investments in police can drive down both crime and incarceration rates. Although this possibility has been noted, for some time, by prominent practitioners, and although scholarly research has hinted that this double dividend might be possible, this is the first research to be aimed at addressing its likelihood directly. That said, we hope that this will not be the last word on this topic as there is more work to be done. The effect of police on the size of the prison population will naturally depend on how police are used—whether new officers will be used in a way that will generate more arrests (or more productive arrests) and how state courts process defendants. Although we leverage

national data to generate a national estimate, it stands to reason that we have ignored important contextual details and that these details will matter in assessing the degree to which a new wave of police hiring will affect downstream incarceration rates. Likewise, even though strong instruments are difficult to find, we are hopeful that future research can generate findings with greater precision around these point estimates. With smaller confidence intervals, it may be possible to draw a more concrete inference about the sign of relationship between law enforcement spending and the use of prison.

Finally, we note that although it is unlikely that investments in law enforcement appreciably increase and may even decrease state prison populations, this is not the only way that an increase in the intensity of policing affects American communities. To the extent that more police surveillance leads to an increase in stop or search rates, this may serve to fray the already tenuous relationship between police officers and many disadvantaged communities. Likewise, it may be the case that police hiring does, in fact, increase the size of local jail populations and the extent to which the net has been widened for low-level offenders who have become "frequent fliers" in jail settings.[40] As most individuals who find themselves under correctional supervision in the United States are either in jail or are supervised in the community, the findings from this research cannot tell us about that margin of the corrections–law-enforcement relationship. Nevertheless, given that prison is the most socially and financially costly of available sanctions, it stands to reason that a strategy that potentially leads to less prison and less crime should, at least, merit further discussion.

ENDNOTES

[1] There is also a rich amount of literature on perceptual deterrence—the extent to which potential offenders are well informed about the risks of offending—with notable contributions including Chiricos and Waldo (1970); Paternoster, Saltzman, Waldo, and Chiricos (1983); and Loughran, Paternoster, Piquero, and Pogarsky (2011). See Apel (2013) for a detailed review of this literature.

[2] In a large amount of literature on "criminal careers," scholars have provided evidence on the size of incapacitation effects by seeking to understand the general productivity level of offenders (Blumstein, Cohen, Roth, & Visher, 1986; D'unger, Land, McCall, & Nagin, 1998; Hirschi & Gottfredson, 1986; Nagin & Land, 1993; Visher & Roth, 1986).

[3] Crime is found to be responsive to redeployments of police after a terror attack (Di Tella & Schargrodsky, 2004; Draca, Machin, & Witt, 2011; Klick & Tabarrok, 2005) or a traffic collision (Weisburd, 2016). Crime has likewise been found to decline in the presence of targeted police surges (MacDonald, Fagan, & Geller, 2016) and to increase when police disengage from the community (Shi, 2009). Quasi-experimental findings are bolstered by numerous experimental studies on deployments of police to crime hot spots (Blattman, Green, Ortega, & Tobón, 2017; Braga & Bond, 2008; Braga et al., 1999; Sherman & Weisburd, 1995; Weisburd, Groff, & Yang, 2012).

[4] An exception in the literature is Kovandzic and Sloan (2002), who found little evidence of an impact of police hiring on crime. See Eck and Maguire (2006), Sherman (2011) and Lee, Eck, and Corsaro (2016) for excellent and detailed reviews of the literature.

[5] This conversation has gone hand in hand with a scholarly and public debate on disproportionate police surveillance of low-income and disadvantaged predominantly minority communities and has generated a great deal of recent scholarly attention with respect to the consequences of aggressive "stop, question, and frisk" policies (Corman & Mocan, 2005; Goel, Rao, & Shroff, 2016; Harcourt & Ludwig, 2006; MacDonald et al., 2016) and whether or not there is a "Ferguson effect" in American policing (Pyrooz, Decker, Wolfe, & Shjarback, 2016; Rosenfeld, 2016; Shjarback, Pyrooz, Wolfe, & Decker, 2017).

[6] Naturally, the mechanisms through which police reduce crime will depend on how they are used. In this article, we consider the impact of resources allocated to law enforcement, holding fixed the mixture of policing strategies and tactics that individual departments employ.

[7] See http://www.nytimes.com/2013/01/26/nyregion/police-have-done-more-than-prisons-to-cut-crime-in-new-york.html

[8] As Levitt (2002) noted, the intuition for this relationship is straightforward—institutional details such as the power of public-sector unions, citizen tastes for government services, and the predilection of politicians to "provide spoils" to their electorates reveal that police and fire safety budgets might co-vary.

