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The Relationship Between Crime Reporting and Police: Implications for the Use of Uniform Crime Reports

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Empirical studies that use reported crime data to evaluate policies for reducing crime will understate the true effectiveness of these policies if crime reporting/recording behavior is also affected by the policies. For instance, when the size of the police force increases, changes in the perceived likelihood that a crime will be solved may lead a higher fraction of victimizations to be reported to the police. In this paper, three data sets are employed to measure the magnitude of this reporting bias. While each of these analyses is subject to individual criticisms, all of the approaches yield similar estimates. Reporting bias appears to be present but relatively small in magnitude: each additional officer is associated with an increase of roughly five Index crimes that previously would have gone unreported. Taking reporting bias into account makes the hiring of additional police substantially more attractive from a cost-benefit perspective but cannot explain the frequent inability of past studies to uncover a systematic negative relationship between the size of the police force and crime rates.

KEY WORDS: crime reporting; police; reporting bias; uniform crime reports.

1. INTRODUCTION

The debate over the validity of reported crime statistics is almost as old as reported crime statistics themselves (see, e.g., Kitsuse and Cicourel, 1963; Sellin and Wolfgang, 1964; Skogan, 1976; Bottomley and Coleman, 1981; Zedlewski, 1983; Pepinsky and Jesilow, 1984; Grove *et al.*, 1985; O'Brien, 1985; Pepinsky, 1987; Biderman and Lynch, 1991; Donohue and Siegelman, 1994; DiIulio, 1996; O'Brien, 1996). While opinions vary about the seriousness of the problems associated with Uniform Crime Report (UCR) data,

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many researchers believe that reported crime data are contaminated by measurement error arising from differences in police department reporting practices across jurisdictions, technological advances in crime recording, and changes in crime reporting by victims over time. Even if such problems exist, however, they do not necessarily preclude the use of reported crime statistics for determining the effectiveness of policies designed to reduce crime. As long as crime is the left-hand side variable in the analysis, *random* measurement error will increase the standard error of the estimates, but will not bias the parameter estimates. Only measurement error in reported crime rates that is systematically related to the policy being evaluated will bias the estimates.[2] Given that UCR data remain the only readily available source of geographically disaggregated data on crime, identifying and quantifying the likely sources of policy-related measurement error in UCR data are important subjects of research.

In this paper, I address this issue for one particular public policy measure: changes in the size of the police force. A large body of criminological literature has examined the impact of police on crime. As noted in surveys by Blumstein *et al.* (1978) and Cameron (1988), empirical studies, almost without exception, have failed to identify a significant negative relationship between the size of the police force and crime.[3] In the face of these discouraging results, research has shifted away from analysis of the number of police toward strategies for using existing police officers more effectively (e.g., Wilson and Boland, 1978; Eck and Spelman, 1987; Sampson and Cohen, 1988; Sherman, 1992; Sparrow *et al.*, 1990).

If the size of the police force systematically affects the willingness of victims to report crime or a police department's propensity officially to record victim crime reports, then UCR data will understate the true effectiveness of police in reducing crime. Victims may be more likely to report crimes to the police when the perceived likelihood of a crime being solved is high. Furthermore, the ready availability of a police officer at the scene of a crime may also lead to more crime reports, and it is easy to imagine

[2]In contrast, when crime is used as an explanatory variable, the use of reported crime statistics will induce two countervailing biases into the estimation. Underreporting will lead estimates of the effect per crime to be overstated. On the other hand, if there is noise in reported crime rates, then standard attenuation bias due to errors in variables will also be present.

[3]In sharp contrast, these same surveys find strong evidence of a negative association between crime rates and the risk of arrest, conviction, or imprisonment. Two recent studies do find a link between the size of the police force and crime. Marvell and Moody (1996), using a Granger-causality approach, find that police reduce crime. Levitt (1997) uses the timing of mayoral and gubernatorial elections as instruments for the size of the police force, obtaining similar results.

that increases in police manpower will affect the likelihood that citizen complaints are officially recorded by police departments.[4] If reporting/recording bias (hereafter referred to as "reporting" bias for brevity) is present, *reported* crime rates may increase with the size of the police force, even if the *true* victimization rate is falling. The fact that substantially less than half of all crimes covered by the FBI's *Uniform Crime Reports* are actually reported to the police heightens concern over the importance of reporting bias.

Reporting bias is frequently cited as an explanation for the failure to uncover a relationship between reported crimes and police presence (e.g., Greenwood and Wadycki, 1973; Swimmer, 1974; Thaler, 1977, 1978; Carr-Hill and Stern, 1979; Cameron, 1988; Devine *et al.*, 1988). Yet while there is a large body of literature examining various determinants of the likelihood that crimes will be reported (Skogan, 1984) including the severity of the offense (Skogan, 1976), positive results from previous reports of victimization (Conway and Lohr, 1994), fear of reprisal (Singer, 1988), and the race of the victim (Rabinda and Pease, 1992), only one empirical study has addressed the relationship between crime reporting and the size of the police force.[5] While not his primary emphasis, Craig (1987) obtains substantively large (but only marginally statistically significant) point estimates of reporting bias using a simultaneous equations model applied to a data set of Baltimore neighborhoods that combines information from the National Crime Survey and data from the Baltimore police department. There are, however, two weaknesses in Craig's estimates. The first is imprecision. The two-standard deviation confidence interval of the estimate covers the entire range of plausible magnitudes. Second, identification of the model relies on excluding a number of socioeconomic and demographic factors including the percentage male, the percentage married, the percentage unemployed, and income variables. There is no theoretical justification for those exclusions. Moreover, many of those variables are found to be systematically related to reporting in the empirical results of the current paper.

