The Effect of Police on Crime: New Evidence from U.S. Cities, 1960-2010

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January 15, 2012

PRELIMINARY DRAFT, SUBJECT TO REVISION

Abstract

This paper shows that errors in the measurement of police are a primary impediment to the accurate estimation of the effect of police on crime. We collect multiple measures of the number of police for a large sample of cities over a long period of time. Correcting for measurement error, we estimate elasticities of crime with respect to police of roughly -0.4 for violent crime and -0.2 for property crime. Elasticities are largest for murder, robbery, and motor vehicle theft.

JEL Classification: C14, C21, C52.

Keywords: Police, crime, measurement error.

I. Introduction

One of the most intuitive predictions of deterrence theory is that, all else equal, an increase in the probability of apprehension decreases participation in crime. This prediction is a core part of Becker's (1968) account of deterrence theory and is also present in the historical articulations of the theory given in Beccaria (1764) and Bentham (1789). The prediction is no less important in more recent treatments, such as the models discussed in Lochner (2004), Burdett, Lagos and Wright (2004), and Lee and McCrary (2009), among others.[1]

On the empirical side, the literature has focused on the specific question of the relationship between police prevalence and crime, where police are viewed as a primary factor influencing the probability of apprehension facing a potential offender. The empirical literature addressing the effect of police on crime encompasses hundreds of articles, and indeed, the literature is sufficiently large that there are many prominent review articles, including Nagin (1978), Cameron (1988), Nagin (1998), Eck and Maguire (2000), Skogan and Frydl (2004), and Levitt and Miles (2006), among others.[2]

Early empirical papers such as Ehrlich (1972) and Wilson and Boland (1978) focused on the cross-sectional association between police and crime. Concern over the potential endogeneity of policing levels, however, led to a predominance of papers using panel data techniques (Cornwell and Trumbull 1994, Marvell and Moody 1996, Witt, Clarke and Fielding 1999, Fajnzylber, Lederman and Loayza 2002, Baltagi 2006) and, more recently, quasi-experimental techniques such as instrumental variables and differences-in-differences (Levitt 1997, Di Tella and Schargrodsky 2004, Klick and Tabarrok 2005, Evans and Owens 2007, Machin and Marie 2011).

In the U.S. context, the typical panel data approach uses information on cities over time and regresses log crime on the log of the number of sworn police as well as additional control variables.[3] Common control variables include city effects, year effects, and measures of the age structure in the population. Frequently, city effects are not estimated using fixed effects, but rather are eliminated by taking first differences, so that the core approach is regressing year-over-year growth rates in crime on year-over-year growth rates in the number of sworn police. Generally speaking, elasticity estimates based on these panel data approaches tend to be persistently negative, but small (e.g., -0.05 to -0.15), at least for large U.S. cities in recent decades.

These findings have convinced many researchers that cities hire police officers during, or perhaps even in anticipation of, crime waves, leading even growth rate regressions to be subject to simultaneity bias (Marvell and Moody 1996, Levitt 1997, Di Tella and Schargrodsky 2004, Klick and Tabarrok 2005). These papers have

[1] Polinsky and Shavell (2000) provide a review of the theoretical deterrence literature that emerged since Becker (1968), with a particular focus on the normative implications of the theory for the organization of law enforcement strategies.

[2] The most recent survey, Lim, Lee and Cuvelier (2010), reviews 258 papers.

[3] Unlike civilian employees, sworn police officers carry a badge and a gun and have the power of arrest.

estimated the police elasticity using a variety of quasi-experimental approaches. In the main, the results from this literature are larger in magnitude than those from the panel data regression papers, which is consistent with the interpretation that cities hire police officers during or before crime waves.

Another explanation for the small magnitude of the police elasticity estimates based on least squares, relative to those from the quasi-experimental literature, is that the number of police is measured with error. As is well-known, measurement error in a covariate leads to bias on the coefficient on that covariate and possibly others as well (Griliches 1977, Ashenfelter and Krueger 1994, Ashenfelter and Rouse 1998, Wooldridge 2002). At first blush, it seems implausible that the number of police would be measured with error. Indeed, as we show, the number of police is measured well, if the metric is the bias exerted on the police coefficient in a crime regression in levels or logs with few covariates. However, the appropriate metric changes if the crime regression of interest involves many covariates, or transformations of the data such as first differences. In such contexts, as we show, the errors in measurement in the number of police are of sufficient magnitude as to exert a first-order influence on the measured elasticity of crime with respect to police.

In this paper, we present estimates of the elasticity of crime with respect to police that correct for measurement error. Our results are based on a large new panel data set on crime and policing pertaining to 242 large U.S. cities over the period 1960-2010. For each city and each year, we utilize two measures of the number of police: one based on the standard data set on police staffing collected by the Federal Bureau of Investigation (FBI) as part of its Uniform Crime Reports (UCR) program and the other based on a rarely used data set on police staffing collected by the Census Bureau as part of its Annual Survey of Government (ASG) program. The crux of our approach is to use one noisy measure of police staffing as an instrument for another noisy measure. Under the classical measurement error model, such instrumental variables estimates have the same probability limit as that of least squares, were the true measure of police available. If, as has been emphasized in the previous literature, simultaneity bias is an important additional source of bias, then the true elasticity of crime with respect to police is likely at least as large as that probability limit. Hence, our analysis may be viewed as conservative. This is an important conclusion from a normative perspective, as we document quite large police elasticities of crime.

We begin the paper with a discussion of some evidence on the extent of measurement error. We then turn to a discussion of the data, outline our methodology, and report our estimated elasticities. Before concluding, we offer suggestive calculations regarding the magnitude of the deterrence effect of police versus the incapacitation effect of police (McCrary 2009, Section 4).

II. Evidence on the Extent of Measurement Error

A. Direct Evidence

In the 2003 version of *Crime in the United States*, the Federal Bureau of Investigation reports that the New York Police Department employed 28,614 sworn police officers on October 31, 2003. Relative to the 37,240 sworn officers employed in 2002 and the 35,513 officers employed in 2004, this is a remarkably low number. If these numbers are to be believed, then the ranks of sworn officers in New York City fell by one-quarter in 2003, only to return to near full strength in 2004.

An alternative interpretion is that the 2003 number is a mistake. Panel A of Figure 1 compares the time series of sworn officers of the New York Police Department based on the UCR reports with that based on administrative data from 1990-2009.[4] These data confirm that the 2003 measure is in error and additionally suggest that the 1999 measure may be in error. These discrepancies may also support a more speculative inference that the numbers for 1963 and 1974 are in error.[5]

Administrative data on the number of officers is difficult to obtain. More readily available are departmental annual reports. However, even these are not easy to obtain; annual reports are largely internal municipal documents and historically did not circulate widely.[6] Moreover, the annual report may or may not report the number of officers employed by the police department.

Nonetheless, we have been able to obtain scattered observations on the number of sworn officers from annual reports for selected other cities in selected years: Los Angeles, Chicago, Boston, and Lincoln, Nebraska. The numbers for Chicago have been further augmented by the strength report data reported in Siskin and Griffin (2007).[7] The time series of sworn officers for these cities is given in Figure 1 in panels B through E. The figure shows that the UCR data for Los Angeles are in close correspondence with the annual report data and that the UCR data for Chicago, Boston, and Lincoln are more accurate than those for New York, but less accurate than those for Los Angeles.

Table 1 summarizes these findings. Columns correspond to the five cities and rows correspond to whether the number of officers are measured in logs or in log differences. The table highlights that, treating the administrative and annual report data as the true measure, (1) there is a broad range of fidelity in reporting to the UCR program, with Los Angeles being the most faithful, New York the least, and the others somewhere between those two book-

[4]See Data Appendix for details on these data. Special thanks to Frank Zimring for pointing us towards public domain information on New York police staffing based on his work on the New York City crime drop (Zimring 2011).

[5]We have discussed this aberrant measurement with other scholars of crime and police, both in economics and in criminology, and have neither thought nor heard of a fully plausible explanation for the source of the measurement aside from simple error.

[6]In recent years, many departments have begun a practice of posting annual reports online, but only a few cities have endeavored to post historical annual reports.

[7]See Data Appendix for details on the annual report and strength report data.

ends, and (2) after taking first differences, the correlation between the UCR data and the alternative measure falls by anywhere from an estimated 8 percent (Los Angeles) to 51 percent (New York). As noted in the introduction this is important, because much of the literature uses first-differenced data. Consequently, the much smaller correlations in the second row of the table are the relevant ones for gauging the magnitude of measurement error.

It may be surprising that there is ambiguity regarding the number of sworn officers. Errors in the measure of the number of sworn officers could arise due to (1) transitory movements within the year in the number of police, (2) conceptual confusion, and (3) typographical or data entry errors.

Figure 2 gives information on transitory movements in police staffing for Chicago for the period 1979-1997. The figure displays the monthly count of the number of sworn officers, with the count for October superimposed as horizontal lines.[8] There is evidently a great deal of within-year volatility in the number of sworn officers. Overall, the series is characterized by hiring bursts followed by the gradual decline associated with losses due to retention or retirement. Transitory movements in police officers is relevant because surveys typically ask for a point-in-time measure, and the snapshot date differs across surveys. The UCR reports a point-in-time measure as of October 31. The ASG reports a point-in-time measure as of November 1 for 1960-1995 and as of June 30 for 1997-2010.[9] Among those we have been able to examine, internal police department documents use different reporting conventions, typically corresponding to the end of the municipal fiscal year, which varies across municipalities and over time. Perhaps responding to the ambiguities of point-in-time measures, the New York City Police Department uses average daily strength in internal documents.

In addition to transitory movements, there may also be conceptual ambiguity over who counts as a sworn police officer. First, there may be confusion between the number of total employees, which includes civilians, and the number of sworn officers. Second, newly hired sworn officers typically attend Police Academy at reduced pay for roughly 6 months prior to swearing in, and there may be ambiguity regarding whether those students count as sworn officers prior to graduation. Third, there is often a discrepancy between authorized and deployed strength.[10] For our main sample of cities, we have measures of the number of authorized and deployed sworn officers for selected recent years from the Law Enforcement Management and Administrative Statistics (LEMAS). These data show that the number of deployed sworn officers ranges from 62 to 128 percent of authorized strength.[11]

[8] We are not aware of any public-use data sets containing information on within-year fluctuations in police staffing. During the period 1979-1997, a unique non-public dataset on sworn officers in Chicago is available to the authors, however, that allows the construction of monthly counts. These data are discussed in Siskin and Griffin (2007) and were previously used in McCrary (2007). See Data Appendix for details.

[9] No annual ASG survey was conducted in 1996.

[10] Authorized strength refers to the number of officers the department has authority from the city government to employ, whereas deployed strength refers to the actual number of employees.

[11] Numbers refer to a pooled analysis of data from 1987, 1990, 1993, 1997, 1999, 2000, and 2003. Population weighted mean

Finally, the measurement of sworn police in the UCR system seems to be subject to errors that are inconsistent with transitory movements within the year in the number of sworn police officers and inconsistent with conceptual confusion. For such errors, we have no other explanation than typographical or data entry error. However, categorizing errors in this way is not meaningfully different from acknowledging that some errors, such as those for New York in 2003 for example, have no easy explanation.

B. Comparison of Two Noisy Measures

Police department internal documents are presumably more accurate than the information police departments report to the UCR program. However, as discussed, these are only available in selected cities and selected years. Trading off accuracy for coverage, we now present a comparison of the UCR series on the number of sworn officers with a series based on the ASG. We use the ASG data to construct an annual series on full-time sworn officers for all the cities in our main analysis sample. We define this sample and give background on the ASG data in Section III, below.

Figure 3 provides visual evidence of the statistical association between the UCR and ASG series for sworn officers, measured in logs (panel A) and first differences of logs, or growth rates (panel B). In panel A, we observe a nearly perfect linear relationship between the two measures, with the majority of the data points massed around the 45° line. The regression line relating the log UCR measure to the log ASG measure is nearly on top of the 45° line, with a slope of 0.99. Panel B makes it clear that differencing the data substantially reduces the statistical association between the UCR and ASG series; the slope coefficient for the log differenced data is just 0.21.

To appreciate the implications of these findings for quantification of the police elasticity of crime, we provisionally turn to a classical measurement error model. This model posits that the two observed series on police are related to a single latent measure as

$$S_i = S_i^* + u_i \tag{1}$$

$$Z_i = S_i^* + v_i \tag{2}$$

Here, S_i is the UCR measure in a given city and year, Z_i is the ASG measure, S_i^* is the latent variable or *signal*, and u_i and v_i are mean zero measurement errors that are mutually independent, independent of the signal, and independent of other measurable factors as well.[12]

and standard deviation are 97 percent and 5 percent, respectively. The LEMAS data also allow us to discount the possibility that there is error due to ambiguities among sworn officers, full-time sworn officers, or full-time-equivalent sworn officers, as only 1 to 2 percent of officers appear to work part-time.

[12]For example, the classical measurement error model typically combines these equations with an equation pertaining to an outcome that depends on S_i^*. Then the additional maintained assumption is that the measurement errors u_i and v_i are independent of the structural error term associated with the outcome equation.

This simple statistical model implies that the covariance between the UCR and ASG is given by the variance of the signal and that the population regression of the UCR measure on the ASG measure yields a coefficient on the ASG measure of $r = V[S_i^*] / (V[S_i^*] + V[v_i])$, a quantity known as the reliability ratio. Under this model, we interpret the slope coefficient of 0.99 in panel A to mean that the variance of the noise is approximately 1 percent as large as the variance of the signal.[13] In panel B, the data are measured in first differences. In that context, the slope is 0.21, indicating that the variance of the noise is 3.8 times as large as the variance of the signal. That first-differencing the data dramatically reduces the reliability ratio has been well-understood since at least Griliches (1977). Intuitively, first-differencing removes variance from the signal, but increases the variance in the measurement errors.

A standard result in econometrics, noted in Wooldridge (2002, p. 75) for example, is that under the classical measurement error model the probability limit of the slope coefficient in a bivariate regression of one variable on the other, where the other variable has a reliability ratio of r, is the target parameter times r. This is referred to as "attenuation bias" because while the estimand retains the correct sign, the magnitude of the estimand is attenuated, or biased towards zero. Consequently, since $r = 0.21$ for the data in growth rates, the classical measurement error model suggests inflating the slope coefficient in a regression of growth rates in crime on growth rates in police by 4.76.

When further control variables are added, all of which are measured without error, then the relevant reliability ratio becomes $r = V[\xi_i] / (V[\xi_i] + V[v_i])$, where ξ_i is the error term in the population regression of S_i^* on all of the control variables in the model (cf., Angrist and Krueger 1999, for example). Since $V[\xi_i] \leq V[S_i^*]$, we conclude that once control variables are added to the model, it may be appropriate to inflate regression estimates by a factor larger than 5. When further control are added, not all of which are measured without error, measurement error bias no longer has the attenuation bias form. We return to this issue in Section IV, below.

III. Data

Virtually all empirical studies of the effect of police on crime use data from the UCR, collected annually by the FBI. Crime measures represent the total number of offenses known to police to have occurred during the calendar year and are part of the "Return A" collection.[14] Sworn police are included in both the Law Enforcement Officers Killed or Assaulted (LEOKA) collection and the Police Employees (PE) collection and represent a snapshot as of October 31st of the given year. Because of the late date of the measurement of the number of police, it is typical to measure police in year t using the LEOKA file from year $t-1$ (cf., Levitt

[13] That is, upon rearrangement, $V[v_i]/V[S_i^*] = (1-r)/r$.

[14] Time series for each of the crime rates utilized for each of our cities are shown in Appendix Figure 1.

1997), and we follow that convention here. Consequently, although we have data on levels from 1960-2010, our regression analyses of growth rates pertain to 1962-2010.