[9] Our identification strategy, in which we use variation in police spending that is explained by spending on fire safety, is valid so long as expenditures on fire safety aggregated to the state level are uncorrelated with new prison admissions, except through police hiring. To the extent that this empirical strategy, like all identification strategies, is imperfect, we argue that this source of identification is sufficient to support the findings in this article because any remaining bias is likely to be positive—that is, toward least-squares estimates—if investments in fire safety are positively correlated with broader investments in public safety. Because our estimates of the effect of police spending on prison admissions point in the negative direction, the fire safety instrument would thus provide an upper bound on this critical policy estimand. We argue that an upper bound is particularly useful in this context.

[10] For simplicity, we assume that each crime has a single perpetrator.

[11] According to the State Court Processing Statistics collected by the U.S. Bureau of Justice Statistics, approximately 55% of individuals convicted of a violent crime spend time in prison. The number for property crimes is 38%. Conviction rates are approximately 50% for violent crimes and 57% for property crimes, respectively. Therefore, the share of index arrestees who spend time in prison is approximately 25%. See bjs.gov/content/pub/pdf/fdluc06.pdf.

[12] In principle, the society could also allocate more overtime hours to the existing officers. We focus on the number of officers hired for simplicity.

[13] Technically, the break-even point is achieved when $|\epsilon_p|$ is slightly larger than $|\epsilon_y|$.

[14] The largest estimates we can find of the effect of police response times on clearance rates comes from a recent paper by Vidal and Kirchmaier (2017), who reported an elasticity of –0.47.

[15] In practice, we follow Owens (2013) and use interacted region-by-year fixed effects that account for time-varying shocks to new prison commitments at the region level.

[16] We note that aggregation at the state level is a common choice in the applied literature, including in several classic papers, both as a means of studying state-level interventions or outcomes and as a way to deal with measurement errors at lower levels of aggregation (Bohn, Lofstrom, & Raphael, 2014; Chiricos, 1987; Donohue, Aneja, & Weber, 2017; Donohue & Levitt, 2001; Donohue & Wolfers, 2006; Duggan, 2001; Levitt, 1996, 1998; Lofstrom, Raphael, & Grattet, 2014; Marvell & Moody, 1996; Raphael & Winter-Ebmer, 2001).

[17] A second type of aggregation bias worth mentioning is temporal aggregation. Here the challenge is that there is a temporal lag between the time when an arrest is made by a police officer and a subsequent prison admission—if the arrest, in fact, results in one. We deal with this by specifying that police affect prison admissions with a 1-year lag. To ensure the robustness of estimates, however, we further aggregate the data up into 2-year bins. Our results indicate that, if anything, our primary estimates are conservative.

[18] See Cope (1992), Lewis (1994), and Rubin (2016) for a detailed discussion of this point.

[19] See Joyce and Mullins (1991), Poterba and Rueben (1995), Shadbegian (1998), and Shavell (1991).

[20] See, for example, LA Times (1966), Ireton (1976), or Recktenwald (1986a, 1986b). A common pattern is for police departments to have hired a large cohort of officers at some point. For some cities, this was after World War II; for other cities, it was the late 1950s; and for other cities, it was the 1960s crime wave. Combined with typical pension plans pegged to 20 years or 25 years of service, many departments face retirement waves approximately two decades after a hiring wave, setting the stage for a 20- to 25-year cycle unless the city exercises foresight. For example, in response to the famous Boston Police Strike of 1919, in which nearly three quarters of the police department went on strike on September 9, then-governor Calvin Coolidge, having assumed control of the department on an emergency basis, refused to allow the strikers to return to work and replaced them all with veterans from World War I (Boston Police Department, 1919; Russell, 2005). This hiring burst, combined with the State-Boston Retirement System, which provides for a defined benefit pension after 10 years if older than 55 and after 20 years if of any age, led to a highly persistent "lumpiness" in the tenure distribution of the department (Boston Police Department, 1940: Table VI).

[21] Relevant federal programs over this time period include the Law Enforcement Assistance Administration (1968–1982), the Edward Byrne Memorial State and Local Law Enforcement Assistance programs (1988–2006), the Local Law Enforcement Block Grant program (1996--2006), the Justice Assistance Grant (2006–present), and the Community

Oriented Policing Services (1994–present). For background on federal programs, see Hevesi (2005), Richman (2006), Varon (1974), and James (2013). At its peak in the late 1970s, LEAA funding accounted for approximately 5% of state and local criminal justice expenditures (Advisory Commission on Intergovernmental Relations, 1977). Background on state programs, which are ubiquitous, is much more scarce, but see Richardson (1980).