In this paper, three approaches to measuring the magnitude of reporting bias are undertaken. While none of the approaches is immune from criticism, the estimates obtained from the various techniques are similar, allowing a

[4]From the perspective of the researcher using official crime statistics to analyze the impact of police on crime, the distinction between reporting and recording is immaterial. From the perspective of better understanding the validity of crime statistics more generally, however, distinguishing between reporting and recording is of fundamental importance. The primary emphasis of this paper is on the first of those two issues, primarily because the methods employed are better suited to addressing the former question than the latter.

[5]Myers (1980, 1982) attempts to correct reported crime statistics for underreporting but does not address the issue of the effect of additional police officers on reporting behavior.

greater level of confidence in the results than would be justified based on any of the analyses in isolation.

The first approach uses cross-sectional variation from the NCVS Cities Surveys performed in the early 1970s. Data on reporting and victimization rates are available for a sample of 26 cities. Combining that information with data on police officers per capita and controlling for other relevant factors, it is straightforward to obtain estimates of reporting bias. The second approach uses data from the annual NCVS conducted over the period 1973–1991. Although this survey does not contain city or state identifiers, the size of the city in which the respondent lives is recorded. Given that police staffing levels vary systematically across city size, these variables provide another potential means of identifying reporting bias. Despite the obvious drawback of not having actual police staffing levels on a city-by-city basis in this data set, these data have the advantage of the availability of repeated cross sections. The third approach differs substantially from the others, relying solely on reported crime statistics rather than victimization data. The underlying premise of this approach is that murders are virtually always reported to the police and consequently will be immune to reporting bias. Therefore, if reporting bias affects other crimes, one might expect the ratio of other crimes to murders to be an increasing function of the number of police per capita. The latter approach, unlike the others, captures not only changes in victim reporting behavior, but also changes in police recording practices (e.g., better technology, creation of rape crisis units, etc.)

The results of this paper suggest that some reporting bias exists, although the evidence is by no means overwhelming. The estimates of reporting bias are almost always positive, but only sometimes statistically significant. Taking the average of the point estimates, each additional officer leads to the reporting of roughly five Index crimes that otherwise would have gone unreported. Ignoring this effect will lead researchers to understate the benefits associated with increases in the size of the police force. While the magnitude of the estimated reporting bias is not sufficient to explain the frequent failure of past studies to uncover a negative relationship between police and crime, it is nonetheless an important consideration to factor in when performing cost–benefit analyses of police manpower or policing strategies.

The structure of the paper is as follows. Section 2 describes the NCVS city samples and presents estimates based on those surveys. Section 3 examines the link between reporting and police staffing using information from the annual National Crime Surveys between 1973 and 1991. Section 4 analyzes the relationship between levels of police staffing and the ratio of other crimes to murders. Section 5 considers the implications of the estimates and offers a brief set of conclusions.

2. THE NCVS CITY SURVEYS

Between 1971 and 1975, the United States Department of Justice conducted victimization surveys in 26 large American cities. Approximately 10,000 household interviews were conducted in each city. Information was collected on victimization and reporting for a variety of crimes and is collected in the United States Department of Justice (1975a, b, 1976) reports. The strength of these data is the availability of city identifiers that can be linked to the police staffing data collected annually in the FBI's *Uniform Crime Reports*. The primary drawback of the data is the limited number of observations, and the fact that the primary source of variation is cross sectional.[6] Failure to control for city-level characteristics that are correlated with both reporting rates and police staffing will lead to biased estimates. Furthermore, the data are two decades old, perhaps decreasing their relevance to the current time period.

Table I presents summary statistics for the sample of 26 cities used in the analysis. Information on reporting rates and victimization rates for the seven crime categories (robbery, aggravated assault, simple assault, burglary, motor vehicle theft, personal larceny, and household larceny) is displayed, along with statistics on sworn police officers and victim characteristics. Almost three-quarters of all motor vehicle thefts are reported, whereas only one-fourth of all household larcenies are reported. Victimization rates for the various crimes differ substantially across the cities in the sample.[7] The likelihood of being robbed, for example, is three times greater in the highest-incidence city relative to the lowest-incidence city (32 vs. 10). Males are more frequently victims of crime (62.5%). Those 19 and under are overrepresented as crime victims relative to their share of the population, whereas senior citizens are less likely to be victims of crime. Police staffing levels also vary substantially across the cities in the sample. The average number of sworn officers per 100,000 residents is approximately 300. The city with the greatest concentration of police, Washington, DC has five times as many officers per capita as San Diego, the city with the lowest concentration.

The general form of the estimating equation for the reporting rate is

$$\ln(\text{REPORT}_i) = \alpha + \beta_1 \ln(\text{SWORN}_i) + \beta_2 \ln(\text{VICTRATE}_i) + \gamma(\text{VICTCHAR}_i) + \varepsilon_i \qquad (1)$$

where i indexes cities, REPORT is the percentage of victimizations reported to the police, SWORN is the number of sworn officers per 100,000

[6]Only 13 of the 26 cities were surveyed on multiple occasions.

