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Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime

By STEVEN D. LEVITT*

Previous empirical studies have uncovered little evidence that police reduce crime, possibly due to simultaneity problems. This paper uses the timing of mayoral and gubernatorial elections as an instrumental variable to identify a causal effect of police on crime. Increases in the size of police forces are shown to be disproportionately concentrated in mayoral and gubernatorial election years. Increases in police are shown to substantially reduce violent crime, but have a smaller impact on property crime. The null hypothesis that the marginal social benefit of reduced crime equals the costs of hiring additional police cannot be rejected. (*JEL* K42, D72)

Crime is a major social and economic issue in the United States. The cost of crime to victims is estimated at approximately $200 billion per year (Ted Miller et al., 1993). The indirect costs of crime also are substantial. Government outlays on the criminal justice system totaled $74 billion in 1990, including $32 billion on police protection. Private expenditures on self-protection are at least the same order of magnitude.

Following the seminal contribution of Gary Becker (1968), a large literature has addressed issues of criminal behavior and sanctions (e.g., George J. Stigler, 1970; Isaac Ehrlich, 1973; Anne Dryden Witte, 1980; Samuel L. Myers, Jr., 1983; Robert E. McCormick and Robert D. Tollison, 1984; James Andreoni, 1991). One of the most surprising empirical results in this literature is the repeated failure to uncover evidence that an increase in the number of police reduces the crime rate. Of the 22 studies surveyed by Samuel Cameron (1988) that attempt to estimate a direct relationship between police and crime using variation across cities, 18 find either no relationship or a positive (i.e. incorrectly signed) relationship between the two.[1]

One probable source of bias in estimating the effect of police on crime is the likely endogeneity of police with respect to crime rates (Franklin Fisher and Daniel Nagin, 1978).[2] Higher crime rates are likely to increase the marginal productivity of police. Cities with high crime rates, therefore, may tend to have large police forces, even if police reduce crime. Detroit has twice as many police officers per capita as Omaha, and a violent crime rate over four times as high, but it would be a mistake to attribute the differences in crime

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[1] In addition to cross-sectional studies, there are other sources of evidence. Quasi-experimental evidence from a study performed in Kansas City in the early 1970's found no statistically significant difference in crime when the number of police assigned to 15 different patrol beats were varied (George Kelling et al., 1974). Analysis of police on the New York subway system in the 1960's suggests that an increased police presence reduces the number of robberies slightly, but at an extremely high taxpayer cost per crime eliminated (James Q. Wilson, 1983). Helen Tauchen et al. (1994) find a deterrent effect of police resources in an analysis that combines individual-level information on arrests with aggregate information on police. Thomas Marvell and Carlisle Moody (1996), using a Granger-causality approach, also find that additional police reduce crime.

[2] In fact, in a recent survey of the effect of police on crime, Lawrence Sherman (1992) dismisses studies of the level of police resources based on cross-sectional variation in a footnote due to this criticism, focusing instead on policing strategies.

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rates to the presence of the police. Similarly, within a particular city, if more police are hired when crime is increasing, a positive correlation between police and crime can emerge, even if police reduce crime.[3] The 1994 crime bill provides a good case study. In response to opinion polls ranking crime as the number one problem facing the country, Congress authorized partial federal funding of an additional 100,000 local police officers.

The primary innovation of the paper is the approach used to break the simultaneity between police and crime. In order to identify the effect of police on crime, a variable is required that affects the size of the police force, but does not belong directly in the crime "production function." The instrument employed in this paper is the timing of mayoral and gubernatorial elections.

Section II of this paper documents a previously unrecognized electoral cycle in police force staffing. Increases in the size of police forces in large cities are disproportionately concentrated in mayoral and gubernatorial election years. The mean percentage change in sworn police officers for the cities in the sample is 2.1 percent in gubernatorial election years, 2.0 percent in mayoral election years, and 0.0 percent in nonelection years. That relationship persists after controlling for a variety of demographic, socioeconomic, and economic factors.

If elections are to serve as valid instruments, then they must be uncorrelated with crime, except through variables that are included in the equation explaining crime. The most obvious ways in which elections might systematically affect the crime rate (other than via changes in the police force) are through electoral cycles in other types of social spending, or through politically induced fluctuations in economic performance. Consequently, spending on education and public welfare programs is included in the equations, as are state unemployment rates. Having controlled for those factors, it seems plausible to argue that election timing will be otherwise unrelated to crime.

Examining a panel of 59 large U.S. cities over the period 1970–1992, it is first demonstrated that, as in previous studies, positive cross-city correlations between police and crime emerge. Using first differences, which identify the parameters using only within-city variation over time and therefore are likely to do a better job of controlling for unobserved heterogeneity, the coefficient on police becomes slightly negative. Instrumenting with election cycles to take account of the endogeneity of police staffing produces estimates that are consistently more negative. The elasticity of violent crime with respect to sworn officers is estimated to be approximately -1.0; for property crime the elasticity is around -0.3. Point estimates are negative for each of the seven crime categories examined. Because the election-cycle instruments are weak, however, the estimated impacts of police on crime are imprecise, making it difficult to draw strong public policy conclusions.

While the particular focus of this paper is on the issue of police and crime, the use of political variation to identify the effects of public policies may prove to be of much broader applicability. Because public policies emerge from a political process, electoral cycles and differences in political institutions are logical instruments for public policy changes. To the extent that the timing of elections and political institutions are relatively fixed, such instruments may provide a more plausibly exogenous source of variation than either cross-sectional or time-series analyses.

The outline of the paper is as follows. Section I summarizes the data set used in the analysis. The second section demonstrates a positive correlation between changes in the size of the police force and both city mayoral races and gubernatorial elections. Section III

[3] A second source of bias against finding that police reduce crime is the use of *reported* crimes rather than *actual* crimes in empirical studies due to the lack of availability of the true measure. As the police presence increases, reporting rates may rise if the perceived likelihood of a crime being solved increases. Furthermore, police officers have a great deal of discretion in choosing whether or not to make arrests in many cases, such as domestic disputes. It is possible that the likelihood of arrest for a given incident decreases with the officer's workload, which in turn may be a function of the level of police staffing. The results of Levitt (1996), which find only a weak relationship between the likelihood a crime is reported and the level of police staffing, suggest that the use of reported crime statistics will be small. To the extent that reporting bias is present, the results of this paper understate the effectiveness of police.

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TABLE 1—SUMMARY STATISTICS (ALL VALUES PER 100,000 RESIDENTS EXCEPT POPULATION)

		Standard deviation			
Variable	Mean	Within-city	Across cities	Minimum	Maximum
Population	721,911	59,190	1,043,450	90,000	7,889,900
Violent crime	1,167	338	685	103	4,353
Murder	19	4	12	0.6	81
Rape	70	20	31	7	188
Assault	531	216	354	42	2,386
Robbery	562	148	377	40	2,338
Property crime	7,740	1,339	2,112	2,707	6,739
Burglary	2,323	435	723	654	4,994
Larceny	4,400	897	1,389	848	10,003
Motor vehicle theft	1,075	377	703	165	5,369
Sworn officers	238	20	99	79	781
Percent black	23.0	2.3	18.1	0.1	78.2
Percent female-headed households	14.9	1.0	4.3	5.6	31.9
Percent ages 15–24	17.2	1.5	17.2	11.5	25.4
Public welfare spending per capita (1992 dollars)	255.2	56.8	126.0	33.5	847.7
Education spending per capita (1992 dollars)	765.2	79.7	122.9	445.9	1,193.4
State unemployment rate	6.5	1.8	2.0	2.0	15.5

Notes: All variables except population are per 100,000 residents. The sample used is a set of 59 large U.S. cities with directly elected mayors over the period 1970–1992. Data on crime, police, and population from *Uniform Crime Reports* issued by the FBI. All other data is available in the *Statistical Abstract of the United States.* Some demographic variables are interpolated from data for decennial census years. For a breakdown of the data by city, see the Appendix.

presents the estimates of the effect of police on crime using the variation in police staffing due to electoral cycles to identify the parameters. Section IV develops a rough cost-benefit appraisal of hiring additional police, although the results of this section must be viewed as highly speculative. Section V offers a brief conclusion.

