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Are U.S. Cities Underpoliced?: Theory and Evidence*

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Abstract

We document the extent of measurement errors in the basic data set on police used in the literature on the effect of police on crime. Analyzing medium to large U.S. cities over 1960-2010, we obtain measurement error corrected estimates of the police elasticity. The magnitudes of our estimates are similar to those obtained in the quasi-experimental literature, but our approach yields much greater parameter certainty for the most costly crimes, which are the key parameters for welfare analysis. Our analysis suggests that U.S. cities are substantially underpoliced.

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I. Introduction

One of the most intuitive predictions of deterrence theory is that an increase in an offender's chances of being caught decreases crime. This prediction is a core part of Becker's (1968) account of deterrence theory and is also present in historical articulations of deterrence theory, such as 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 (2017), among others.

On the empirical side, a large literature focuses on the effect of police on crime, where police are viewed as a primary factor influencing the chances of apprehension.[1] This literature is ably summarized by Cameron (1988), Nagin (1998), Eck and Maguire (2000), Skogan and Frydl (2004), and Levitt and Miles (2006, 2007), all of whom provide extensive references.

The early panel data literature tended to report small elasticity estimates that were rarely distinguishable from zero and sometimes even positive, suggesting perversely that police increase crime.[2] The ensuing discussion in the literature was whether police reduce crime at all. Starting with Levitt (1997), the dominant narrative in the quasi-experimental literature has been that simultaneity bias is the culprit for the small and sometimes perversely signed elasticities found in the panel data literature.[3] The specific concern articulated is that if police are hired in anticipation of an upswing in crime, then there will be a positive bias associated with regression-based strategies, masking the true negative elasticity. This literature has focused instead on instrumental variables

[1] A related literature considers the efficacy of adoption of "best practices" in policing. Declines in crime have been linked to the adoption of "hot spots" policing (Sherman and Rogan 1995, Sherman and Weisburd 1995, Braga 2001, Braga 2005, Weisburd 2005, Braga and Bond 2008, Berk and MacDonald 2010), "problem-oriented" policing (Braga, Weisburd, Waring, Mazerolle, Spelman and Gajewski 1999, Braga, Kennedy, Waring and Piehl 2001, Weisburd, Telep, Hinckle and Eck 2010) and a variety of similarly proactive approaches.

[2] Prominent panel data papers include Cornwell and Trumbull (1994), Marvell and Moody (1996), Witt, Clarke and Fielding (1999), Fajnzylber, Lederman and Loayza (2002), and Baltagi (2006).

[3] Prominent quasi-experimental papers after Levitt (1997) include Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), Evans and Owens (2007), Draca, Machin and Witt (2011), Machin and Marie (2011), and Vollaard and Hamed (2012). See our earlier working paper for a discussion of some problems with this narrative.

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(IV) or difference-in-difference strategies designed to overcome this bias. These strategies consistently demonstrate that police do reduce crime. However, the estimated elasticities display a wide range, roughly -0.1 to -1, depending on the study and the type of crime.

Because of the extraordinary cost of most violent crimes and the comparatively minor cost of most property crimes, from a welfare perspective the central empirical issue for the literature is not whether police affect crime, but the extent to which police reduce violent crime, particularly murder. We formalize this point in in Section II. The analysis shows that at current staffing levels U.S. cities are almost surely underpoliced if police appreciably reduce violent crimes, particularly murder.

Unfortunately, papers in the recent quasi-experimental literature present suggestive but not persuasive evidence regarding the effect of police on violent crime. Compounding the fact that quasi-experimental research designs purposefully disregard most of the variation in police staffing levels, a further empirical challenge is that the most costly crimes are rare. Rare crimes have highly variable crime rates and even more variable growth rates, leading to parameter uncertainty. Consequently, we still know little about the elasticities that are central to a social welfare evaluation.

The leading example of parameter uncertainty in this literature is the police elasticity of murder. Two prominent papers using U.S. data are Levitt (1997, murder elasticity of -3.05 ± 4.06) and Evans and Owens (2007, elasticity of -0.84 ± 0.94).[4] Both confidence intervals are wide enough to incorporate very large elasticities (e.g., -1.5) as well as zero. Meanwhile, another prominent study estimates a police elasticity of violent crime of zero and argues that it is implausible that police affect murder (Klick and Tabarrok 2005, fn. 24).

As noted, many recent studies disregard most of the variation in police due to concerns over simultaneity bias. An obvious way to improve the precision of police elasticities is to return to regression-based methods with appropriate controls, as in Marvell and Moody (1996), for example. Importantly, however, this type of approach has the potential to run afoul of the "iron law of econometrics," or the tendency of regression coefficients to be too small because of errors in the measurement of the variable of interest (Hausman 2001). Most quasi-experimental approaches, such

[4]For Levitt (1997), we cite the corrected numbers from McCrary (2002).

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as IV, do not suffer from the same bias (Bound, Brown and Mathiowetz 2001), at least under the hypotheses of the classical measurement error model.

In this paper, we present evidence on the degree of measurement error in the basic dataset on police used in the U.S. literature, the Uniform Crime Reports (UCR), and we present estimates of the police elasticity that correct for measurement error. The implications of measurement errors in police for the estimated police elasticity of crime has, prior to this work and perhaps surprisingly, gone unaddressed in the crime literature. Our results show that prior regression-based estimates are too small by a factor of four to five, providing an alternative explanation for the small size of the elasticities from the prior panel data literature. Our evidence on measurement errors in the UCR is based on a new data set we collect that combines information on municipal police from the UCR with analogous information collected independently as part of the Annual Survey of Government (ASG). We frame our discussion of these data with the classical measurement error model. In a methodological contribution, we obtain a more efficient estimator of the policy parameter by exploiting the inherent symmetry of the classical measurement error model. We also show how that symmetry implies new tests for the restrictions of the classical measurement error model. We find little evidence against those restrictions in our data.

Our estimated police elasticities are substantively large, roughly four to five times as large as those from the traditional literature using natural variation and in line with some of the larger estimates from the quasi-experimental literature. For example, our best guess regarding the elasticity for murder is -0.67$\pm$0.47. Combining our empirical analysis with the social welfare framework suggests reduced victim costs of $1.63 for each additional dollar spent on police in 2010, implying that U.S. cities are in fact underpoliced. To the extent that lingering simultaneity bias affects our estimates, this conclusion is conservative. On the other hand, however, and as we show, our estimates are robust to controlling for the confounders mentioned in the quasi-experimental literature, including demographic factors, the local economy, city budgets, social disorganization, the presence of crack cocaine in the city, and any possible state-level policy changes that have the same effect across cities (e.g., sentencing reform, education policy 10-20 years ago, and so on). This robustness to controls might suggest a more minor role for simultaneity bias.

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II. Conceptual Framework

In this section, we outline a framework for deriving the optimal number of police. This framework shows that additional investments in police are unlikely to be socially beneficial unless police reduce violent crimes to at least a moderate degree. Reductions in property crime are simply not sufficiently costly to justify the expense of additional police officers. Violent crimes, on the other hand, are extremely costly and consequently even relatively small effects of police on violent crime would be sufficient to justify additional investment in police. Table 1 presents estimates of the annual cost of crime for different crime categories. A review of the table reveals that violent crimes are dramatically more costly than property crimes. The extreme case is murder. Even though it is exceedingly rare—occurring at a rate one-third that of the second rarest crime, rape, and one-fiftieth that of motor vehicle theft—murder accounts for fully *sixty* percent of the per capita expected cost of all crime. The framework we next outline motivates from a welfare perspective the econometric modeling of the cost-weighted sum of crimes, which gives more weight to more costly offenses.

Suppose society consists of n identical individuals, each of whom confronts a probability of criminal victimization $\phi(S)$, where S is the number of police employed by the government.[5,6] Each individual faces a victimization cost of k and has assets A that could be spent on consumption. To keep the presentation as simple as possible, we restrict attention to the case of linear utility.[7] Individuals pay a lump-sum tax τ to fund police, and the cost of an officer is w. For reference, Table 1 presents an estimate of w that is based on the "fully-loaded" 2010 cost of a police officer of $130,000.[8] On

[5]In the Theory Appendix, we extend this basic analysis to accomodate heterogeneity across persons, crowd-out of private precautions by government investments in policing, and externalities in private precaution.

[6]We assume that $\phi(\cdot)$ is differentiable and strictly convex.

[7]More generally, a third-order Taylor approximation to utility in conjunction with typical estimates of the coefficients of relative risk aversion and prudence (Chetty 2006) suggests that linear utility is a good approximation.

[8]This estimate, which is specific to the 242 large U.S. cities we study empirically below, is based on total police operating budgets relative to the total number of officers. This is closer to the concept employed in Levitt (1997) (who obtains $133,000 in 2010 dollars) than to the pure marginal cost concept employed in Evans and Owens (2007) (who obtain $73,000 in 2010 dollars). The data on operating budgets are taken from the Annual Survey of Government (ASG) Finance files, and the data on the number of officers are taken from the ASG Employment files. To accomodate

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a per capita basis, this works out to \$341, or about 1.3 percent of annual income.

In our model, the social planner maximizes the expected utility of the representative agent, subject to the financing constraint that tax receipts must equal the total wages paid police, or $n\tau = wS$. This implies a social welfare function of

$$V(S) \quad = \quad y(S) - C(S) \tag{1}$$

where $C \equiv C(S) = k\phi(S)$ is the expected cost of crime and $y(S) = A - \tau = A - wS/n$ is consumption in the absence of crime and subject to the financing constraint.[9] The first-order necessary condition for this problem, which is also sufficient, is of course $0 = V'(S)$, but it is convenient to analyze instead the proportional condition

$$0 \quad = \quad V'(S)\frac{S}{C} = y'(S)\frac{S}{C} - C'(S)\frac{S}{C} \equiv -\frac{wS}{nC} - \varepsilon \tag{2}$$

where $\varepsilon \equiv \partial \ln C / \partial \ln S$ is the police elasticity of the cost of crime, and $y'(S) = -w/n$. Next, note that in this framework, an increase in policing improves the welfare of the representative agent when policing passes a cost-benefit test. Formally,

$$V'(S) > 0 \qquad \Longleftrightarrow \qquad |\varepsilon| > \frac{wS}{nC} \tag{3}$$

Now suppose there are multiple crime categories.[10] The probability of victimization is $\phi_j(S)$ and the cost of crime is k_j, where j ranges from 1 to J. This leads to a redefinition of the expected cost of crime: $C(S) = \sum_{j=1}^{J} k_j \phi_j(S)$. With these redefinitions, equations (1), (2), and (3) remain the same as above.

However, it is useful to rewrite the aggregate police elasticity, ε, in terms of the elasticities for specific crime categories. Minor rearrangement shows that the aggregate elasticity is a weighted

outliers in the budget data, which are prevalent, we compute a city-specific median of the per sworn officer budget from 2003 to 2010, after adjusting each year's budget to 2010 dollars.

[9]Our definition of expected utility can either be thought of as implying that society is comprised exclusively of potential victims or as implying that the social planner refuses to dignify the perpetrator's increased utility, as in Stigler (1970). See Cameron (1989) for a valuable discussion of these conceptual issues.

[10]Without loss of generality, we define crime categories to be mutually exclusive so that the probability of being victimized by no crime is $1 - \sum_{j=1}^{J} \phi_j(S)$.

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average of elasticities for individual crime categories, or

$$\varepsilon = \frac{\sum_{j=1}^{J} k_j \phi_j(S) \varepsilon_j}{\sum_{j=1}^{J} k_j \phi_j(S)} \tag{4}$$

where the weights, $k_j \phi_j(S)$ are the expected cost of the crime categories and $\varepsilon_j = \partial \ln \phi_j(S) / \partial \ln S$ is the police elasticity for crime type j.

