Using Electoral Cycles in Police Hiring to Estimate the Effects of Police on Crime: Reply

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Justin McCrary (2002) demonstrates that a paper of mine on the subject of elections, police, and crime published in this journal (Levitt, 1997) suffers from programming and classification errors. It is with tremendous personal embarrassment that I acknowledge these mistakes, and I thank McCrary for his careful work that has laid bare these errors.

While any mistake in published work is unacceptable, I take some small solace in the fact that the particular errors I made have a relatively small impact on the results. For individual crime categories, the point estimates are not greatly affected by my errors, and the corrected standard errors that McCrary reports are more often than not *smaller* than those in the original paper. The estimates that pooled crime categories were more significantly affected, with the point estimates shrinking substantially and losing statistical significance (although the point estimates are still "large" in the sense of implying important effects of police on crime if they are correct).[1] Even in the original version of the paper it was difficult to draw strong conclusions of the impact of police on crime because of the imprecision of the estimates. McCrary's new evidence rightly points out that even greater caution is warranted when interpreting the pooled regressions.

If electoral cycles can provide no more than suggestive evidence of a causal impact of police on crime, are there other identification strategies that can do better? In the pages that follow, I first discuss the results of two prior papers of note on the subject. I then present new results using the number of firefighters and other municipal workers as instruments for the number of police officers. All three of these approaches confirm a large, negative impact of police on crime consistent with those of Levitt (1997), but with estimates much more precise than those attainable using electoral cycles as instruments.

I. Existing Evidence on the Impact of Police on Crime

The challenge in estimating a causal impact of police on crime is to overcome simultaneity bias: increases in crime are likely to induce politicians to hire more police. Virtually every cross-sectional or correlational study published on the topic finds either no impact of police on crime, or even a positive relationship between these two variables (Samuel Cameron, 1988).

To date there have been three studies that take seriously the identification issues: Levitt (1997), Thomas Marvell and Carlisle Moody (1996), and Hope Corman and H. Naci Mocan (2000). These three papers have utilized very different estimation strategies. Levitt (1997), as noted earlier, uses electoral cycles as instrumen-

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[1] Correcting the other error that I made regarding the classification of mayoral elections, while marginally improving the first-stage fit, does not materially affect either the precision or the magnitude of the two-stage least-squares estimates. The differences that do arise are the consequence of McCrary (2002) using a different model of the timing of elections affecting police hiring. The size of the police force is reported as of October 31 each year, but many mayoral elections occur in the first half of the year. In my paper, I assumed that the increase in the police force would occur in the months immediately prior to the election, and thus would not affect the reported police statistics until the year of the election, whereas McCrary chose to model the increase as occurring in the previous calendar year (in some cases nine months or more before the election).

As an aside that illustrates just how difficult it is to gather reliable information on the timing of mayoral elections, I contacted the mayor's office of the 13 cities in the sample with the greatest discrepancies between my coding and McCrary's coding of mayoral elections. In seven of these cities (Dallas, Milwaukee, Minneapolis, Oakland, Phoenix, Sacramento, and St. Petersburg), the information they provided me is not consistent with McCrary's classification of elections (nor with mine, for that matter). Thus, it appears that McCrary's mayoral election measure continues to suffer from substantial misclassification error.

tal variables. Marvell and Moody (1996) employ a Granger-causality approach, demonstrating that increases in police Granger-cause reductions in crime. Using the city-level panel data set constructed and used in Levitt (1997), the elasticity of total index crime (violent and property combined) is estimated to be −0.30. Marvell and Moody (1996) also show that increases in crime (especially violent crime) predict future increases in the size of the police force. Corman and Mocan (2000) use monthly time-series data for New York City over the period 1970–1996. Corman and Mocan argue that the use of monthly data minimizes any simultaneity bias because of lags in the political response to rising crime. If it takes policy makers three months to increase the police force when crime rises, than annual data will be contaminated by simultaneity, but monthly data will not.[2] They report elasticities of crime with respect to the number of police that range from −0.29 to −1.385 across crime categories, with a median value of −0.452. As in Levitt (1997), police are found to be most effective in reducing homicide among the index crimes.