I. The Data

The data used are a panel of 59 U.S. cities, with observations running from 1970–1992. These cities represent all U.S. cities satisfying two criteria: (i) the city population exceeds 250,000 at some point in the time period analyzed, and (ii) the mayor is directly elected. Because mayoral elections are critical to identifying the model, six cities (Cincinnati, Virginia Beach, Norfolk, Wichita, Santa Ana, and Colorado Springs) that satisfy the population cutoff, but do not have direct election of mayors, are excluded from the sample.

Data on crime are taken from the *Uniform Crime Reports* issued by the Federal Bureau of Investigation (FBI), and are available annually on a city-level basis for seven types of crime: murder and nonnegligent manslaughter, forcible rape, assault, robbery, burglary, larceny, and motor vehicle theft. Data on the number of sworn officers are also taken from the FBI *Uniform Crime Reports.* Sworn officers carry a gun and have the power of arrest; other police employees do not.

Summary statistics, expressed in values per 100,000 population where applicable, are presented in Table 1. Overall, there is slightly less than one reported crime per ten individuals,

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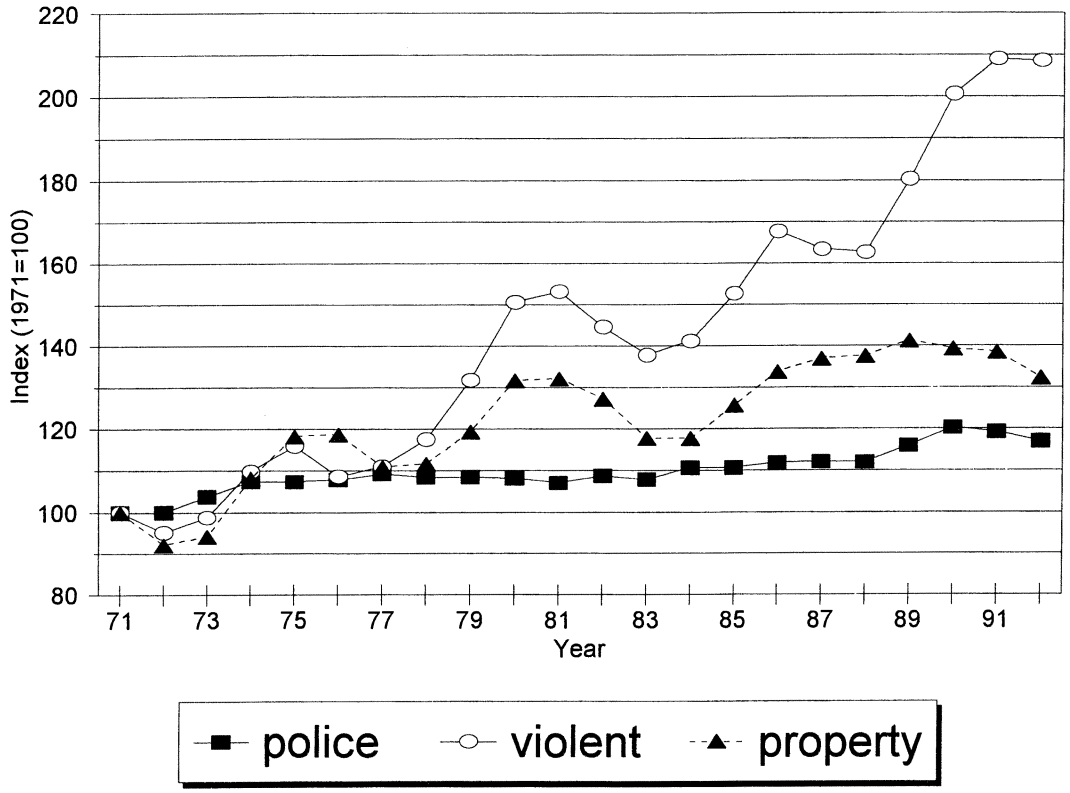

FIGURE 1. TRENDS IN CRIME AND POLICE

the great majority of which are relatively minor property crimes.[4] Violent crime rates for the cities in the sample are more than twice as high as for the nation as a whole; property crimes per capita are almost twice as frequent in these cities. One notable feature of the data on reported crime is the wide variation across cities. The crime rates for an individual crime category often vary by orders of magnitude across cities.[5] There are approximately 240 sworn police officers per 100,000 population, representing approximately 80 percent of total police employees. While the analysis that follows focuses on sworn officers, similar results are obtained throughout the paper when total police employees are used in place of sworn officers.

Figure 1 shows the time series (in per capita terms) of violent crime, property crime, and sworn officers for the cities in the sample. In each case, the 1971 value of the category is indexed as 100. Violent crime has seen the greatest increase, more than doubling in these cities between 1970 and 1992. Until the mid-1980's, violent crime and property crime tracked each other fairly closely. Since that time, violent crime has steadily increased, while property crime has flattened.[6] The

[4] In fact, this number greatly understates the true crime rate for two reasons. First, less than one-half of all crimes are reported to the police. Second, when multiple offenses occur in the commission of a single crime, the FBI only records the most serious of these offenses. Thus, if a robber kills someone in the process of a holdup, and then steals a car to flee the scene, only the murder would be included in the FBI statistics.

[5] Edward L. Glaeser et al. (1996) undertake an in-depth analysis of this issue.

[6] Victimization surveys, unlike the reported crime statistics used in this analysis, show a declining trend in

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number of sworn officers has grown less rapidly than crime rates.

In addition to the data on police and crime, a number of demographic, government-spending, and economic variables are included in the regressions. Ideally, those variables would be available at the city level on an annual basis. Unfortunately, the data limitations with respect to city-level data necessitate a number of compromises. While city populations are available annually, the only convenient data source for the percent of a city's population that is black and the percent living in female-headed households is the decennial census. Consequently, a linear interpolation of those variables is made for noncensus years. Since demographic variables tend to evolve slowly, this may serve as a reasonable approximation. The data on the percentage of the population between the ages of 15 and 24 used in this paper also are linearly interpolated from the decennial census, and suffer from the further defect that calculation is by Standard Metropolitan Statistical Area (SMSA) rather than by city.

Data on government spending for education and public welfare programs have a different complication. While annual city government outlays on such programs are available, less than 10 percent of total state and local expenditures on both of those categories originate at the city level.[7] State outlays, however, are not broken down according to the city that receives the money. Therefore, the spending variables that are employed in this paper are combined state and local outlays per capita (in 1992 dollars) on a particular category in a given state and year. While this variable misses some of the city-level variation, it is assumed that any mayoral election cycles in such spending will be small since city budgets for such activities also are small. Annual state unemployment rates are used to control for economic fluctuations. Finally, a series of city-size indicators (corresponding to populations below 250,000, between 250,000 and 500,000, between 500,000 and one million, and over one million) are included as controls.

II. Mayoral and Gubernatorial Election Cycles in Police Staffing

There are many reasons to suspect a link between elections and the timing of changes in the size of city police forces, particularly in big cities. First, crime is a critical political issue in these cities, and has been since the crime rate began to rise in the early 1960's. Crime consistently ranks among the most important issues facing the nation in opinion surveys, and is frequently *the* most critical issue when the economy is performing well. Given the importance of crime as a political issue, incumbents will have incentives to increase the police force in advance of elections, either in hopes of actually reducing crime, or simply to demonstrate that they are "tough on crime."[8]

For mayors, especially, police are an ideal target for political manipulation since police departments are organized at the city level with only a few exceptions.[9] Ultimate decision-making authority on police issues therefore resides with the mayor, as does credit or blame concerning police performance. Furthermore, the high rate of turnover among police officers (who can typically retire after 20 years with full pension) and the ease of altering the size of an incoming class of cadets makes both upward and downward shifts in the size of the police force relatively easy to accomplish. In contrast, a city's economic performance is largely outside the control of the mayor. For that reason, it is not clear that voters hold mayors responsible for a city's economic situation; John Chubb (1988) finds that even governors bear little responsibility for the state economy in the eyes of voters.

crime rates per capita (for the nation as a whole). Unfortunately, more disaggregated data on victimization is not available. For an extensive discussion of crime data in the United States, see Robert O'Brien (1985).

[7] In 1992, only eight of the 59 cities in the sample spent an appreciable amount on education. Local school boards typically are financed independently from city governments.