The crime-specific elasticities ε_j are the focus of most of the empirical literature on the effect of police on crime. Estimates are available for the seven so-called "index offenses" captured by the Uniform Crime Reports (UCR) system of the Federal Bureau of Investigation (FBI). For reference, Table 1 displays the costs associated with these crimes (k_j) as well as their prevalence in the population (ϕ_j scaled by 100,000) and the expected cost ($k_j \phi_j$).[11] Totalling across crime categories yields $C = \$995$, which is about 3.8 percent of per capita income in our sample. The cost figures in Table 1 thus imply that $wS/(nC)$ is about 0.34.

Some simple arithmetic using the cost figures in Table 1 in connection with the framework sketched above allows us to substantiate the claim we made above that the key policy question for this literature is not *whether* police affect crime, but the extent to which police affect *violent* crime, particularly murder.[12] Suppose that the police elasticity of crime was -1 for each property crime category, but 0 for each violent crime category. Then using equation (4) and the cost and incidence figures from Table 1, we see that the cost-weighted elasticity would be a scant -0.07—a notable departure from -0.34, the value of the cost-weighted elasticity that would justify hiring additional police.[13]

[11]The figures on the cost of crime are drawn from the literature, the most recent of which is Cohen and Piquero (2009), augmented by estimates of the value of a statistical life (VSL). The *ex ante* perspective adopted in constructing VSL figures is the appropriate one for this context. Unfortunately, for crimes other than murder, the only study to utilize an *ex ante* perspective is Cohen, Rust, Steen and Tidd (2004). Their methodology involved a contingent valuation survey in which individuals were asked to choose from among several different hypothetical dollar amounts in order to protect themselves from crime. The resulting cost estimates are much larger—often 1 to 2 orders of magnitude larger—than those given in Table 1. We use the more conventional victim cost approach to be conservative.

[12]Levitt (1997) makes a similar point in emphasizing the reliance of his cost-benefit calculation on the magnitude of the murder elasticity.

[13]We note that there are certainly benefits from policing that are not captured by the seven index offenses (e.g., arrests for other crime categories, or emergency medical response), and there may also be costs (e.g., civil liberties infringements). In this section, we are pointing out that a cost-benefit analysis focused on the seven index offenses would not justify the existing number of police.

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In a similar exercise, we might suppose that the police elasticity was -0.75 for all crimes except murder. In that case, the murder elasticity would have to be at least as negative as -0.2 to lead to a cost-weighted elasticity of -0.34.

III. The Extent of Measurement Error in the Number of Police

We begin our discussion of the nature and extent of measurement errors in police personnel data using as an example the case of New York City in 2003. The UCR data for New York show 28,614 sworn police officers in 2003. Relative to the 37,240 and 35,513 sworn officers employed in 2002 and 2004, respectively, this is a remarkably low number. If the UCR figures are to be believed, New York lost a quarter of their sworn officers in 2003 and then hired most of them back the next year.[14]

An alternative interpretion is that the 2003 number is a mistake. Internal documents from New York are available that shed light on the UCR records. 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.[15] Setting aside the data for 2003, the UCR series and the internal documents series track reasonably well; after discarding the data for 2003 and 2004, the correlation is 0.92 in levels and 0.56 in growth rates. The internal documents show that the number of sworn officers in 2003 was 36,700, not 28,614, indicating that the UCR data is incorrect.

Administrative data on police such as these are difficult to obtain. Some departmental annual reports are available, but they are not practical for econometric research. Annual reports do not circulate widely and even for cities and years where they are available, they do not always report the number of officers.[16] Trading off the accuracy of administrative data for the coverage of survey data, we now present a comparison of the UCR series on the number of sworn officers with a series based

[14]The UCR data also indicate that New York lost a fifth of their civilian police employees in 2003 and then gained them all back in 2004, arguing against confusion over sworn officers versus civilian employees.

[15]Thanks to Franklin Zimring, the internal documents of the New York City Police Department cited are available at `http://www.oup.com/us/companion.websites/9780199844425`.

[16]See the working paper version for some limited comparisons of the UCR with administrative data and data from annual reports. An interesting and econometrically problematic pattern in annual reports is the tendency to omit police numbers when other sources indicate declining police force size.

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on a separate survey collecting information on police officers, the ASG. These data are collected by the U.S. Census Bureau, rather than the FBI, and are filled out by officials in city-wide government, rather than by the police department specifically.[17] 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 more background on the ASG data in Section V, below.

Figure 2 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 ("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 association between the two series; the slope coefficient for the data in growth rates is just 0.22. This much smaller relationship is the expected pattern when the true number of officers changes slowly (Cameron and Trivedi 2005, Section 26.2.5).

Many people are surprised that there are errors in measuring the number of police officers. After all, a great deal of ink has been spilled on the topic of errors in the measurement of crime, but nearly nothing has been written on the subject of errors in the measurement of police.[18] Aside from obvious problems with transcription errors or computer programming errors, errors in measuring police could arise due to (1) transitory movements within the year in the number of sworn officers, (2) conceptual confusion, or (3) organizational confusion.

Regarding the first source of error, we are not aware of any public-use data sets containing information on within-year fluctuations in police. However, during the period 1979-1997, a unique

[17]The ASG collects information on all city government employees, while the UCR collects information only on police officers.

[18]Extensive references to the large literature on measurement errors in crime data are given in Mosher, Miethe and Hart (2011). Within economics, non-classical measurement errors in crime are the subject of two papers using U.S. data (Levitt 1998a,b), and a paper using British data (Vollaard and Hamed 2012). None of these papers contemplates measurement errors in police. The degree to which estimates of the total number of police nationally are compromised by measurement errors in the UCR data has been noted by Eck and Maguire (2000) and by King, Cihan and Heinonen (2011). However, these papers do not discuss the potential for measurement errors at the city level to bias estimates of the police elasticity derived from panel data.

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non-public dataset on sworn officers in Chicago is available that allows the construction of monthly counts.[19] In that data set, a regression of the year-over-year growth rate in sworn officers on year indicators yields an R^2 of 0.71, suggesting that more than a fourth of the movement in police growth rates is transitory.[20] This point is particularly relevant, as different data sources ask for a count of officers as of different snapshots in time, or are ambiguous about the relevant date.

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, due to frictions associated with the hiring process, there is often a discrepancy between the number of officers the department has authority from city government to employ ("authorized strength") and the number of officers currently employed ("deployed strength").[21] Using auxiliary data from the Law Enforcement Management and Administrative Survey (LEMAS), described below in Section V, we collected measures of the number of authorized and deployed sworn officers for selected recent years. These data indicate that the number of deployed sworn officers ranges from 62 to 128 percent of authorized strength.[22]

Finally, the UCR measure of sworn police has errors that may be the product of organizational confusion. For example, the internal documents for New York discussed above list the total number of sworn officers in the department as well as the number of officers assigned to one of the six largest bureaus.[23] For 2003, that latter figure was 26,367, which is notably below the average daily total

[19]These data are discussed in Siskin and Griffin (1997) and were previously used in McCrary (2007).

[20]This does not reflect seasonality, as monthly indicators raise the R^2 by only 0.0001.

[21]Typical steps include a written examination, a drug test, a background check, an interview, and a series of physical and psychological tests (Police Executive Research Forum 2005, Wilson and Grammich 2009).

[22]The population weighted mean and standard deviation of the ratio are 97 percent and 5 percent, respectively. Numbers refer to a pooled analysis of all available years of the LEMAS data. The LEMAS data also allow us to discount the possibility that there is error due to different rules for accounting for full- or part-time workers, as they show that at most 2 percent of sworn officers work part-time.

[23]These are patrol (71 percent of total), detective (9 percent), transit (8 percent), housing (7 percent), narcotics (4 percent), and vice (1 percent). Numbers taken from 2009 data, but other years are similar.

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staffing of 36,700, but close to the 28,614 reported to the UCR system. Alternatively, the 2003 number may have reflected ongoing confusion over the 1995 consolidation of the New York Police Department with the police departments of the New York City Transit Authority (April 1995) and the New York City Housing Authority (May 1995), which in 2003 together comprised approximately 5,550 officers.[24]

Since there is little hope of obtaining perfect data, it is reasonable to propose simple models of the measurement process and ask what they might imply about the econometric quantities being measured in the literature. The workhorse model in this context is the classical measurement error model, which we introduce below. As a preamble to that topic, we pause first to describe the standard econometric specification for estimating the effect of police on crime, because that is relevant to how the measurement model is specified.

In the literature, the police elasticity of crime is typically measured using regressions specified in growth rates, with the outcome being year-over-year growth rates in crime in a given year and the covariate of interest being year-over-year growth rates in sworn officers from the year prior. Taking growth rates eliminates time-invariant differences across cities and is preferred to fixed effects in this context because it requires only an assumption of weak exogeneity as opposed to strict exogeneity. The use of police once-lagged as opposed to contemporaneously is the product of several considerations. First, observed crime counts are annual totals, but the observed police numbers are a snapshot as of October 31. Second, crime may respond to police with some delay. Third, doing so may limit to some extent concerns over simultaneity. To maintain conformity with the prior literature, we follow the basic approach.

Consistent with that approach, and yet acknowleding the possibility of measurement error, suppose that in growth rates the two observed series on police (UCR and ASG) are related to true police as

$$S_i = S_i^* + u_i \tag{5}$$
$$Z_i = S_i^* + v_i \tag{6}$$

[24]That is, the individual filling out the form in 2003 may have thought transit and housing officers were not supposed to be included in the department total. Based on the 2003 internal document (see above), we compute a total of 3,986 officers uniquely assigned to transit or housing, and applying a department-wide adjustment factor of 36,700/26,367=1.39 leads to an estimated 5,548 transit and housing sworn officers in 2003. Adding that figure back in to the UCR figure of 28,614 yields 34,162 officers, which again is in the ballpark of the correct figure.

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and suppose crime growth rates, Y_i, are given by

$$Y_i = \theta S_i^* + X_i'\gamma + \epsilon_i \tag{7}$$

Here, S_i is the UCR measure in a given city and year, Z_i is the ASG measure, S_i^* is the "true" police growth rate or *signal*, X_i are other covariates measured without error, u_i and v_i are mean zero measurement errors that are mutually uncorrelated and uncorrelated with ϵ_i, S_i^*, and X_i, and ϵ_i is mean zero and uncorrelated with S_i^*, X_i, u_i, and v_i. Equations (5) through (7) and the stochastic restrictions just named together constitute the classical measurement error model (Fuller 1987).

A famous result from the prior econometric literature (see, for example, Wooldridge (2002, Section 4.4.2) or Cameron and Trivedi (2005, Section 26.2.4)) is that, under the assumptions of the classical measurement error model, the probability limit of the OLS estimate of θ, based on using the covariates X_i and the proxy S_i, is related to the scope of measurement errors and the relationship between the signal and the included covariates as follows:

$$\text{plim}_{n\to\infty}\ \widehat{\theta}_{OLS} = \theta \frac{\sigma_*^2(1-R^2)}{\sigma_*^2(1-R^2)+\sigma_u^2} \equiv \theta\ \pi \tag{8}$$

where σ_*^2 is the variance of the signal, σ_u^2 is the variance of the measurement error from equation (5), and R^2 is the population R-squared from a regression of the signal S_i^* on the covariates X_i. The parameter π is commonly referred to as the reliability ratio.

This formula stands for three ideas. First, since the reliability ratio is positive but smaller than one, OLS will be correctly signed, but too small in magnitude, or attenuated. Second, while it is a staple of empirical work to see whether a regression estimate is robust to the inclusion of various control variables, equation (8) indicates that the cure of additional covariates may be worse than the disease of omitted variables bias. Adding more controls increases the R^2, which exacerbates any attenuation bias. This is intuitive, since controls will explain the signal but fail to explain the measurement error. Third, since the estimates of θ and γ will generally covary, the bias in the estimate of θ will spill over to result in bias in the estimate of γ. This also implies that when more than one variable is measured with error, the probability limit of OLS may no longer be of the correct sign.