As noted above, we augment data from the UCR with data from the Annual Survey of Government (ASG) Employment, an annual survey of municipal payrolls that has been administered by the Bureau of Labor Statistics and reported to the U.S. Census annually since 1952. The ASG data provide payroll data for a large number of municipal functions including elementary and secondary education, judicial functions, public health and hospitals, streets and highways, sewerage and police and fire protection among others. The survey generally provides information on the number of full-time, part-time and full-time equivalent sworn and civilian employees for each function and for each municipal government.[15]

Our sample of 242 cities includes nearly all cities with a population exceeding 50,000 individuals for each year during our 1960-2010 study period.[16] Information on police staffing is available in both the UCR data and ASG data for each of these cities for the entire study period.[17] The LEOKA data provide the number of full-time sworn police officers and the total number of police officers in each year. The ASG data provide the same information beginning in 1977. Prior to 1977, the ASG series reports only the number of full-time equivalent police personnel, without differentiating between sworn officers and civilian employees. In order to extend the series, we generate a city- and year-specific estimate of the proportion of police personnel who are sworn officers using the LEOKA data. This was accomplished by regressing the proportion of police personnel who are sworn on an exhaustive set of city and year dummies using the 1960-1977 sample and generating a predicted value for the sworn percentage in each city-year. The predicted values were then multiplied by full-time equivalent officers from the ASG series prior to 1977 to generate a predicted number of sworn FTE officers. Next, in order to generate an estimate of the number of full-time sworn officers, a city-specific estimate of the average ratio of full-time equivalent officers to full-time officers was generated using the ASG data from 1977-2010.[18] Multiplying this ratio by the number of sworn FTEs yields an estimated number of sworn full-time officers for each city prior to 1977.[19]

In addition to these data we have collected historical information on several important covariates. One such control variable is city revenues. We were particularly concerned with collecting this series because of a particular causal channel which might lead regression-based estimates of the effect of police on crime to

[15]Full-time equivalent employees represent the number of full-time employees who could have been employed if the hours worked by part-time employees had instead been dedicated exclusively to full-time employees. The statistic is calculated by dividing the number of part-time hours by the standard number of full-time hours and then adding this number to the number of full-time employees.

[16]Excluded from the sample are approximately 30 cities for which more than seven years of data were missing for one or more key variables.

[17]We fill in missing observations using linear interpolation. For example, the ASG was not administered in 1996 and is taken as the average of the 1995 and 1997 levels.

[18]This ratio ranges from a low of 83% to a high of 100%, with a mean of 99.8%.

[19]Time series of the number of full-time sworn officers according to the LEOKA and ASG measures for each city are provided in Appendix Figure 2.

be negatively biased. According to this story, cities lay off police officers when the budget is tight, which coincides with a period of a weak local economy and a possible labor market link to crime. These data are from the Annual Survey of Government Finance.[20]

Another obvious control variable is the overall population. The population measure utilized in the majority of crime research is drawn from the FBI's Return A file. While this series contains valid observations for nearly all city-years, it is potentially contaminated by measurement error, particularly in the years immediately prior to the decennial Census. Accordingly, we also utilize a measure of each city's population that is contained in the annual ASG files. The ASG population measure is likewise noisy and is also often not smooth across Census year thresholds. As a result, measures of police and crime that are deflated by either of the two population measures are also not smooth across these thresholds even when the raw numbers officers and crimes are. In order to smooth the series, we generated a moving average population measure using the predictions from local linear regression with a bandwidth of 5 and the triangle kernel (Fan and Gijbels 1996).[21] These population imputations, as well as the raw data underneath them, are shown for each city in the sample in Appendix Figure 3.

We provide further evidence for the necessity of smoothing the raw population in Figures 6A and 6B. These figures present scatterplots of the growth rate in violent and property crimes against the growth rate in the the raw and smoothed population measures from both the LEOKA and the ASG file. Referring to Panel A of Figure 6A, we see that a one percent increase in the population growth rate is associated with a 0.25 percent increase in the number of violent and property crimes. This is suprising as, while the crime-population elasticity need not equal 1, on average, population and crime should closely track one another. Panel B plots the number of crimes against the smoothed LEOKA population measure. Here, the regression slopes are 0.94 and 0.84, respectively, neither of which is statistically significantly different from 1. Figure 6B reports similar results for the ASG population measure. We interpret these findings as evidence that the smoothed population measures paint a more accurate picture of changes in city population.

We additionally consider population disaggregated by age, sex, and race/ethnicity. These data, collected by the Census Bureau as part of its Population Estimates program, are only available starting in 1970.

We turn now to Table 2, which provides summary statistics for each of our two primary police measures as well as each of the seven so-called index offenses—murder, rape, robbery, aggravated assault, burglary, larceny exclusive of motor vehicle theft ("larceny"), and motor vehicle theft. We additionally report summary statistics for the aggregated crime categories of violent and property crime. The left-hand panel of Table 2 gives statistics for the levels of crime and police in per capita terms, specifically as a measure of the value

[20]See Data Appendix for details on these data.

[21]We describe the procedure we employ in greater detail in the Data Appendix to this paper.

per 100,000 population. The right-hand panel gives statistics for log differences of crime and police.

Several features of the data are worth noting. First, a typical city employs approximately 250 police officers per 100,000 population, one officer for every 4 violent crimes, and one officer for every 24 property crimes. There is considerable heterogeneity in this measure over time, with the vast majority of cities hiring additional police personnel over the study period. However, there is even greater heterogeneity accross cities, with between city variation accounting for nearly 90% of the overall variation in the measure. The pattern is somewhat different for the crime data, with a roughly equal proportion of the variation arising between and within cities.

Second, it is worth pointing out that the vast majority (87%) of crimes are property crimes with the most violent crimes (murder and rape) comprising less than 1% of all crimes reported to police. It is likewise important to note that each of the crime aggregates is dominated by a particular crime type with assault comprising nearly half of all violent crimes and larceny comprising 59% of all property crimes.

Third, and turning to the growth rates, perhaps the most relevant feature of the data is that taking first differences of the series comes close to eliminating time invariant cross-sectional heterogeneity in log crime and log police. For each measure of crime and police, the within standard deviation in growth rates is essentially equal to the overall standard deviation. Moreover, in results not shown, taking first differences of a per capita measure fully eliminates cross-sectional heterogeneity.

Figure 4 highlights long-run trends in crime and police. Panels A, B, and C present the time series for total violent crime, total property crime, and total sworn officers for our sample of 242 cities, 1960-2010. The series show a remarkable 30 year rise in criminality from 1960 to 1990, followed by an equally remarkable 20 year decline in criminality from 1990 to 2010. These swings are spectacular in magnitude. Violent crimes are below 200,000 in 1960, rise to well over 800,000 by 1990, and then decline to just below 500,000 by 2010. Property crimes are below 1.5 million in 1960, rise to 4.5 million by 1990, and then decline to below 3 million by 2010.

The series for sworn police shows quite different secular trends. The 1960s is a decade of strong gains, from 110,000 officers to 150,000 officers, with acceleration evident after the wave of riots 1965-1968, followed by a slower rate of increase during the first half of the 1970s. During the second half of the 1970s, we see an era of retrenchment, perhaps related to urban fiscal problems. From 1980 to 2000, sworn police generally increase, with particularly strong increases in the 1990s. Since 2000 the numbers are roughly flat, with the exception of 2003, which is driven entirely by the erroneous estimate provided by the New York City Police Department to the UCR program (cf., Figure 1).

Throughout our analysis, we focus on year-over-year growth rates in crime and police and further absorb the secular trends by including year effects as covariates. Interestingly, this is also the performance metric used by most police departments, at least based on the annual reports we have been able to examine. That is, they discuss year-over-year growth rates and compare their numbers to year-over-year growth rates in national averages.

Our focus on this transformation implies that the source of identifying information in our estimation strategies is related to the temporal changes in the standard deviation of year-over-year growth rates. The bottom part of panels A, B, and C show these standard deviations and how they have evolved over time. The figure shows that there has been some slight decline in the standard deviation of the growth rate of sworn police over time. An interesting pattern is the strong spike in the standard deviation of the crime growth rates around 1990. This pattern is attributable to differences across cities in the date of the peak of crime. Around 1990, some cities are still experiencing the wave of violence related to the crack epidemic, while other cities are already seeing the beginnings of the crime decline. Generally speaking, however, all time periods seem equally likely on an a priori basis to be informative regarding the effect of police on crime and so we focus on estimates that are based on all available years.

IV. Econometric Approach

Our first equation of interest is

$$Y_i = \theta_0 S_i^* + \gamma_0' X_i + \epsilon_i \tag{3}$$

where Y_i is the first difference of log crime in a given city and year, S_i^* is the first difference of the log of the true number of police, and X_i is a vector of control variables such as log revenues per capita, log population, the demographic structure of the population, all measured in first differences, as well as year effects or state-by-year effects. We interpret the parameter θ_0 as what might be termed the short-term police elasticity of crime. We do not address in this paper the interesting question of whether a short-term innovation to the level of police in a city has a smaller or larger effect than a long-term innovation to the level of police.

We can combine this equation with the measurement error model given in equations (1) and (2) by substituting equation (1) into equation (3) and by linearly projecting S_i onto Z_i and X_i. This yields

$$Y_i = \theta_0 S_i + \gamma_0' X_i + \varepsilon_i \tag{4}$$

$$S_i = \pi_{01} Z_i + \pi_{02}' X_i + \nu_i \tag{5}$$

where $\varepsilon_i = \epsilon_i - \theta_0 u_i$ and ν_i is a linear projection error. This is then a standard simultaneous equations model where Z_i is potentially an instrument for S_i. Estimation proceeds straightforwardly by IV since the model is just-identified, and 2010 city population is used as a weight to obtain a police elasticity estimate representative of the typical person living in our sample of cities. We next articulate the precise assumptions that justify the exclusion of Z_i from equation (4). We assume that

$$\begin{array}{lrcl} (A1) & (u_i, v_i) & \perp\!\!\!\perp & \epsilon_i \\ (A2) & (u_i, v_i) & \perp\!\!\!\perp & (S_i^*, X_i) \\ (A3) & u_i & \perp\!\!\!\perp & v_i \\ (A4) & \epsilon_i & \perp\!\!\!\perp & (S_i^*, X_i) \end{array}$$

where u_i and v_i are the measurement errors from equations (1) and (2).

Assumptions $(A1)$ through $(A3)$ assert that the measurement error in the ASG measure of police is independent of the structural error term in equation (3), the true growth rate in police, and of the measurement error in the UCR measure. We discuss empirical implications of assumptions $(A1)$ through $(A3)$ below.

Assumption $(A4)$ is innocent if we maintain that we would be interested in running a regression of crime growth rates on police growth rates and controls X_i, were police growth rates observed without error. On the other hand, $(A4)$ may reasonably be called into question. In particular, we present evidence below that city population growth rates are measured with error. City population growth is a sufficiently important confounder that we feel the (infeasible) regression model implied by equation (3) and assumption $(A4)$ would not be of interest unless X_i included it.[22] We discuss the challenges of mismeasurement of city population growth in greater detail below.

Most of the previous empirical papers utilizing instrumental variables strategies to address measurement error have focused on the estimated return to education among samples of twins (see Card (1999) for a review of this literature). The set of econometric issues raised in those papers is slightly different than in our context. In particular, in those papers, surveyed twins are asked about their own schooling and that of their twin. Within each twin pair, individuals are selected at random to be "twin 1" and "twin 2", and twin 2's report of the twin schooling difference is used as an instrument for twin 1's report of the difference. Even in the event that classical measurement error were violated, the IV estimates in those papers are expected to be statistically and economically indistinguishable from IV estimates that instead used twin 1's report as an instrument for twin 2's report. This follows from the simple fact that twin number is randomly assigned.

In our context, however, the different measures arise from substantively different measurement processes. In the absence of classical measurement error, we would expect to get different results when using the UCR measure as an instrument for the ASG measure than when using the ASG measure as an instrument for the UCR measure. For example, one could imagine that since the UCR data have been the basis for most of the papers in the literature, specification searching might have led to a correlation between u_i and ϵ_i. Since no previous paper has utilized the ASG series, specification searching would not have led to a similar correlation between v_i (the ASG error) and ε_i. A variety of alternative violations of classical measurement error would

[22] In times of population growth, police force size and crime both grow mechanically. This leads to a positive bias in the estimated police elasticity for specifications that omit population growth.

lead to differences between "forward" (ASG as an instrument for UCR) and "reflected" (UCR as an instrument for ASG) IV estimates. In particular, note that under classical measurement error, the same steps we used to motivate the simultaneous equations model in equations (4) and (5) can be used to motivate a second simultaneous model with the roles of S_i and Z_i reversed and identical parameters in equation (4).

This raises the possibility of an omnibus test of the classical measurement error model, testing the equality of the "forward" and "reflected" IV estimands. To implement this test, and to present more efficient estimates of the police elasticity, we consider generalized method of moments (GMM) estimates using moments

$$g_i(\beta) = W_i \begin{pmatrix} Z_i(Y_i - \theta_1 S_i - \gamma_1' X_i) \\ X_i(Y_i - \theta_1 S_i - \gamma_1' X_i) \\ S_i(Y_i - \theta_2 Z_i - \gamma_2' X_i) \\ X_i(Y_i - \theta_2 Z_i - \gamma_2' X_i) \end{pmatrix} \tag{6}$$

where W_i is 2010 city population in levels and all other variables are as defined before. When the parameters θ_1 and θ_2 and γ_1 and γ_2 are allowed to differ, estimating those same parameters by GMM is equivalent to estimating them separately by IV and correcting the standard errors for the common dependent variable.

Once we impose the restrictions $\theta_1 = \theta_2$ and possibly $\gamma_1 = \gamma_2$, however, GMM estimates implicitly average the unrestricted IV estimates, leading to efficiency gains. The omnibus test of the classical measurement error model is then available as the standard GMM test of overidentifying restrictions.

The system of moments in equation (6) has $2(K+1)$ moments and, with the fully restricted model with $\theta_1 = \theta_2$ and $\gamma_1 = \gamma_2$, has $K+1$ parameters, where X_i has K elements. Since the degree of overidentification is somewhat large, there is value in exploring empirical likelihood (EL) estimates of the police elasticity and associated overidentifying tests (Imbens 1993, Qin and Lawless 1994, Imbens 2002).

Above, we noted that our context differs from that of the twins literature in that the sources of our two measures have the well-defined labels "UCR" and "ASG" which may convey information about the measurement errors themselves. In the twins context, the sources of the two measures have the labels "twin 1" and "twin 2", which have no substantive content, since they were randomly assigned. One interesting implication of this simple fact, as discussed, is that we have available an omnibus test of the classical measurement error model, namely a test of the equality of the "forward" and "reverse" IV estimands. A second interesting implication is that more focused tests of aspects of the classical measurement error model are available as well. For example, under the model in equations (1) and (2), the difference $S_i - Z_i$ represents the measurement error difference $u_i - v_i$. Under assumptions $(A1)$ and $(A2)$, this quantity should be unrelated to crime growth rates, which is a testable implication. Moreover, assumption $(A3)$ is testable with a third measure of police, which we have for selected years from the LEMAS survey. For those years, $(A3)$ implies that $S_i - Z_i$ should be unrelated to the LEMAS measure, which is again testable. Similarly, letting $\widetilde{Z}_i$ denote the LEMAS measure, the difference $S_i - \widetilde{Z}_i$ should be unrelated to Z_i and $Z_i - \widetilde{Z}_i$ should be unrelated to S_i. Anticipating what is to come, there is little evidence in our data against these stochastic restrictions.

As noted, a challenge we face in implementing the above ideas is that both police and population growth rates may be measured with error. As is well-known, measurement error bias has the form of attenuation bias only if a single covariate is measured with error (cf., Angrist and Krueger 1999). In our context, the important elements of X_i are fixed effects, which are innocent, and city population growth, which may be problematic. We next discuss our approach to addressing the measurement error in population.

There is no consensus choice of reliable information on city population annually. Both the UCR and ASG data systems ask cities for information on population, and as noted the UCR measure is used throughout the crime literature. However, the accuracy of these measures is not obvious, and they often disagree. A potential solution to the measurement problems with city population growth is to again use the UCR measure as an instrument for the ASG measure.