[8] Eric Monkkonen (1992) details various other political uses of urban police forces over the last century, although no mention is made of election cycles in police staffing.

[9] In recent years, a few cities have formed a joint force with surrounding communities. Nashville, for instance, shares its police force with Davidson County.

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While the motives of incumbent governors are likely to be similar to those of incumbent mayors, the mechanism by which governors might affect levels of city police staffing is less straightforward since the state government does not typically directly hire local police. State governments do, however, provide substantial local aid to city governments (representing more than 20 percent of general revenues for large cities), as well as a more limited amount of intergovernmental grants tied specifically to local law enforcement. Tim Besley and Anne Case (1995) document gubernatorial election cycles for a range of fiscal variables. Although intergovernmental grants is not among the categories Besley and Case examine, the existence of cycles in intergovernmental grants would not be implausible in light of their other results.

Empirically, changes in the size of police forces do, indeed, tend to mirror the political cycle in large cities. A simple comparison of the mean percentage change in the number of police officers per capita in the sample across election and nonelection years, presented below, shows that the number of sworn officers grows by approximately 2 percent on average in both gubernatorial and mayoral election years, but is completely flat in nonelection years (standard deviations in parentheses):[10]

	Gubernatorial election year ($N = 302$)	Mayoral election year ($N = 391$)	No election ($N = 621$)
Δln Sworn police officers per capita	0.021 (0.006)	0.020 (0.007)	0.000 (0.006)

The propensity to increase police forces around elections also emerges when the data is analyzed on a year-by-year basis. Figure 2 provides a comparison of increases in sworn officers for cities with and without elections in a given year (either mayoral or gubernatorial). While there is substantial year-to-year variability in the average change in the number of sworn officers, cities with elections in the current year exhibit higher rates of increase (or smaller decreases) in 20 of the 23 years. If changes in sworn officers are independent across cities and are unrelated to the timing of elections, the likelihood that cities holding elections would have higher rates of increase in 20 or more of 23 cases is less than one in 4,000.[11]

Another way of examining the robustness of the relationship between sworn officers and elections is to analyze the data on a city-by-city basis. A full list of cities, along with information on mean changes in sworn officers per capita in gubernatorial, mayoral, and nonelection years, is provided in the Appendix. Excluding Washington, DC, which does not have gubernatorial elections, 43 of the 58 cities in the sample have higher mean rates of increase in gubernatorial election years compared to years in which there are neither gubernatorial nor mayoral elections. If cities represent independent observations, the odds of a pattern this extreme are less than one in 5,000 if elections do not affect police hiring. In 37 of the 59 cities there are greater mean increases in sworn officers in mayoral election years versus nonelection years (with one tie). Again assuming independence across cities, the likelihood of an outcome this extreme is less than one in 30 if mayoral elections have no effect on police staffing.

Those simple averages, of course, do not take into account possible correlation between the timing of elections and other factors that might influence growth of the police force, such as the state of the economy. To allow for such considerations, the relationship between police and elections is modeled more formally as follows:

$$(1) \quad \Delta \ln P_{it} = \Theta_1 M_{it} + \Theta_2 G_{it} + \mathbf{X}_{it}\delta + \gamma_t + \lambda_i + \nu_{it},$$

where P_{it} is the number of sworn officers per capita for city i in year t; M is an indicator

[10] Approximately 3 percent of the observations in the sample have both mayoral elections and gubernatorial elections in the same year. Consequently, there is a small amount of double counting in these simple averages. The patterns are unaltered if such observations are discarded.

[11] Of course, changes in police forces across cities are not truly independent since grants from state to local governments will tend to be positively correlated for cities in the same state.

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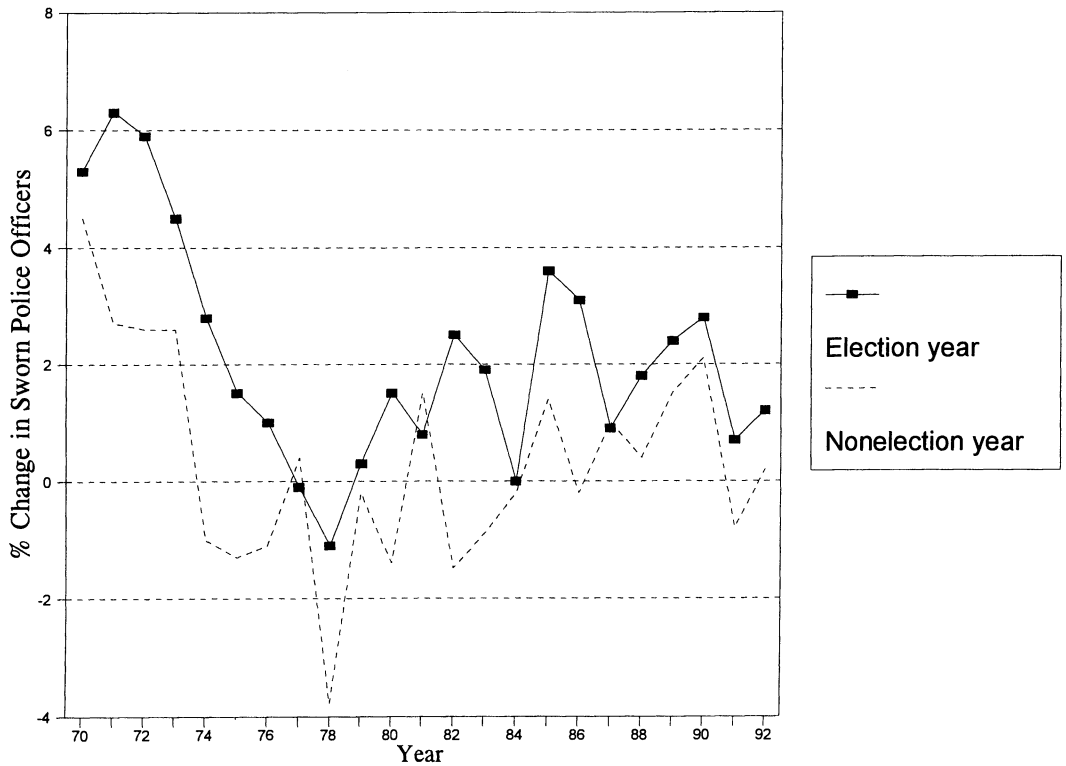

FIGURE 2. YEARLY CHANGES IN SWORN POLICE (ELECTION YEARS VERSUS NONELECTION YEARS)

variable equal to one in mayoral election years and zero otherwise; G is an indicator variable equal to one in gubernatorial election years and zero otherwise; and $\mathbf{X}$ is a matrix of covariates including demographic variables, state and local spending controls, city-size indicators, and region and year dummies. With the exception of the indicator variables, the covariates are log-differenced.[12]

Table 2 presents regression estimates for three variations of equation (1). Column (1) includes only year and region dummies as covariates. Column (2) adds city-size indicators and demographic and economic controls. Column (3) replaces region dummies with city-fixed effects. The city-fixed effects in column (3) capture city-level trends in police staffing because the dependent variable is differenced.

[12] First-differencing the election indicators would imply a model where all election-year increases in police are completely undone in the following year. The data, however, strongly reject such a model. When indicator variables for all years of the election cycle were included, the null hypothesis of equal coefficients across all nonelection years could not be rejected, suggesting that the simple specification employed in Table 2 adequately captures the electoral cycles in police hiring.

Empirically, estimates of equation (1) with the election indicators first-differenced yield both point estimates and standard errors on the election variables that are roughly one-half as large as those presented in Table 2. The point estimates are smaller for the reasons discussed in the preceding paragraph of this footnote. The standard errors shrink because of the negative serial correlation in the election-year data (i.e., an election last year guarantees that no election will be held this year because of the even spacing of elections). First-differencing a data series that is negatively serially correlated *increases* the variation in the election data, yielding smaller standard errors. If equation (1) is run with neither police nor elections first-differenced, the point estimates are similar to the first-differenced case and the standard errors are similar to those in Table 2. Consequently, in that specification, unlike the first-differenced case and Table 2, the election coefficients are not statistically significant.