Now return to equation (7) and suppose that X_i is measured without error. It is straightforward

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to show that under the assumptions given, the coefficient on S_i in a regression of Z_i on S_i and X_i is consistent for the reliability ratio, π. The indirect least squares interpretation of IV then shows that IV is consistent for θ, as we discuss in the next section.[25]

IV. Econometric Approach

The three equation model introduced in Section III leads naturally to a simultaneous equations model. Substituting equation (5) into equation (7) and linearly projecting S_i onto Z_i and X_i yields

$$Y_i = \theta S_i + X_i'\gamma + e_i \tag{9}$$
$$S_i = \pi Z_i + X_i'\phi + \eta_i \tag{10}$$

where Y_i is the year-over-year change in log crime in a given city and year, S_i is the year-over-year difference in observed log police, and X_i is a vector of controls such as the year-over-year change in log revenues per capita, log population, the demographic structure of the population, as well as year effects or state-year effects. In this model, $e_i = \epsilon_i - \theta u_i$, and η_i is a linear projection error. This is then a standard simultaneous equations model where Z_i is potentially an instrument for S_i. In words, when one has two noisy measures of the same thing, instrumenting the one with the other leads to consistent estimates of ideal regression parameters under the classical measurement error model.

Estimation of the parameters in equations (9) and (10) proceeds straightforwardly by IV, and we weight observations by 2010 city population to obtain a police elasticity estimate representative of the typical person.[26] Sufficient conditions for excluding Z_i from equation (9) are

[25] The indirect least squares interpretation of IV is the familiar result that IV is the ratio of two OLS estimates, namely the reduced form and first stage coefficients. An alternative to IV that is suggested in the panel data literature is to take long differences (Griliches and Hausman 1986). This approach assumes that long differences are just as likely to be exogenous as short differences, which is unlikely in this context. In particular, in the medium- to long-term, it is possible that cities may be able to respond to perceptions of lawlessness by adjusing the size of the police force. The scope for this form of endogeneity is likely to be much weaker in a short-run context, which is one rationale for the literature's focus on the short-run police elasticity of crime.

[26] We are aware of the econometric critique of regression weighting (Deaton 1997, Solon, Haider and Wooldridge 2012). See Section VI.B, below, for discussion.

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$$(A1) \qquad \mathbb{C}[u_i,\epsilon_i]=\mathbb{C}[v_i,\epsilon_i]=0$$
$$(A2) \qquad \mathbb{C}[u_i,(S_i^*,X_i')']=\mathbb{C}[v_i,(S_i^*,X_i')']=0$$
$$(A3) \qquad \mathbb{C}[u_i,v_i]=0$$
$$(A4) \qquad \mathbb{C}[\epsilon_i,(S_i^*,X_i')']=0$$

where u_i and v_i are the measurement errors from equations (5) and (6), ϵ_i is the structural error term from equation (7), and $\mathbb{C}[\cdot,\cdot]$ is covariance.[27]

Assumptions $(A1)$ through $(A3)$ assert that measurement errors in the UCR and ASG measures of police are not associated with the structural error term in equation (7), and are not associated with the true growth rate in police and the covariate vector X_i, and that the UCR and ASG measurement errors are mutually uncorrelated, respectively. We discuss empirical implications of assumptions $(A1)$ through $(A3)$ below. Assumption $(A4)$ would justify running a regression of crime growth rates on police growth rates and controls X_i, were police growth rates observed without error.[28]

Under the classical measurement error model, the exact same steps we used to motivate the simultaneous equations model in equations (9) and (10) can be used to motivate a second simultaneous model with the roles of S_i and Z_i reversed and identical parameters in equation (9). In words, when one has two noisy measures of the same thing, one can either use the one as the instrument for the other, or the other as the instrument for the one.

Since, under the classical measurement error model, both IV estimates are consistent for the police elasticity of crime, it is possible to pool the estimates, which increases efficiency. This result is apparently new.[29] We refer to IV models that use the ASG measure of police as an instrument for the UCR measure as *forward* IV estimates and to models that use the UCR measure of police as an instrument for the ASG measure as *reflected*. Practically, to pool the forward and reflected IV estimates, we stack

[27] Assumptions $(A1)$ through $(A4)$ together imply that $\mathbb{E}[Z_i\epsilon_i]=\mathbb{E}[Z_iu_i]=0$, which implies that $\mathbb{E}[Z_ie_i]=0$, where $\mathbb{E}[\cdot]$ is expectation. Assumptions $(A2)$ and $(A4)$ imply that $\mathbb{E}[X_ie_i]=0$. Of course, $\mathbb{E}[(Z_i,X_i')'e_i]=0$ is the exclusion restriction justifiying the use of IV with Z_i as an excluded instrument and X_i as an included instrument.

[28] As our aim in this paper is to recover estimates of the police elasticity of crime that correct for measurement error, we maintain the assumption of $(A4)$, conditional on state-year effects. However, we emphasize that $(A4)$ is not guaranteed to hold and particularly will not hold in the context of simultaneity bias.

[29] For example, it is discussed in neither the classic monograph by Fuller (1987) nor in the recent review paper by Chen, Hong and Nekipelov (2011).

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the orthogonality conditions for the forward and reflected IV programs into the broader set of moments

$$g_i(\beta)=W_i\begin{pmatrix} Z_i(Y_i-\theta_1 S_i-X_i'\gamma_1) \\ X_i(Y_i-\theta_1 S_i-X_i'\gamma_1) \\ S_i(Y_i-\theta_2 Z_i-X_i'\gamma_2) \\ X_i(Y_i-\theta_2 Z_i-X_i'\gamma_2) \end{pmatrix} \tag{11}$$

where W_i is 2010 city population in levels and all other variables are as defined before and estimate the parameters using the generalized method of moments (GMM). 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 a method for estimating them by IV and allowing testing procedures to acknowledge the common dependent variable. We can also estimate the system imposing the restriction $\theta_1=\theta_2=\theta$.[30] A further benefit of pooling the two IV estimates using GMM is that the standard GMM test of overidentifying restrictions (Hansen's J) then provides a test of the classical measurement error model.[31]

A challenge we face in implementing the above ideas is that population growth is an important confounder, yet is also likely measured with error. As noted above, measurement error bias may not have the attenuation bias form if more than one covariate is measured with error. Measurement errors in the population variable in the UCR data are, to the best of our knowledge, not discussed in the literature, but are an important consideration in our view. We follow an approach suggested by Lubotsky and Wittenberg (2006) and include as controls growth rates in both the UCR and ASG population measures.[32]

[30] If we additionally seek to impose the restriction that $\gamma_1=\gamma_2=\gamma$, then in an interesting twist, the implied moments can become linearly dependent, raising computational problems for a GMM approach. In a working paper version of this paper, we discuss how empirical likelihood (EL, Owen 2001) is a natural solution to this problem. Estimates using EL and two-step GMM differ in at most the third decimal place, and we suppress those results here in the interest of space.

[31] In the Online Econometric Appendix, we provide extensive discussion of new results regarding tests of the classical measurement error model. These new results complement the use of Hansen's J and also clarify what aspects of the classical measurement error model are and are not being tested when we examine Hansen's J. The results discussed there provide little evidence against the assumptions of the classical measurement error model.

[32] In the interest of simplicity, we refer to this as "controlling for population" throughout the paper. In a working paper version of this paper, we present evidence that this procedure is sufficient to avoid bias from failure to control for city population growth.

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V. Data

In this section, we introduce our sample of cities and describe the main sources of information for our data. Our sample of 242 cities is drawn from all cities with more than 50,000 population each year from 1960-2010 and contains at least one city in 44 of the U.S. states as well as the District of Columbia.[33] For each city in our sample, we collect information from public data sources on a variety of different measures. We obtain data on crimes and sworn police officers from the UCR. We collect additional information on sworn police officers from the ASG and from the LEMAS data mentioned above. These data series form the core of our analysis, but we also collect auxiliary data on city revenues, police payroll, and police operating budget from the finance files of the ASG; city demographic structure from the Census Bureau; county-level economic data from the Bureau of Economic Analysis and Internal Revenue Service; and proxies for social disorganization from the Centers for Disease Control and the National Center for Educational Statistics. We now provide more detail regarding each of these data sources. We focus our discussion on our measures of crimes, police, and population, and provide more information regarding our auxiliary data in the Online Data Appendix.

The UCR crime data we collect are the standard measure used in the empirical literature. These data are collected monthly by the FBI and, following the literature, are aggregated to the annual level in our analysis. 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. The offenses recorded in this system are limited to the so-called index offenses—murder, forcible rape ("rape"), robbery, aggravated assault ("assault"), burglary, larceny exclusive of motor vehicle theft ("larceny"), and motor vehicle theft.[34]

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

[33] Alaska, Idaho, North Dakota, Vermont and Wyoming are unrepresented in our sample. In addition, there are 10 states for which our sample contains only a single city, which is relevant for understanding parameter estimates that condition on state-year effects.

[34] For each of our cities, the time series of index crimes, police (UCR and ASG), population (UCR and ASG, smoothed and raw), and budgets may be found at `https://econ.berkeley.edu/~jmccrary/chalfin_and_mccrary2012webappendix.pdf` under Figures 1, 2, 4, and 3, respectively.

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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 measure from year $t-1$ (cf., Levitt 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, to assess the extent of measurement errors in personnel data we augment data from the UCR with data from the employment files of the ASG. The ASG data provide annual employment counts for a large number of municipal functions, including police protection. 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. As with the UCR system, the ASG reports a point-in-time measure of police. For 1960-1995, the ASG reference period is the pay period including October 12, but beginning with 1997 the reference date has been March 12.[35] For selected analyses we also draw upon a third measure of police, as noted. This measure is drawn from two sources: the Law Enforcement Management and Administrative Statistics series and the Census of State and Local Law Enforcement Agencies. These data, which we refer to simply as the LEMAS series, have been collected at regular intervals from 1987-2008.[36]

The measure of city population used in the majority of crime research is from the FBI's Return A file, because Census data are not available annually. While this series contains observations for nearly all city-years, it is potentially contaminated by measurement error, particularly in the years leading up to the decennial Census. The population entries are contemporaneous; the FBI does not retroactively correct any of the population figures. As noted, we additionally use the annual

[35]No annual ASG survey was conducted in 1996. We impute data for 1996 using the average of the 1995 and 1997 levels. Other than this one missing year and occasional missing data, information on police is available in both the UCR data and ASG data for each of these cities for the entire study period. The UCR 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 (FTE) police personnel, without differentiating between sworn officers and civilian employees. In order to extend the series, we use the UCR data to generate a city- and year-specific estimate of the proportion of police personnel who are sworn officers. This was accomplished by regressing the proportion of police personnel who are sworn on city and year indicators using the 1960-1977 sample and generating a predicted value for the sworn percentage in each city-year. The ASG FTE numbers before 1977 were then multiplied by the estimated proportion.

[36]Data are available for 1987, 1990, 1992, 1993, 1996, 1997, 1999, 2000, 2003, 2004, 2007, and 2008.

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population estimate recorded in the ASG. For both the UCR and the ASG, we smooth the level of the series over time using local linear regression prior to computing growth rates.

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 index offenses. We additionally report summary statistics for the aggregated crime categories of violent and property crime, which simply add together the relevant corresponding individual crime categories, respectively, and for the cost-weighted crime index. The sample pertains to 10,589 city-year observations for which measures of crime, police and population growth rates are nonmissing. The left-hand panel gives statistics for levels per 100 thousand population and the right-hand panel gives statistics for growth rates. In addition to the standard reporting of mean, standard deviation, minimum, and maximum, we also break the overall standard deviations down into between- and within-city.