This brings us to our second equation of interest,

$$Y_i = \theta_0 S_i^* + \phi_0 P_i^* + \gamma_0' X_i + \epsilon_i \tag{7}$$

where now X_i is redefined to include only fixed effects. First differenced log population, measured without error, is now written explicitly as P_i^*. We couple this structural equation with an expanded version of the classical measurement error model given above,

$$\begin{aligned} S_i &= S_i^* + u_{1i} && (8) \\ Z_i &= S_i^* + v_{1i} && (9) \\ P_i &= P_i^* + u_{2i} && (10) \\ Q_i &= P_i^* + v_{2i} && (11) \end{aligned}$$

Substituting equations (8) and (10) into equation (7) and linearly projecting S_i onto Z_i, Q_i, and X_i, as well as P_i onto Z_i, Q_i, and X_i, we arrive at the simultaneous equations model

$$\begin{aligned} Y_i &= \theta_0 S_i + \phi_0 P_i + \gamma_0' X_i + \varepsilon_i && (12) \\ S_i &= \pi_{011} Z_i + \pi_{012} Q_i + \pi_{013}' X_i + \nu_{1i} && (13) \\ P_i &= \pi_{021} Z_i + \pi_{022} Q_i + \pi_{023}' X_i + \nu_{2i} && (14) \end{aligned}$$

where now $\varepsilon_i = \epsilon_i - \theta_0 u_{1i} - \phi_0 u_{2i}$. The precise conditions justifying the exclusion restriction in this simultaneous equations model are now given by

$$
\begin{array}{lrcl}
(A1') & (u_{1i}, v_{1i}, u_{2i}, v_{2i}) & \perp\!\!\!\perp & \epsilon_i \\
(A2') & (u_{1i}, v_{1i}, u_{2i}, v_{2i}) & \perp\!\!\!\perp & (S_i^*, P_i^*, X_i) \\
(A3a') & u_{1i} & \perp\!\!\!\perp & (v_{1i}, u_{2i}, v_{2i}) \\
(A3b') & v_{1i} & \perp\!\!\!\perp & (u_{1i}, u_{2i}, v_{2i}) \\
(A3c') & u_{2i} & \perp\!\!\!\perp & (v_{1i}, u_{2i}, v_{2i}) \\
(A3d') & v_{2i} & \perp\!\!\!\perp & (v_{1i}, u_{2i}, v_{2i}) \\
(A4') & \epsilon_i & \perp\!\!\!\perp & (S_i^*, P_i^*, X_i)
\end{array}
$$

Above, the symmetry of the roles of S_i and Z_i led to a GMM system with $2(K+1)$ moments. Here, S_i and Z_i continue to play symmetric roles, but so also do P_i and Q_i. This turns out to lead to a GMM system with $4(K+2)$ moments. Specifically, rather than substituting into equation (7) equations (8) and (10), we could instead substitute equations (8) and (11), or (9) and (10), or (8) and (11). Coupled with the associated linear projections, this would lead to four related IV problems, all of which have the same structural parameters as equation (7). The corresponding moments for the GMM estimator are

$$
g_i(\beta) = W_i \begin{pmatrix}
Z_i(Y_i - \theta_1 S_i - \phi_1 P_i - \gamma_1' X_i) \\
Q_i(Y_i - \theta_1 S_i - \phi_1 P_i - \gamma_1' X_i) \\
X_i(Y_i - \theta_1 S_i - \phi_1 P_i - \gamma_1' X_i) \\
S_i(Y_i - \theta_2 Z_i - \phi_2 P_i - \gamma_2' X_i) \\
Q_i(Y_i - \theta_2 Z_i - \phi_2 P_i - \gamma_2' X_i) \\
X_i(Y_i - \theta_2 Z_i - \phi_2 P_i - \gamma_2' X_i) \\
Z_i(Y_i - \theta_3 S_i - \phi_3 Q_i - \gamma_3' X_i) \\
P_i(Y_i - \theta_3 S_i - \phi_3 Q_i - \gamma_3' X_i) \\
X_i(Y_i - \theta_3 S_i - \phi_3 Q_i - \gamma_3' X_i) \\
S_i(Y_i - \theta_4 Z_i - \phi_4 Q_i - \gamma_4' X_i) \\
P_i(Y_i - \theta_4 Z_i - \phi_4 Q_i - \gamma_4' X_i) \\
X_i(Y_i - \theta_4 Z_i - \phi_4 Q_i - \gamma_4' X_i)
\end{pmatrix} \tag{15}
$$

Imposing the restrictions $\theta_1 = \theta_2 = \theta_3 = \theta_4$, $\phi_1 = \phi_2 = \phi_3 = \phi_4$, and $\gamma_1 = \gamma_2 = \gamma_3 = \gamma_4$ results in increased efficiency, yet also raises a new issue: redundancy of moments. In particular, define the row vectors $R_1 = (-1, 0, 0_K', 1, 0, 0_K', 1, 0, 0_K', -1, 0, 0_K')$ and $R_2 = (0, -1, 0_K', 0, 1, 0_K', 0, 1, 0_K', 0, -1, 0_K')$ and the $K \times 4(K+2)$ matrix $R_3 = (0_K, 0_K, -I_K, 0_K, 0_K, I_K, 0_K, 0_K, I_K, 0_K, 0_K, -I_K)$ and note that

$$
\begin{aligned}
((1/\phi_1)R_1 - (1/\theta_1)R_2)\ g_i(\beta) &= 0 \qquad (16) \\
R_3\ g_i(\beta) &= 0_K \qquad (17)
\end{aligned}
$$

These turn out to be the only linear combinations of the moments that yield zero. This implies that once we impose the full set of restrictions, we only have $4(K+2) - (K+1) = 3K+7$ linearly independent moments, rather than $4(K+2)$.

The most direct way to address redundancy is simply to drop equations from the overall system until the moments are linearly independent. However, an unfortunate feature of GMM is that estimates are not invariant to which set of $K+1$ moments are dropped. This turns out to affect our elasticity estimates in the second decimal place. This is related to the lack of invariance of over-identified GMM to linear transformations of the moments.[23]

In recent years a number of competitors to GMM have emerged for solving method of moments problems (see Imbens (2002) for a review). These estimators have the same first-order asymptotic properties as GMM, but different higher-order properties. An important advantage of this class of estimators is that estimates are numerically invariant to which set of moments we elect to drop. We focus here on EL, which is one such estimator. EL is known to exhibit small second-order asymptotic bias (Newey and Smith 2004), good power (Owen 2001), and potentially a more reliable test of the overidentifying restrictions (Imbens et al. 1998). We briefly describe the idea of EL estimation and our computational approach, but refer the reader to the literature for additional details.

Following Imbens (1993) and Qin and Lawless (1994), EL estimates are defined as the solution to

$$\max_{p_1,p_2,\ldots,p_n,\beta} \sum_i \ln p_i \quad \text{s.t.} \quad p_i \geq 0,\ 1 = \sum_i p_i,\ \text{and}\ \ 0 = \sum_i p_i g_i(\beta) \tag{18}$$

where now we redefine $g_i(\beta)$ to be a set of $L \equiv 3K + 7$ linearly independent moments from among the original $g_i(\beta)$. Computing the empirical likelihood estimator is not straightforward, however, due to the non-linearity of the program, even for moment problems where $g_i(\beta)$ is linear in β. Following Imbens (1997), Newey and Smith (2004), and Guggenberger and Hahn (2005), the empirical likelihood estimator for β can be viewed as the solution to a program in terms of β, and a vector of L Lagrange multipliers λ, given by

$$\min_\beta \max_\lambda \sum_i \log\left(1 + \lambda' g_i(\beta)\right) \tag{19}$$

and the Lagrange multipliers are related functionally to the empirical likelihood probabilities as $np_i = (1 + \lambda' g_i(\beta))^{-1}$. Let $\alpha = (\lambda', \beta')'$. The saddlepoint problem above has first order conditions

$$m_i(\alpha) = \frac{1}{1 + \lambda' g_i(\beta)} \begin{pmatrix} g_i(\beta) \\ G_i' \lambda \end{pmatrix} \tag{20}$$

which have a derivative matrix

$$M_i(\alpha) \;=\; \frac{1}{1 + \lambda' g_i(\beta)} \begin{pmatrix} 0_{L\times L} & G_i \\ G_i' & 0_{J\times J} \end{pmatrix} - \frac{1}{(1 + \lambda' g_i(\beta))^2} \begin{pmatrix} g_i(\beta) g_i(\beta)' & g_i(\beta)\lambda' G_i \\ G_i' \lambda g_i(\beta)' & G_i' \lambda\lambda' G_i \end{pmatrix} \tag{21}$$

Following Guggenberger and Hahn (2005), we use Newton iteration with a stepsize of one, starting from the initial condition $\alpha = (0_L, \widehat{\beta}_2')'$ where $\widehat{\beta}_2$ is the two-step GMM estimator.[24] As those same authors note, three iterations suffice for second-order asymptotic equivalence to the fully iterated empirical likelihood estimator.

[23]On this point from a more general perspective see, for example, Hansen, Heaton and Yaron (1996), Imbens (1997), Imbens, Spady and Johnson (1998), or Hall (2005).

[24]That is, we estimate $\widehat{\Omega} = \frac{1}{n}\sum_i g_i(\widehat{\beta}_1) g_i(\widehat{\beta}_1)'$, where $\widehat{\beta}_1 = \arg\min_\beta \overline{g}(\beta)'\overline{g}(\beta)$, and compute $\widehat{\beta}_2 = \arg\min_\beta \overline{g}(\beta)'\widehat{\Omega}^{-1}\overline{g}(\beta)$.

Moreover, in our application, iterating more than three times is sufficient for convergence and changes the estimates only in the 7th or 8th decimal place.

The EL test of the overidentifying restrictions is computed as

$$T = 2\sum_{i=1}^{n} \ln(1 + \widehat{\lambda}' g_i(\widehat{\beta})) \tag{22}$$

We have not yet explored the Lagrange multiplier approach advocated by Imbens et al. (1998).

V. Results

We begin our discussion of the results with an examination of the first stage relationship between growth rates in the two measures of police, presented in Table 3A. The first five columns of Table 3A present coefficients and standard errors from models in which the growth rate in the LEOKA measure is regressed on the growth rate in the ASG measure. These models correspond to what we term our "forward" regressions, models in which the LEOKA measure is employed as the endogenous measure of police that is measured with error and the ASG measure is employed as the instrumental variable. The final five columns present results arising from a regression of the ASG measure on the LEOKA measure. We refer to models arising from this formulation as our "reflected" regressions. We begin, in specification (1), by presenting a regression of the growth rate in the LEOKA measure on the growth rate in the ASG measure, conditional on the growth rate in the city's population and a vector of year dummies. This specification is standard in the literature. In column (2), we add the first difference in the log of the city's total budgetary expenditures exclusive of police expenditures to capture time-varying shocks to a city's budget cycle. Each of the first two columns pertains to the entire 1960-2010 sample period. Columns (3)-(5) refer to the period spanning 1970-2010, the years for which detailed city-level demographic data are available. Column (3) presents estimates that are equivalent to those in column (2) with the exception that results are based on the 1970-2010 sample. In column (4), we add a series of log differenced demographic controls. These capture year-over-year changes in the proportion of a city's population that is comprised of sixteen age-race-gender subgroups.[25] Finally, in column (5), we include an unrestricted set of polynomials and interactions between each of the demographic variables in order to flexibly model the effect of changes in a city's demographic composition on its growth rate in crime. Comparisons between column (3) and columns (4) and (5) reveal the extent to which the first stage relationship between the growth rates in the police measures is robust to demographic controls. Columns (6)-(10) are equivalent to columns (1)-(5) but

[25]The demographic variables arise from a fully interacted set of variables that consist of two races (white and nonwhite), four age groups (0-14, 15-24, 25-39 and >40) and both genders).

pertain to the reflected first stage regressions. Throughout Table 3A, and in subsequent tables, we report two sets of standard errors: Huber-Eicker-White standard errors that are robust to the presence of heteroskedasticity are reported in parentheses below the coefficient estimates and robust standard errors clustered at the city level are reported below in square brackets. These different standard error approaches are highly similar. The F-statistic on the excluded instrument is reported below the coefficient estimates as a standard test of instrument relevance. As the smallest F-statistic we report exceeds 100, subsequent IV estimates reported throughout the paper do not suffer from common problems that are associated with weak instruments.

The coefficients reported in Table 3A provide a measure of the relatedness of the growth rates in each of the two sworn officer series. Consistent with the scatterplots presented in Figure 3, the coefficients reported in Table 3A are relatively small in magntiude, indicating that each measure contains an appreciable amount of noise. Referring for example, to column (1) of Table 3A, we observe that, conditional on the growth rate in population, a one percent increase in the ASG measure is associated with only a 0.17 percent increase in the LEOKA measure. Put differently, the growth rate in the ASG measure explains just 13 percent of the variation in the growth rate of the LEOKA measure. Referring to the remaining columns in the table, we likewise report evidence that the magnitude of the coefficients is relatively insensitive to the inclusion of controls for budget cycles and demographics.[26] Referring to columns (6)-(10) which report results for the reflected first stage regressions, we observe coefficients that are substantially larger in magnitude than the coefficients in columns (1)-(5). These differing magnitudes are expected since the LEOKA measure of police growth rates exhibits less variance than the ASG measure, as the first stage coefficient is the covariance between the two measures, relative to the variance of the predicting variable. The results differ only slightly when additional controls are added to the specification.

In Table 3B, we present coefficients arising from a series of least squares models of the effect of police on each of seven crime types and two crime aggregates, maintaining the same table structure introduced in Table 3A. Consistent with least squares results reported by prior researchers, we report modest elasticities of crime with respect to police. We begin our discussson referring to column (1) of Table 3B, which conditions only on the growth rate in population and year fixed effects. Using the LEOKA measure of police officers, these elasticities are largest for murder (-0.27), robbery (-0.20) and motor vehicle theft (-0.19). All three elasticities are statistically significant at conventional significance levels. Overall, the elasticity is greater for violent crime (-0.12) than for property crime (-0.07). As with the first stage results, the estimated elasticites are surprisingly insensitive to the inclusion of control variables for either budget cycles or demographic composition.[27] Columns

[26]We note that the estimated coefficient is approximately 10 percent smaller with the inclusion of polynomials and interactions in demographics.

[27]Referring to column (2) which adds controls for the budget cycle and changes in a city's demographic composition, the results are nearly identical to those in column (1). Likewise, estimates reported in columns (4) and (5) are similar to those reported in column (3).

(6)-(10) report results for models in which the growth rate in crimes is regressed on the growth rate in the ASG measure of police.[28] While the coefficients in columns (6)-(10) are smaller in magnitude, reflecting a weaker association between this measure of police and crime, they are also more precisely estimated with significant coefficients for murder (-0.23), robbery (-0.09) and motor vehicle theft (-0.09). Taken as a whole, least squares estimates of the elasticity of crime with respect to police point to a modest but persistent relationship between changes in police manpower and criminal activity. To underscore this point, regardless of whether we rely on the forward or the reflected regressions, we note that a 10 percent increase in the size of a city's police force (which would correpond to an unusually large and costly change in the policy regime) is predicted to lead to only a 1 percent reduction in the rate of violent and property crimes. Many researchers confronting such estimates have concluded that least squares estimates are inconsistent due to simultaneity bias.

In Tables 3C we report IV estimates of each crime elasticity that correct for measurement error. These estimates are typically four to five times larger in magnitude than those estimated via least squares.[29] Referring to column (1) which uses the full 1960-2010 sample to estimate the "forward" IV regressions, the largest elasticites are those for murder (-1.34), motor vehicle theft (-0.50), robbery (-0.48) and burglary (-0.23). In addition, we report precisely estimated elasticities for each of the two crime aggregates of -0.32 for violent crimes and -0.14 for property crimes. The elasticities arising from the "reflected" IV regressions reported in columns (6)-(10) exhibit a similar pattern though the estimated coefficients are substantially smaller in magnitude with elasticities for murder, robbery and motor vehicle theft of -0.72, -0.52 and -0.52, respectively. Elasticities for the crime aggregates are -0.32 for violent crimes and -0.18 for property crimes.

The elasticities reported in Table 3C reveal considerable attenuation in least squares coefficients resulting from the presence of measurement errors in the police series. Given the degree of the attenuation, it should be clear that measurement error is a prominent factor underlying discrepancies between least squares and IV coefficients that have been estimated in prior research. However, because the potential for simultaneity bias remains, the models estimated in Table 3C do not convincingly identify a "state-of-the-art" causal estimate of the effect of police on crime. That is, while these models remove between-city variation via differencing and control for national crime trends, city-specific budget cycle shocks and changes in a city's demographic composition, we are unable to rule out the existence of unit and time-varying confounders which are correlated with both changes in the size of a city's police force and its crime rate. In particular, it is possible that

[28]To our knowledge, this is the first time a panel data regression of crime on the ASG measure has been run. This important, because of the possibility of specification searching mentioned above.

[29]A familiar result is that the IV estimate can be recovered by dividing the "reduced form" estimate of the police elasticites in Table 3B by the first stage estimate presented in Table 3A. In this context, to recover the forward IV coefficients presented in columns (1)-(5) of Table 3C, we would divide the reflected least squares coefficients in columns (6)-(10) of Table 3B by the first stage coefficient. Due to the presence of missing data, this arithmetic does not exactly reproduce the relevant IV coefficients.

changes in regional macroeconomic conditions, idiosyncratic shocks to regional crime markets or changes in state-level criminal justice policies, each of which is unaccounted for in the models presented in Table 3, will lead to inconsistent parameter estimates. The omission of time-varying state-level policy variables is especially concerning as the adoption of a "get tough on crime" attitude among a state's lawmakers (or its citizens) might plausibly lead to both increases in police and more punitive sentencing policies. The result would be a negatively biased police elasticity (too large in magnitude) as we would mistakenly attribute some portion of increased punitiveness to the effect of increases in police manpower.