[7]Among others, Sampson (1986), Land *et al.* (1990), and Glaeser *et al.* (1996) analyze the possible explanations for the dramatic variation in crime rates across cities.

Table I. Summary Statistics for NCVS City Sample[a]

Variable	Mean	SD	Minimum	Maximum
Reporting rate				
Robbery	0.52	0.054	0.44	0.65
Aggrav. assault	0.50	0.046	0.41	0.60
Simple assault	0.33	0.039	0.27	0.45
Burglary	0.53	0.033	0.46	0.58
Motor vehicle	0.74	0.045	0.63	0.79
Personal larceny	0.29	0.038	0.19	0.36
Household larceny	0.26	0.036	0.19	0.32
Victimization rate (per 1000 residents)				
Robbery	20.0	6.5	10	32
Aggrav. assault	14.1	4.3	4	22
Simple assault	16.8	6.5	5	28
Burglary	130.6	30.9	68	177
Motor vehicle	37.4	15.5	15	86
Personal larceny	94.8	26.4	44	141
Household larceny	105.9	40.1	33	190
Sworn police (per 100,000 residents)	304.3	112.2	133.9	672.6
Victim characteristics (all crimes combined)				
% white	67.8	15.2	35.5	92.2
% male	62.5	4.0	53.0	68.8
% 19 and under	33.4	6.2	19.9	43.9
% 65 and older	6.9	1.5	3.6	10.0

[a]All data from United States Department of Justice (1975a, b, 1976), except sworn officers and arrest rates, which are from *Uniform Crime Reports*. For cities that were sampled twice, only the first survey outcome is included. Sample means are unweighted city averages. Summary statistics for victim characteristics are an unweighted average across all crimes; the victim characteristic variables included in the regressions in Table II, however, are for the particular crime in question. Victim characteristics for percentage male and percentage 19 and under refer to personal crimes only, not to household crimes.

residents,[8] VICTRATE is the victimization rate per 1000 residents, and VICTCHAR is a vector of victim demographic characteristics that includes the percentage of victims that fall into the following categories: male, white, 19 years or younger, and 65 years and older.[9] Males and those 19 and under tend to report crimes less frequently, whereas those 65 and older are more

[8]An alternative to sworn officers is overall police employment, including civilians. The inclusion of civilians, who generally comprise less than 25% of the total police force, does not alter the basic findings.

[9]Because the demographic variables are already defined in terms of percentages, they are not logged in Eq. (1). Household income level of victims was also considered as a possible control but was insignificant in all specifications for all crime categories and therefore is not included in the results presented in the tables.

Table II. Reporting Bias Estimates Using the NCVS City Survey[a]

Variable	(1) Robbery	(2) Aggrav. assault	(3) Simple assault	(4) Burglary	(5) Motor vehicle	(6) Personal larceny	(7) Household larceny
ln(SWORN)	0.114	0.060	0.067	0.089	−0.035	0.100	0.154
	(0.038)	(0.044)	(0.066)	(0.052)	(0.032)	(0.077)	(0.115)
ln(VICTRATE)	−0.189	−0.033	−0.042	0.073	−0.016	−0.082	0.116
	(0.058)	(0.049)	(0.067)	(0.060)	(0.018)	(0.072)	(0.072)
% of victims							
White	−0.267	−0.067	0.078	−0.038	−0.178	0.046	−0.112
	(0.134)	(0.090)	(0.261)	(0.070)	(0.057)	(0.091)	(0.202)
Male	−0.156	−0.528	−0.165	—	—	0.098	—
	(0.308)	(0.272)	(0.332)			(0.320)	
19 and under	−0.216	0.614	−0.450	—	—	−0.203	—
	(0.286)	(0.197)	(0.281)			(0.161)	
65 and older	0.569	3.149	1.368	0.835	0.746	−0.33	0.848
	(0.402)	(1.06)	(0.865)	(0.458)	(0.448)	(0.106)	(1.730)
Constant	−0.459	−0.871	−1.242	−1.555	0.021	2.840	−2.768
	(0.452)	(0.358)	(0.578)	(0.566)	(0.213)	(0.672)	(1.060)
Adjusted R^2	0.49	0.54	0.27	0.22	0.17	0.19	0.08
P value of controls	0.01	<0.01	0.22	0.01	<0.01	0.01	0.30
Estimated reporting							
bias without controls	0.130	0.115	0.131	0.079	0.029	0.194	0.083
	(0.046)	(0.041)	(0.047)	(0.027)	(0.042)	(0.073)	(0.081)

[a] Dependent variable is ln(Reporting Rate). White heteroskedasticity-consistent standard errors in parentheses. Data taken from United States Department of Justice (1975a, b, 1976) and *Uniform Crime Reports*. All data are city-level averages. Number of observations equals 26 in all regressions. The bottom row is the coefficient from a simple regression of ln(Reporting Rate) on ln(SWORN).

likely to report (United States Department of Justice, 1983). The log form of Eq. (1) is adopted primarily for convenience of interpretation across crime categories (the estimated coefficients are elasticities). A number of alternative functional forms were examined, with little impact on the conclusions.