Several features of the data are worth noting. First, a typical city employs approximately 250 police officers per 100 thousand 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 across cities, with between city variation accounting for nearly 90 percent 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, 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.[37]

To assess the extent to which our sample of cities is representative of broader trends in crime and policing in the country, Figure 3 displays long-run trends in crime and police for our sample of 242 cities and for all cities from 1960 to 2010. The dotted lines in Panels A present the time series for total violent crimes per 100 thousand persons while the solid lines present the time series

[37]In results not shown, the first difference of a log per capita measure exhibits essentially no cross-sectional heterogeneity.

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for cost of violent crimes per person.[38] Panel B presents the same time series evidence for property crimes while Panel C presents the time series for total sworn officers. While the levels of crime and police are higher for our sample of large cities than for all cities, the trends are generally similar.

Focusing on the trends among our sample of cities, it is clear that regardless of whether crimes are cost-weighted, the series show a 30 year rise in criminality from 1960 to 1990, followed by a 20 year decline from 1990 to 2010. The magnitude of these swings is spectacular. For violent crime, costs in 2010 dollars per person quadrupled from 1960 to 1990 and then fell by half from 1990 to 2010. For property crime, costs in 2010 dollars per person tripled from 1960 to 1990 and then returned to just above the 1960 cost by 2010. Notably, our sample of cities, which covers approximately one third of the U.S. population over the 1960-2010 time period, closely parallels national trends.

VI. Results

A. Main Results

The central results of this paper are contained in Table 3. The table presents OLS, IV, and GMM estimates of the police elasticity of crime for the seven major index crimes and for the three crime aggregates—violent crimes, property crimes and the cost-weighted crime index. Columns (1)-(4) correspond to OLS models in which we regress the growth rate in crime on the growth rate in police, conditioning on population growth and either year or state-year effects. Columns (5)-(8) correspond to IV models, and column (9) corresponds to GMM models.[39,40] In addition to OLS, IV, and GMM

[38]This is simply the cost-weighted sum of crimes, computed for the subset of violent crimes, relative to the number of persons and is presented in units of dollars per person.

[39]In Table 3, and in subsequent tables, we report Huber-Eicker-White standard errors that are robust to heteroskedasticity. We note that the heteroskedasticity robust standard errors are similar in magnitude to robust standard errors, clustered at the city level. We favor the robust standard errors as they are generally slightly larger in magnitude and hence conservative.

[40]Many colleagues have asked us to compare our IV estimates to OLS applied to a subsample that excludes obvious errors. For example, out of our primary sample of 10,589 observations, roughly 1,000 have a growth rate of exactly zero (potentially consistent with simply filling out the survey with a copy of the numbers for last year) or have a growth rate in excess of 20 percent in absolute value (potentially consistent with a gross error such as New York in 2003). Applying OLS to all but those observations (the "restricted sample") improves upon OLS applied to all the data, but is far from the IV estimate. For example, the OLS estimate for the police elasticity of murder is -0.204 in the primary sample and

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estimates of the police elasticity, the bottom panel of Table 3 presents first stage coefficients.

Consistent with the scatterplots presented in Figure 2, the first stage coefficients are relatively small in magnitude, indicating that both the UCR measure and the ASG measure contain a great deal of noise once measured in growth rates. Referring, for example, to column (5) where the ASG measure is the instrument and the UCR measure is the endogenous regressor, we observe that conditional on population growth a 10 percent increase in the ASG measure is associated with only a 1.8 percent increase in the UCR measure. Column (6) shows that this result is robust to the inclusion of state-year effects with the coefficient value falling by roughly 10 percent, from 0.18 to 0.16. Turning to columns (7) and (8), which present the results from the reflected first stage regressions, we see that these coefficients are substantially larger in magnitude than the coefficients in columns (5) and (6). These differing magnitudes are expected since the UCR measure of police growth rates exhibits less variance than the ASG measure, and since the first stage coefficient is the covariance between the two measures, relative to the variance of the predicting variable. As with the forward first stage regressions, results differ only slightly when the state-year effects are added.[41]

The F-statistic on the excluded police measure is reported below the coefficient estimates. Since the F-statistics we report are all above 140, standard asymptotic approximations will be highly accurate in the context of our application. That is, weak instruments are not a concern in this context (for references to this literature, see for example Cruz and Moreira (2005)).

We turn now to our estimated police elasticities, presented in the top panel of Table 3. Beginning in column (1), we see that using the UCR measure of police officers, the OLS estimate of the police elasticity of crime is largest for murder (-0.27), motor vehicle theft (-0.19) and robbery (-0.18). 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). Reflecting the large weight on

-0.359 in the restricted sample. The IV estimate using the ASG as an instrument is -0.889, or more than twice as large as the estimate from the restricted sample (all three estimates control for two measures of population growth rates and state-year effects). If we perform the same analysis with the ASG measure as the endogenous regressor, the analogous three estimates are -0.143, -0.171, and -0.572.

[41]First stage results are similar when we condition additionally on a large number of local-level control variables, as in Table 4, below.

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murder, the cost-weighted crime elasticity is -0.21 indicating that a 10 percent increase in police is associated with a 2 percent decline in the cost of crime to victims.

The OLS estimates in column (2) control for state-year effects rather than just year effects. The explanatory power of the state-year effects indicates substantial unobserved heterogeneity in crime growth rates. For most crime types, models including state-year effects have an R^2 of about 0.60, which is remarkably high for a model specified in growth rates. However, the unobserved heterogeneity appears to be unrelated to changes in police staffing. Comparing the OLS elasticity estimates in columns (1) and (2) reveals generally minor differences, with the possible exception of murder and motor vehicle theft, which fall by close to one-third. Conditioning on state-year effects, the largest elasticities are for murder (-0.20), robbery (-0.20), and motor vehicle theft (-0.13). Elasticities for violent, property, and cost-weighted crimes are -0.12, -0.06, and -0.14, respectively.

Columns (3) and (4) report results for models in which the growth rate in crimes is regressed on the growth rate in the ASG measure of police. The coefficients in columns (3) and (4) are all of the same sign as those in columns (1) and (2). To the best of our knowledge, this is the first time a city-level panel data regression of crime on the ASG measure of police has been run.[42] Reassuringly, the results confirm the sign of the estimates based on the UCR data. While the estimates in columns (3) and (4) are generally smaller in magnitude, this is expected since the ASG measure exhibits more variance than the UCR measure.[43] The greater variance of the ASG measure also explains the somewhat greater precision of the ASG estimates. The OLS police elasticities are largely similar when the full set of state-year effects are included in column (4), with the exception of motor vehicle theft which falls by roughly half. Taken as a whole, OLS estimates of the elasticity of crime with respect to police point to a persistent but modest relationship between changes in police and criminal activity.

We turn now to the IV estimates in columns (5)-(8). These estimates are typically five times larger in magnitude than the OLS estimates. Referring to column (5), the largest elasticities are those for

[42]Marvell and Moody (1996) use the ASG police measure in regressions of the growth rate in crime on the growth rate in police at the state level.

[43]Abstracting from covariates and under the classical measurement error model, recall that the probability limit of the OLS police elasticity based on S_i is given by $\theta\mathbb{V}[S_i^*]/\mathbb{V}[S_i]$ and that based on Z_i is given by $\theta\mathbb{V}[S_i^*]/\mathbb{V}[Z_i]$.

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murder (-0.80), motor vehicle theft (-0.59), robbery (-0.46) and burglary (-0.22). Violent, property, and cost-weighted crimes have elasticities of -0.29, -0.15, and -0.61, respectively. The elasticities from the reflected IV specification reported in column (7) are generally similar in magnitudes, with elasticities for murder, motor vehicle theft and robbery of -0.74, -0.51 and -0.49, respectively, and estimates for the crime aggregates are similar to those from the forward specification.

Columns (6) and (8) present IV results that condition on state-year effects. These results are somewhat more variable and also somewhat less similar between the forward and reflected specifications. In both (6) and (8), the violent crime elasticity is approximately -0.35 and a property crime elasticity that is approximately -0.17. However, the cost-weighted crime elasticity in the forward specification is -0.61 while that in the reflected specification is -0.40.[44] With regard to the individual crimes, elasticities are largest for murder (between -0.57 and -0.89), robbery (between -0.52 and -0.57), motor vehicle theft (between -0.30 and -0.37) and burglary (between -0.17 and -0.34). While the coefficient on robbery does not change appreciably when conditioning on state-year effects, coefficients on motor vehicle theft are approximately 30 to 50 percent smaller with the inclusion of state-year effects, indicating some correlation between police growth rates and unobserved heterogeneity in the growth rate of motor vehicle theft.

In column (9), we present two-step GMM estimates of the police elasticity of crime.[45] These estimates efficiently combine the information from the forward and reflected IV estimates presented in columns (6) and (8). The GMM estimates are -0.67 for murder, -0.56 for robbery, -0.34 for motor vehicle theft and -0.23 for burglary. With regard to the crime aggregates, we report an elasticity of -0.34 for violent crimes, -0.17 for property crimes and -0.47 for cost-weighted crimes. The estimates in column (9) represent our best guess regarding the police elasticity.

[44] As noted, our estimates are essentially the same whether we include both population growth measures, instrument the one with the other, or instrument the other with the one. For example, the forward IV cost-weighted elasticity in (6) is -0.614. If we instead instrument UCR (ASG) population with ASG (UCR) population, we obtain -0.613 (-0.629). Results for the reflected estimate are similar, with corresponding numbers of -0.403 in (8) and -0.403 (-0.410) for the population IV estimates.

[45] These estimates correspond to the GMM framework in equation (11) and utilize the identity weighting matrix in the first step. We have explored other flavors of GMM and empirical likelihood approaches to the moments in equation (11), obtaining extremely similar results.

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We also calculate Hansen's J-test of overidentifying restrictions corresponding to the GMM estimates in column (9). This test provides a measure of the discrepancy between the two parameter estimates and, as the Online Econometric Appendix shows, can be thought of as a test of the classical measurement error model.[46] Under the null hypothesis, Hansen's J is distributed χ^2_1. We suppress these test statistics in the interest of space, but they are generally quite small and uniformly below the 95 percent critical value of 3.84. This implies that the diffferences between the forward and reflected IV estimates in columns (5) and (7) and (6) and (8) are consistent with sampling variability.

B. Robustness

Before turning to a discussion of the results presented above, we consider several robustness checks. The estimates in Table 3 assume the exogeneity of police conditional on population growth and state-year effects. While state-year effects absorb important time-varying state-level variation, results will nevertheless be inconsistent if there are time-varying city-level factors which are correlated with both police and crime growth rates. In Table 4, we explore the extent to which the elasticities reported in Table 3 are robust to the inclusion of a variety of city-level covariates. Unfortunately, these covariates are not available for our entire sample period and we thus restrict attention to the 1970-2002 subsample.[47]

We begin in column (1) of Table 4 by replicating the GMM estimates for the 1970-2002 subsample of our data. These estimates condition on population growth and state-year effects and correspond to column (9) of Table 3. For the 1970-2002 subsample, the violent crime elasticity is -0.22 and the property crime elasticity is -0.18. The largest elasticities are for murder, robbery, and motor vehicle theft (-0.62, -0.59, and -0.34, respectively). Interestingly, the coefficients on rape and assault for this time period are perversely signed. However, the police elasticity of cost-weighted crimes

[46] In principle, the test statistic is available using several different estimators. We compute the test statistic using two-step GMM. Because we are unwilling to assert that the variance matrix of the errors is spherical, the two-step GMM estimator is no longer the efficient estimator in its class, which implies that the test of over-identifying restrictions is not equal to the minimized value of the objective function. However, the proper test statistic can nonetheless be constructed; see Newey (1985) for a discussion and the proper formula for this case.