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TABLE 2—THE ELECTION CYCLE AS A PREDICTOR OF CHANGES IN THE POLICE FORCE

Variable	(1) Δln Sworn officers	(2) Δln Sworn officers	(3) Δln Sworn officers	(4) Violent crime	(5) Property crime
Mayoral election year	0.013	0.011	0.012	−0.008	−0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Gubernatorial election year	0.025	0.024	0.024	−0.006	−0.009
	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)
Δln Public welfare spending per capita	—	−0.012	−0.013	−0.010	0.008
		(0.009)	(0.009)	(0.013)	(0.011)
Δln Education spending per capita	—	0.088	0.094	0.054	−0.022
		(0.044)	(0.045)	(0.043)	(0.035)
Δ State unemployment rate	—	−0.323	−0.319	−0.286	0.645
		(0.256)	(0.258)	(0.224)	(0.182)
Δ(Percent ages 15–24 in SMSA)	—	2.41	4.69	−4.03	4.24
		(2.24)	(2.56)	(2.76)	(2.24)
Δ(Percent black)	—	0.001	−0.007	−0.018	−0.012
		(0.007)	(0.010)	(0.009)	(0.007)
Δ(Percent female-headed households)	—	−0.003	0.012	−0.002	0.013
		(0.014)	(0.019)	(0.018)	(0.014)
Year indicators?	Yes	Yes	Yes	Yes	Yes
City-size indicators?	No	Yes	Yes	Yes	Yes
City-fixed effects?	No	No	Yes	Yes	Yes
P-value: Joint significance of election years?	<0.001	<0.001	<0.01	0.082	0.062
R^2	0.06	0.09	0.11	0.21	0.37

Notes: Dependent variable in columns (1)–(3) is Δln sworn police officers per capita. The dependent variable in columns (4) and (5) is the average Δln in the number of crimes per capita over the current and following year. All four violent crime categories are stacked in estimating column (4); all three property crime categories are stacked in estimating column (5). Sample includes 59 large U.S. cities with directly elected mayors, 1970–1992. Number of observations is 1,276 in columns (1)–(3), 4,801 in column (4), and 3,606 in column (5). Columns (4) and (5) allow for heteroskedasticity across crime categories. Year dummies included in all regressions. Three city-size indicators are included in columns (2)–(5); city-fixed effects are included in columns (3)–(5).

Mayoral election years are associated with a greater than 1-percent increase in per capita sworn officers. The effect of gubernatorial elections is even larger.[13] The election coefficients are both individually and jointly statistically significant. In contrast, the other variables in the regression generally are statistically insignificant and carry coefficients that are substantively small.

Although the election variables are statistically significant, they account for only a small fraction of the overall variation in police staffing. Eliminating the election variables from column (4), for instance, reduces the R^2 by only 0.016. Consequently, the election variables are fairly weak instruments for identifying the impact of police on

[13] Following Besley and Case (1995), who find differential tax and fiscal behavior by incumbent governors conditional on the presence of binding term limits, I divided governors between those constrained from seeking reelection and those who are not constrained. While there was some evidence in the raw data that unconstrained governors induce more extreme cycles in police hiring, the restriction of identical behavior could not be rejected in the specifications shown in Table 2.

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crime. One partial solution to that problem is to exploit variation in the size of electoral impacts on police by city size or region (results not shown in tabular form). When the two election variables are interacted with four city-size indicators, yielding a total of eight instruments, all eight variables carry a positive sign, demonstrating the robustness of the election-cycle effects. The eight election*city-size interactions are jointly statistically significant at the 0.01 level (F-stat=3.96), with five of the eight terms individually significant. The amount of variation explained by the election*city-size interactions is, however, only 20 percent greater than that of the noninteracted election variables. When elections are interacted with nine region dummies corresponding to the census definitions, 15 of the 18 interactions carry a positive coefficient. The interactions are jointly statistically significant at the 0.01 level (F-stat=3.14), with five individually significant. Moreover, the election*region interactions more than triple the amount of variation in police staffing explained vis-à-vis the uninteracted election variables.

Given that police staffing increases in election years, then if police affect crime, a reduced-form relationship between election years and crime should emerge. The relationship between elections and crime may, however, be complicated by lags in police impacting on crime. Police reduce crime either via deterrence (preventing the commission of the initial crime due to an increased likelihood of being caught), or through incapacitation (catching repeat offenders so they cannot commit future crimes). If there are lags in the response of criminal behavior to the probability of being caught, the deterrence effect will not be immediate. Similarly, the benefits of incapacitation are realized for as long as the offender is in prison. Thus, an arrest today can reduce crime well into the future.[14] Consequently, in the reduced-form specifications, the dependent variable examined is the average change in crime rates over the current and following year.

The reduced-form specifications estimated are as follows:

$$(2)\quad \Delta \ln C_{ijt} = \Psi_1 M_{it} + \Psi_2 G_{it} + \mathbf{X}_{it}\kappa_j + \gamma_{tj} + \lambda_i + \nu_{ijt},$$

where $\Delta \ln C_{ijt}$ is shorthand for the average annual percent change in city i for crime j over the years t and $t + 1$. The other right-hand-side variables are identical to equation (1), except that the coefficients on the other covariates and the year dummies are allowed to vary by crime. The city-fixed effect, however, is constrained to be the same across all crimes.

Equation (2) is estimated jointly for all violent crime and similarly for all property crime.[15] In each case, the point estimates are constrained to be identical across crimes for the election year and demographic variables, but not for the year dummies. The estimation allows for heteroskedasticity across crime categories. Columns (4) and (5) of Table 2 present the results of estimating equation (2) for violent crime and property crime, respectively. Elections are associated with a decrease in both violent and property crimes. The point estimates imply that crime rates are slightly less than 1 percent lower than otherwise would be expected over the election year and the year following the election. The effects of mayoral and gubernatorial elections on crime appear to be similar. In both columns (4) and (5), the election indicators are jointly significant at the 0.05 level. The other covariates in the regression generally are statistically insignificant and carry an unexpected sign as often as not. In specifications omitting the covariates (not

[14] On a purely practical level, another reason for including lagged changes in the police force is the staggered nature of the available data. Data on reported crimes are collected on a calendar-year basis, whereas the size of the police force is a snapshot as of October 31. As a consequence, if changes in the police force occur shortly before October 31, there is little opportunity for those changes to affect crime in the current year.

[15] Stacking the various crime categories provides a more effective means of incorporating the information contained in the time series of individual crimes than simply summing the total number of crimes across categories, as is typically done. Because some crimes are far more frequent than others, the information provided by the rarer (but more severe) crimes is washed out when the crime numbers are summed.

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shown in table), similar election-year coefficients are obtained.

III. Estimating the Effect of Police on Crime

The preceding section demonstrates a positive correlation between elections and changes in the police force, as well as a negative correlation between crime and election years. Together those results suggest a direct relationship between police and crime that is examined in this section using election timing as an instrument for changes in the size of the police force.

In order for election timing to serve as an instrument to identify the effect of police on crime, it must be the case that elections are validly excluded from the crime "production function." Such an exclusion is invalid if there are other variables that are both correlated with crime and affected by electoral cycles. In particular, spending on public welfare or education might fall into that category since such spending may have effects on criminal activities by changing the opportunity sets of potential criminals. It is also possible that state and local elections induce economic fluctuations akin to the political business cycle observed at the national level (Alberto Alesina and Jeffrey Sachs, 1988). In estimating a relationship between police and crime, therefore, controls are included for state and local spending on both public welfare and education, as well as state unemployment rates. Having controlled for such factors, election cycles would appear to be plausible instruments.

The impact of police on crime is estimated using two-stage least squares (2SLS), treating the police variables as endogenous and the other right-hand-side variables as exogenous. The particular form of the equation to be estimated is as follows:

$$(3)\quad \Delta \ln C_{ijt} = \beta_{1j}\Delta \ln P_{ijt} + \beta_{2j}\Delta \ln P_{ijt-1} + \mathbf{X}_{it}\eta_j + \gamma_{tj} + \lambda_i + \varepsilon_{ijt},$$

where C_{ijt} is the number of crimes per capita in city i for crime category j in year t; P is the number of sworn officers; and $\mathbf{X}$ is the same matrix of covariates described in the preceding section. The seven crime categories are jointly estimated in equation (3), including city-fixed effects to capture city-level trends in crime across the crime categories. Crime-specific year dummies also are included to reflect national trends in individual crime categories. The estimation allows for heteroskedasticity across crime categories. Following the logic of the previous section, police are allowed to affect crime both contemporaneously and with a one-year lag.[16] The inclusion of longer lags did not substantively affect the results. With ordinary least squares (OLS) the police variables are likely to be correlated with the error term, leading to inconsistent estimates. If the exclusion of election years from equation (3) is valid, however, election timing can be used as an instrument to identify the police parameters.