Table II presents the results. Each column corresponds to a different crime category. All of the control variables are included for personal crimes. For household crimes, controls for sex and age 19 and under are not relevant and are therefore excluded. The coefficient associated with the variable SWORN captures reporting bias. Note that the variables included in the regression do a good job of explaining differences in reporting. While most of the regressors are not individually statistically significant, the variables are jointly highly statistically significant in five of the seven regressions. While the point estimate on sworn officers carries the predicted positive sign and generally has a t statistic greater than one, only for robbery can the null hypothesis of no reporting bias be rejected at the 0.05 significance level. For household larceny, the largest of the point estimates, the reporting elasticity is 0.154, meaning that a 10% increase in the number of sworn officers per capita corresponds to a 1.54% increase in the reporting rate of household larcenies (i.e., from 26.0% of household larcenies reported to 26.4% reported).

Sworn officers may be related to changes in reporting behavior either because the perceived likelihood that a case will be solved increases or simply because of a greater availability of officers. Including the arrest rate for a given crime provides a possible means of distinguishing between those two alternatives. When arrest rates are added to the specification in Table II (results not shown in tabular form, but available from the author on request), the coefficients on sworn officers are essentially unchanged. The arrest rates enter with small coefficients and mixed signs across crimes. Thus, it appears that the likelihood of crime reporting by victims is a function of the sheer numbers of sworn officers, rather than a response to an increased probability of the criminal being caught.

The other control variables generally take on the expected sign. As expected, cities with higher victimization rates have lower reporting rates in five of the seven categories. Whites, males, and those 19 and under are less likely to report crimes, whereas the elderly are more likely to report. If, for instance, the percentage of elderly burglary victims fell from 7 to 0%, simple calculations show that the average reporting rate for burglary would be estimated to fall from 53 to 50%.

To determine whether the estimated reporting biases are sensitive to the inclusion of the demographic controls, the bottom row in Table II presents the estimated reporting elasticities from a simple regression of reporting rate on sworn officers per capita, excluding the other controls.[10] The estimates obtained from the simple regressions all carry a positive sign and are slightly larger than those when controls are included. That fact, along with more precision in the estimates, allows the null hypothesis of no reporting bias to be rejected for five of the seven crime categories.[11]

[10]One reason for excluding the control variables from the regression is their possible endogeneity. If criminals respond to increases in the police force by targeting demographic groups that are less likely to report crimes, then including the composition of victims will tend to bias estimates of the reporting elasticity downward.

[11]One shortcoming of this analysis is the inability to control completely for differences across cities that may be correlated with reporting rates and/or levels of police staffing. The use of repeated surveys of the same city helps to avoid that problem. To the extent that reporting is prevalent, one would expect reporting rates to rise in a given city when the number of sworn officers per capita increases. The primary shortcoming of this approach is the limited number of cities that were surveyed on multiple occasions and the short period of elapsed time between repeat surveys (roughly 3 years). Estimates of reporting bias based on city changes over time (full details of estimation available from the author on request) range from 0.062 to 0.192, similar in magnitude to the results in Table II. The estimates based on changes, however, are estimated less precisely and therefore are not significantly different from zero.

3. USING THE ANNUAL NCVS TO OBTAIN ESTIMATES OF REPORTING BIAS

The U.S. Department of Justice conducts victimization surveys of over 50,000 households nationwide annually.[12] The primary strengths of this data source are the sheer volume of observations (over 300,000 separate incidents), the presence of individual-specific demographic information, and the pooled cross-section, time-series nature of the data. In addition, because the victimization data reflects only whether a crime was reported to police, and not whether it was officially recorded, this analysis distinguishes true reporting effects, whereas the other sections of this paper cannot differentiate between changes in victim reporting and changes in official recording practices of the police. From the perspective of analyzing the question at hand, the clear drawback of this data set is the absence (to preserve anonymity) of city, MSA, or state identifiers. As a consequence, no direct measure of police is available. This data set does, however, contain information on the population of the city inhabited by the victim. Therefore, a proxy for the number of police per capita can be constructed using the mean for cities of the relevant size for the year in which the incident occurs.[13] *Uniform Crime Reports* provides annual tallies of sworn officers per capita for cities in six groupings: over 250,000, 100,000–250,000, 50,000–100,000, 25,000–50,000, 10,000–25,000, and below 10,000.

Because police per capita is available only at a highly aggregated level, direct estimation of a reporting regression such as Eq. (1) at the individual level using the mean police intensity for cities of a particular size is likely to be severely contaminated by measurement error. When individuals are aggregated by city size, however, so that the dependent variable is the *mean* reporting rate for cities of that size, the errors-in-variables problem is eliminated. Therefore, in what follows, all individual-level data for each crime category and year are aggregated by city size.[14] Because the point estimate

[12]These data are available from ICPSR on CD-ROM. For the purposes of this analysis, observations on all individuals categorized as "not residing in a place" are dropped from the sample because of the absence of *Uniform Crime Report* police staffing levels for such individuals, as are all observations with missing data for any of the right-hand-side variables.

[13]One small problem with this proxy is that city size in *Uniform Crime Reports* is based on the most recent available population estimates, whereas the NCVS place size code is drawn from the last decennial census. Since there is relatively little movement across categories, however, this difference should not pose a major problem.