[47] In a working paper version of this paper, we document small associations between police growth rates and these city-level covariates.

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is -0.43, which is close to our preferred full-sample estimate of -0.47 from Table 3.

In column (2) we add a series of economic covariates that capture the growth rate in personal income and total employment as well as revenue and employment in 4 leading industrial sectors (construction, manufacturing, wholesale trade and retail trade). We also include city public expenditures exclusive of police to capture other municipal programs that might correlate with both police and crime. In column (3), we include the lags of each of these variables to capture a potentially lagged response of crime to local economic conditions. In column (4), we capture changes in a city's demographic composition by adding control variables for the population share of sixteen age-gender-race groups within each city. In order to control flexibly for the effect of changes in a city's demographic composition, column (5) adds squares and cross-products of the demographic controls. Finally, column (6) adds city-specific linear time trends.

Looking across columns (1)-(6) of Table 4, it is apparent that the estimated elasticities change very little with the inclusion of the controls. Referring, for example, to cost-weighted crimes, the estimated elasticity moves from -0.43 when conditioning only on population and state-year effects to -0.41 when economic covariates are included. Conditioning also on the lags of the economic covariates brings the estimated elasticity up to -0.42 while controlling extensively for demographics brings the elasticity back to -0.39. When time trends are included, the elasticity increases to -0.41, just 2.5 percent lower than the original elasticity. A similar pattern holds for each of the other crime types with the largest change from column (1) to column (6) occurring for murder, which falls by roughly 5 percent. The results in Table 4 thus imply a relatively minor role for the confounding of police growth rates with unobserved determinants of crime growth rates, at least conditional on population growth rates and state-year effects.[48]

Finally, in columns (7)-(9) of Table 4 we additionally consider the robustness of our estimates to controls for the emergence of crack cocaine, characterized by a number of scholars as a prominent exogenous shock to urban crime markets during the 1980s and early 1990s (Grogger and Willis 2000, Fryer, Jr., Heaton, Levitt and Murphy 2013). Using data on the prevelance of crack-cocaine in 123 U.S. cities compiled by Fryer, Heaton, Levitt and Murphy (2013) for the 1980-2000

[48]In unreported results, we find a similar lack of confounding when state-year effects are replaced by the more parsimonious year effects.

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period, we augment the model estimated in column (6) with a control for the "crack index," a variable that captures various proxies for the prominence of crack, including cocaine arrests, cocaine-related emergency room visits, cocaine-induced drug deaths, crack mentions in newspapers and DEA drug busts. Column (7) reports the same model that is reported in column (1) for the 123 city subsample of our data over 1980-2000. In column (8) we add the crack index, and in column (9), we condition on the full set of covariates used in column (6). The estimates do not change with the inclusion of the crack index (column (8)) and indeed move very little even when the full set of covariates are included (column (9)). The cost-weighted elasticity in column (9) (-0.48) is nearly the same as our preferred full-sample elasticity estimate from Table 3 (-0.47).

We conduct three final robustness checks that are worth mentioning. First we assess the sensitivity of our preferred estimates (the GMM estimates in column (9) of Table 3) to the exclusion of several theoretically-motivated groups of cities—the two largest cities in the sample, cities that have merged with their respective counties (e.g., Jacksonville), and cities which have been recently found to have misreported data to the FBI's Uniform Crime Reporting System (e.g., Milwaukee). When these cities are excluded from the sample, the estimates are nearly identical to those reported in Table 3 (unreported results). Second, we address the possibility of displacement ("reshuffling")—the idea that an increase in policing in one jurisdiction might displace crime to a nearby jurisdiction—by aggregating the data to the MSA level. Estimates from the higher level of aggregation are, if anything, somewhat larger than those from the city level analysis, suggesting displacement is an unlikely explanation for our results unless crime is as likely to be displaced between MSAs as it is between cities (cf., Appendix Table 2, working paper version). Third, following Solon et al. (2012), we examine interactions of the police growth rates with population to assess whether our weighted least squares procedure recovers the average partial effect. These results support the interpretation of our main results as the police elasticity of crime for a typical person in our sample of cities.[49]

[49]As noted by Solon et al. (2012), weighted least squares will not necessarily estimate the average partial effect in the presence of unmodeled heterogeneous effects. They suggest an alternate procedure whereby population is interacted with the main effect of interest. We re-estimated the population-weighted OLS estimates in Table 3 using this formulation, centering population around the population of the city in which a typical individual lives in our sample, which we write as $\overline{w}$, and including the

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VII. Discussion

The 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. Our preferred estimates are the GMM estimates from column (9) of Table 3. In this section, we compare our reported elasticities to those in the prior literature.

Table 5 presents selected police elasticities from eight recent papers utilizing U.S. data.[50] Each seeks to correct for simultaneity bias, for which our estimates do not adjust. The dominant methodologies are quasi-experimental, but two papers focus on regression strategies with high quality control variables (Marvell and Moody 1996, Corman and Mocan 2005). None of these papers discuss the possibility of measurement errors in police. As noted above, IV and difference-in-difference strategies will correct for both simultaneity bias and measurement error bias under the classical measurement error hypothesis. The two papers using regression strategies may nonetheless suffer from measurement error bias.[51]

The results in these papers display four evident tendencies. First, the estimates are generally negative. Some of the estimates are zero—Levitt (1997) for property crime and Klick and Tabarrok (2005) for violent crime—but almost none are positive.[52] Second, the estimates from the quasi-experimental U.S. literature tend to be similar to, or perhaps slightly larger in magnitude than, our own estimates. It is difficult to know how to interpret this pattern. For example, there is a good deal of sampling variability associated with each estimate, indicating that not too much stock should

population weight as an additional regressor. Under a linear approximation to the heterogeneity, i.e., $\theta(W_i)=\theta(\overline{w})+(W_i-\overline{w})\theta'(\overline{w})$, where the prime indicates differentiation, the coefficient on the growth rate in police represents the average partial effect. The estimates we obtain are similar to those reported in Table 3. For example, we obtain OLS estimates (standard errors) for the UCR and ASG measure of of -0.123 (0.042) and -0.092 (0.037) for violent crime and -0.049 (0.030) and -0.030 (0.026) for property crime, respectively. The degree of similarity between these results and those in columns (2) and (4) of Table 3 provide little evidence in favor of important unmodeled heterogeneity.

[50] We note that our focus on U.S. data here excludes several interesting and well-executed papers utilizing data from other countries, including Di Tella and Schargrodsky (2004), Draca et al. (2011), Machin and Marie (2011), and Vollaard and Hamed (2012).

[51] On the other hand, Corman and Mocan (2005) use administrative data on the number of police which may be less subject to error.

[52] It is important to note that elasticity estimates reported in Klick and Tabarrok (2005) are based on their estimates of the change in police strength resulting from a natural experiment, not an explicit measure of the change in police manpower.

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be placed in any differences. Bracketing the issue of sampling variability, however, one obvious interpretation for the discrepancy is that the papers cited in Table 5 correct for simultaneity bias, whereas our estimates do not. On the other hand, the samples involve quite different time periods and this alone may be a sufficient explanation for any discrepancies. For example, when we restrict our analysis to the years analyzed by the very careful study by Evans and Owens (2007), namely 1990 to 2001, our estimated elasticities are -0.83 and -0.31 for violent and property crimes, respectively. These are extremely close in magnitude to those reported by Evans and Owens (-0.99 and -0.26).

Third, there is considerable variability in the prior literature with regard to whether police have a larger protective effect on violent crimes than on property crimes. In particular, two papers find violent crime elasticities that are larger than property crime elasticities (Levitt 1997, Evans and Owens 2007); two find violent and property crime elasticities that are roughly equal (Levitt 2002, Corman and Mocan 2005); and two find property crime elasticities that are larger than violent crime elasticities (Klick and Tabarrok 2005, Lin 2009).[53] Our estimates are closest to those of Evans and Owens.

Fourth, the estimated elasticities tend to be quite imprecise, with estimated standard errors ranging from 0.2 to 0.7 for violent crimes and 0.2 to 0.9 for property crimes. As a result, it is often the case that even large elasticities (on the order of -1) cannot be rejected as being different from zero. Similarly, the cross-crime pattern of the elasticities is difficult to discern. For example, one of the more precise studies is that of Evans and Owens (2007). In that study, the magnitude of the estimated elasticities and standard errors suggest that it would be difficult to reject tests of the equality of various crime-specific elasticities. As a result, though the general pattern of the elasticities is suggestive, it is difficult to draw inferences about even the most basic policy questions such as the relative effectiveness of police in reducing violent versus property crimes. As we noted in Section II, the core question for the literature is not whether police affect crime, but whether they affect violent crime, particularly murder.

The elasticities reported above in Table 3 are estimated with considerably greater precision than those from the prior literature, with standard errors that are between one-quarter and one-half the

[53]We note that Corman and Mocan (2005) employ monthly data on New York, whereas the other papers in the table employ annual data on multiple cities. This may imply that the estimand from the Corman and Mocan study differs from that of other studies.

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size of even those reported by Evans and Owens (2007), and up to an order of magnitude smaller than those reported in other papers. The result is that we are able to generate considerably stronger inferences regarding the core question of interest.

In Table 6, we formalize this idea and test the equality of all pairs of individual crime elasticities. The table reports p-values from each of these tests, operationalized by stacking up crime categories into a broader GMM system. Each entry in the table reports the p-value associated with a test of the equality of the coefficients for the crime categories in the row and column. The table suggests that we can be confident that police reduce murder to a greater extent than assault and larceny and perhaps burglary. Likewise, the effect of police on robbery is greater than it is for assault, burglary and larceny, and the effect of police on motor vehicle theft is greater than the effect of police on larceny. Referring to the aggregates, the elasticities for murder and robbery are greater than the property crime elasticity. We can also reject, at the 10 percent level, the equality of the violent and property crime elasticities.

The cross-crime pattern of the police elasticity estimates could reflect non-classical measurement error, relative deterrence effects, or relative incapacitation effects. The variety of non-classical measurement error that could lead to the cross-crime patterns we observe is simple: if reporting to police is increasing in police, then *measured* crimes could be increasing in police, even if the true count of crimes is decreasing in police.[54] In a particularly well-known recent episode, the Sacramento police department responded to deep budget cuts by announcing they would no longer respond to "burglary, misdemeanors, or minor traffic accidents" and would only conduct follow-up investigations for murder and sexual assault (Goode 2012). Similar policy changes have been reported for Camden (Goldstein 2011), Chicago (Spielman 2013), Oakland (Preuitt and Sanchez 2010), and Stockton (DuHain 2012). Such policies seem likely to result in a reduced reporting rate. If reporting is affected by changes in police staffing, this should amplify our central policy conclusion. Our analysis shows that the police elasticity of *reported* crime is negative and large enough in magnitude to suggest underpolicing (cf., Section VIII, below). Correcting for reporting bias would lead to police elasticities that are more negative than those we document and strengthen our ultimate policy conclusion.

[54]Levitt (1998a) finds little evidence for this effect in U.S. data, but Vollaard and Hamed (2012) do in British data.

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The cross-crime pattern of police elasticities may also reflect differences in deterrence and incapacitation effects across crimes. The deterrence effect of police is that some crimes will not occur, because a person notes the increase in police presence and thereby is deterred from committing the offense. The incapacitation effect of police is that some crimes will not occur because additional police will result in arrests, pre-trial detention, and jail or prison time for the convicted (McCrary 2009). It is clear that deterrence effects could differ across crime types. To see why incapacitation effects could lead to differences across crimes in elasticity estimates, note that in an efficient criminal justice system the most serious recidivists are those most likely to be placed in pre-trial detention and are most likely to be incarcerated if convicted. To the extent that police, prosecutors, and other actors focus on the population of serious recidivists, we might indeed expect strong cross-crime differences.