Two sets of results from estimating equation (3) are presented. In Tables 3 and 4, a number of cross-crime parameter restrictions are imposed. In particular, the coefficients on the police, demographic, and state and local spending variables are constrained to be identical for all violent crime and all property crime, but are allowed to vary between the two groups.[17] The benefits of such restrictions are that they provide a more concise summary of the effects and that they yield more precise point estimates. The potential drawback of the restrictions is that they may impose unwarranted structure on the data. Table 5, therefore, presents crime-by-crime estimates of the impact of police, removing all cross-crime parameter restrictions. In all cases, the coefficient reported for sworn officers, an elasticity, is the sum of the coefficients for the contemporaneous and once-lagged values.

[16] The police variable is indexed P_{ijt}, despite the fact that the number of police in a given city and year does not vary by crime category. This notation is required, however, because the coefficient on police is allowed to vary across crime categories, necessitating that the police variable enter the regression interacted with the crime-category indicators.

[17] Allowing the coefficients on the demographic and spending variables to differ across crimes had little effect on the estimates of the police variables; the more restrictive specification is displayed simply to economize on space.

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TABLE 3—ESTIMATES OF THE ELASTICITY OF VIOLENT CRIME RATES WITH RESPECT TO SWORN POLICE OFFICERS

Variable	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) LIML
ln Sworn officers per capita	0.28 (0.05)	−0.27 (0.06)	−1.39 (0.55)	−0.90 (0.40)	−0.65 (0.25)	−1.16 (0.38)
State unemployment rate	−0.65 (0.40)	−0.25 (0.31)	−0.00 (0.36)	−0.19 (0.33)	−0.13 (0.32)	−0.02 (0.33)
ln Public welfare spending per capita	−0.03 (0.02)	−0.03 (0.02)	−0.03 (0.02)	−0.03 (0.02)	−0.02 (0.02)	−0.03 (0.02)
ln Education spending per capita	0.04 (0.07)	0.06 (0.06)	0.02 (0.07)	0.03 (0.07)	0.05 (0.06)	0.03 (0.06)
Percent ages 15–24 in SMSA	1.43 (1.00)	−2.61 (3.71)	−1.47 (4.12)	−2.55 (3.88)	−2.02 (3.76)	−1.50 (3.86)
Percent black	0.010 (0.003)	−0.017 (0.011)	−0.034 (0.015)	−0.025 (0.013)	−0.022 (0.012)	−0.031 (0.013)
Percent female-headed households	0.003 (0.006)	0.007 (0.023)	0.040 (0.030)	0.023 (0.027)	0.018 (0.025)	0.033 (0.027)
Data differenced?	No	Yes	Yes	Yes	Yes	Yes
Instruments:	None	None	Elections	Election * city-size interactions	Election * region interactions	Election * region interactions
P-value of cross-crime restriction on police elasticity	<0.01	<0.01	0.09	0.13	0.33	0.28

Notes: Dependent variable is Δln crime rate per capita for one of the four violent crimes (murder and nonnegligent manslaughter, rape, robbery, and aggravated assault), except in column (1) where log-levels, rather than log-differences, are used. Right-hand-side variables also are differenced in columns (2)–(6). Estimates are obtained estimating all crime categories jointly, allowing for a city-fixed effect across crime rates and heteroskedasticity across crime categories. The reported parameter estimates are constrained to be the same across all violent crime. Corresponding results for property crime are reported in Table 4. Number of observations is 1,136 per crime category. Crime-specific year dummies, region dummies, and city-size indicators also are included in all regressions. The reported coefficient for sworn officers is the sum of the contemporaneous and once-lagged coefficients. In columns (3)–(6), sworn officers are treated as endogenous. Column (3) instruments using mayoral and gubernatorial election-year indicators. Column (4) instruments using interactions between the city-size indicator variables and mayoral and gubernatorial elections. Columns (5) and (6) instruments using interactions between region dummies and mayoral and gubernatorial elections. The last row of the table reports the *p*-value of the restriction that the effect of sworn officers is identical across all four crime categories.

Table 3 shows the estimates for violent crime, imposing the cross-crime parameter restrictions described above. Column (1) presents OLS estimates of equation (3) in log-levels. The positive coefficient on sworn officers (0.28 with a standard error of 0.05) implies that more police are associated with higher crime rates. This result is consistent with previous estimates in the literature that rely on cross-city variation and do not take into account the endogeneity of the size of the police force (Cameron, 1988). Column (2) shows OLS results of equation (3) in log-differences. By first-differencing, all of the parameters are identified using only within-city variation over time. The coefficient on sworn officers now becomes negative (−0.27 with a standard error of 0.06), suggesting that unobserved heterogeneity across cities imparts an upward bias on the coefficient.

Columns (3)–(5) of Table 3 provide 2SLS estimates of the impact of police on crime using a varying set of election-year interactions as instruments for sworn officers. The other variables continue to be assumed exogenous. In column (3), separate indicator variables for contemporaneous and once-lagged may-

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TABLE 4—ESTIMATES OF THE ELASTICITY OF PROPERTY CRIME RATES WITH RESPECT TO SWORN POLICE OFFICERS

Variable	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) LIML
ln Sworn officers per capita	0.21	−0.23	−0.38	−0.05	−0.24	−0.34
	(0.05)	(0.09)	(0.83)	(0.59)	(0.36)	(0.54)
State unemployment rate	1.40	0.99	1.04	0.94	1.01	1.04
	(0.46)	(0.46)	(0.55)	(0.50)	(0.47)	(0.49)
ln Public welfare spending per capita	0.01	−0.02	−0.02	−0.02	−0.02	−0.02
	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)
ln Education spending per capita	0.51	0.01	0.01	0.02	0.01	0.01
	(0.08)	(0.09)	(0.11)	(0.10)	(0.09)	(0.10)
Percent ages 15–24 in SMSA	1.43	1.15	−1.47	−2.55	−2.02	2.69
	(1.00)	(4.92)	(4.12)	(3.88)	(3.76)	(5.16)
Percent black	−0.002	−0.019	−0.029	−0.021	−0.022	−0.027
	(0.003)	(0.014)	(0.018)	(0.016)	(0.015)	(0.016)
Percent female-headed households	0.007	0.013	0.025	0.012	0.016	0.022
	(0.006)	(0.030)	(0.039)	(0.034)	(0.031)	(0.034)
Data differenced?	No	Yes	Yes	Yes	Yes	Yes
Instruments:	None	None	Elections	Election * city-size interactions	Election * region interactions	Election * region interactions
P-value of cross-crime restriction on police elasticity	<0.01	0.18	0.91	0.92	0.96	0.93

Notes: Dependent variable is Δln crime rate per capita for one of the three property crimes (burglary, larceny, or motor vehicle theft), except in column (1) where log-levels, rather than log-differences, are used. Right-hand-side variables also are differenced in columns (2)–(6). Estimates are obtained estimating all crime categories jointly, allowing for a city-fixed effect across crime rates and heteroskedasticity across crime categories. The reported parameter estimates are constrained to be the same across all property crime. Number of observations is 1,136 per crime category. Corresponding results for violent crime are reported in Table 3. Crime-specific year dummies, region dummies, and city-size indicators also are included in all regressions. The reported coefficient for sworn officers is the sum of the contemporaneous and once-lagged coefficients. In columns (3)–(6), sworn officers are treated as endogenous. Column (3) instruments using mayoral and gubernatorial election-year indicators. Column (4) instruments using interactions between the city-size indicator variables and mayoral and gubernatorial elections. Columns (5) and (6) instruments using interactions between region dummies and mayoral and gubernatorial elections. The last row of the table reports the *p*-value of the restriction that the effect of sworn officers is identical across all three crime categories.

oral and gubernatorial election years are used as instruments (since both contemporaneous and once-lagged police are included as regressors).[18] The police coefficient, while imprecisely estimated, is nonetheless statistically significant at the 0.05 level and is five times larger than the OLS estimates in column (2).[19] Columns (4) and (5) expand the set of instruments by interacting the election-year dummies with four city-size indicators and nine census-region dummies, respectively. As the number of instruments increases in columns (4) and (5), both the coefficient estimates and the standard errors shrink. The estimates remain statistically significant,

[18] Because the seven crime categories are stacked, each of the instruments is interacted with seven crime-category indicator variables in the actual estimation. A similar procedure is used in columns (4)–(6).