Fortunately, since sentencing policy is determined almost entirely at the state level, we can address this potential source of bias with the inclusion of a set of unrestricted state-by-year fixed effects. These state-by-year effects add an additional 1,700 parameters to each set of IV estimates, but also increase the R^2 to nearly 60 percent for most crime categories.

Tables 4A, 4B and 4C report first stage, least squares and IV estimates for models that include an exhaustive set of unrestricted state-by-year effects. In Table 4A, we observe that the relatedness between the growth rates LEOKA and ASG measures of police is very similar to results reported in Table 3A, with the estimated coefficient in the forward regression declining from approximately 0.17 to 0.15. Consistent with the extraordinary explanatory power of the state-by-year effects, we note that in both the forward and reflected first stage regressions, the effect of the control variables for budget cycles and demographic composition is greatly diminished conditioning on the state-by-year effects. However, as the relationship between the police measures is not heavily related to the state-by-year effects, the F-statistic on the excluded instrument remains quite high, continuing to exceed a value of 100 in all cases.

Table 4B reports least squares estimates of the effect of police on crime, inclusive of the state-by-year effects. Referring to column (1), the elasticities for the violent and property crime aggregates are -0.13 and -0.05 respectively, with both elasticities meeting the standard threshold for statistical significance. Elasticities are largest for murder (-0.22), robbery (-0.21) and motor vehicle theft (-0.13). The reflected least squares estimates are likewise similar to those reported in Table 3B with a violent crime elasticity of -0.06 and a property crime elasticity of -0.02. [30] Finally, in Table 4C, we present IV results that correct for attentuation bias in least squares. Conditional upon state-by-year effects, we report a violent crime elasticity that is approximately -0.35 and a property crime elasticity that is approximately -0.14. With regard to the individual crimes, elasticities are largest for murder (between -0.51 and -0.83), robbery (between -0.50 and -0.59), motor vehicle theft (between -0.26 and -0.37) and burglary (between -0.16 and -0.29). While the coefficient on robbery does not change appreciably from Table 3C to

[30] Notably, the murder elasticity is substantially smaller at -0.13.

Table 4C, coefficients on murder and motor vehicle theft are approximately 50 percent smaller with the inclusion of the unrestricted state-by-year effects as compared to the standard first diffrencing specification. We interpret this as evidence in favor of the presence of substantial time-varying unobserved heterogeneity at the state-level.

We have, thus far, privileged estimates in columns (1)-(5) of Tables 3 and 4 to estimates reported in columns (6)-(10). We do so because the primary measure of police that is employed in prior research is the LEOKA measure drawn from the FBI's Uniform Crime Reports and, as such, mismeasurement in this series is of greater relevance in comparing our results to those reported in the extent literature. However, with regard to estimating a police elasticity, it is important to note that we have no *a priori* reason to prefer the estimated elasticities in columns (1)-(5) to those in columns (6)-(10). In principle, both the forward and reflected IV regressions contain valuable information in estimating the responsiveness of crime to changes in the number of police personnel. Accordingly, a state-of-the-art estimate of the effect of police on crime should draw upon information contained in both sets of estimates. In Table 5, we present pooled estimates of the elasticity of crime with respect to police that efficiently combine information from both the forward and reflected IV regressions presented in Table 4C. For each crime type and for each of two measures of the city's population, Table 5 computes an estimated elasticity with robust standard errors in parentheses below the reported coefficients. Pooled estimates are computed both via one-step and two-step GMM estimation and via empirical likelihood (EL).[31] Panel A reports pooled elasticities for models which use the growth rate in the city's population in the LEOKA file as a control variable while Panel B reports pooled elasticities using the growth rate in population measure from the ASG file. As we are pooling information conatined in the forward and the reflected IV regressions, the elasticities reported in Panel A are smaller in magnitude than those reported in column (1) of Table 4C, but larger in magnitude than those in column (5), and are estimated with enhanced precision as standard errors are approximately 12 percent smaller than the smaller of the elasticities estimated via 2SLS. We likewise note the extraordinary similarity between two-step GMM and EL estimates of the police elasticity. Pooling the estimates using empirical likelihood, we report precisely estimated elasticities of between -0.51 and -0.59 for murder, between -0.52 and -0.56 for robbery, between -0.28 and -0.33 for motor vehicle theft and between -0.15 and -0.20 for burglary. With regard to the crime aggregates, we report an elasticity of between -0.27 and -0.35 for violent crimes and between -0.09 and -0.14 for property crimes. Treating annual population growth as being measured without error, these estimates represent our best guess regarding the police elasticity and are our preferred estimates.

As we have noted, under classical measurement error, the forward and reflected IV regressions provide two estimates of the same underlying parameter. This observation gives rise to an overidentification test

[31]Since the derivatives are constant in the parameters, the resulting GMM estimates converge after the second step.

in which we can test the equality of the forward and reflected IV coefficients which form the basis for the pooled parameter estimates in Table 5. As we demonstrate in the preceeding section of the paper, this test of overidentifying restrictions provides an omnibus test for the presence of classical measurement errors. In the bottom panel of Table 5, for the estimates in panels A and B, we report a likelihood ratio test statistic which provides a measure of the degree to which the two parameter estimates differ.[32] Under the null hypothesis of classical measurement error, the test statistic has a χ^2 distribution with one degree of freedom. Given a critical value for the test of 3.84, an examination of Table 5 reveals that we fail to reject the null hypothesis of classical measurement error in each of nine tests. We interpret the equivalence of the IV coefficients reported in Table 4C as providing little evidence against the existence of classical measurement error in the police measures and consequently, as evidence in favor of the consistency of the estimated elasticities in Table 4C and in Table 5.

We further supplement the results of the tests of overidentifying restrictions presented in Table 5 with several additional analyses that are designed to either directly or indirectly test each of the assumptions of the classicial measurement error model individually. Recall conditions ($A1$), ($A2$), and ($A3$), discussed above. Condition (A1) states that the measurement errors must be independent of the residual in the outcome equation. A testable implication of (A1) is that the mesurement errors must be independent of the growth rates in each of the seven crimes we test and the two crime aggregates. Condition (A2) requires that the measurement errors be independent of the signal while condition (A3) requires that the measurement errors be independent of each other. To test these two conditions, we introduce a third measure of police manpower and modify conditions (A1)-(A3) in the obvious ways to reflect a third measurement error. With three measures of manpower, and under the classical measurement error hypothesis, the difference between any two measures is the difference in measurement errors. Under (A2) and (A3), the difference in two measurement errors cannot be related to the third manpower measure, because the third measure is comprised of the signal, which the measurement error difference should not predict, and a third measurement error, which the measurement error difference should not predict.

Table 6 presents additional evidence on the existence of classical measurement error for three incarnations of the measurement errors, each of which corresponds to the difference between two different measures of police manpower. Our third measure of police manpower is drawn from the Law Enforcement Management and Administrative Statistics (LEMAS) series. These data, which have been collected at regular intervals from 1987-2007 provide an additional measure of police in our sample of 242 cities.[33] In Table 6, Column (1) expresses the measurement error as the difference between the growth rate in the LEOKA measure and the growth rate in the LEMAS measure while columns (2) and (3) use the difference between the LEOKA measure and the ASG

[32]Here, the test statistic is computed via two-step GMM.

[33]For additional details regarding the LEMAS series, please see the data appendix to the paper.

measure and the difference between the LEMAS measure and the ASG measure, respectively. We begin in Panel A of Table 6, by regressing the growth rate in each of the nine crime types on each incarnation of the measurement error, conditional on the growth rate in population. To the extent that the measurement error is not correlated with the growth rate in each type of crime, it should not be correlated with ϵ, the error term in the structural equation. Table 6 provides twenty-seven tests of this hypothesis, three for each crime type. We fail to reject the null hypothesis of a relationship between the measurement errors and growth rate in crime in all but five cases. Moreover, when each of the seven crime rates are included as regressors in the same regression, in each case, we fail to reject that the coefficients are jointly different from zero. Finally, in Panel B of Table 6, we provide a joint test of conditions (A2) and (A3), as discussed above. An examination of the coefficients for each of the three incarnations of the measurement errors reveals that we fail to reject that the measurement errors are related to the signal. Notably, the estimated coefficients are extremely small as a one percent increase in the growth rate of a given police measure is found to be associated with only a 0.3-0.8 percent change in the measurement error.

In Table 5, we reported pooled estimates of the police elasticity under two different measures of the growth rate in a city's population. Conditional on choosing one of these two measures of the population growth rate, we presented evidence that IV coefficients from the forward and reflected regressions did not differ significantly from one another which we interpreted as providing little evidence against the classical measurement error hypothesis. However, we did not directly address the degree to which the estimates in Panel A and Panel B differ from one another. In particular, it is possible that the magnitude of the police elasticities are sensitive to mismeasurement of the growth rate in a city's population. This is an especially salient concern as the coefficient arising from a regression of the growth rate in one population measure on the growth rate in another is just 0.65. In order to assess the degree to which the police elasticity is responsive to such mismeasurement, we instrument for each measure of the population growth rate with its counterpart and report the results in Table 7. In Table 7, Panel A reports resulting parameter estimates in models where the ASG population measure is used as an instrument for the LEOKA population measure while Panel B corresponds to the reflected configuration. Here, we see that the resulting police elasticities are slightly larger in magnitude than those reported in Table 5. In particular, using EL, in panel A, we report elasticities for murder, robbery and motor vehicle theft of approximately -0.58, -0.61 and -0.36, respectively. These compare with -0.59, -0.57 and -0.33 in Panel A of Table 5. Referring to Panel B, we see that the degree to which the estimated police elascities differ based on which population measure is employed as the instrument is extremely limited. Formally, this can be seen via an examination of the test statistics reported in Table 7. These test statistics represent tests of the equality of the empirical likelihood parameters in Panels A and B. As the test is distributed chi_2 with

3 degrees of freedom, the test has a critical value of 9.35. Accordingly, there is extremely little evidence that the pooled estimates in Panels A and B of the table differ. In Panel C, we implement the EL version of the GMM estimator described in (15) and estimate police elasticities that pool the elasticities reported in Panels A and B. These estimates represent our best guess regarding the police elasticity. Here, we report precisely estimated elasticities of -0.59 for murder, -0.58 for robbery, -0.35 for motor vehicle theft and -0.22 for burglary. With regard to the aggregates, we report an elasticity of -0.36 for violent crimes and -0.15 for property crimes.

VI. Discussion

The IV estimates reported in the previous section of this paper can be thought of as police elasticities that are robust to errors in the measurement of police. Conditional upon only year fixed effects, we find elasticities of violent and property crimes with respect to police of approximately -0.3 and -0.15, respectively. Conditioning on a set of fully interacted state-by-year effects, and pooling estimates from our forward and reflected IV regressions and accoutning for mismeasurement of population, we report precisely estmated elasticities of -0.36 for violent crimes and -0.15 for property crimes, with especially large elasticities for murder (-0.59) robbery (-0.58), motor vehicle theft (-0.35) and burglary (-0.22).

In this section, we contextualize these findings by comparing our reported elasticities to those in the prior literature. Table 8 presents police elasticities from seven recent papers, each of which aims to correct for simultaneity bias, which our estimates do not adjust for. Under the classical measurement error hypothesis, these estimates jointly address bias arising from simultaneity and measurement errors. We compare each of these estimated elasticities to those reported in this paper.

Prior research typically finds that police have a larger protective effect on violent crimes than on property crimes. Violent crime elasticities that meet the standard threshold for statistical significance range from -0.44 to -0.99. An additional set of estimates using mayoral and gubernatorial elections as instruments, reported by Levitt (1997) and McCrary (2002) report an elasticity that is similar in magnitude though is not precisely estimated. With regard to the individual crimes, elasticities that meet the threshold of significance are typically largest for murder (-0.84 and -0.91) and robbery (-1.34). However, despite consistently large point estimates, results often remain insignificant due to the presence of correspondingly large standard errors. For example, McCrary (2002) and Levitt (2002) report robbery elasticities of -0.98 and -0.45, respectively, though both estimates are small relative to their standard error. With regard to property crimes, overall elasticities are insignificant in two of three aggregate data analyses, with point estimates ranging from 0 to -0.5. Elasticities for motor vehicle theft and burglary are typically largest with reported elasticities for motor vehicle theft of between -0.3 and -0.8 and for burglary of between -0.3 and -0.6.

Though each of the studies spans different numbers of cities and time periods, it is apparent that the elasticities reported in this paper are quite similar to those reported in prior research. Since our estimated elasticities are robust to measurement error and state and time-varying omitted variables but not the presence of simultaneity between police and crime, our research implies a smaller role for simultaneity than has been suggested by prior studies. Moreover, there is evidence that appears to support the proposition that changes in police hiring are often idiosyncratic and that it is difficult for cities to hire police during, or in anticipation of, a crime wave - at least in the short run. In particular, cities may have other objectives in regards to police staffing than the intertemporal smoothing of the marginal disutility of crime. Consider the example of Detroit's police numbers over the period 1975-1984. Mayor Coleman Young sought to aggressively hire officers under an affirmative action plan (Deslippe 2004). In 1977, 1245 officers were hired under the plan, increasing the size of the police force by some 20 percent. The next year, a further 227 officers were hired under the plan. After Detroit hired those officers, the city confronted a serious budget crisis. The city was compelled to lay off 400 and 690 officers in 1979 and 1980, respectively. In 1981 and 1982, the city was able to recall 100 and 171 of the laid off officers, respectively. However, a new round of cuts in 1983 undid this effort, as 224 officers were again laid off. In 1984, 135 of those officers were recalled.[34]

These boom and bust patterns in police hiring are somewhat common and seem to reflect some combination of city constraints and lack of foresight (Koper, Maguire and Moore 2001). For example, municipalities operate under many borrowing constraints, including tax and expenditure limitations (Joyce and Mullins 1991, Advisory Commission on Intergovernmental Relations 1995, Poterba and Rueben 1995, Shadbegian 1999), and balanced budget requirements (Cope 1992, Rubin 1997, City of Boston 2007).[35] Lewis (1994) reports that 99 of the 100 largest U.S. cities are required to balance the budget by state constitution, state statute, or city charter.

Perhaps in part because of these constraints, fiscal crises emerge with some regularity in cities, and this leads to police layoffs. Responding to the recent financial crisis, Camden laid off 45 percent of its sworn officers in early 2011 (Katz and Simon 2011). More historically, in 1981, Boston confronted a sluggish to recessionary economy, Proposition 2½, and a major Massachusetts Supreme Court decision that led to large reductions in Boston's property tax revenue.[36] Seeking to balance the budget, the city reduced the police

[34] *NAACP v. Detroit Police Officers Association*, 591 F. Supp. 1194 (1984).

[35] Of course, balanced budget requirements have more bite in some jurisdictions than in others. New York City famously required a last minute loan in 1975 from the federal government to avoid insolvency, yet the city charter requires a balanced budget (Gramlich 1976). At the other end of the spectrum, Atlanta's charter holds members of the city budget commission personally liable for any deficit (Chang 1979).

[36] *Tregor v. Assessors of Boston*, 377 Mass. 602, *cert. denied* 44 U.S. 841 (1979). For background on Proposition 2½, see Massachusetts Department of Revenue (2007).

department budget by over 27 percent. The department eliminated all capital expenditures, closed many police stations, and reduced the number of sworn officers by 24 percent (Boston Police Department 1982).

Cities also frequently fail to anticipate the ripple effects of past booms in hiring. Pension rules lead to spikes in retirement after 20 and 25 years, so a hiring boom two or more decades ago may result in a police officer shortage. Describing the situation in Chicago in 1986, Recktenwald (1986) notes that "[i]n 1983, an average of 32 officers a month left the force. Today the monthly average stands at 71, the records show. This comes at a time when the department's largest branch... is more than 1,000 officers short of the 7,940 level authorized by the Chicago City Council".

On the other hand, cities facing a difficult crime problem may be able to obtain extra funding from the state or federal government, and this may lead to simultaneity bias. Describing the situation in Washington, D.C., Harriston and Flaherty (1994) note that "[t]he [1994] hiring spree was a result of congressional alarm over the rising crime rate and the fact that 2,300 officers—about 60 percent of the department—were about to become eligible to retire. Congress voted to withhold the $430 million federal payment to the District for 1989 and again for 1990 until about 1,800 more officers were hired." Boston, in response to the 1981 crisis in police staffing, ultimately obtained a lump sum disbursement from the state government that helped Boston avoid deeper cuts to police department staffing.[37]

Overall, we suspect that our estimates are likely compromised somewhat by simultaneity bias. As noted in McCrary (2002), criminologists and economists have argued for several decades now that the sign of the bias is positive, leading to an underestimate of the magnitude of the policing elasticity. Thus, the correct magnitude is likely at least as large as what our results indicate. While we continue to view our results as representing a lower bound on the police elasticity, we note that an advantage of this approach is that we report elasticities that are more precisely estimated than a majority of the results in the prior literature.