Whether our estimates are similar to or different from those in the preceding literature is important for getting the magnitude of police elasticities right, but is also interesting because it speaks to the broader issue of whether simple regression techniques are compromised by simultaneity bias. If our estimates are deemed to be similar to those reported in prior research, then our research implies a smaller role for simultaneity than has been suggested by prior studies. If, on the other hand, simultaneity bias is a lingering issue (Nagin 1978, Nagin 1998, Levitt and Miles 2007), our approach underestimates the magnitude of the policing elasticity, thus strengthening our ultimate policy conclusion.

VIII. Social Welfare Analysis

A. Simple Estimates of the BCR

The results presented in Table 3 represent our best estimates of the police elasticity of crime. We now connect those estimates to the broader policy question of whether U.S. cities are underpoliced, drawing on the analysis of Section II. The rule-of-thumb outlined in that section is that hiring police improves welfare when

$$|\varepsilon|/\frac{wS}{nC} \equiv \kappa > 1 \tag{12}$$

where κ can be viewed as a benefit-cost ratio (BCR). The Online Theory Appendix outlines how this rule-of-thumb remains relevant under a broad array of conditions, such as when there is heterogeneity

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across individuals, when investments in public police can crowd out individual investments in precaution, and when there are externalities to individual precautions. That analysis shows that the rule-of-thumb is quite robust to alternative considerations.[55]

Our preferred estimate of the police elasticity of cost-weighted crimes is -0.47 ± 0.34. This elasticity estimate is based on a model including state-by-year effects and controls for population (Table 3, column (9)). The cost-weighted elasticity is powerfully affected by the assumed monetized value of an averted murder, which in Table 1 was taken to be \$7 million, based on the VSL literature (cf., footnote 11). To assess the sensitivity of our conclusions to this figure, we recomputed our GMM estimate of the police elasticity of cost-weighted crimes using VSL figures ranging from \$1 to \$28 million. These estimates range from -0.32 ± 0.18 to -0.55 ± 0.52.

As noted above, scaling the cost-weighted elasticity by the ratio of the expected cost of crime to the cost of police produces an estimate of the 2010 social dollars saved from increasing spending on police by one dollar, or the BCR. As with the cost-weighted elasticity, the BCR is sensitive to the assumed VSL value.[56] Figure 4 demonstrates this visually, plotting the BCR on the vertical axis as a function of possible VSL values on the horizontal axis. The BCR ranges from approximately 0.4 at a VSL of \$1 million to approximately 6.0 at a VSL of \$28 million.

To give some sense of what is a reasonable value for the VSL, we superimpose a kernel density estimate of the density of 64 VSL estimates drawn from the recent literature or currently in use by the federal government.[57] While the estimates vary considerably, approximately 80 percent of the data lies below \$10 million which is associated with an approximate BCR of 2. At \$7 million, the mean value of the VSL, the resulting BCR is 1.63, indicating that, in a typical U.S. city, an additional dollar allocated towards policing would save \$1.63 in costs to crime victims. This would

[55] More specifically, the conclusion that U.S. cities are underpoliced holds unless (1) public policing crowds out private investments in precaution, (2) private precautions have *positive* externalities on average, *and* (3) the externality effect is fully 39 percent as big as the direct effect of police. If instead private precautions have beggar-thy-neighbor effects on average, the return to a dollar invested in policing *exceeds* \$1.63. See the Online Theory Appendix for details of this argument.

[56] In the literature, it is not uncommon for the results of a benefit-cost analysis of a given policy to depend on the researcher's choice between two reasonable alternative values of the cost of a murder.

[57] See the working paper version of this manuscript for greater detail.

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be consistent with classical notions of the underprovision of public goods. On the other hand, as noted there is substantial ambiguity regarding VSL estimates. The estimated VSL from Ashenfelter and Greenstone (2004) implies a BCR of roughly $0.80, indicating overpolicing.[58]

Since we are using cost-of-crime figures from 2010, these figures assume we are seeking to apply a cost-benefit analysis as of 2010. We are unaware of any reliable basis for changes over time in these cost figures. However, since these costs are dominated by the cost of murder, a reader with strong prior grounds for believing that the VSL should have been historically lower or higher than at present can approximate the benefit-cost ratio that would result by reference to Figure 4.

B. Police Incapacitation Effects

The estimates in the preceeding sub-sections are valid under the assumption that either (i) the decline in crime resulting from increased police is entirely due to deterrence or that (ii) the cost of incarcerating offenders is fixed in the short run so that the downstream cost of incapacitating offenders need not be counted as a cost of increased police personnel. Here, we re-frame the analysis, treating the expected short-term increase in incarceration resulting from more police as an additional cost of hiring a new officer. Because we are interested in the short-run costs and benefits of new police hiring, we count only the costs of incarceration that are borne in the first year. We note that the long run effect on incarceration would be smaller than the effect in the first year, as the prison population would be smaller due to the lower prevalence of crime (c.f., McCrary 2009, equation (34)).

We begin with an estimate of the number of arrests per officer. Using our sample of 242 cities, an officer in the typical city made 19.45 arrests in 2010.[59] Next, we employ an estimate of the conditional probability of a conviction given an arrest. In 2006, the most recent year available for convictions, there were 14,380,370 arrests made by police officers in the United States while there

[58]We note that estimates pertaining to individuals' labor market behavior tend to yield larger VSL values (on average, $9.5 million), while estimates from non-labor market behaviors tend to yield much smaller VSL values (on average, $4 million).

[59]The working assumption here is that a new officer's productivity, and the lost productivity associated with laying off an officer, can be approximated using the productivity of an average officer. We obtain 18.7 (20.2) arrests using the UCR (ASG) officer count, which we average to obtain 19.45.

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were 1,132,290 convictions in state courts and another 81,934 convictions in federal courts.[60] Dividing convictions by arrests yields an estimated conditional probability of a conviction of 8.4 percent. This implies about 1.64 convictions per police officer.

Of defendants sentenced in state courts, 40 percent were sentenced to state prison (with a mean sentence length of 4.92 years) 28 percent were sentenced to a term in local jail (with a mean sentence of 0.5 years) and the remaining 32 percent were sentenced to a term of probation or an alternate penalty that did not involve incarceration. This implies an expected sentence length given conviction of 2.11 years. Using the National Corrections Reporting Program data for 2006, we estimate that for the seven index offenses, individuals serve 47.5 percent of their nominal sentence, for an effective sentence per conviction of 0.98 years.[61] Thus, a typical officer is associated with 19.45 arrests, 1.64 convictions, and 0.98 incarceration-years.[62] At an incarceration cost of $33,089 per year, each new officer is thus associated with $32,344 in additional incarceration costs.[63] Augmenting the salary figure with this estimate yields a benefit-cost estimate of $1.31 using the $7 million estimate of the value of a statistical life.

IX. Conclusion

In this paper, we have advanced an argument and presented evidence relevant to it. Our argument is that from a welfare perspective the effect of police on property crime is not nearly as important as the effect of police on violent crime, particularly murder. The essence of this claim is that violent crimes, especially murder, are highly relatively costly for society, so costly as to dwarf the cost of most property crimes. For example, the most costly of property crimes, motor vehicle theft, is 46 times more likely to happen to a citizen than is their murder. Yet even accounting for its rarity, the expected cost of murder is 27 times that of motor vehicle theft. Citizens would pay an enormous sum

[60] The national arrest figure is from FBI (2006), and the conviction and court processing figures here and in the remainder of this passage are from BJS (2009).

[61] See `http://www.bjs.gov/index.cfm?ty=pbdetail&iid=2045`, particularly Table 0610.

[62] Note that we ignore the possible role of pre-trial detention, since time served is typically taken off of any sentence received.

[63] This figure is based on the ratio of state corrections expenditures in 2007 to the number of prisoners, adjusted to 2010 dollars. See `http://www.bjs.gov/index.cfm?ty=pbdetail&iid=4332`.

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of money to reduce slightly their chances of being murdered, just as they implicitly pay (through state and federal government tax and spending decisions) enormous sums to reduce slightly their chances of being involved in a fatal car or mining accident, and we have argued that this is the perspective government ought to adopt in choosing how much to invest in policing.

However, the recent crime literature has focused more on establishing that police reduce crime generally, rather than the extent to which police reduce violent crimes, or specific crimes such as murder. While these papers exhibit extraordinary creativity in grappling with the simultaneity bias problem, there nonetheless remains substantial ambiguity about the effect of police on the most costly crimes. As we have noted, some studies find larger effects for violent crime than for property crime, some studies find the opposite, and some studies find similar sized effects for violent and property crimes. Moreover, there is additional ambiguity due to simple parameter uncertainty, and this is particularly pronounced for murder. Almost none of the papers in the prior literature documents an effect of police on murder that is distinguishable statistically from no effect at all.

The evidence we have marshalled regarding our argument is a series of measurement error corrected estimates of the police elasticity of crime. Combining these estimates with the social welfare approach we outline suggests that increases in police in medium to large U.S. cities in recent years would have substantially improved social welfare. We estimate that as of 2010 in our study cities, a dollar invested in policing yields a social return of $1.63.

Several considerations suggest that this estimate of the social return to investing in police is, in fact, conservative. First, the literature has consistently argued that simultaneity induces a positive bias on regression-based elasticity estimates—that is, the elasticity estimates are not as negative as they should be, because when crime increases so do police. Our estimates do not correct for simultaneity bias and from this perspective may be conservative. Second, the cost of crime that we have used in quantifying the return to investing in police is limited to what a victim would be willing to pay to obtain a reduced probability of victimization for murder, rape, robbery, assault, burglary, larceny, and motor vehicle theft. While these are the only police outputs that are reliably measured in our data, these are not the only crimes police are tasked with preventing and solving. Presumably police activities in these other

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arenas (e.g., domestic violence or driving under the influence) are also socially beneficial and outweigh unmeasured costs of policing (e.g., civil liberties infringements). Finally, crime has an extraordinary ripple effect on economic life, leading to expensive population and economic reorganization within space (Cullen and Levitt 1999, Rosenthal and Ross 2010) as individuals relocate activities to keep safe. These considerations underscore the primary conclusion of this paper that U.S. cities are underpoliced.

Finally, in a methodological contribution, we show how the fundamental symmetry of the classical measurement error model implies that there are two consistent estimators for the parameter of interest, rather than one, indicating a role for a more efficient generalized method of moments (GMM) estimator. The GMM framework then also suggests a series of specification tests that, as we demonstrate, have good power to reject violations of the classical measurement error model. We find little evidence against the classical measurement error model in these data, ratifying the validity of these methods and the conclusions we have drawn from them.