[14]Probit regressions using individual-level data with whether or not a given crime is reported as the dependent variable, and the demographic variables and police proxy as the right-hand-side variables, yielded estimated reporting elasticities near zero for all crime categories. In light of the measurement error, however, it is impossible to determine whether this is simply the result of attenuation bias due to measurement error.

for any given crime category is imprecise, all seven crime categories examined in Table II are pooled in this analysis. The precise specification of the regressions is as follows:

$$\ln(\text{REPORT}_{ict}) = \beta_1 \ln(\text{SWORN}_{it}) + \gamma(\text{VICTCHAR}_{ict}) + \delta_c \text{ATTEMPT}_{ict} + \lambda_c + \eta_i + \theta_t + \varepsilon_{ict} \qquad (2)$$

where REPORT_{ict} is the reporting rate in city-size category i, for crime c, in year t. β_1 captures reporting bias. The variable ATTEMPT controls for the fraction of victimizations that are attempted but not successfully completed. Such crimes are less likely to be reported to the police. Fixed effects for each of the city-size categories, crime categories, and years of data are also included as controls. In some instances, crime-specific trends are also included to pick up changes in the rates at which particular crimes are reported over time (Jensen and Karpos, 1993). Because all crimes are estimated jointly, only one estimate of reporting bias across all crimes is obtained. This estimate is a weighted average of each of the individual crime categories and, thus, may not reflect the actual degree of reporting bias for any one crime. The estimation technique employed is weighted least squares, with the weights determined by the number of victimizations underlying each observation.

The empirical results are presented in Table III. The columns in Table III include varying combinations of demographic controls and crime-specific trends. While the presence of autocorrelation or serial correlation across crime categories within a given city will not bias the parameter estimates themselves, it will affect the standard errors. Consequently, White heteroskedasticity-consistent standard errors, which correct for a general form of correlation across observations, are reported in parentheses. In addition, the reported standard errors correct for correlation in the SWORN variable resulting from the grouping of seven different crime categories into one regression.

The estimated reporting bias in Table III is somewhat larger than those presented in the preceding section, ranging from 0.118 to 0.187. These estimates, however, are not statistically significant at the 0.05 level. Adding demographic covariates (columns 2 and 4) somewhat lowers the magnitude of the estimates. Crime-specific trends (columns 3 and 4), although highly statistically significant as reported at the bottom of Table III, have little impact on the estimated magnitude of reporting bias.

The other variables in the regressions appear to be reasonably estimated. The coefficient on male victims and the age of victims fit the expected pattern and, in the latter instance, are highly statistically significant. The parameters on the city-size indicator variables confirm that reporting rates are substantially lower in the largest cities. Cities with populations over 250,000 is the

Table III. Reporting Bias Estimates from the Annual NCVS (1973–1991): Pooled Time-Series, Cross-Sectional Data by City Size[a]

Variable	(1)	(2)	(3)	(4)
ln(SWORN)	0.185	0.123	0.187	0.118
	(0.106)	(0.082)	(0.113)	(0.074)
Δ% of victims				
White	—	−0.162	—	−0.172
		(0.126)		(0.108)
Male	—	−0.091	—	−0.095
		(0.120)		(0.101)
19 and under	—	−0.290	—	−0.532
		(0.164)		(0.106)
65 and older	—	0.486	—	0.543
		(0.179)		(0.205)
City				
100,000–250,000	0.131	0.130	0.132	0.134
	(0.044)	(0.026)	(0.046)	(0.028)
50,000–100,000	0.135	0.143	0.136	0.149
	(0.054)	(0.031)	(0.058)	(0.035)
25,000–50,000	0.146	0.156	0.147	0.165
	(0.055)	(0.029)	(0.058)	(0.034)
10,000–25,000	0.171	0.191	0.172	0.203
	(0.053)	(0.026)	(0.056)	(0.033)
<10,000	0.118	0.140	0.119	0.150
	(0.033)	(0.021)	(0.034)	(0.024)

[a]Dependent variable is ln(Reporting Rate). Data aggregated from individual incidents according to city size (six categories), year (1973–1991), and the seven crime categories noted in the text; consequently, there are 798 observations in each column. Year dummies, fixed-effects for crime categories, and terms interacting the crime categories with the percentage of incidents in which a crime was attempted but not successfully completed are included in all specifications. Omitted category for city sizes is cities over 250,000 in population. All columns estimated by weighted-least squares, where the weights are the number of incidents on which the aggregated values is based. White heteroskedasticity-consistent standard errors in parentheses. The reported standard errors have also been corrected to account for correlation of the right-hand-side variables due to joint estimation of the seven crime categories.

omitted category, so all coefficients are relative to that baseline. For all city categories, the coefficients are positive and, usually, statistically significant. If the reporting rate for a given crime category is 0.50 in a small city, the parameter estimates imply that the likelihood of reporting the same crime in a city of over 250,000 is 0.45.

4. USING THE RATIO OF OTHER CRIMES TO MURDERS TO ESTIMATE REPORTING BIAS

While the preceding sections have attempted to estimate reporting bias directly using victimization data, in this section the issue is addressed indirectly through the use of reported crime statistics. Under the set of assumptions specified below, reporting bias can be identified solely from reported crime data. The premise of this approach is that murder, in contrast to other crimes, is likely to be immune from reporting bias since virtually all murders are reported to the police. When the number of police officers increases, the number of reported murders decreases at the same rate as the actual number of murders. The number of reported nonmurder crimes, however, does not decline as quickly as the actual number of nonmurder crimes due to increased reporting. Thus, in the presence of reporting bias, the *ratio* of nonmurder crimes to murders might be expected to be an increasing function of the level of police staffing.[15] Unlike the earlier analyses, which capture only changes in victim reporting, this approach reflects both victim reporting and police recording practices.