II. New Evidence on the Impact of Police on Crime Using Firefighters as Instruments

To identify the impact of police on crime using instrumental variables, one needs an instrument that predicts changes in the size of the police force, but is unrelated to changes in crime (after controlling for other relevant factors). In what follows, I propose the number of municipal firefighters per capita as plausible instruments. Factors such as the power of public sector unions, citizen tastes for government services, affirmative action initiatives, or a mayor's desire to provide spoils might all be expected to jointly influence the number of firefighters and police. Empirically, changes in the number of police officers and firefighters within a city are highly correlated over time.

Whether firefighters may be plausibly excluded from the equation determining crime is a more difficult question. There is little reason to believe that the number of firefighters has a direct impact on crime. However, if the set of factors that influences the number of firefighters, (e.g., the local economy, fiscal crises) also affect crime, and these are not properly accounted for in the estimation, the exogeneity of the instrument may be called into question. In the empirical work, therefore, I attempt to control for factors like the local economy, and in some cases, the number of municipal streets and highway employees.[3]

The data set used is an annual, city-level panel covering the period 1975–1995. In order to be included in the sample, a city needed to have at least 100,000 residents in 1973. A total of 122 cities are included in the sample. Because the data on firefighters is incomplete, however, there are typically only about 100 observations per year in the data set.[4] Table 1 provides summary statistics for the variables used in the analysis.[5] The final column of the table reports standard deviations after city-fixed effects and year dummies have been removed, since this is the relevant source of variation used in the paper. There are roughly twice as many police as there are firefighters. There is also substantially greater variation in the number of police, especially within a city over time. One hypothesis to explain this fact is that police staffing responds to crime fluctuations, whereas as the number of firefighters may not.

[2] To the extent that policy makers are able to effectively anticipate future changes in crime and respond proactively, monthly data may also be affected by simultaneity. The same criticism can be leveled at the Granger causality approach.

[3] One can imagine either a positive or negative bias on the two-stage least-squares estimate of the impact of police as a consequence of the instrument failing to satisfy the exclusion restriction. The number of firefighters might positively covary with the unobserved factors increasing crime. For instance, if for political reasons, increases in police also tend to trigger increases in firefighters, leading the estimates to understate the true impact of police on crime. On the other hand, with a fixed municipal budget, an exogenous increase in crime may lead to more police and fewer firefighters.

[4] The missing data is sometimes due to cities failing to report the data, but more often to cities reporting identical data for many years in a row for all employment categories, suggesting that the data for the latter years is invalid. The reporting problems for streets and highway workers is even more severe (cutting the data set another 25 percent). It is for that reason that I do not include that variable in the baseline specifications.

[5] The data are drawn from a variety of sources. See the notes to Table 1 for details.

TABLE 1—SUMMARY STATISTICS

		Standard deviation	
Variable	Mean	Overall	Removing city fixed effects and year dummies
Police per capita	0.0033	0.0011	0.0003
Firefighters per capita	0.0016	0.0005	0.0001
Violent crime per capita	0.0136	0.0068	0.0022
Property crime per capita	0.0765	0.0191	0.0097
City population	2,005,602	2,539,372	105,809
Effective abortion rate (per 1,000 live births)	32.3	60.7	30.5
Unemployment rate	0.068	0.022	0.012
Real state income per capita (1999 dollars)	22,509	3,290	850
Percentage black	0.251	0.159	0.018
State prisoners per capita	0.0020	0.0011	0.0003

Notes: The unit of observation is a city-year pair. Data cover the period 1975–1995. To be included in the sample, a city had to have a population greater than 100,000 in 1973. Police, crime, and city population data are from the Federal Bureau of Investigation's *Uniform Crime Reports*. Firefighter data are from *Annual Survey of Governments*. The effective abortion rate is the weighted average of the abortion rate of crime-aged individuals, as calculated by Donohue and Levitt (2001). Unemployment data corresponds to a city's MSA and is from Department of Labor reports. State income per capita is from *Statistical Abstract of the United States*. The variable Percentage black is linearly interpolated between census years. The state prison population is from Bureau of Justice Statistics reports. There is a substantial amount of missing data for the firefighter variable. All values in the table are weighted by city population.