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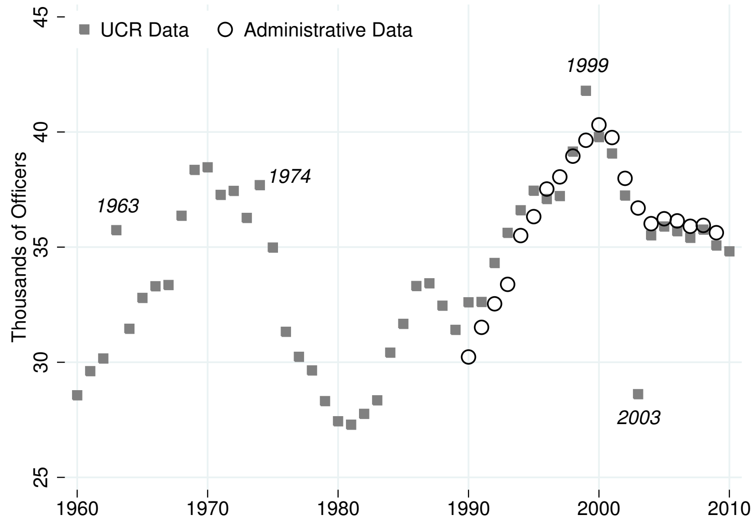
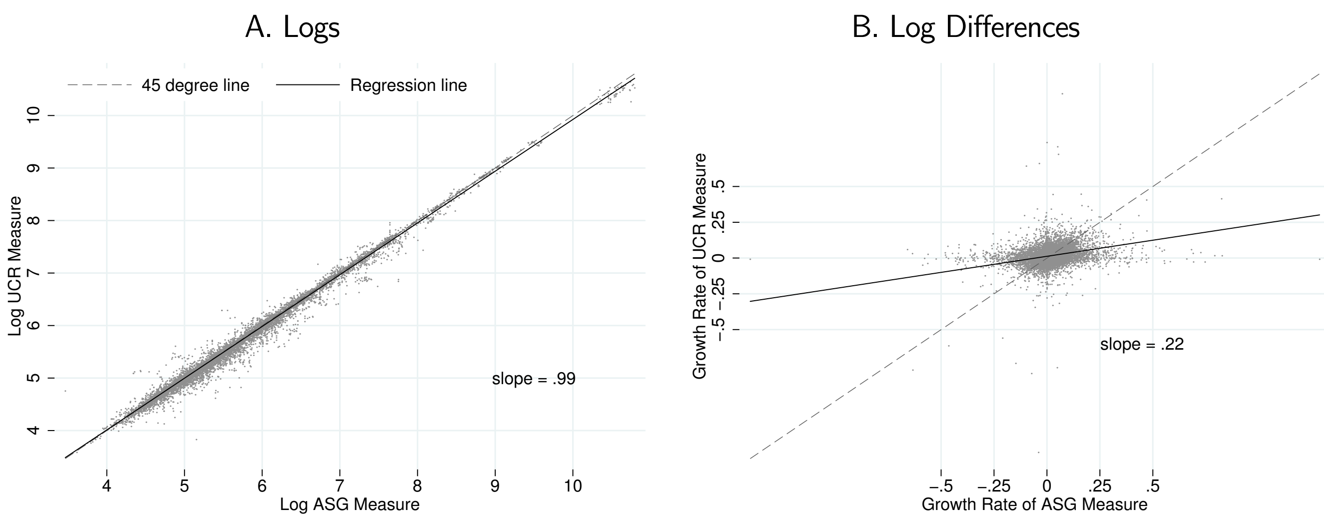
Figure 1. Sworn Officers in New York City: the Uniform Crime Reports and Police Administrative Data



Note: Graph compares the UCR measure of police from the FBI to administrative data on police personnel reported directly by New York City.

Figure 2. Two Leading Measures of Sworn Officers: the Uniform Crime Reports and the Annual Survey of Government



Note: Panel A plots the relationship between the UCR and ASG measures of police in logs. Panel B plots the relationship in log differences ("growth rates"). For ease of visual comparison, in Panel B, we have focused on data points for which the growth rates are smaller than 50 percent in magnitude. The vast majority (99.9 percent) of the data are in this space. The regression slope (0.22) is drawn through the entirety of the data. See text and online Data Appendix for details.

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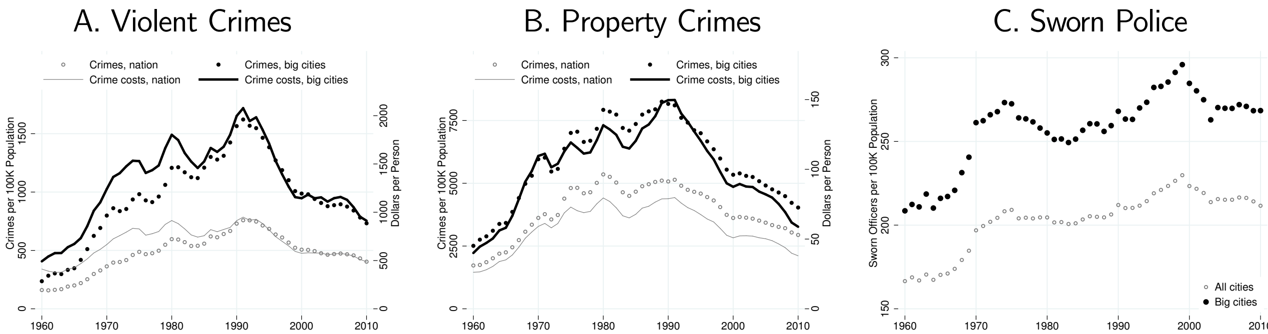
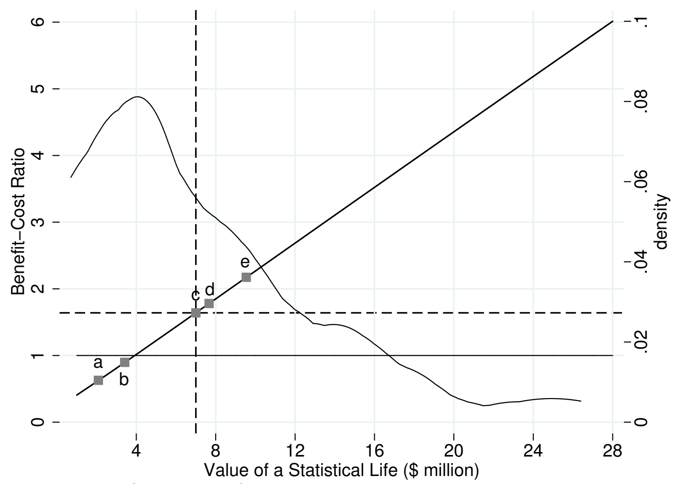
Figure 3. Aggregate Trends in Violent and Property Crime and Police: Evidence from the Uniform Crime Reports



Note: In panels A and B, data on crimes nationally are taken from `http://www.ucrdatatool.gov`. In panel C, no such data are available, and we construct an index using all municipalities ever reporting to the UCR system 1960-2010 and imputation. In all panels, "big cities" refer to the sample of 242 cities analyzed in the paper. See text and online Data Appendix for details.

Figure 4. Cost-Benefit Analysis
Benefit-Cost Ratio as a Function of the Value of a Statistical Life



Note: The table plots the value of the benefit-cost ratio calculated using the two-step GMM procedure that pools the "forward" and "reflected" IV regressions 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, conditional on both the UCR and the ASG measure of the growth rate in the population size and a vector of unrestricted state-by-year dummies. For each measure of police, expenditures on personnel are estimated by multiplying the number of personnel by $130,000, an estimate of the "fully-loaded" annual salary of a police officer. Victimization costs for rape, robbery, assault, burglary, larceny and motor vehicle theft are drawn from Cohen and Piquero (2009). As there is a great deal of variation in extant estimates of the value of a statistical life, the cost of murder is allowed to vary. Using the solid black line, we plot the benefit-cost ratio on the vertical axis as a function of the value of a statistical life, plotted on the horizontal axis in millions of dollars. The horizontal line corresponds to a benefit-cost ratio of 1. In addition, we superimpose a kernel density function that plots the distribution of the extant estimates of the value of a statistical life. Key estimates include the $2.1 million VSL estimated by Ashenfelter and Greenstone (2004) ("a"), $3.4, the mean VSL among studies of non-labor market behavior ("b"), $7 million, the mean VSL among all studies in the literature ("c"), $7.7 million, the mean VSL used by various federal agencies for the 2004-2010 period ("d") and $9.5 million, the mean VSL among studies of U.S. labor market behavior ("e"). The dotted lines show the BCR ($1.63) at the mean value of a statistical life ($7 million). The majority of these estimates are drawn from Viscusi and Aldy (2003). We supplement these estimates with several that are drawn from the more recent literaure.

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Table 1. Costs of Police and Crime

	Cost per Officer	Officers per 100K Population	Annual Cost per Capita
Sworn police	$130,000	262.7	$341

	Cost per Crime	Crimes per 100K Population	Annual Expected Cost per Capita
Murder	$7,000,000	9.9	$693
Rape	$142,020	30.9	$44
Robbery	$12,624	286.4	$36
Assault	$38,924	418.9	$163
Burglary	$2,104	976.2	$21
Larceny	$473	2,623.3	$12
Motor vehicle theft	$5,786	454.3	$26
		Grand Total:	$995
		Income per capita:	$26,267

Note: Numbers pertain to a sample of 242 large U.S. cities in 2010, which have a collective population of 73,820,297. Data on crimes from the Uniform Crime Reports. Data on income per capita are taken from the American Communities Survey five-year estimates (2007-2011). Data on costs of police and crime taken from the literature. See text for details.

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Table 2. Summary Statistics on Police and Crime

		Levels				Log Differences			
Variable		**Mean**	**S.D.**	**Min.**	**Max.**	**Mean**	**S.D.**	**Min.**	**Max.**
Sworn police, UCR	O	245.5	111.6	54.4	786.6	0.016	0.058	-1.359	1.148
(per 100K pop)	B		105.7				0.012		
	W		36.0				0.056		
Sworn police, ASG	O	257.7	128.0	50.1	779.8	0.016	0.078	-1.401	1.288
(per 100K pop)	B		120.4				0.012		
	W		42.4				0.078		
Violent crimes	O	972.7	630.5	8.2	4189.0	0.035	0.162	-1.804	1.467
(per 100K pop)	B		440.3				0.019		
	W		451.4				0.161		
Murder	O	14.6	10.6	0.0	110.9	0.014	0.382	-2.792	2.446
(per 100K pop)	B		8.4				0.021		
	W		6.5				0.382		
Rape	O	49.0	29.6	0.0	310.5	0.035	0.291	-4.384	4.199
(per 100K pop)	B		17.4				0.028		
	W		23.9				0.289		
Robbery	O	438.0	344.5	1.1	2,358.0	0.035	0.202	-1.792	1.946
(per 100K pop)	B		257.5				0.019		
	W		228.9				0.201		
Assault	O	471.1	329.5	1.2	2,761.4	0.037	0.213	-2.833	3.129
(per 100K pop)	B		209.5				0.024		
	W		254.4				0.212		
Property crimes	O	6,223.4	2,355.0	667.3	18,345.2	0.015	0.113	-1.304	1.248
(per 100K pop)	B		1,366.2				0.014		
	W		1,918.2				0.112		
Burglary	O	1,671.9	810.9	143.0	6,713.5	0.010	0.149	-1.549	1.411
(per 100K pop)	B		433.8				0.018		
	W		685.1				0.148		
Larceny	O	3,655.4	1,500.2	84.2	11,590.7	0.017	0.122	-1.435	2.146
(per 100K pop)	B		982.6				0.015		
	W		1,133.7				0.121		
Motor vehicle	O	896.0	574.5	42.5	5,294.7	0.014	0.169	-1.516	1.447
theft	B		428.6				0.016		
(per 100K pop)	W		435.3				0.169		
Cost-Weighted	O	1,433.9	904.9	15.36	8,909.2	0.019	0.271	-2.363	3.033
Crimes	B		699.6				0.018		
($ per capita)	W		573.9				0.270		

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 three 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. Summary statistics are reported both in levels per 100,000 population and in growth rates. All statistics are weighted by 2010 city population. The sample size for all variables is N=10,589.