VII. Conclusion

In this paper, we have presented estimates of the elasticity of crime with respect to police for index offenses: murder, rape, robbery, assault, burglary, larceny, and motor vehicle theft. These estimates are based on annual data on crime and police in a panel data set of 242 cities observed from 1960-2010. Our specifications model year-over-year growth rates in crime as a function of the year-over-year growth rate in the number of sworn officers from the year preceding, as well as a large number of control variables including year effects, state-by-year effects, budget cycles, and demographic controls.

[37] A succinct discussion of the local public finance implications of Proposition 2½ and the Tregor decision is given in *Boston Firefighters Union Local 718 v. Boston Chapter NAACP, Inc.*, 468 U.S. 1206 (1984).

Our main focus is on IV estimates where one noisy measure of the growth rate in police per capita is instrumented using another noisy measure of the growth rate in police. Under the classical measurement error model, the errors in measurement in one proxy are independent of the errors in measurement of the other proxy and of unobserved factors influencing the growth rate in crime. These assumptions imply that IV is consistent for the elasticity of crime with respect to police.

One implication of the classical measurement error hypothesis is that there are two consistent IV estimators. The first instruments one noisy measure of the growth rate in police per capita with another noisy measure of the growth rate in police. The second instruments the other noisy measure of the growth rate in police per capita with the one. That two consistent estimators are available for the police elasticity suggests pooling the two estimates to arrive at an efficient minimum chi-square estimate of the police elasticity. This approach also yields an immediate test of the classical measurement error hypothesis in the form of the minimized value of the test statistic. Generally speaking, there is little evidence in these tests against the null hypothesis of classical measurement error.

Our focus on measurement error stands in contrast to the previous literature, which has instead emphasized the potential for simultaneity bias, whereby cities or perhaps police departments take heed of ongoing and possibly upcoming trends in crime and hire police officers accordingly. We have instead emphasized the variety of institutional considerations that make such lifecycle optimization challenging for cities and departments, including tax and expenditure limitations, the *de facto* requirement to balance the city budget under state constitution, state statute, or city charter, and the predominance of policing costs as a fraction of the city budget. An additional consideration is that cities and departments may simply fail to optimize appropriately. For example, many cities report staffing difficulties in the wake of retirement booms, but these are of course largely predictable based on simple actuarial projections using years of service and year hired. As an empirical matter, cities seem to engage in boom and bust hiring with relatively little attention paid to the level of crime and relatively more attention paid to shortfalls of police staffing from recent norms.

Consistent with this reasoning, our estimates are robust to the inclusion of a variety of control variables that have direct bearing on crime, in particular budget cycles and demographic variables. Indeed, our estimates are robust to the inclusion of unrestricted state-by-year effects. This implies that our estimates represent a pure effect of policing that cannot be attributed to, for example, changes to punishment policy since those are made at the state level. Our best guess regarding the elasticity of crime with respect to police is -0.5 for violent crime and -0.25 for property crime. Crime categories where police seem to be most effective are murder, robbery, and motor vehicle theft. Our estimates are similar to those found in the previous literature, but are somewhat more precisely estimated.

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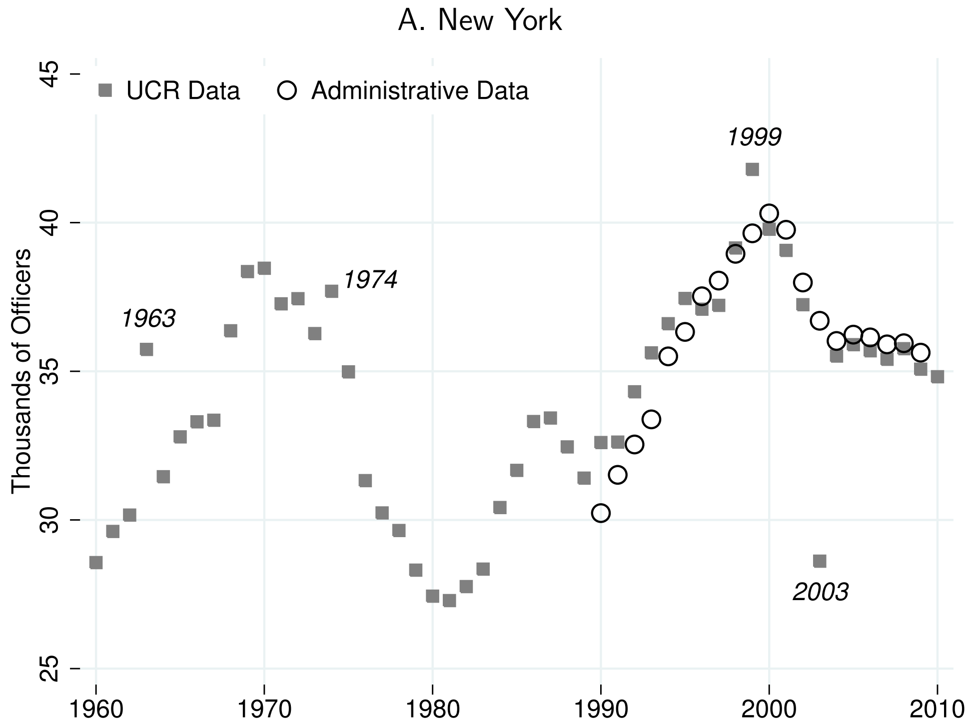
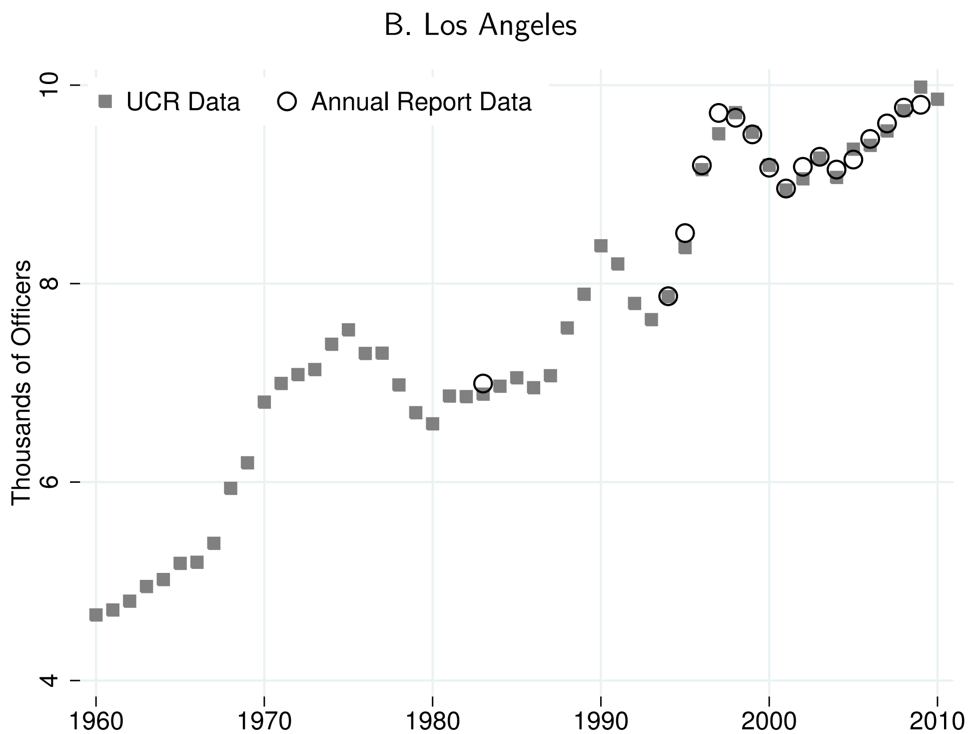
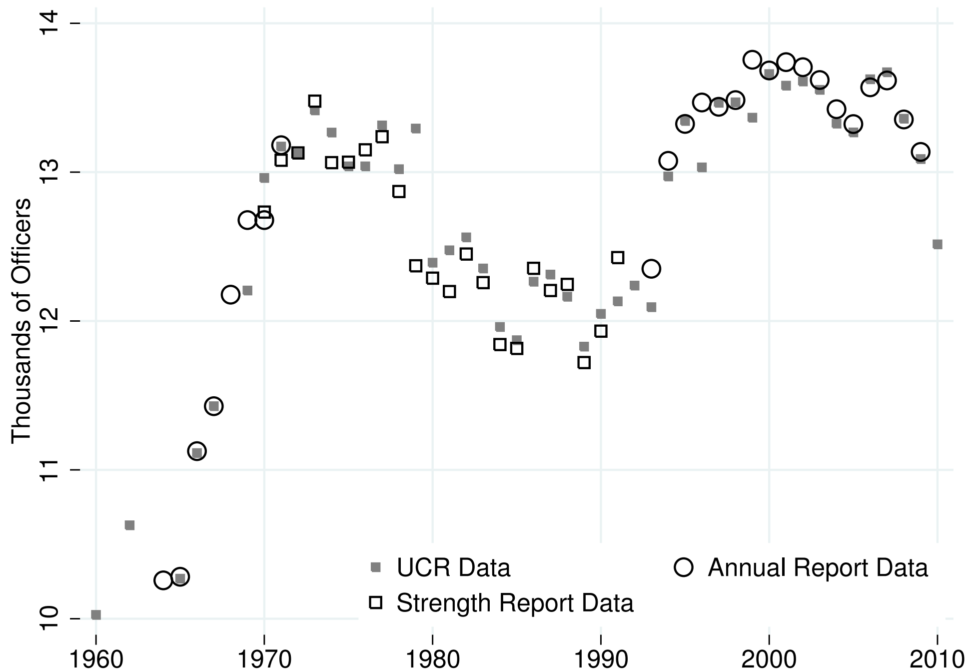
Figure 1. Sworn Officers in Five Cities: the Uniform Crime Reports and Direct Measures from Departments

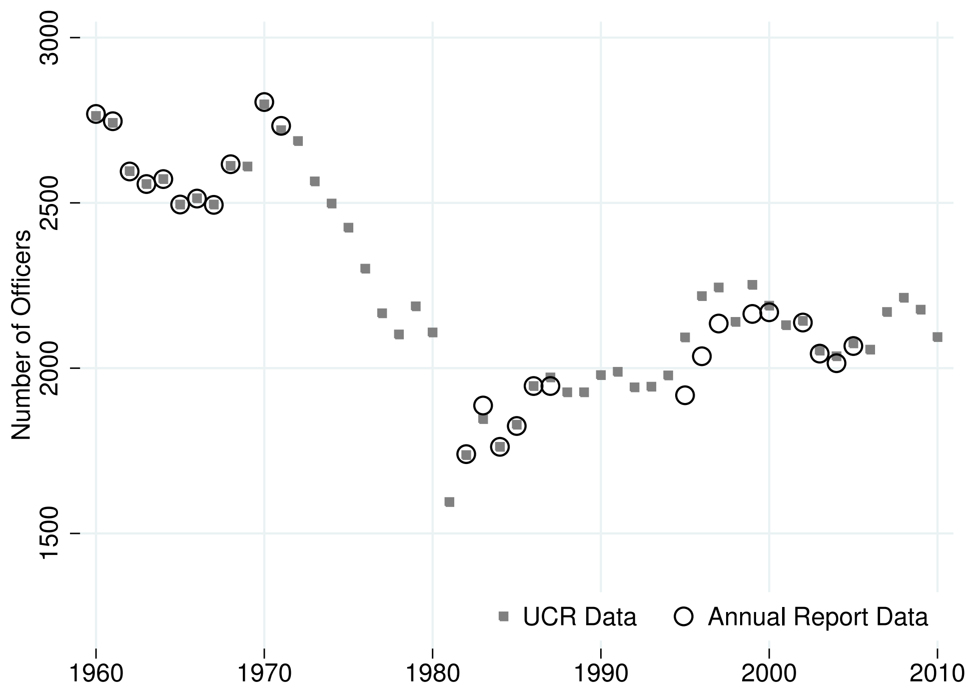




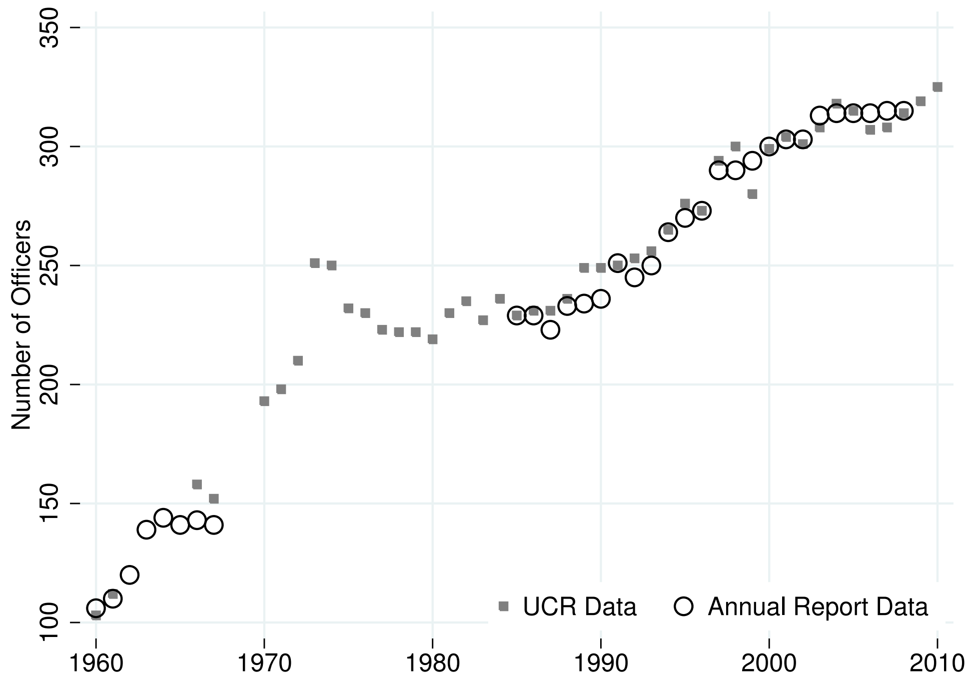
C. Chicago



D. Boston

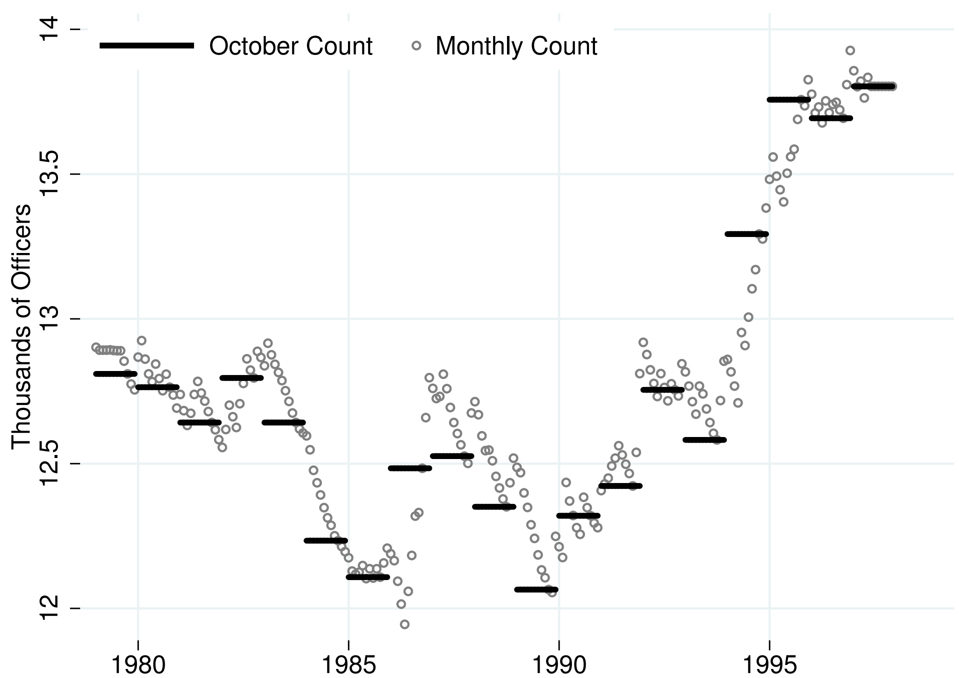


E. Lincoln, Nebraska



Note: In panel A, numbers for 1960-1994 are adjusted to account for the 1995 merger of NYPD with housing and transit police. See Data Appendix for details.