[22] Chalfin and McCrary (2017a) demonstrated that differences between prior OLS and two-stage least-squares (2SLS) estimates in the literature in which the effect of police hiring on crime is studied differs mainly because of measurement errors in police manpower data in the FBI's *Uniform Crime Reports*, rather than as a result of endogeneity bias, as has been supposed. We note here that we are aggregating agency-level data to the state level, and as such, measurement error bias should be a far less concerning issue in our data as the resulting errors are likely to smooth out in the aggregation. Thus, in the absence of considerable endogeneity bias, OLS and 2SLS estimates should be more similar to each other than in the city-level analyses in the extant literature.

[23] Our reading of Kovandzic et al. (2016) is that they questioned the strength of the first stage in Levitt's (2002) original analytic sample but not the underlying validity of the identification strategy.

[24] Another concern is that a third factor might affect both expenditures on police and firefighters as well as prisons— for example, a recession. To account for this possibility, we control extensively for changes in the local economy and, critically, for the size of a state's total budget and its spending on corrections. Thus, we estimate the first stage regression of police spending on firefighter spending, holding other state budgets constant.

[25] We further estimate standard errors using the cluster-bootstrap. Estimates (not reported) are extremely similar to the traditional estimates.

[26] We also estimate the effect of law enforcement spending on all new prison admissions as a robustness check, and all results are very similar.

[27] Since then, the sole year in which the ASPEP was not conducted is 1996.

[28] Prior to 1996, the ASPEP collected information from the October payroll.

[29] Twice per decade (in years ending in a "2" or a "7"), information is collected from all cities.

[30] Index crimes include murder and non-negligent manslaughter, rape, robbery, aggravated assault, burglary, larceny (greater than $50), and motor vehicle thefts. More detail on UCR data can be found in the FBI's *Uniform Crime Reporting Handbook*.

[31] We include only agencies that report all 12 months of the year. As a result of inconsistent reporting by the Chicago Police Department (e.g., no data on rape), we exclude Illinois.

[32] For both crime and arrests, we use the monthly data provided by NACJD and aggregate annually. The crime data include a variable indicating how many months the agency reported for that year; the arrest data do not. We include only agencies that report 12 months of the year for every year we studied. As the arrest data do not contain such a variable, our inclusion criterion is only that the agency reports every year studied. This leads to arrest data being noisier than crime data because some agencies may change their reporting practices over time. On any given year, agencies that report crime data represent a greater proportion of the population than do agencies reporting arrest data. The stricter criteria for crime data, however, lead to some crime agencies being excluded and to results in a similar percentage of the population being covered by each data set. In both cases—crime data and arrest data—we are covering approximately 75% of the population of the United States.

[33] All other data come from the same sources as used in the national analysis with data aggregated to the county level rather than to the state level. The American Community Survey began providing county-level indicators in 2005 and limits these indicators to counties with a population of at least 100,000. We linearly interpolate data for the years 1997– 2004 using the decennial 1990 and 2000 censuses as well as the 2012 5-year American Community Survey data. With this population restriction as well as one county not being present in ASPEP data, of California's 58 counties, 33 remain in our data.

[34] Standard errors estimated using the cluster-bootsrap are extremely similar.

[35] The Stock–Yogo critical value for 10 percent maximal IV size is 14.6.

[36] These computations use state-level data generated by the FBI in which agency crime counts are imputed in years in which an agency does not report to the *Uniform Crime Reports*. When we estimate the effect of police spending on crime using only the subsample of consistent reporters, we obtain an estimate that is very similar.

[37] In a working paper version, Chalfin and McCrary (2017a) provide a detailed discussion of officer cost calculations.

[38] For instance, this is close to the estimate obtained by Chalfin and McCrary (2017a) as well as to the estimate by Evans and Owens (2007), in two of the most recent papers in which a national effect of police on crime is estimated.

[39] Consistent with Table 1, we use the following conditional probabilities to construct an estimate: murder (0.93), rape (0.72), robbery (0.71), aggravated assault (0.43), burglary (0.49), larceny (0.34), motor vehicle theft (0.42), drug possession (0.33), and drug sale (0.41). These data come from the Bureau of Justice Statistics' Felony Sentences in State Courts 2006 report, the last year a report is available.

[40] Indeed, the results of an analysis of California jail admissions data (not reported) indicate that spending on police is more likely to increase jail admissions than prison admissions.

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