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Table 3. Estimates of the Effect of Police on Crime

	Least Squares Estimates				2SLS Estimates				GMM Estimates
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	UCR Measure		ASG Measure		*Forward Models* UCR Measure		*Reflected Models* ASG Measure		*Pooled Models*
Violent crimes	-0.117 (0.037)	-0.120 (0.040)	-0.053 (0.024)	-0.058 (0.023)	-0.289 (0.128)	-0.361 (0.143)	-0.321 (0.100)	-0.336 (0.106)	-0.344 (0.096)
Murder	-0.270 (0.071)	-0.204 (0.097)	-0.148 (0.047)	-0.143 (0.059)	-0.804 (0.260)	-0.889 (0.364)	-0.742 (0.198)	-0.572 (0.262)	-0.666 (0.238)
Rape	-0.066 (0.069)	-0.074 (0.092)	-0.038 (0.043)	-0.054 (0.050)	-0.208 (0.234)	-0.339 (0.301)	-0.181 (0.188)	-0.208 (0.248)	-0.255 (0.219)
Robbery	-0.180 (0.048)	-0.204 (0.047)	-0.085 (0.032)	-0.084 (0.029)	-0.459 (0.176)	-0.521 (0.177)	-0.493 (0.128)	-0.572 (0.125)	-0.559 (0.117)
Assault	-0.052 (0.044)	-0.037 (0.050)	-0.010 (0.030)	-0.013 (0.035)	-0.052 (0.164)	-0.079 (0.209)	-0.143 (0.120)	-0.104 (0.136)	-0.099 (0.127)
Property crimes	-0.071 (0.028)	-0.059 (0.026)	-0.028 (0.020)	-0.030 (0.015)	-0.152 (0.109)	-0.189 (0.090)	-0.195 (0.077)	-0.167 (0.068)	-0.174 (0.062)
Burglary	-0.061 (0.043)	-0.062 (0.037)	-0.041 (0.027)	-0.054 (0.021)	-0.222 (0.144)	-0.339 (0.128)	-0.166 (0.118)	-0.174 (0.098)	-0.225 (0.089)
Larceny	-0.038 (0.031)	-0.025 (0.027)	-0.002 (0.021)	-0.018 (0.017)	-0.012 (0.115)	-0.113 (0.103)	-0.103 (0.085)	-0.070 (0.074)	-0.083 (0.067)
Motor vehicle theft	-0.187 (0.049)	-0.131 (0.043)	-0.109 (0.031)	-0.047 (0.025)	-0.592 (0.169)	-0.292 (0.151)	-0.514 (0.130)	-0.367 (0.115)	-0.343 (0.101)
Cost-Weighted crime	-0.213 (0.054)	-0.144 (0.071)	-0.112 (0.034)	-0.099 (0.041)	-0.605 (0.184)	-0.614 (0.250)	-0.583 (0.147)	-0.403 (0.192)	-0.473 (0.171)
Instrument:	—	—	—	—	ASG		UCR		
First stage coefficient	—	—	—	—	0.184 (0.014)	0.161 (0.013)	0.364 (0.029)	0.356 (0.029)	— —
F-statistic on excluded instrument	—	—	—	—	169.1	144.7	154.2	146.4	—
year effects	yes	—	yes	—	yes	—	yes	—	—
state-year effects	no	yes	no	yes	no	yes	no	yes	yes

Note: Columns (1)-(4) report results of a least squares regression of the growth rate in each of ten crime rates on the first lag of the growth rate in the number of sworn police officers. For each set of models, the first column reports regression results, conditional on both the UCR and the ASG measures of the growth rate in the city's population and year effects. The second column adds state-by-year effects. Columns (5)-(8) reports results from a series of 2SLS regressions of the growth rate in each of ten crime rates on the first lag of the growth rate in the number of per capita sworn police officers. Finally, column (9) reports generalized method of moments (GMM) estimates of the growth rate in each of ten crime rates on the first lag of the growth rate in the number of sworn police officers. Below estimates of the effect of police on crime, we report coefficient estimates and standard errors from a least squares regression of the growth rate in a given measurement of the number of police officers on the the growth rate in the other measurement of police. We also report the F-statistic on the excluded police measure. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors that are robust to heteroskedasticity are reported in the second row below the coefficient estimates.

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Table 4. Robustness of Results to the Inclusion of Covariates

	1970-2002 Sample						1980-2000 Sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Violent crimes	-0.221 (0.093)	-0.216 (0.094)	-0.216 (0.094)	-0.199 (0.094)	-0.208 (0.096)	-0.238 (0.097)	-0.201 (0.117)	-0.198 (0.117)	-0.209 (0.132)
Murder	-0.617 (0.238)	-0.570 (0.240)	-0.588 (0.240)	-0.562 (0.241)	-0.565 (0.246)	-0.583 (0.248)	-0.592 (0.266)	-0.581 (0.266)	-0.618 (0.304)
Rape	0.020 (0.168)	0.025 (0.170)	0.013 (0.170)	0.035 (0.172)	0.060 (0.176)	0.041 (0.177)	-0.094 (0.187)	-0.096 (0.186)	0.036 (0.215)
Robbery	-0.595 (0.114)	-0.590 (0.115)	-0.588 (0.114)	-0.589 (0.115)	-0.594 (0.117)	-0.607 (0.118)	-0.411 (0.134)	-0.413 (0.134)	-0.417 (0.145)
Assault	0.149 (0.122)	0.144 (0.123)	0.148 (0.125)	0.180 (0.126)	0.162 (0.127)	0.151 (0.128)	0.108 (0.154)	0.113 (0.155)	0.044 (0.172)
Property crimes	-0.184 (0.070)	-0.190 (0.070)	-0.187 (0.070)	-0.180 (0.070)	-0.185 (0.071)	-0.198 (0.070)	-0.105 (0.092)	-0.105 (0.093)	-0.043 (0.097)
Burglary	-0.224 (0.096)	-0.223 (0.097)	-0.219 (0.098)	-0.212 (0.100)	-0.202 (0.102)	-0.213 (0.100)	-0.075 (0.115)	-0.076 (0.115)	0.019 (0.131)
Larceny	-0.092 (0.078)	-0.102 (0.077)	-0.099 (0.077)	-0.095 (0.077)	-0.109 (0.078)	-0.116 (0.077)	-0.011 (0.101)	-0.011 (0.101)	0.009 (0.102)
Motor vehicle	-0.342 (0.115)	-0.325 (0.114)	-0.339 (0.112)	-0.325 (0.112)	-0.329 (0.114)	-0.345 (0.116)	-0.443 (0.162)	-0.441 (0.163)	-0.300 (0.184)
Cost-Weighted Crimes	-0.434 (0.159)	-0.408 (0.160)	-0.415 (0.160)	-0.389 (0.161)	-0.392 (0.164)	-0.411 (0.165)	-0.474 (0.181)	-0.466 (0.183)	-0.477 (0.202)
state-by-year effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
crack index	no	no	no	no	no	no	no	yes	yes
economic covariates	no	yes	yes	yes	yes	yes	no	no	yes
lagged economic covariates	no	no	yes	yes	yes	yes	no	no	yes
demographic variables	no	no	no	yes	yes	yes	no	no	yes
polynomials and interactions	no	no	no	no	yes	yes	no	no	yes
linear time trends	no	no	no	no	no	yes	no	no	yes

Note: Each column reports generalized method of moments (GMM) estimates of the growth rate in each of ten crime rates on the first lag of the growth rate in the number of sworn police officers, conditional on both the UCR and ASG measures of the growth rate in population, unrestricted state-by-year effects, and various control variables. Estimates are reported for two time periods: the 1970-2002 subsample (in columns (1)-(6)) and the 1980-2000 subsample (columns (7)-(8)). Column (1) reports GMM estimates conditional on both the UCR and the ASG measures of the growth rate in the city's population and unrestricted state-by-year dummies only. Column (2) adds economic covariates while column (3) adds lags of those covariates. Column (4) adds 16 demographic controls capturing the fraction of a city's population that is in various age-gender-race groups. Column (5) adds polynomials and interactions of the demographic variables, while column (6) adds city-specific linear time trends. Columns (7) and (8) report estimates for the 1980-2000 subsample to condition on a city-specific "crack index." Column (7) reports estimates that condition on the UCR and ASG measures of the growth rate in population and unrestricted state-by-year effects, as in column (1). Column (8) adds the crack index to that baseline model, while column (9) adds the full set of covariates in column (6) in addition to the crack index. All models are estimated using 2010 city population weights. Huber-Eicker-White standard errors that are robust to heteroskedasticity are reported below the coefficient estimates.

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Table 5. Comparison of Estimates of the Police Elasticity of Crime

Source	Years	Cross-Sectional Units	Research Design	Violent Crime	Murder	Robbery	Property Crime	Burglary	Motor Vehicle Theft
Marvell and Moody (1996)	1973-1992	56 cities	OLS: lags as control variables		-0.24	-0.22		-0.15	-0.30
					(0.09)	(0.06)		(0.04)	(0.07)
Levitt (1997)	1970-1992	59 cities	IV: mayoral elections	-0.79	-3.03	-1.29	0.00	-0.55	-0.44
				(0.61)	(2.03)	(1.00)	(0.34)	(0.67)	(0.98)
McCrary (2002)	1970-1992	59 cities	IV: mayoral elections	-0.66	-2.69	-0.98	0.11	-0.47	-0.77
				(0.65)	(2.07)	(1.09)	(0.43)	(0.77)	(1.08)
Levitt (2002)	1975-1995	122 cities	IV: number of firefighters	-0.44	-0.91	-0.45	-0.50	-0.20	-1.70
				(0.23)	(0.33)	(0.26)	(0.24)	(0.26)	(0.57)
Corman and Mocan (2005)	1974-1999	NYC	OLS: monthly time series		-0.50	-0.39		-0.28	-0.58
					(1.04)	(0.45)		(0.23)	(0.25)
Klick and Tabarrok (2005)	2002-2003	DC	DiD: high terrorism alert days	0.00				-0.30	-0.84
				(na)				(0.18)	(0.25)
Evans and Owens (2007)	1990-2001	2,074 cities	IV: COPS grants	-0.99	-0.84	-1.34	-0.26	-0.59	-0.85
				(0.33)	(0.47)	(0.52)	(0.16)	(0.18)	(0.35)
Lin (2009)	1970-2000	51 states	IV: state sales tax	-1.13	-2.73	-1.86	-2.18	-1.59	-4.14
				(0.74)	(1.31)	(1.12)	(0.93)	(0.80)	(1.82)
Our preferred estimates	1960-2010	242 cities	measurement error correction	-0.34	-0.67	-0.56	-0.17	-0.23	-0.34
				(0.10)	(0.24)	(0.12)	(0.06)	(0.09)	(0.10)

Note: Table reports implied elasticities and standard errors from recent articles employing a novel identification strategy to estimate the effect of police on crime. Under research design, studies are classified as either using least squares ("OLS"), instrumental variables ("IV") or differences-in-differences ("DiD") to identify the effect of police on crime. 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; McCrary (2002) estimates refer to the estimates using a different measure of mayoral elections. Our preferred estimates which account for the presence of measurement errors are shown below.

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Table 6. Tests of the Equality of Cross-Crime Elasticities

Type	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor Vehicle Theft	Violent Crimes	Property Crimes
Murder	-	0.213	0.649	0.036	0.058	0.015	0.181	-	.035
Rape	-	-	0.181	0.485	0.917	0.452	0.689	-	0.731
Robbery	-	-	-	0.002	0.008	0.001	0.120	-	0.001
Assault	-	-	-	-	0.382	0.922	0.114	-	0.554
Burglary	-	-	-	-	-	0.109	0.287	0.295	-
Larceny	-	-	-	-	-	-	0.010	0.010	-
Motor vehicle theft	-	-	-	-	-	-	-	0.997	-
Violent crimes	-	-	-	-	-	-	-	-	0.075

Note: Each element of the table reports a p-value for a test of the equality between the two-step GMM parameters reported in Table 3 for an exhaustive combination of any two crime categories. For example, the p-value arising from a test of the equality of the pooled murder and burglary elasticities is 0.058. The p-values are generated using a GMM procedure in which we stack data pertaining to each of the two crime categories. All models are estimated using 2010 city population weights and condition on two measures of population as well as an unrestricted vector of state-by-year effects.