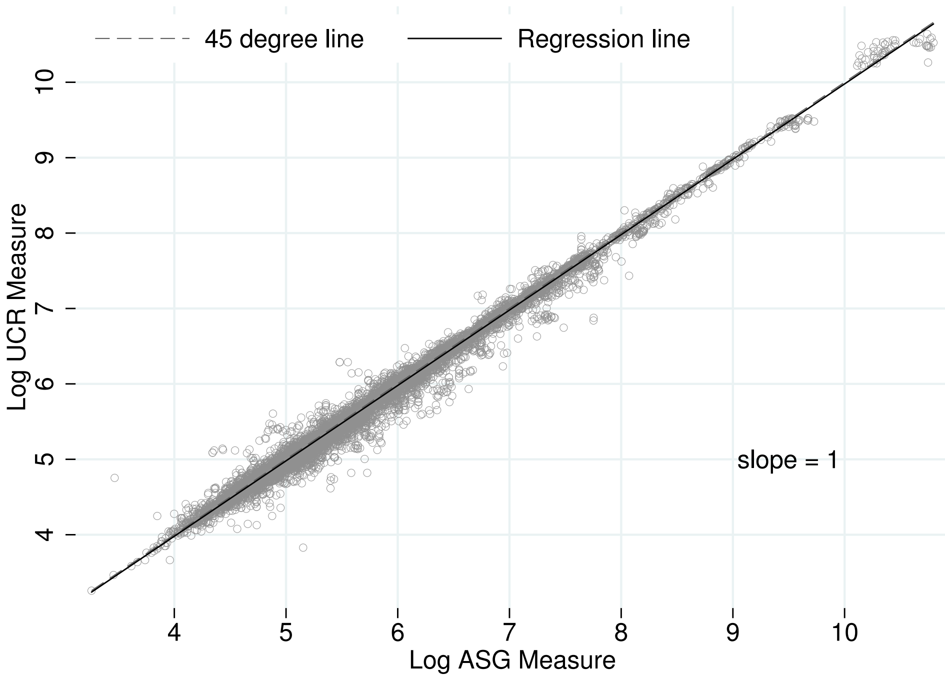
Figure 2. Sworn Officers in Chicago 1979-1997, by Month



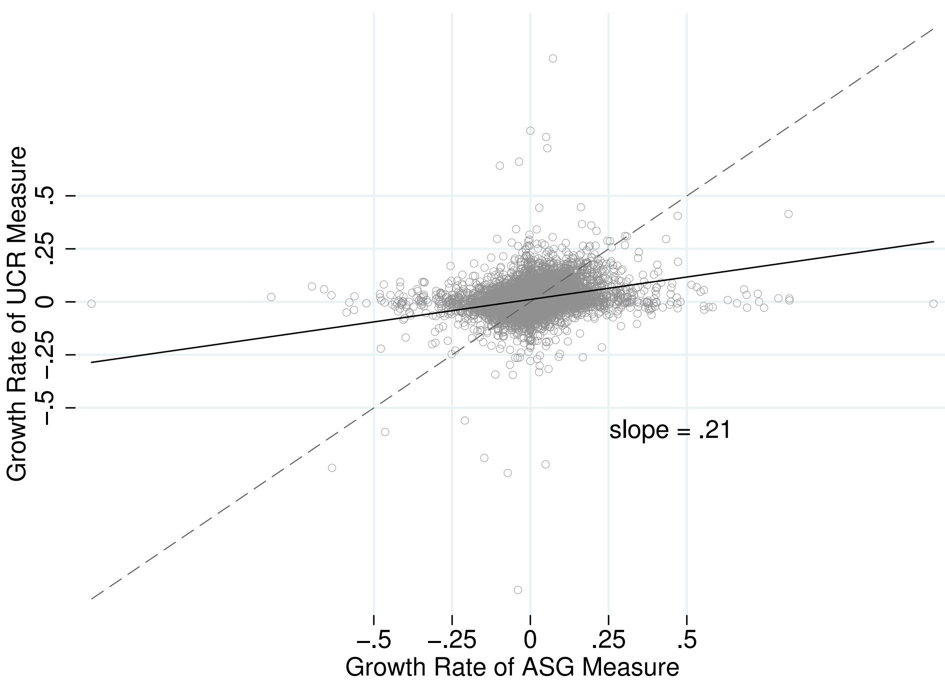
Note: See text for details.

Figure 3. Two Leading Measures of Sworn Officers: the Uniform Crime Reports and the Annual Survey of Government

A. Logs



B. Log Differences



Note: See text and Data Appendix for details.

Figure 4. Location of Cities in Sample

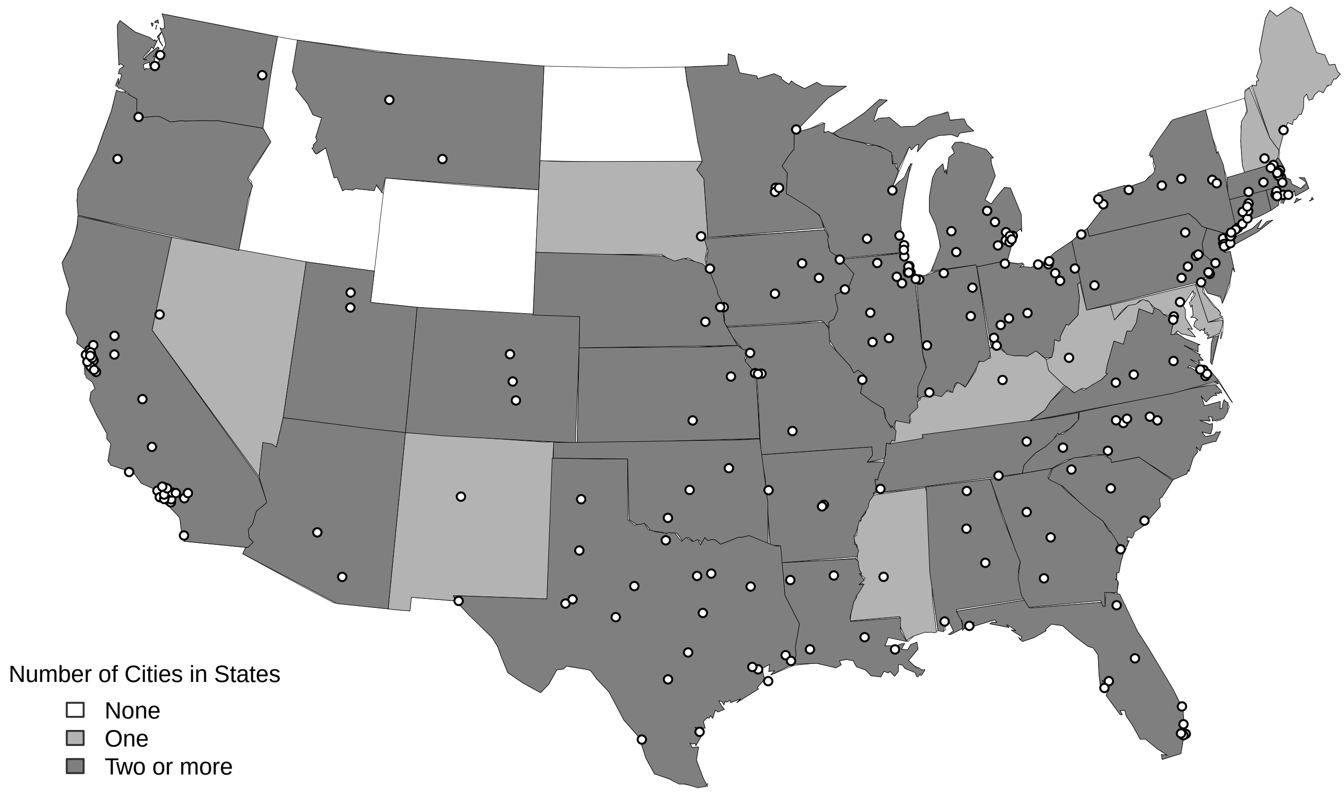
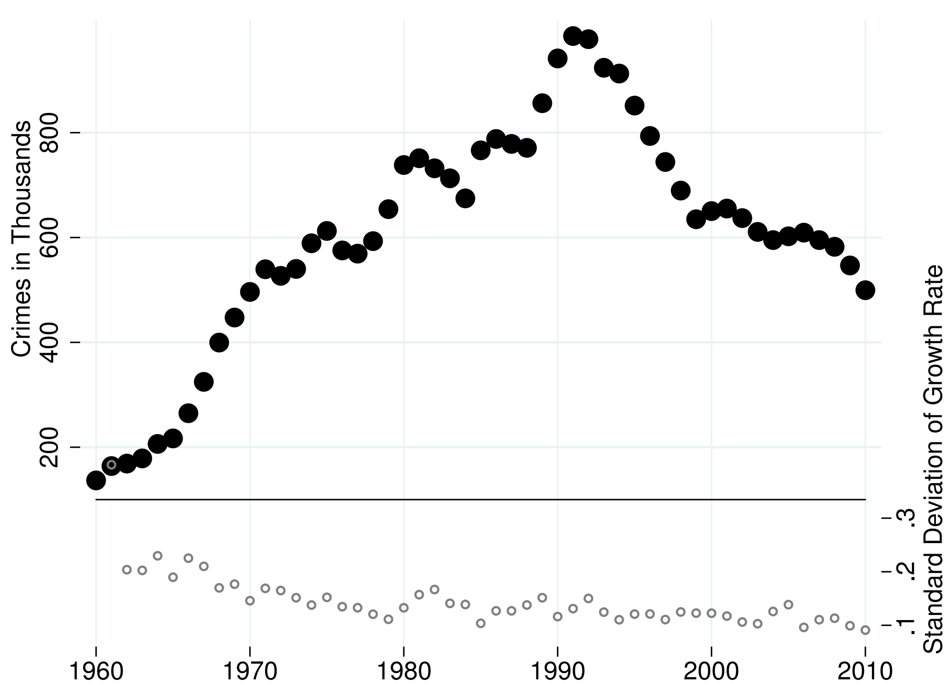
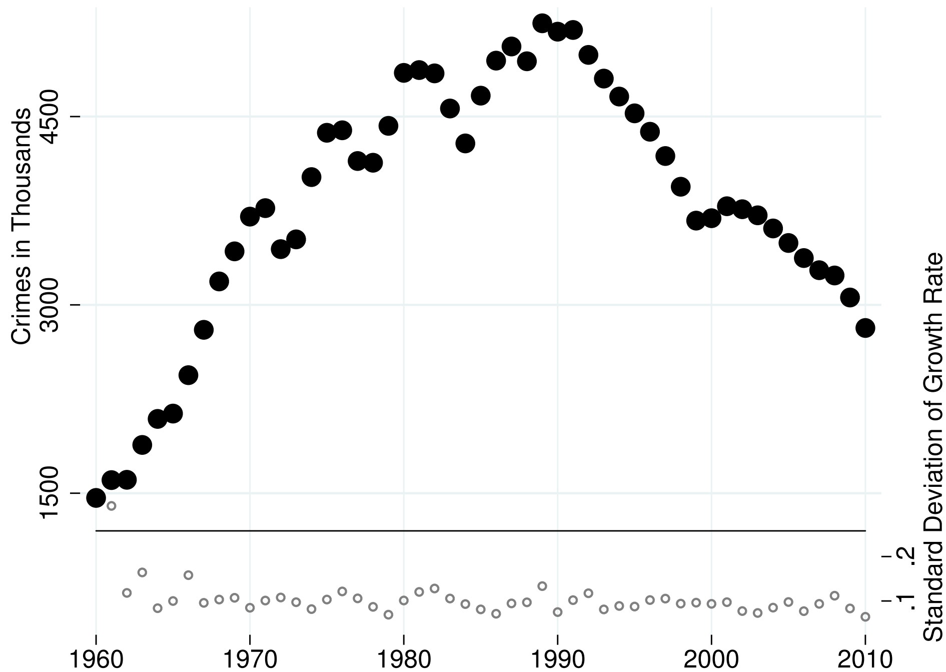


Figure 5. Aggregate Trends in Violent and Property Crime and Police: Evidence from the Uniform Crime Reports

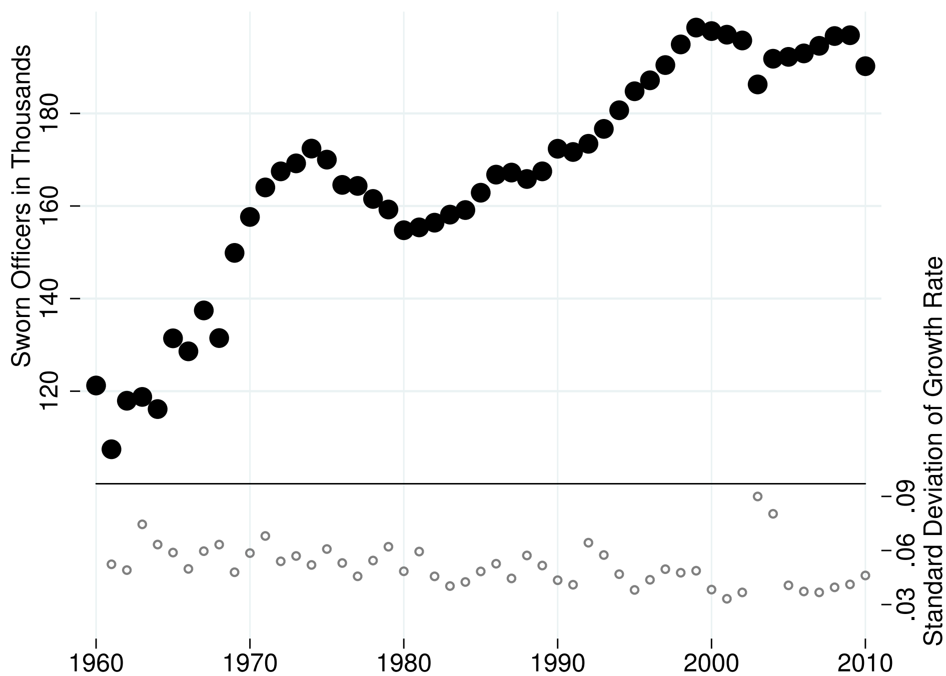
A. Violent Crime: Murder, Rape, Robbery, Aggravated Assault



B. Property Crime: Burglary, Larceny, Motor Vehicle Theft



C. Sworn Police



Note: In the UCR data, larceny is defined to exclude motor vehicle theft. Solid circles give totals and open circles give standard deviations of year-over-year growth rates. See text and Data Appendix for details.

Table 1. Correlation of UCR and Police Department Measures of Number of Sworn Personnel

Measure	New York	Los Angeles	Chicago	Boston	Lincoln
Log Sworn Police	0.65	0.99	0.96	0.98	0.99
Growth Rate	0.32	0.92	0.65	0.94	0.45

Note: Table entries are correlation coefficients between the UCR measure of the number of sworn police and a measure of the number of sworn police taken from police department reports. Annual report data for Boston in 1982 are omitted from the calculations.

Table 2. Summary Statistics on Police and Crime

			Levels (per 100,000 population)				Log Differences			
Variable	**N**		**Mean**	**S.D.**	**Min.**	**Max.**	**Mean**	**S.D.**	**Min.**	**Max.**
Sworn police (LEOKA)	12,157	O	248.5	114.0	52.6	786.6	0.014	0.056	-1.359	1.148
		B		107.2				0.012		
		W		39.1				0.055		
Sworn police (ASG)	11,960	O	255.0	122.3	40.3	779.8	0.016	0.080	-1.401	1.288
		B		110.3				0.011		
		W		51.7				0.079		
Violent crimes	12,021	O	930.8	629.4	6.6	4189.0	0.035	0.171	-1.804	1.767
		B		411.9				0.020		
		W		473.3				0.170		
Murder	12,274	O	14.2	10.5	0.0	110.9	0.015	0.410	-4.277	4.091
		B		8.1				0.015		
		W		6.7				0.410		
Rape	12,101	O	46.4	29.8	0.0	310.5	0.035	0.323	-4.384	4.199
		B		16.6				0.029		
		W		24.7				0.322		
Robbery	12,187	O	424.9	344.2	1.1	2,358.0	0.034	0.212	-2.639	2.565
		B		242.5				0.018		
		W		244.3				0.211		
Assault	12,176	O	465.2	338.9	1.0	2,761.3	0.037	0.228	-2.833	3.129
		B		204.4				0.023		
		W		270.7				0.226		
Property crimes	12,177	O	5,980.4	2,415.2	155.6	18,345.2	0.015	0.124	-2.330	1.769
		B		1,316.3				0.014		
		W		2,025.3				0.124		
Burglary	12,192	O	1,588.2	815.9	37.7	6,713.5	0.011	0.158	-2.457	2.030
		B		417.7				0.018		
		W		701.1				0.157		
Larceny	12,185	O	3,528.3	1,513.0	84.2	11,590.7	0.017	0.135	-2.228	2.146
		B		934.4				0.015		
		W		1,191.0				0.134		
Motor vehicle theft	12,186	O	862.7	570.7	8.4	5,294.7	0.012	0.178	-2.833	1.899
		B		363.2				0.017		
		B		440.4				0.177		

Note: This table reports descriptive statistics for the two measures of sworn police officers used throughout the article as well as for each of the seven crime categories and two crime aggregates. For each variable, we report the overall mean, the standard deviation decomposed into overall ("O"), between ("B"), and within ("W") variation, as well as the minimum and maximum values, in levels and growth rates. Results are weighted by 2010 city population.