[19] Estimates using only mayoral or only gubernatorial elections yield similar results, but are less precise.

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TABLE 5—CRIME-SPECIFIC ESTIMATES OF THE EFFECT OF CHANGES IN SWORN OFFICERS

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor vehicle theft
OLS (levels)	0.27	−0.07	0.64	0.34	0.08	0.14	0.38
	(0.06)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.06)
OLS (differences)	−0.60	−0.06	−0.31	0.11	−0.25	−0.10	−0.29
	(0.19)	(0.13)	(0.10)	(0.13)	(0.08)	(0.06)	(0.10)
2SLS (elections as instruments)	−3.05	0.67	−1.20	−0.82	−0.58	0.26	−0.61
	(0.91)	(1.22)	(1.31)	(1.20)	(1.55)	(1.66)	(1.31)
2SLS (election * city-size interactions as instruments)	−2.09	0.08	−0.38	−0.36	−0.39	0.06	0.14
	(0.64)	(0.84)	(0.89)	(0.81)	(1.06)	(1.20)	(0.89)
2SLS (election * region interactions as instruments)	−1.18	−0.11	−0.49	−0.41	−0.11	−0.21	−0.34
	(0.39)	(0.49)	(0.53)	(0.50)	(0.62)	(0.67)	(0.53)
LIML (election * region interactions as instruments)	−1.98	−0.27	−0.79	−1.09	−0.05	−0.43	−0.50
	(0.59)	(0.77)	(0.79)	(0.73)	(0.90)	(1.01)	(0.80)

Notes: Dependent variable is Δln crime rate per capita for the named crime category, except in row 1 where log-levels, rather than log-differences, are used. Right-hand-side variables also are differenced in columns 2–6. Each row of the table presents crime-specific coefficients on the sworn-officer variables from a separate regression. The reported coefficients, which are elasticities, represent the sum of the contemporaneous and once-lagged coefficients. In all cases, specifications are identical to that used in Tables 3 and 4, except that the cross-crime restrictions on police elasticities in Tables 3 and 4 have been removed in this table. All crime categories are estimated jointly, allowing for a city-fixed effect across crime rates and heteroskedasticity across crime categories. Controls for state unemployment rates, public welfare, and education spending per capita, percent of the population between the ages of 15 and 24, percent of blacks, and percent of female-headed households also are included in all regressions, as are crime-specific year dummies, region dummies, and city-size indicators. In row 3, sworn officers are treated as endogenous. Column 3 instruments using mayoral and gubernatorial election-year indicators. Row 4 instruments using interactions between the city-size indicator variables and mayoral and gubernatorial elections. Rows 5 and 6 instruments using interactions between region dummies and mayoral and gubernatorial elections.

however, and are two to three times larger than the OLS estimates.

Expanding the set of instruments may lead to more efficient estimation, but also increases the likelihood that 2SLS will perform poorly. In the case of instruments that are only weakly correlated with the endogenous regressor, it has been demonstrated that 2SLS is both likely to be biased towards OLS and to converge to asymptotic properties at a slow rate (Paul A. Bekker, 1994; Douglas Staiger and James Stock, 1994; John Bound et al., 1995). The shrinking pattern of coefficients moving across columns (3)–(5) is consistent with poor finite-sample performance of 2SLS biasing the estimates towards the OLS estimates. As Joshua Angrist et al. (1995) demonstrate, however, limited information maximum-likelihood (LIML) estimation, while sensitive to other forms of misspecification, has favorable finite-sample properties in the presence of many weakly correlated instruments. Column (6) of Table 3 presents the analog to column (5), estimated using LIML rather than 2SLS.[20] The coefficient on sworn officers is now −1.16, almost as large as the 2SLS estimate with the smaller set of instruments, suggesting that the 2SLS estimates in columns (4) and (5) may be biased towards OLS. The standard error also increases vis-à-vis column (5), implying that the asymptotic standard errors reported in column (5) are exaggeratedly low.

Table 4 presents estimates of equation (3) for property crime. As in Table 3, cross-crime parameter restrictions are imposed. The results for property crime display a somewhat different pattern of coefficients than was the case

[20] Using LIML on the specification in column (3) yields almost identical police coefficients as 2SLS. The LIML estimates of column (4) appear exaggeratedly negative, highlighting the sensitivity of LIML to the particular choice of specification.

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for violent crime. OLS estimates once again find a positive coefficient on police when using cross-city variation [column (1)] and a weak negative relationship between police and crime (an elasticity of −0.23 with a standard error of 0.09) when first differences are used in column (2). In contrast to violent crime, instrumenting does not have a large impact on the parameter estimates for property crime. The point estimates on sworn officers are somewhat more negative in columns (3) and (6), but smaller in column (4). The imprecision of the estimates precludes any strong conclusions. The pattern of coefficients in Tables 3 and 4 suggests that police are more effective in reducing violent crime, and also that the size of the police force is more sensitive to violent crime rates. The fact that OLS and 2SLS yield similar estimates for property crime suggests that any endogenous responses of police staffing to crime are limited to violent crime.

The other coefficients in Tables 3 and 4 generally are statistically insignificant and often carry an unexpected sign once the data is differenced, eliminating cross-city variation. Changes in the unemployment rate appear to have a positive effect on property crime, but have little effect on violent crime. A 1-percentage-point increase in the unemployment rate leads to a jump of approximately 1 percent in property crime. That value is larger than those obtained using national-level time-series data (David Cantor and Kenneth C. Land, 1985; Joel A. Devine et al., 1988). Evaluated at the 1992 sample means, a 1-percentage-point increase in unemployment leads to approximately 70 additional reported property crimes per year per 100,000 population.

The percentage of the population between the ages of 15 and 24 has the expected positive sign when estimated in log-levels, but generally is negative in the differenced regressions and is never statistically significant. There is little apparent effect of state and local spending on public welfare of education; the coefficients both are substantively and statistically insignificant. Adding lagged values of the spending variables did not improve their performance. The percentage of the population that is black generally is negatively correlated with crime, while the percentage of female-headed households always is positively correlated with crime. Because of the extremely high positive correlation between those two variables (ρ = 0.86), however, it is difficult to interpret those coefficients separately.

In light of the generally poor performance of the demographic, socioeconomic, and state and local spending variables, it is reassuring that the police coefficients are insensitive to the exclusion of those variables. OLS estimates of the police coefficient in the differenced specification go from −0.27 to −0.26 for violent crime and from −0.23 to −0.21 for property crime. The 2SLS and LIML police coefficients, when demographic controls are omitted, range from −0.56 to −1.38 for violent crime and from −0.09 to −0.51 for property crime.

Because the number of instruments exceeds the number of endogenous regressors in estimating equation (3), the equation is overidentified, allowing for a test of the exogeneity of the extra instruments. To test those restrictions, the residuals from the second-stage regression of 2SLS are regressed on all of the exogenous variables included in the specification, as well as the full set of election-cycle instruments. The test statistic for the validity of the overidentifying restrictions is computed as $N*R^2$, where N is the number of observations and R^2 is the unadjusted R^2 from the regression of the residuals on the exogenous variables and the instrutments. That test statistic is distributed χ^2 with degrees of freedom equal to the number of overidentifing restrictions. The tests of overidentifying restrictions yield mixed results. When the election years are not interacted with city size or region [column (3)], the exogeneity of the overidentifying restrictions cannot be rejected (p-value = 0.16). As the number of instruments is expanded, however, the model fares less well. In column (4), the overidentifying restrictions are rejected at the 0.03 level; in column (5), the model is rejected at the 0.01 level. The failure of tests of overidentifying restrictions largely is driven by the observations in the sample with small city populations. If the 106 city-year pairs with city populations less than 250,000 are dropped from the sample, the null of exogeneity of

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overidentifying restrictions is well within acceptable bounds for columns (3) and (4), but just rejected at the 0.05 level for column (5). Excluding the demographic variables has little effect on the tests of overidentifying restrictions.