To make the intuition underlying this approach more concrete, imagine for a moment that changes in the number of police have no impact on the actual number of murders or robberies but that the reporting rate of robberies increases with the number of police. As the number of police increases, the number of reported (and actual) murders is unchanged, but the number of reported robberies rises, even though the actual number of robberies is constant. The ratio of reported robberies to reported murders will therefore be an increasing function of the number of police. The greater the magnitude of reporting bias, the larger is the effect of changes in the size of the police force on that ratio.

A critical identifying assumption of this approach is that, on average, the *true* ratio of murders to other crimes is the same across the observations that are being compared. For that reason, this section focuses only on changes in a particular city over time, rather than looking across cities. Different cities may have widely varying ratios of crimes for many reasons unrelated to police staffing. Reporting procedures across police departments may also differ, further distorting the analysis (O'Brien, 1985). For a particular city over time, however, the underlying ratio of crimes is likely to be more consistent.

Even focusing on a particular city over time, a further assumption is required to identify the model: changes in the number of police must result

[15]In independent research, O'Brien (1996) develops a line of argument similar to that presented here. O'Brien demonstrates that fluctuations in first-differenced UCR homicide and robbery rates closely mirror one another. UCR robberies, however, have trended up, whereas UCR homicides have not. An increased tendency of police to report robberies is one explanation for this pattern. The logic I put forth parallels that of O'Brien, except that I focus not on aggregate crime patterns but, rather, on the linkage between changes in the size of a particular city's police department and the ratio of homicide to other crimes in that city.

in similar decreases in the crime rate across the crime categories being considered (i.e., murder and any of the nonmurder crimes). To make this logic clearer, assume that additional police affect both actual murders and another crime, e.g., robbery, as follows:

$$\ln(\text{Murder}_t) = \alpha_1 + \beta_1 \ln(\text{Sworn}_i) + \varepsilon_t \tag{3}$$

$$\ln(\text{Robbery}_t) = \alpha_2 + \beta_2 \ln(\text{Sworn}_t) + \eta_t \tag{4}$$

where t indexes time periods, and Murder and Robbery reflect *actual crimes committed*, not reported crimes. The a priori expectation is that β_1 and β_2 are both negative but not necessarily equal.

It is assumed that all murders are reported, whereas the reporting rate for robberies is an increasing function of the number of sworn officers:

$$\ln(\text{Murder}_t^{\text{R}}) = \ln(\text{Murder}_t) \tag{5}$$

$$\ln(\text{Robbery}_t^{\text{R}}) = \ln(\text{Robbery}_t) + \ln(\text{RRate}_t) \tag{6}$$

$$\ln(\text{RRate}_t) = \theta \ln(\text{Sworn}_t) + v_{it} \tag{7}$$

where the superscript R denotes reported crimes, and RRate is the reporting rate for robberies. Equations (5) and (6) are identities, and Eq. (7) allows for reporting bias θ with respect to robberies.[16] Rearranging Eqs. (2) through (6) yields an expression for *the log ratio of reported robberies to murders:*

$$\ln(\text{Robbery}_t^{\text{R}}/\text{Murder}_t^{\text{R}}) = (\alpha_2 - \alpha_1) + (\theta + \beta_2 - \beta_1)\ln(\text{Sworn}_t) + (v_t + \eta_t + \varepsilon_t) \tag{8}$$

Thus θ, the coefficient reflecting reporting bias, cannot be separately identified except under the assumption that $\beta_1 = \beta_2$, i.e., additional police affect both crimes proportionately. While this is clearly an important weakness of the approach, it is convenient that the direction of the bias induced by a violation of this assumption is easy to sign. If additional police are more effective at reducing nonmurder crimes vis-à-vis murders (i.e., β_2 is more negative than β_1), then the coefficient of ln(Sworn) will understate reporting bias. Conversely, if extra police reduce the murder rate more than other crimes, the reporting bias will be overstated. While it is impossible to know with certainty the impact of additional police on a particular crime, looking at a wide range of crime categories increases the likelihood that some crimes are deterred less successfully than murder by additional police, while others are more effectively deterred.

[16]Equation (5) is obtained by taking the log of the identity Reported Crimes = Actual Crimes * Reporting Rate.

Equation (8) is estimated using the ratio of reported values of six crimes (rape, aggravated assault, robbery, burglary, larceny, and motor vehicle theft) to murders. The data set is comprised of annual, city-level data for the period 1970–1992 and includes all 59 cities that satisfy the following two criteria: (1) a population of over 250,000 at any point between 1970 and 1992 and (2) direct election of mayors.[17] Cities that do not directly elect mayors are excluded from the sample because election cycle variables are used as instruments for changes in the size of the police force. Six cities satisfy the population cutoff but are excluded due to indirect election of mayors: Cincinnati, Virginia Beach, Norfolk, Wichita, Santa Ana, and Colorada Springs.

In addition to the variables specified in Eq. (8), a range of other controls is also included: the city population, the percentage of the city population that is black, the percentage of the city population residing in female-headed households, the percentage of the city population between 18 and 24 years of age, the state unemployment rate, and the combined state and local spending on two types of program—education and public welfare.[18] In addition, year dummies and city fixed effects are also included as controls. The demographic variables control for the possibility that changes in social circumstances have a differential effect across crimes. The state employment rate captures differences in the responsiveness of particular crimes to economic conditions; one might expect property crimes to be more sensitive to the economy than murder. City-fixed effects ensure that the parameters are identified from within-city variation over time.