Table 3A. First Stage Models

	EC = LEOKA Measure INS = ASG Measure					EC = ASG Measure INS = LEOKA Measure				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ASG measure	0.168	0.168	0.144	0.141	0.152					
	(0.014)	(0.014)	(0.013)	(0.013)	(0.014)					
	[0.018]	[0.018]	[0.017]	[0.017]	[0.018]					
LEOKA measure						0.359	0.359	0.342	0.338	0.353
						(0.029)	(0.029)	(0.033)	(0.033)	(0.035)
						[0.034]	[0.034]	[0.038]	[0.039]	[0.043]
F-statistic	153.0	152.2	116.7	124.7	122.5	150.6	150.2	109.3	107.5	100.4
N	11,036	11,036	8,869	8,869	8,869	11,036	11,036	8,869	8,869	8,869
sample	1960-	1960-	1970-	1970-	1970-	1960-	1960-	1970-	1970-	1970-
year effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
budget cycles	no	yes	yes	yes	yes	no	yes	yes	yes	yes
demographics	no	no	no	yes	yes	no	no	no	yes	yes
polynomials and interactions	no	no	no	no	yes	no	no	no	no	yes

Note: Each column reports results of a least squares regression of the growth rate in a given measurement of the number of per capita police officers on the the growth rate in the other measurement. Columns (1)-(5) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (6)-(10) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and a vector of year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. The third column is the same as column (2) with the exception that estimation is done using the 1970-2010 sample. In the fourth column we add demographic controls which capture the proportion of a city's population that is comprised of each of twleve age-gender-race groups. Finally, in the fifth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. The top row reports Huber-Eicker-White standard errors that are robust to heteroskedasticity. The standard errors reported in the second row are clustered at the city level.

Table 3B. Least Squares Models of the Effect of Police on Crime

	EC = LEOKA Measure INS = ASG Measure					EC = ASG Measure INS = LEOKA Measure				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Violent crimes	-0.123	-0.124	-0.064	-0.069	-0.068	-0.063	-0.064	-0.050	-0.052	-0.051
	(0.037)	(0.037)	(0.036)	(0.035)	(0.034)	(0.024)	(0.024)	(0.021)	(0.021)	(0.021)
	[0.035]	[0.036]	[0.036]	[0.031]	[0.032]	[0.019]	[0.019]	[0.021]	[0.022]	[0.021]
Murder	-0.272	-0.275	-0.265	-0.268	-0.258	-0.234	-0.235	-0.239	-0.241	-0.243
	(0.071)	(0.071)	(0.076)	(0.076)	(0.076)	(0.063)	(0.063)	(0.071)	(0.071)	(0.072)
	[0.089]	[0.089]	[0.081]	[0.080]	[0.080]	[0.066]	[0.067]	[0.078]	[0.079]	[0.079]
Rape	-0.080	-0.080	0.019	0.003	0.009	-0.070	-0.070	-0.076	-0.081	-0.076
	(0.069)	(0.069)	(0.060)	(0.058)	(0.057)	(0.066)	(0.066)	(0.072)	(0.073)	(0.073)
	[0.067]	[0.067]	[0.047]	[0.046]	[0.047]	[0.064]	[0.064]	[0.072]	[0.072]	[0.072]
Robbery	-0.196	-0.196	-0.172	-0.178	-0.178	-0.085	-0.085	-0.091	-0.093	-0.092
	(0.047)	(0.047)	(0.048)	(0.046)	(0.045)	(0.032)	(0.032)	(0.029)	(0.028)	(0.028)
	[0.053]	[0.054]	[0.054]	[0.048]	[0.048]	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]
Assault	-0.056	-0.058	0.018	0.015	0.016	-0.025	-0.026	-0.002	-0.005	-0.004
	(0.043)	(0.043)	(0.041)	(0.041)	(0.040)	(0.029)	(0.029)	(0.025)	(0.025)	(0.025)
	[0.037]	[0.037]	[0.040]	[0.038]	[0.039]	[0.026]	[0.026]	[0.026]	[0.027]	[0.027]
Property crimes	-0.071	-0.069	-0.048	-0.049	-0.048	-0.024	-0.023	-0.030	-0.031	-0.030
	(0.028)	(0.028)	(0.030)	(0.028)	(0.027)	(0.020)	(0.020)	(0.021)	(0.020)	(0.020)
	[0.022]	[0.022]	[0.028]	[0.027]	[0.028]	[0.014]	[0.014]	[0.019]	[0.020]	[0.019]
Burglary	-0.060	-0.058	-0.021	-0.023	-0.025	-0.039	-0.038	-0.044	-0.048	-0.048
	(0.042)	(0.042)	(0.046)	(0.041)	(0.039)	(0.027)	(0.027)	(0.024)	(0.024)	(0.024)
	[0.033]	[0.033]	[0.036]	[0.035]	[0.035]	[0.016]	[0.016]	[0.021]	[0.022]	[0.022]
Larceny	-0.040	-0.038	-0.024	-0.023	-0.020	0.001	0.001	-0.008	-0.008	-0.007
	(0.030)	(0.030)	(0.033)	(0.031)	(0.031)	(0.021)	(0.021)	(0.022)	(0.021)	(0.021)
	[0.021]	[0.021]	[0.029]	[0.029]	[0.030]	[0.014]	[0.014]	[0.016]	[0.015]	[0.016]
Motor vehicle	-0.190	-0.189	-0.170	-0.175	-0.173	-0.085	-0.084	-0.072	-0.070	-0.067
	(0.049)	(0.049)	(0.058)	(0.054)	(0.052)	(0.033)	(0.033)	(0.038)	(0.037)	(0.036)
	[0.042]	[0.042]	[0.046]	[0.045]	[0.047]	[0.043]	[0.043]	[0.050]	[0.051]	[0.050]
sample	1960-	1960-	1970-	1970-	1970-	1960-	1960-	1970-	1970-	1970-
year effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
budget cycles	no	yes	yes	yes	yes	no	yes	yes	yes	yes
demographics	no	no	no	yes	yes	no	no	no	yes	yes
polynomials and interactions	no	no	no	no	yes	no	no	no	no	yes

Note: Each column reports results of a least squares regression of thegrowth rate in each of nine crime rates on the first lag of the growth rate in the number of per capita sworn police officers. Columns (1)-(5) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (6)-(10) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and a vector of year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. The third column is the same as column (2) with the exception that estimation is done using the 1970-2010 sample. In the fourth column we add demographic controls which capture the proportion of a city's population that is comprised of each of twleve age-gender-race groups. Finally, in the fifth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. The top row reports Huber-Eicker-White standard errors that are robust to heteroskedasticity. The standard errors reported in the second row are clustered at the city level.

Table 3C. 2SLS Models of the Effect of Police on Crime

	EC = LEOKA Measure INS = ASG Measure					EC = ASG Measure INS = LEOKA Measure				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Violent crimes	-0.318	-0.320	-0.314	-0.334	-0.330	-0.323	-0.326	-0.158	-0.181	-0.171
	(0.130)	(0.131)	(0.135)	(0.133)	(0.134)	(0.101)	(0.101)	(0.104)	(0.100)	(0.099)
	[0.104]	[0.104]	[0.125]	[0.132]	[0.134]	[0.088]	[0.089]	[0.097]	[0.086]	[0.088]
Murder	-1.339	-1.350	-1.584	-1.627	-1.655	-0.721	-0.729	-0.718	-0.736	-0.710
	(0.426)	(0.428)	(0.555)	(0.564)	(0.571)	(0.237)	(0.239)	(0.228)	(0.235)	(0.236)
	[0.392]	[0.393]	[0.499]	[0.507]	[0.514]	[0.202]	[0.203]	[0.228]	[0.229]	[0.228]
Rape	-0.360	-0.360	-0.488	-0.529	-0.500	-0.199	-0.198	0.097	0.045	0.063
	(0.379)	(0.379)	(0.477)	(0.489)	(0.494)	(0.182)	(0.181)	(0.142)	(0.138)	(0.140)
	[0.383]	[0.384]	[0.473]	[0.483]	[0.488]	[0.192]	[0.193]	[0.179]	[0.173]	[0.172]
Robbery	-0.477	-0.477	-0.592	-0.611	-0.613	-0.519	-0.518	-0.465	-0.495	-0.491
	(0.179)	(0.180)	(0.187)	(0.184)	(0.185)	(0.129)	(0.129)	(0.133)	(0.127)	(0.125)
	[0.155]	[0.155]	[0.158]	[0.164]	[0.168]	[0.124]	[0.125]	[0.128]	[0.117]	[0.118]
Assault	-0.075	-0.080	-0.014	-0.031	-0.027	-0.140	-0.145	0.083	0.069	0.078
	(0.165)	(0.166)	(0.161)	(0.164)	(0.165)	(0.120)	(0.120)	(0.119)	(0.120)	(0.119)
	[0.146]	[0.147]	[0.171]	[0.178]	[0.179]	[0.104]	[0.104]	[0.117]	[0.114]	[0.115]
Property crimes	-0.135	-0.130	-0.190	-0.198	-0.192	-0.182	-0.178	-0.119	-0.128	-0.115
	(0.114)	(0.113)	(0.134)	(0.130)	(0.128)	(0.077)	(0.077)	(0.087)	(0.081)	(0.078)
	[0.073]	[0.073]	[0.112]	[0.117]	[0.116]	[0.058]	[0.058]	[0.076]	[0.076]	[0.079]
Burglary	-0.229	-0.224	-0.291	-0.320	-0.325	-0.161	-0.155	-0.054	-0.064	-0.058
	(0.149)	(0.149)	(0.158)	(0.156)	(0.156)	(0.119)	(0.119)	(0.135)	(0.123)	(0.115)
	[0.090]	[0.090]	[0.121]	[0.133]	[0.132]	[0.087]	[0.087]	[0.103]	[0.101]	[0.104]
Larceny	0.003	0.007	-0.049	-0.050	-0.043	-0.096	-0.092	-0.045	-0.049	-0.034
	(0.118)	(0.118)	(0.145)	(0.141)	(0.138)	(0.085)	(0.085)	(0.097)	(0.091)	(0.090)
	[0.080]	[0.080]	[0.101]	[0.101]	[0.103]	[0.060]	[0.060]	[0.085]	[0.086]	[0.089]
Motor vehicle	-0.502	-0.499	-0.459	-0.457	-0.438	-0.517	-0.515	-0.478	-0.507	-0.491
	(0.185)	(0.185)	(0.235)	(0.233)	(0.233)	(0.132)	(0.133)	(0.165)	(0.155)	(0.148)
	[0.222]	[0.223]	[0.297]	[0.314]	[0.309]	[0.104]	[0.105]	[0.121]	[0.128]	[0.131]
sample	1960-	1960-	1970-	1970-	1970-	1960-	1960-	1970-	1970-	1970-
year effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
budget cycles	no	yes	yes	yes	yes	no	yes	yes	yes	yes
demographics	no	no	no	yes	yes	no	no	no	yes	yes
polynomials and interactions	no	no	no	no	yes	no	no	no	no	yes

Note: Each column reports results of a 2SLS regression of the growth rate in each of nine crime rates on the first lag of the growth rate in the number of per capita sworn police officers. Columns (1)-(5) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (6)-(10) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and a vector of year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. The third column is the same as column (2) with the exception that estimation is done using the 1970-2010 sample. In the fourth column we add demographic controls which capture the proportion of a city's population that is comprised of each of twleve age-gender-race groups. Finally, in the fifth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. Two sets of standard errors are reported below the coefficient estimates. The top row reports Huber-Eicker-White standard errors that are robust to heteroskedasticity. The standard errors reported in the second row are clustered at the city level.

Table 4A. First Stage Models
Within-State Differences

	EC = LEOKA Measure INS = ASG Measure					EC = ASG Measure INS = LEOKA Measure				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ASG measure	0.149	0.149	0.129	0.128	0.129					
	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)					
LEOKA measure						0.350	0.350	0.335	0.334	0.335
						(0.029)	(0.029)	(0.031)	(0.031)	(0.031)
F-statistic	138.2	138.2	137.1	136.6	135.7	149.7	149.5	117.2	116.5	116.0
N	11,036	11,036	8,869	8,869	8,869	11,036	11,036	8,869	8,869	8,869
sample	1960-	1960-	1970-	1970-	1970-	1960-	1960-	1970-	1970-	1970-
state-by-year effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
budget cycles	no	yes	yes	yes	yes	no	yes	yes	yes	yes
demographics	no	no	no	yes	yes	no	no	no	yes	yes
polynomials and interactions	no	no	no	no	yes	no	no	no	no	yes

Note: Each column reports results of a least squares regression of the growth rate in a given measurement of the number of per capita police officers on the the growth rate in the other measurement. Columns (1)-(5) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (6)-(10) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and an unrestricted set of state-by-year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. The third column is the same as column (2) with the exception that estimation is done using the 1970-2010 sample. In the fourth column we add demographic controls which capture the proportion of a city's population that is comprised of each of twleve age-gender-race groups. Finally, in the fifth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. As clustered and robust standard errors are very similar across all models, we report Huber-Eicker-White standard errors that are robust to heteroskedasticity. in parentheses below the coefficient estimates.

Table 4B. Least Squares Models of the Effect of Police on Crime
Within-State Differences

	EC = LEOKA Measure INS = ASG Measure					EC = ASG Measure INS = LEOKA Measure				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Violent crimes	-0.126	-0.126	-0.080	-0.077	-0.075	-0.063	-0.063	-0.034	-0.032	-0.029
	(0.038)	(0.038)	(0.037)	(0.037)	(0.037)	(0.023)	(0.023)	(0.021)	(0.021)	(0.021)
Murder	-0.217	-0.218	-0.190	-0.183	-0.179	-0.130	-0.131	-0.119	-0.116	-0.118
	(0.094)	(0.094)	(0.099)	(0.099)	(0.100)	(0.059)	(0.059)	(0.062)	(0.062)	(0.062)
Rape	-0.090	-0.089	0.052	0.054	0.054	-0.010	-0.009	0.030	0.033	0.035
	(0.090)	(0.090)	(0.069)	(0.069)	(0.069)	(0.054)	(0.054)	(0.049)	(0.049)	(0.049)
Robbery	-0.214	-0.214	-0.217	-0.214	-0.212	-0.082	-0.082	-0.075	-0.074	-0.071
	(0.046)	(0.046)	(0.047)	(0.047)	(0.047)	(0.029)	(0.029)	(0.027)	(0.027)	(0.027)
Assault	-0.053	-0.053	0.008	0.013	0.014	-0.014	-0.014	0.020	0.024	0.028
	(0.049)	(0.049)	(0.047)	(0.046)	(0.046)	(0.035)	(0.035)	(0.029)	(0.029)	(0.029)
Property crimes	-0.052	-0.051	-0.036	-0.031	-0.030	-0.018	-0.018	-0.016	-0.013	-0.014
	(0.025)	(0.025)	(0.029)	(0.028)	(0.028)	(0.015)	(0.015)	(0.015)	(0.016)	(0.015)
Burglary	-0.057	-0.055	-0.034	-0.032	-0.032	-0.042	-0.041	-0.032	-0.029	-0.028
	(0.036)	(0.036)	(0.039)	(0.039)	(0.039)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Larceny	-0.023	-0.022	-0.013	-0.008	-0.006	-0.001	-0.001	-0.006	-0.004	-0.004
	(0.027)	(0.027)	(0.031)	(0.030)	(0.030)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Motor vehicle	-0.130	-0.129	-0.105	-0.101	-0.098	-0.035	-0.035	-0.008	-0.004	-0.004
	(0.042)	(0.042)	(0.048)	(0.048)	(0.048)	(0.025)	(0.025)	(0.027)	(0.027)	(0.027)
sample	1960-	1960-	1970-	1970-	1970-	1960-	1960-	1970-	1970-	1970-
state-by-year effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
budget cycles	no	yes	yes	yes	yes	no	yes	yes	yes	yes
demographics	no	no	no	yes	yes	no	no	no	yes	yes
polynomials and interactions	no	no	no	no	yes	no	no	no	no	yes

Note: Each column reports results of a least squares regression of thegrowth rate in each of nine crime rates on the first lag of the growth rate in the number of per capita sworn police officers. Columns (1)-(5) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (6)-(10) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and a vector of year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. The third column is the same as column (2) with the exception that estimation is done using the 1970-2010 sample. In the fourth column we add demographic controls which capture the proportion of a city's population that is comprised of each of twleve age-gender-race groups. Finally, in the fifth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. As clustered and robust standard errors are very similar across all models, we report Huber-Eicker-White standard errors that are robust to heteroskedasticity. in parentheses below the coefficient estimates.