The first-stage relationship between police and firefighters is estimated as follows:

$$(1)\quad \ln(\text{Police}_{ct}) = \gamma \ln(\text{Fire}_{ct}) + \mathbf{X}'_{ct}\boldsymbol{\Gamma} + \lambda_t + \phi_c + \varepsilon_{ct}$$

where c indexes cities and t captures time. The Police and Fire variables are per capita. A range of covariates are included in the vector $\mathbf{X}$: state prisoners per capita, the unemployment rate in the metropolitan area, state income per capita, the percentage of blacks in the city, and the effective abortion rate in the state. The effective abortion rate (John J. Donohue and Levitt, 2001) is a weighted average of the abortion exposure at birth of the crime-aged population. Year dummies and city-fixed effects are also included. Weighted least squares is used, with weights determined by city population. Heteroscedasticity-robust standard errors are reported in parentheses.

The first three columns of Table 2 present first-stage estimates of equation (1). The elasticity of police with respect to firefighters ranges from 0.206 to 0.251, depending on the set of covariates included. In all cases the coefficient on firefighters is highly statistically significant. In contrast, note that the number of streets and highway workers is not strongly related to the number of police (elasticity of 0.014 with a standard error of 0.014). Thus, the link between police and firefighters does not appear to merely be reflecting municipal hiring more generally, but rather, some specific link between those two functions.

The last two columns of Table 2 show the reduced-form relationship between firefighters and crime rates. The number of firefighters is negatively related to both violent and property crime. Under the assumption that exclusion of firefighters from the crime equations is valid, the combination of a positive first-stage coefficient on firefighters and a negative reduced-form coefficient implies that the instrumental variables estimate will be negative.

The relationship between police and crime is modeled as

$$(2)\quad \Delta \ln(\text{Crime}_{ct}) = \beta_1 \ln(\text{Police}_{ct-1}) + \mathbf{X}'_{ct}\boldsymbol{\Theta} + \lambda_t + \phi_c + \mu_{ct}$$

where, depending on the specification, Crime is the per capita city rate of either violent crime, property crime, or an individual crime category (e.g., murder). The police variable enters once lagged to minimize endogeneity, as does the state prison population, which is among the covariates. Equation (2) is estimated both using weighted least squares, with city populations as weights, and using two-stage least squares with the logged, once-lagged number of per capita firefighters as an instrument for the police variable.[6]

Estimation results are presented in Table 3. Results are shown separately for violent crime [columns (1)–(3)] and property crime [columns (4)–(6)]. Columns (1) and (4) present

[6] The mayoral election instrument cannot be used on this sample because many of the cities included do not have directly elected mayors.

TABLE 2—THE RELATIONSHIP BETWEEN FIREFIGHTERS, POLICE, AND CRIME

	First-stage estimates (dependent variable = ln(Police per capita)			Reduced-form estimates	
Variable	(i)	(ii)	(iii)	Dependent variable: ln(violent crime per capita)	Dependent variable: ln(property crime per capita)
ln(Firefighters per capita)	0.251	0.236	0.206	−0.103	−0.118
	(0.050)	(0.054)	(0.050)	(0.050)	(0.042)
ln(Street and highway workers per capita)	—	—	0.014	—	—
			(0.014)		
ln(State prisoners per capita)	—	−0.101	−0.077	−0.130	−0.255
		(0.022)	(0.022)	(0.036)	(0.030)
Unemployment rate	—	0.571	0.265	−0.723	0.945
		(0.276)	(0.314)	(0.365)	(0.277)
State income per capita (×10,000)	—	0.150	0.211	0.004	0.002
		(0.004)	(0.005)	(0.006)	(0.005)
Effective abortion rate (×100)	—	0.033	0.045	−0.156	−0.127
	(0.013)	(0.013)	(0.026)	(0.025)	(0.025)
ln(City population)	—	0.040	−0.014	0.161	−0.374
		(0.040)	(0.047)	(0.067)	(0.068)
Percentage black	—	0.361	0.493	0.222	0.336
		(0.204)	(0.264)	(0.334)	(0.271)
City-fixed effects and year dummies included?	yes	yes	yes	yes	yes
R^2:	0.947	0.952	0.962	0.931	0.817
Number of observations:	2,032	2,032	1,445	2,005	2,032