Summary statistics for the variables used in the analysis are provided in Table IV, and the crime-by-crime estimation results are presented in Table V. The coefficient on sworn officers, as before, is an estimate of reporting bias. Although correlation in the residuals will not lead to biased coefficient estimates, it will affect the standard errors. Therefore, White heteroskedasticity-consistent standard errors are reported in parentheses. The estimated reporting biases are somewhat larger than those obtained using the NCVS city sample, ranging from −0.01 to 0.44. These larger coefficients are consistent with the hypothesis that some portion of the observed reporting bias is due not to changes in victim reporting but, rather, to an increased propensity for police forces to officially record citizen complaints. Four of the six estimated reporting biases are statistically different from zero at the 0.05 level. The largest coefficients are obtained for assault. Motor vehicle

[17]This is the same data set used by Levitt (1997). For further details on the construction of the data, see that paper.

[18]The percentage of the city population that is black, the percentage in female-headed households, and the percentage of the population between 18 and 24 years of age are available only in decennial census years and are, therefore, linearly interpolated between those years.

Table IV. Summary Statistics for the Ratio of Reported Nonmurder Crimes to Murders[a]

Variable	Mean	SD	Minimum	Maximum
Ratio of crime:murder				
Rape/murder	4.8	3.0	0.4	24.0
Robbery/murder	32.2	15.8	5.7	140.9
Assault/murder	33.5	23.7	4.0	209.7
Burglary/murder	162.2	102.9	24.2	926.8
MVT/murder	69.0	47.4	14.2	520.6
Larceny/murder	326.0	243.8	41.9	1649.0
Sworn officers (per 100,000 population)	245.2	100.0	111.6	781.0
% age 15–24	17.1	2.1	11.5	25.1
% black	23.4	18.1	0.2	78.2
% female-headed households	15.0	4.3	6.0	31.9
City population (×1000)	687.1	926.2	90.0	7418.6
State and local education spending	769.8	121.1	445.9	1193.4
State and local welfare spending	258.4	126.0	33.5	847.7
State unemployment rate	6.8	1.9	2.1	15.5

[a]All crime and police data drawn from *Uniform Crime Reports*. Crime data are annual city-level police reports for a sample of 59 cities with populations over 250,000 for the period 1971–1992. Other variables taken from a variety of sources including the decennial census, *Statistical Abstract of the United States*, and Survey of Current Business. For a more detailed description of the data used and data sources, see Levitt (1995).

theft, which is typically well reported because of insurance requirements, is the only crime with a negative (but statistically insignificant) point estimate.[19].

Because the dependent variable is the ratio of other crimes to murders, there is often not a strong prediction for the sign of the coefficients associated with other controls. A positive sign implies that an increase in the variable makes the nonmurder crime relatively more frequent. When a larger fraction of the population is between 18 and 24 years of age, there tend to be more burglaries and fewer assaults relative to murders. As the fraction of the population that is black increases, assault, robbery, and rape rise at a faster rate than murder, while motor vehicle theft increases at a slower rate. The signs of the coefficients associated with female-headed households tend to

[19]As discussed in Section 2, the inclusion of the arrest rate as a control provides a possible means of distinguishing between the reasons reporting rises in response to increased police officers. When the arrest rate for murder and the crime being examined are added to the specifications in Table V, the estimated reporting biases fall slightly for most crime categories, now ranging from −0.10 to 0.34. As was the case in Section 2, the increased likelihood of arrest does not seem to be the primary channel through which increases in sworn officers lead to higher reporting rates.

Table V. Regression Analysis of Ratios of Reported Nonmurders to Murders[a]

	Rape/ murder (1)	Robbery/ murder (2)	Assault/ murder (3)	Burglary/ murder (4)	Motor vehicle/ murder (5)	Larceny/ murder (6)
ln(Sworn Officers)	0.44	0.10	0.30	0.40	−0.01	0.11
	(0.14)	(0.08)	(0.13)	(0.16)	(0.09)	(0.14)
% age 15–24	−1.81	−5.77	−16.70	1.42	−8.29	−5.56
	(1.15)	(1.01)	(1.11)	(1.04)	(0.81)	(0.99)
% black	0.037	0.006	0.021	−0.016	−0.024	0.014
	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)
% female-headed households	−0.028	−0.022	−0.034	0.014	0.059	−0.043
	(0.011)	(0.009)	(0.011)	(0.011)	(0.009)	(0.011)
ln(City Population)	−0.24	0.08	−0.50	−0.97	−0.11	−0.54
	(0.19)	(0.15)	(0.17)	(0.19)	(0.13)	(0.17)
ln(Education Spending)	−0.33	−0.05	−0.17	−0.41	0.59	−0.15
	(0.20)	(0.13)	(0.15)	(0.20)	(0.11)	(0.15)
ln(Welfare Spending)	0.01	−0.07	0.08	−0.12	−0.01	−0.06
	(0.07)	(0.04)	(0.06)	(0.04)	(0.04)	(0.04)
State unemployment rate	2.62	3.77	1.08	4.63	−0.27	3.53
	(0.66)	(0.48)	(0.65)	(0.64)	(0.51)	(0.58)
N	1323	1330	1330	1330	1330	1330
Adjusted R^2	0.717	0.727	0.713	0.819	0.750	0.858
2SLS coefficient on ln(Sworn Officers)	0.47	0.29	0.34	0.14	0.05	0.17
	(0.53)	(0.40)	(0.57)	(0.44)	(0.46)	(0.45)