Table 4C. 2SLS Models of the Effect of Police on Crime
Within-State Differences

	EC = LEOKA Measure INS = ASG Measure					EC = ASG Measure INS = LEOKA Measure				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Violent crimes	-0.357	-0.357	-0.251	-0.233	-0.210	-0.336	-0.335	-0.207	-0.205	-0.200
	(0.144)	(0.144)	(0.153)	(0.154)	(0.154)	(0.107)	(0.107)	(0.110)	(0.111)	(0.111)
Murder	-0.826	-0.829	-0.903	-0.891	-0.902	-0.506	-0.508	-0.417	-0.401	-0.383
	(0.390)	(0.391)	(0.464)	(0.467)	(0.467)	(0.265)	(0.265)	(0.297)	(0.298)	(0.299)
Rape	-0.060	-0.058	0.223	0.246	0.263	-0.241	-0.239	0.202	0.203	0.202
	(0.343)	(0.343)	(0.352)	(0.357)	(0.357)	(0.254)	(0.254)	(0.211)	(0.213)	(0.214)
Robbery	-0.496	-0.494	-0.559	-0.553	-0.533	-0.588	-0.586	-0.637	-0.638	-0.633
	(0.179)	(0.179)	(0.192)	(0.194)	(0.194)	(0.127)	(0.127)	(0.132)	(0.132)	(0.133)
Assault	-0.041	-0.040	0.150	0.180	0.210	-0.117	-0.116	0.082	0.087	0.092
	(0.210)	(0.210)	(0.208)	(0.210)	(0.210)	(0.137)	(0.138)	(0.141)	(0.142)	(0.142)
Property crimes	-0.144	-0.141	-0.117	-0.095	-0.097	-0.143	-0.141	-0.099	-0.091	-0.088
	(0.091)	(0.090)	(0.107)	(0.108)	(0.107)	(0.068)	(0.068)	(0.083)	(0.083)	(0.083)
Burglary	-0.292	-0.288	-0.244	-0.220	-0.217	-0.162	-0.159	-0.103	-0.097	-0.098
	(0.130)	(0.130)	(0.149)	(0.151)	(0.152)	(0.099)	(0.099)	(0.115)	(0.116)	(0.116)
Larceny	-0.043	-0.040	-0.041	-0.024	-0.030	-0.056	-0.054	-0.023	-0.014	-0.007
	(0.105)	(0.104)	(0.125)	(0.126)	(0.125)	(0.075)	(0.075)	(0.092)	(0.092)	(0.092)
Motor vehicle	-0.261	-0.258	-0.058	-0.028	-0.029	-0.367	-0.365	-0.315	-0.310	-0.303
	(0.150)	(0.150)	(0.186)	(0.187)	(0.188)	(0.114)	(0.114)	(0.141)	(0.142)	(0.142)
sample	1960-	1960-	1970-	1970-	1970-	1960-	1960-	1970-	1970-	1970-
state-by-year effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
budget cycles	no	yes	yes	no	no	no	yes	yes	no	no
demographics	no	no	yes	no	yes	no	no	yes	no	yes
polynomials and interactions	no	no	no	yes	yes	no	no	no	yes	yes

Note: Each column reports results of a 2SLS regression of the growth rate in each of nine crime rates on the first lag of the growth rate in the number of per capita sworn police officers. Columns (1)-(5) report results for the models in which the LEOKA measure is employed as the endogenous covariate and the ASG measure is employed as the instrumental variable while columns (6)-(10) report results for models in which the ASG measure is employed as the endogenous covariate and the LEOKA measure is employed as the instrumental variable. For each set of models, the first column reports regression results, conditional on the growth rate in the city's population and a vector of year effects. The second column adds a control variable for the city's per capita expenditures exclusive of police expenditures. The third column is the same as column (2) with the exception that estimation is done using the 1970-2010 sample. In the fourth column we add demographic controls which capture the proportion of a city's population that is comprised of each of twleve age-gender-race groups. Finally, in the fifth column, we add polynomial terms and selected interactions of the demographic variables. All models are estimated using 2010 city population weights. As clustered and robust standard errors are very similar across all models, we report Huber-Eicker-White standard errors that are robust to heteroskedasticity. in parentheses below the coefficient estimates.

Table 5A. Pooled Estimates of the Effect of Police on Crime
With Exogenous Population Growth
GMM and Empirical Likelihood Estimation

	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Motor Vehicle Theft
Panel A. Popoulation Measure = LEOKA									
GMM(1)	-0.346	-0.661	-0.154	-0.544	-0.080	-0.144	-0.224	-0.050	-0.306
	(0.104)	(0.253)	(0.246)	(0.125)	(0.142)	(0.067)	(0.094)	(0.073)	(0.104)
GMM(2)	-0.342	-0.591	-0.187	-0.565	-0.099	-0.143	-0.202	-0.052	-0.333
	(0.097)	(0.243)	(0.232)	(0.118)	(0.128)	(0.062)	(0.090)	(0.068)	(0.100)
EL	-0.342	-0.591	-0.188	-0.565	-0.099	-0.143	-0.200	-0.052	-0.330
	(0.097)	(0.241)	(0.229)	(0.118)	(0.127)	(0.062)	(0.088)	(0.068)	(0.102)
Panel B. Population Measure = ASG									
GMM(1)	-0.271	-0.570	-0.091	-0.499	0.015	-0.092	-0.176	0.010	-0.270
	(0.101)	(0.243)	(0.242)	(0.124)	(0.141)	(0.063)	(0.089)	(0.070)	(0.101)
GMM(2)	-0.268	-0.511	-0.127	-0.522	-0.007	-0.093	-0.155	0.006	-0.287
	(0.095)	(0.235)	(0.227)	(0.106)	(0.127)	(0.060)	(0.086)	(0.066)	(0.097)
EL	-0.268	-0.509	-0.129	-0.522	-0.007	-0.093	-0.153	0.005	-0.283
	(0.095)	(0.234)	(0.224)	(0.117)	(0.127)	(0.060)	(0.084)	(0.067)	(0.099)
Test statistic: LEOKA population	0.02	0.70	0.30	0.30	0.14	0.00	1.11	0.02	0.49
Test statistic: ASG population	0.01	0.68	0.31	0.31	0.18	0.01	1.07	0.04	0.51
N	10,074	10,389	10,179	10,254	10,237	10,239	10,257	10,248	10,251

Note: Each column reports results of a pooled IV regression of the growth rate in each of nine crime rates on the first lag of the growth rate in the number of sworn police officers and the population size. Estimates are computed via one-step GMM (GMM1), two-step GMM (GMM2) and empirical likelihood (EL) estimation. Panel A presents the pooled police elasticity from the forward and reflected IV regressions using the growth rate in the LEOKA population measure as a control variable while Panel B presents police elasticites from pooled IV regressions using the growth rate in the ASG population measure as a control variable. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors are reported in parentheses below the coefficient estimates. Below the parameter estimates and the standard errors, we report the value of two test statistics. The first test statistic corresponds to the pooling restriction that we estimate a common parameter on the growth rate in police for the LEOKA population measure. The second test statistic corresponds to the same pooling restriction for the ASG population measure. In both cases, the test statistic uses the result from the two-step GMM procedure and refers to a test of the equality of the individual parameter estimates. The test statistic is distributed χ_1 under the null hypothesis of classical measurement error. The criticial value of the test is 3.84.

Table 6. Further Tests of Classical Measurement Errors

	Measurement Error Type		
	LEOKA-LEMAS (1)	LEOKA-ASG (2)	LEMAS-ASG (3)
Panel A: Test of Assumption A1			
Violent crimes	-0.022	-0.004	0.014
	(0.013)	(0.006)	(0.022)
Murder	0.003	-0.004	0.014
	(0.003)	(0.002)	(0.005)
Rape	0.003	-0.002	0.025
	(0.007)	(0.003)	(0.012)
Robbery	0.002	-0.002	0.040
	(0.010)	(0.005)	(0.017)
Assault	-0.016	0.004	-0.011
	(0.010)	(0.004)	(0.016)
Property crimes	0.005	-0.011	0.038
	(0.015)	(0.011)	(0.025)
Burglary	0.001	-0.016	0.023
	(0.012)	(0.008)	(0.021)
Larceny	-0.003	0.001	0.023
	(0.014)	(0.009)	(0.023)
Motor vehicle theft	0.017	-0.007	0.029
	(0.011)	(0.007)	(0.019)
F-statistic	1.05	1.34	1.73
Panel B: Test of Assumptions A2 and A3			
ASG Measure	-0.026		
	(0.023)		
LEMAS Measure		-0.050	
		(0.095)	
LEOKA Measure			-0.085
			(0.061)

Note: Each column corresponds to a particular incarnation of measurement error. In column (1), the measurement error is calculated as the difference between the LEOKA series and the LEMAS series. In column (2) the measurement error is calculated as the difference between the LEOKA series and the ASG series. Finally, in column (3), the measurement error is calculated as the difference between the LEMAS series and the ASG series. Due to the limitied availability of LEMAS data, estimates in columns (1) and (3) are calculated using the following years of data: 1987, 1990, 1992, 1993, 1996, 1997, 1999, 2000, 2003, 2004, 2007 and 2008. Estimates in column (2) use the full 1960-2008 sample period. Panel A of the table reports the results of a series of regressions of growth rate in the number of crimes on the measurement error, conditional on the growth rate in population. Panel B reports the results of a series of regressions of a given proxy for the number of police on the measurement error, calculated as the difference between the two remaining measures. Each of the models contains a full set of state by year fixed effects. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors that are robust to heteroskedasticity are reported in parentheses below the coefficient estimates.

Table 7. Pooled Estimates of the Effect of Police on Crime
With Endogenous Population Growth
GMM and Empirical Likelihood Estimation

	Violent Crime	Murder	Rape	Robbery	Assault	Property Crime	Burglary	Larceny	Motor Vehicle Theft
Panel A. Instrumental Variable = ASG Population Measure									
GMM(1)	-0.363	-0.635	-0.196	-0.593	-0.069	-0.150	-0.247	-0.034	-0.345
	(0.103)	(0.259)	(0.249)	(0.130)	(0.145)	(0.066)	(0.094)	(0.075)	(0.105)
GMM(2)	-0.360	-0.576	-0.232	-0.613	-0.091	-0.150	-0.225	-0.038	-0.361
	(0.099)	(0.247)	(0.239)	(0.123)	(0.133)	(0.064)	(0.092)	(0.072)	(0.102)
EL	-0.360	-0.578	-0.233	-0.613	-0.093	-0.151	-0.224	-0.040	-0.359
	(0.100)	(0.245)	(0.237)	(0.124)	(0.132)	(0.064)	(0.090)	(0.071)	(0.104)
Panel B. Instrumental Variable = LEOKA Population Measure									
GMM(1)	-0.358	-0.671	-0.167	-0.555	-0.094	-0.152	-0.233	-0.057	-0.326
	(0.101)	(0.250)	(0.239)	(0.127)	(0.127)	(0.065)	(0.093)	(0.073)	(0.104)
GMM(2)	-0.354	-0.596	-0.197	-0.574	-0.111	-0.151	-0.212	-0.059	-0.341
	(0.098)	(0.247)	(0.230)	(0.119)	(0.120)	(0.063)	(0.091)	(0.069)	(0.101)
EL	-0.354	-0.599	-0.286	-0.198	-0.574	-0.152	-0.210	-0.061	-0.338
	(0.098)	(0.245)	(0.228)	(0.120)	(0.120)	(0.063)	(0.090)	(0.069)	(0.103)
Panel C. Pooled Estimates									
GMM(1)	-0.361	-0.654	-0.181	-0.572	-0.083	-0.151	-0.239	-0.047	-0.334
	(0.101)	(0.259)	(0.242)	(0.118)	(0.140)	(0.065)	(0.086)	(0.073)	(0.104)
GMM(2)	-0.356	-0.588	-0.182	-0.579	-0.109	-0.151	-0.218	-0.056	-0.347
	(0.098)	(0.245)	(0.229)	(0.111)	(0.129)	(0.063)	(0.084)	(0.069)	(0.101)
EL	-0.356	-0.591	-0.191	-0.582	-0.110	-0.151	-0.217	-0.057	-0.346
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Test statistic: pooling restriction (A=B)	0.14	1.00	0.37	2.48	0.61	0.10	1.94	1.68	1.59
N	10,074	10,389	10,179	10,254	10,237	10,239	10,257	10,248	10,251

Note: Each column reports results of a pooled IV regression of the growth rate in each of nine crime rates on the first lag of the growth rate in the number of sworn police officers and the population size. As in table 5, we continue to instrument for one noisy measure of police with a second noisy measure. However, while Table 5 treat both the LEOKA and the ASG population measures as exogenous, in this table, we assume that both the LEOKA and ASG population measures are measured with error. Accordingly, both the police and population measures are instrumented using their counterparts in the corresponding dataset. Estimates are computed via one-step GMM (GMM1), two-step GMM (GMM2) and empirical likelihood (EL) estimation. Panel A presents the pooled police elasticity from the forward and reflected IV regressions, instrumenting the growth rate in the population measure in the LEOKA series with the growth rate in the population measure in the ASG series. Panel B presents police elasticites from pooled IV regressions, instrumenting the growth rate in the ASG population measure with the growth rate in the LEOKA population measure. In Panel C, we pool the police elasticity across the exhaustive combination of choices of instruments and endogenous covariates. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors are reported in parentheses below the coefficient estimates. Below the parameter estimates and the standard errors, we report the value of three test statistics. The test statistic below Panel C tests the equality of the empirical likelihood parameters in Panels A and B. This test statistic is distributed χ_3 under the null hypothesis of classical measurement error. The critical value of this test is 9.35.

Table 7. Extant Estimates of the Effect of Police on Crime
Implied Elasticities

Article	**Country**	**Years**	**Cross-Sectional Units**	**Research Design**	**Violent Crime**	**Property Crime**
Marvell and Moody (1996)	USA	1973-1992	56 cities	lags as control variables	-0.13* (murder) -0.22* (robbery)	-0.15* (burglary) -0.30* (auto theft)
Levitt (1997)	USA	1970-1992	59 cities	mayoral elections	-0.79 -3.03 (murder) -1.29 (robbery)	0.00 -0.55 (burglary) -0.44 (auto theft)
McCrary (2002)	USA	1970-1992	59 cities	mayoral elections	-0.66 -2.69 (murder) -0.98 (robbery)	0.11 -0.47 (burglary) -0.77 (auto theft)
Levitt (2002)	USA	1975-1995	122 cities	number of firefighters	-0.44* -0.91* (murder) -0.45 (robbery)	-0.50* -0.20 (burglary) -1.70* (auto theft)
DiTella and Schargrodsky (2004)	Argentina	4/1994 -12/1994	876 city blocks	redeployment of police following a terrorist attack	n/a	-0.33* (auto theft)
Klick and Tabarrok (2005)	USA	3/12/2002 - 7/30/2003	7 districts	high terrorism alert days	0.0	-0.30* (burglary) -0.84* (auto theft)
Evans and Owens (2007)	USA	1990-2001	2,074 cities	COPS grants	-0.99* -0.84* (murder) -1.34* (robbery)	-0.26 -0.59* (burglary) -0.85* (auto theft)
Our preferred estimates	USA	1960-2010	242 cities	measurement errors	-0.36* -0.59* (murder) -0.58* (robbery)	-0.15* -0.22* (burglary) -0.35* (auto theft)

Note: This table reports implied elasticities that arise from six recent articles each of which employs a novel identification strategy to estimate a *causal* effect of police on crime. Elasticities are reported for the violent and property crime aggregates as well as for murder, robbery, burglary and auto theft. In place of the original elasticities reported in Levitt (1997), we have included elasticity estimates from McCrary (2002) which correct for a coding error in the original paper. Our preferred estimates which account for the presence of measurement error in the Uniform Crime Reports police series are shown below. Asterisks denote results that are significant, at a minimum, at the 10% level.