If there are electoral cycles in state and local education and welfare spending, it would be incorrect to treat those variables as exogenous in equation (3). There is, however, little evidence for such electoral cycles. In identical specifications as those shown in Table 2, except that police are replaced with spending on either public welfare or education, the election-year coefficients are neither individually nor jointly significant in any of the specifications, implying that education and public welfare spending, unlike police spending, are not systematically affected by election years. Furthermore, reestimating the specifications of Tables 3 and 4 treating state and local spending as endogenous (instrumenting with electoral variables) has virtually no impact on the estimated police coefficients using either 2SLS or LIML.

The estimates in Tables 3 and 4 restrict the police, demographic, and spending variables to have the same coefficients across all violent crime categories and property crime categories, respectively. Allowing the coefficients on the demographic and spending variables to vary by crime has virtually no effect on the police estimates. A more pertinent question is whether the restriction of identical effects of police on the various crime categories is supported by the data. The bottom row of Tables 3 and 4 presents the results of an F-test of that restriction. For violent crime, the OLS specifications strongly reject the equality of police coefficients. For the instrumented cases, the p-values, while low, are above the 0.05 threshold. The restriction appears less binding for property crime.

Table 5 presents estimates eliminating all cross-crime restrictions. To save space, only the sworn-officer coefficients are presented (complete results are available from the author on request). The seven columns in Table 5 correspond to the seven crime categories. Each row of the table represents a different specification, i.e., the rows of Table 5 correspond to the columns of Tables 3 and 4. The same patterns that were observed in the earlier tables emerge when looking at crime categories individually. OLS in levels yields positive coefficients on police for six of the seven crime categories. OLS in first differences leads to negative coefficients in all but one instance. Instrumenting for police leads to more negative estimates in almost all cases, although the individual point estimates are extremely imprecise. In spite of this imprecision, the estimates are negative for all seven crime categories in the bottom two rows of the table.

The crime category yielding the greatest apparent effect of police is murder, a result that is perhaps surprising. This result is very robust, even emerging in the first-differenced OLS estimates. Large negative impacts of police also are observed for robbery, aggravated assault, and auto theft. These are three categories that one might suspect would be most affected by increased policing. Detection of auto theft is likely to be closely correlated with the number of patrols and traffic stops. With robbery and assault, there is a victim to identify the assailant, making it more likely that increased police effort will lead to an arrest. In the case of domestic assaults, there is a great degree of police discretion in making arrests. It may be the case that increases in police staffing make it more likely that an arrest will occur in such situations. In contrast, there is little chance of catching a burglar if the apprehension is not immediate, regardless of the level of police resources devoted. Neither rape nor larceny appears to be greatly affected by the size of the police force.

IV. Implications of the Estimated Effects of Police on Crime for Public Policy

Having obtained estimates of the impact of police on crime in the preceding section, one would like to understand the public policy implications of these estimates. There are, however, a number of important obstacles to making such a determination. First, the impact of police is imprecisely estimated in this paper. Any calculations based on

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these estimates consequently also will be subject to large standard errors. Second, even if the impact of police on crime was known with certainty, the social costs of crime are not, introducing further uncertainty into the exercise. Third, one cannot be certain from the analysis of this paper whether the observed police-related decreases in crime within cities reflects a true reduction in crime or merely a shifting of the crime across jurisdictions. Finally, some consideration must be given to the social value of police activities that are not related to reductions in the subset of crimes considered in this paper. Less than one-quarter of all arrests are for crimes included in this analysis. Among the criminal activities not included in this paper that impose substantial social costs are driving under the influence, drug-related activities, arson, fraud, and vandalism. Moreover, police spend only one-half of their time on crime-related activities (Jack Greene and Carl Klockars, 1991). Presumably, non-crime-related police activities also provide social benefits.

Recognizing the inherent limitations of the exercise, this section provides what must be considered a highly speculative cost-benefit analysis of increasing the number of police in large cities.

The best available measures of the costs of crime to victims are the estimates of Mark Cohen (1988) and Miller et al. (1993). Those two papers attempt to capture monetary costs of crime (medical bills, property loss, lost productivity) and quality of life reductions due to pain and suffering. To gauge the quality of life reductions, jury awards in civil suits, excluding punitive damages, are estimated for a wide range of injuries. The jury awards then are mapped to the distribution of injuries associated with the various crime categories. These cost estimates do not include the costs of additional preventative measures taken by victims, lifestyle changes associated with the marginal crime, costs to employers, or legal costs and, therefore, may understate the true costs of crime. On the other hand, these estimates reflect the cost for the average crime rather than the marginal crime. To the extent that the crimes eliminated due to increased police are less serious than the average crime, the damages may be overstated.

Using these values of the cost of crime, the typical violent crime, weighted by the frequency observed in the sample, carries a social cost of approximately $60,000 (in 1992 dollars). This estimate, however, is extremely sensitive to the value assigned to human life.[21] If a life is valued at $1 million rather than the $2.7 figure used by the authors of the previous studies, the cost per violent crime falls to $33,000. The typical property crime costs $1,100. The estimated elasticities of violent crime with respect to sworn officers from the instrumented regressions in Table 3 range from −0.65 to −1.39. Evaluated at the sample mean, this translates into a reduction of between 3.2 and 7.0 reported violent crimes per additional sworn officer per year. For property crime, the estimated elasticities range from −0.05 to −0.38, implying that the number of reported property crimes falls by 1.6 to 12.4 per officer annually.[22] Even using the most conservative of these estimates, an additional officer provides a social benefit of almost $200,000. In comparison, the OLS estimate of the benefit from reduced crime due to one additional officer, based on column (2) of Tables 3 and 4, is roughly $75,000. Both because the social costs of violent crime far outweigh those of property crime, and because the elasticities of violent crime with respect to sworn officers are estimated to be larger, virtually all of the social benefit of

[21] According to their estimates, each murder carries a social cost of approximately $2.7 million dollars (in 1992 dollars). The social costs of other crimes are as follows: rape, $50,600; robbery, $17,800; aggravated assault, $12,000; burglary, $1,600; larceny, $200; auto theft, $4,000.

[22] These estimates consider only changes in the number of crimes reported to the police. Victimization surveys suggest that less than 40 percent of all index crimes are reported to the police. Given that a criminal does not know with certainty whether a crime will be reported to the police, it is likely that the number of unreported crimes will decline as well. It also has been hypothesized that increases in the size of the police force lead to a greater percentage of crimes being reported to the police—due, perhaps, to an increased likelihood that a case will be solved. Such a reporting bias will lead the estimates of this paper to understate the true effectiveness of police in reducing crime.

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additional officers is due to the reduction in violent crime.

An additional police officer will receive a salary of approximately $40,000, and impose nonsalary overhead costs of a roughly equal magnitude.[23] In addition, there is deadweight loss associated with tax distortions in raising the public funds to pay for the additional officer, with recent estimates suggesting that such distortions may be large (Martin Feldstein, 1995). Another consideration that is ignored in this cost-benefit analysis is the possible change in prison costs associated with the increase in the number of police.[24]

Simply comparing the point estimates of the apparent costs and benefits of an additional officer, it would appear that the number of police in large cities is below the optimal level. Given the imprecision of the estimates, however, it is impossible to draw strong policy conclusions from the estimates of the paper. Even ignoring the inherent variability in calculating the costs per crime, the deadweight loss in raising tax revenue, potential increases in prison costs, and possible shifting of crime across cities, the null hypothesis of equal costs and benefits can only be rejected at the 0.05 level in one of the specifications [column (6)].

V. Conclusions

This paper uncovers a heretofore unrecognized link between police staffing and state/local electoral cycles, with increases in the size of the police force disproportionately concentrated in election years. The existence of such cycles presents a potential avenue for solving the problem of joint determination of crime and police staffing that has plagued previous research. Although the instrumental variables estimates are imprecise, since election cycles explain only a small fraction of the overall variation in police, the results do provide evidence suggesting that additional police reduce crime. While the point estimates generally are not statistically significant for individual crime categories, they are significant for violent crime taken as a whole. Although the point estimates suggest that the benefits of increased police outweigh the costs, the imprecision of the estimates makes it impossible to reject the null hypothesis that the current level of police is set optimally in large cities.