[a]Dependent variables are ln(Reported Nonmurder Crimes/Reported Murders). Data used are a pooled time series of city-level crime reports from *Uniform Crime Reports* for a sample of 59 cities with populations greater than 250,000 over the time periods 1971–1992. City-fixed effects included in all regressions. Under the assumption that additional police are equally effective in reducing murders and other crimes, the coefficient on ln(Sworn Officers) is an estimate of reporting bias for the crime in question. The estimation technique used is weighted least-squares, with observation rates proportional to city population. Standard errors (in parentheses) have been corrected using White heteroskedasticity-consistent standard errors. The 2SLS coefficient reported in the bottom row is the coefficient on ln(Sworn Officers) instrumenting for the police variables with mayoral and gubernatorial election years by city.

be reversed from those on percentage black. Given the strong positive correlation between those two variables ($r = 0.86$), strong inferences should not be drawn from those coefficients individually. Increases in population are correlated with a greater increase in murders than property crimes. Increases in spending on education are associated with higher levels of other crimes relative to murder, while the effects of public welfare spending are mixed. With the exception of motor vehicle theft, other crimes respond more dramatically to unemployment than do murders.

One concern in interpreting the foregoing analysis is the potential endogeneity of sworn officers, although the case for endogeneity is not as straightforward as usual. In the typical regression of police on crime, endogeneity arises because politicians respond to rising crime by increasing spending on police resources. In the regressions presented in Table V, that alone is not sufficient to bias the coefficients. Rather, it must be the case that an increase in murders leads to a different political response than a proportionate increase in other, nonmurder crimes. It is possible that politicians are more responsive to changes in murders than changes in other crimes, which would lead to a downward bias in the estimation of the reporting bias coefficient.

In an attempt to counteract that endogeneity, the years of the mayoral and gubernatorial election cycle are used as instruments. To serve as valid instruments, election timing must be correlated with changes in the size of the police force but, otherwise, uncorrelated with the ratio of nonmurders to murder since other covariates are included. Levitt (1997) demonstrates that increases in big-city police forces are disproportionately concentrated in election years. It is difficult, however, to argue that the election cycle is correlated with the ratio of nonmurders to murders, except through changes in the police force.[20]

The specifications in Table V were therefore reestimated using two-stage least-squares. The number of sworn officers and arrest rates were treated as endogenous, using the years of the mayoral and gubernatorial election cycle as instruments. The other variables were treated as exogenous. To economize on space, only the coefficients on sworn officers are presented in the bottom row in Table V. The other parameter estimates are consistent with the OLS results, although less precisely estimated. Full results are available from the author on request.

The standard errors on the 2SLS parameter estimates are three to four times higher than with OLS due to the instrumenting. Consequently, it is difficult to draw strong conclusions about individual parameter estimates.

[20]For complete documentation of the relationship between election cycles and police staffing, as well as details of the estimation of the first-stage equation, see Levitt (1997).

The overall pattern of coefficients, however, suggests that the results are not very sensitive to the presence of endogeneity. Four of the six 2SLS estimates are higher than the corresponding OLS estimates, while two are lower. The mean reporting bias across all crime categories is 0.178 for OLS and 0.215 for 2SLS.

5. IMPLICATIONS AND CONCLUSIONS

This paper investigates three data sets in an attempt to measure reporting/recording bias. While each of the techniques has prominent shortcomings, it is reassuring that the three sets of estimates are roughly similar in magnitude. The likelihood that a crime will be officially reported appears to be an increasing function of the number of sworn officers per capita, although the results are by no means definitive. Of the 30 separate point estimates of reporting bias presented in this paper, 28 are positive, but only 9 are statistically significant at the 0.05 level. The median reporting elasticity obtained in this papers is approximately 0.12; the mean reporting elasticity is 0.16. Assuming a reporting elasticity of 0.12 for all nonmurder crimes, adding one police officer in the typical large city would result in the additional reporting of roughly five Index I crimes that would not previously have been reported. Based on the estimated cost of crime to victims of Cohen (1988), these five crimes represent a social cost of approximately $20,000. Thus, a naive cost-benefit analysis of the value of an additional police officer that did not take reporting bias into consideration would substantially underestimate the benefits of increasing the police force.

The largest estimates in this paper are obtained in Section V, where the coefficients capture not only victim reporting behavior and police recording practices. This suggests that changes in the diligence of police recording victim crime complaints may be an important part of the story. This conjecture is consistent with the observation that propensity of victims to report crimes to the police has changed little in the NCVS since 1973, but the gap between victims' claims of crimes reported to police and the number of crimes officially recorded has steadily decreased.[21] Better distinguishing between crime reporting and recording is a subject that warrants future attention.

Although the focus of this paper is reporting bias resulting from changes in the size of police forces, parallel arguments can be made for changes in policing strategies. Given that the measured impact of changes in policing

[21]I would like to thank Patrick Langan not only for bringing this fact to my attention, but also for raising my awareness of the important distinction between reporting and recording more generally.

strategies are often small, ignoring the possibility of reporting bias may lead to overly pessimistic assessments of the value of policy interventions. It is widely recognized, for instance, that the adoption of "community policing" practices may lead to higher reporting rates, obscuring any benefits associated with the approach. The results of this paper suggest that such considerations need to be taken seriously.

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