The estimates presented in this paper are derived from "natural experiments" brought about by electoral cycles. An alternative approach to determining the effect of police on crime is a large-scale, long-term randomized experiment.[25] A randomized experiment also might be helpful in assessing the degree of spatial spillovers in crime. It is impossible to determine from the analysis of this paper whether the observed decrease in crime in central cities in response to increased police represents a geographical reallocation of crime rather than a true reduction. The finding that city police hiring is affected by the timing of elections adds to the short, but growing, literature documenting the effects of politics on economic decision-making (e.g., Kenneth Rogoff, 1990; James M. Poterba, 1994; Besley and Case, 1995). This paper goes beyond those previous works, however, by using the political budget cycle as a source of exogenous variation to identify the value of public expenditures. The approach taken in this paper could in theory be applied to a wide range of public policies at all levels of government, and is likely to lead to more reliable estimates than studies based on either cross-sectional or time-series variation.

[23] John J. Donohue and Peter Siegelman (1994) report that there were 812,000 state and local police employees in 1990, and total salary expenditure for this group was $33.4 billion in 1990, for an average salary of $41,000.

[24] As McCormick and Tollison (1984) demonstrate, however, the effect of increasing the number of police on the number of arrests is ambiguous. When the likelihood of detection increases, a greater fraction of the crimes committed results in arrests, but some crimes that previously would have been committed are now successfully deterred and, therefore, do not lead to arrests. In McCormick and Tollison's data on fouls in the ACC basketball tournament, the number of fouls (arrests) drops substantially when the number of officials (police) is increased.

[25] Robert J. LaLonde (1986) and Gary Burtless (1995) argue the merits of randomized experiments; James J. Heckman and Jeffrey A. Smith (1995) offer an opposing view.

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APPENDIX: DATA FOR CITIES IN SAMPLE

	Mayor's term	Violent crime per 100,000	Property crime per 100,000	Annual percent change in crime	Police per 100,000	Percent change in sworn officers		
						Gubernatorial election	Mayoral election	No election
Akron	4 yrs.	704	6,544	3.6	204	0.0	0.6	0.5
Albuquerque	4 yrs.	971	8,151	0.7	255	0.9	4.6	−0.1
Anaheim	2 yrs.	535	6,953	−0.1	186	−0.1	−0.9	0.1
Arlington	2 yrs.	424	6,389	0.5	153	4.1	4.1	0.9
Atlanta	4 yrs.	2,417	10,634	3.5	369	2.5	6.9	1.6
Austin	3 yrs.	498	8,067	2.7	219	8.1	2.8	−3.9
Baltimore	4 yrs.	2,031	6,974	1.3	460	0.7	−1.6	0.3
Birmingham	4 yrs.	1,209	8,235	3.2	286	3.4	1.0	1.9
Boston	4 yrs.	1,839	9,208	1.7	436	2.6	−9.0	−0.3
Buffalo	4 yrs.	1,033	6,222	3.4	333	0.9	−0.1	−1.2
Charlotte	2 yrs.	1,264	8,199	2.6	246	−0.9	2.5	−2.0
Chicago	4 yrs.	1,165	6,316	2.5	475	1.6	1.1	−0.5
Cleveland	4 yrs.	1,468	6,880	0.4	362	−1.9	4.5	−1.3
Columbus	4 yrs.	781	7,516	1.5	250	−2.5	4.1	1.6
Corpus Christi	2 yrs.	662	7,751	2.2	176	2.1	−0.5	0.3
Dallas	2 yrs.	1,437	10,401	1.6	293	2.0	1.0	2.3
Denver	4 yrs.	906	8,780	−0.7	319	2.6	1.0	0.1
Detroit	4 yrs.	2,139	9,071	0.3	437	0.7	14.3	−5.6
El Paso	2 yrs.	680	6,715	2.1	188	1.3	2.1	−1.3
Fort Worth	2 yrs.	1,118	10,311	3.1	242	1.0	1.0	2.3
Fresno	4 yrs.	918	10,126	1.4	210	−0.9	−1.8	−2.9
Honolulu	4 yrs.	250	5,865	1.2	252	−0.7	0.0	0.2
Houston	2 yrs.	940	7,529	1.3	265	5.5	1.5	0.7
Indianapolis	4 yrs.	887	5,647	1.6	258	1.4	−0.7	−1.2
Jacksonville	4 yrs.	1,079	7,079	2.8	275	2.7	−1.6	2.0
Jersey City	4 yrs.	1,271	6,102	4.4	416	6.2	6.2	−1.4
Kansas City	4 yrs.	1,602	8,341	2.3	360	0.4	3.7	0.4
Los Angeles	4 yrs.	1,632	7,250	0.7	320	−0.6	0.7	−1.0
Louisville	4 yrs.	766	5,488	0.5	284	0.6	1.1	1.8
Memphis	4 yrs.	1,058	6,861	3.8	243	1.8	−0.9	1.7
Mesa	2 yrs.	420	6,510	2.0	181	5.5	6.6	−2.1
Miami	4 yrs.	2,577	10,567	2.3	329	1.8	4.7	−0.3
Milwaukee	4 yrs.	580	6,051	3.4	360	−1.5	1.6	0.4
Minneapolis	4 yrs.	1,102	8,417	2.3	225	−0.5	2.1	0.9

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APPENDIX—*Continued.*

	Mayor's term	Violent crime per 100,000	Property crime per 100,000	Annual percent change in crime	Police per 100,000	Percent change in sworn officers		
						Gubernatorial election	Mayoral election	No election
Nashville	4 yrs.	857	6,044	3.1	245	2.3	1.6	1.7
Newark	4 yrs.	2,709	8,736	1.8	431	13.6	3.3	−6.9
New Orleans	4 yrs.	1,419	7,140	1.4	321	3.0	−0.4	−1.1
New York	4 yrs.	1,902	6,594	0.7	441	3.5	−1.3	0.0
Oakland	4 yrs.	1,753	10,318	0.2	267	0.0	0.0	−0.3
Oklahoma City	4 yrs.	845	8,160	3.3	217	2.0	2.0	1.4
Omaha	4 yrs.	655	5,743	1.4	201	3.9	2.4	−1.4
Philadelphia	4 yrs.	942	4,162	2.8	469	2.5	−1.4	−0.6
Phoenix	4 yrs.	797	8,889	0.3	264	−2.9	5.8	−0.2
Pittsburgh	4 yrs.	1,100	5,939	1.2	319	−1.3	−0.1	−0.5
Portland	4 yrs.	1,521	10,557	1.3	240	4.0	1.5	−2.6
Sacramento	4 yrs.	1,018	9,147	1.1	247	0.7	−2.5	−0.6
Saint Louis	4 yrs.	2,181	10,235	1.2	507	1.6	2.0	−1.3
Saint Paul	4 yrs.	756	6,883	0.9	233	−2.7	1.1	2.2
Saint Petersburg	2 yrs.	1,209	7,254	1.9	257	6.4	−0.7	1.9
San Antonio	2 yrs.	582	8,213	2.5	181	4.9	−2.6	2.4
San Diego	4 yrs.	676	6,927	1.6	199	6.6	0.4	−1.2
San Francisco	4 yrs.	1,454	7,959	−0.0	333	2.0	−2.3	−0.6
San Jose	4 yrs.	497	6,083	−1.6	167	1.2	1.2	0.9
Seattle	4 yrs.	1,026	9,488	1.6	285	1.5	−0.6	−0.4
Tampa	4 yrs.	1,868	10,444	4.0	300	6.0	−3.0	1.7
Toledo	2 yrs.	728	7,408	1.4	211	−2.9	3.9	−5.0
Tucson	4 yrs.	695	8,938	2.7	218	−3.2	1.3	3.7
Tulsa	2 yrs.	741	7,076	2.1	214	1.2	2.5	0.7
Washington, DC	4 yrs.	1,890	7,101	0.2	739	—	4.5	−0.6

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