Notes: The dependent variable is specified at the top of each column. All estimates are weighted least squares using city population as weights. Heteroscedasticity-robust standard errors are in parentheses. The unit of observation is a city-year pair for cities with population greater than 100,000 in 1973, for the period 1975–1995. Sample size varies due to missing data, especially for municipal streets and highway workers. In the final two columns, the firefighter and prison population variables are once-lagged. For further description of the variables used and data sources, see the notes to Table 1.

estimates of pooled OLS estimates without city-fixed effects. These estimates are identified using cross-city variation, and thus are similar in spirit to the cross-sectional approach that dominated the earlier literature on police and crime (Cameron, 1988). As is typically the case in such specifications, police are positively correlated with crime, with elasticities of 0.56 and 0.11 for violent and property crime respectively. A number of the other covariates also yield unexpected results: more prisoners and higher state per capita income are also strongly positively related to crime rates in this specification.

Columns (2) and (5) add city-fixed effects to the regressions, still using weighted least squares. Consequently, the parameter estimates are now identified in within-city changes over time. The police coefficient now becomes negative, with elasticities of −0.076 and −0.218 for violent and property crime respectively. Only in the latter case is the estimate statistically different than zero. The other variables in the regression now carry the signs suggested by previous research in almost all cases. Higher state imprisonment rates are associated with lower crime (Marvell and Moody, 1994; Levitt, 1996). Higher unemployment rates are strongly positively related to property crime, but the opposite is true for violent crime (e.g., Stephen Raphael and Rudolf Winter-Ebmer, 2001). The effective abortion rate, which is a weighted average of the state abortion rate at the time of birth of the criminal-aged population, is strongly negatively related to crime rates, consistent with Donohue and Levitt (2001). The coefficients on state income per capita and percent of the population that is Black are not statistically significant for either crime category.

Two-stage least-squares estimates are presented in columns (3) and (6). The once-lagged, logged number of firefighters per capita is used as an instrument for the police variable For both violent and property crime, the point estimate on police becomes more negative, with elasticities

TABLE 3—THE IMPACT OF POLICE ON CRIME

	Violent crime			Property crime		
Variable	OLS	OLS	IV	OLS	OLS	IV
ln(Police per capita)$_{t-1}$	0.562	−0.076	−0.435	0.113	−0.218	−0.501
	(0.056)	(0.061)	(0.231)	(0.038)	(0.052)	(0.235)
ln(State prisoners per capita)$_{t-1}$	0.250	−0.131	−0.171	0.189	−0.273	−0.305
	(0.039)	(0.036)	(0.044)	(0.030)	(0.028)	(0.037)
Unemployment rate	3.573	−0.741	−0.480	1.283	1.023	1.231
	(0.473)	(0.365)	(0.404)	(0.312)	(0.274)	(0.326)
State income per capita (×10,000)	0.050	−0.003	0.003	0.010	0.005	0.009
	(0.005)	(0.006)	(0.007)	(0.003)	(0.004)	(0.006)
Effective abortion rate (×100)	−0.214	−0.150	−0.141	−0.184	−0.118	−0.111
	(0.045)	(0.023)	(0.025)	(0.020)	(0.021)	(0.024)
ln(City population)	0.072	0.203	0.178	−0.064	−0.333	−0.355
	(0.012)	(0.063)	(0.067)	(0.006)	(0.063)	(0.066)
Percentage black	0.627	0.233	0.398	−0.136	0.411	0.517
	(0.074)	(0.334)	(0.345)	(0.057)	(0.271)	(0.291)
City-fixed effects and year dummies included?	only year dummies	yes	yes	only year dummies	yes	yes
R^2:	0.601	0.930	—	0.238	0.819	—
Number of observations:	2,005	2,005	2,005	2,032	2,032	2,032

Notes: The dependent variable is listed at the top of each column. The police and prison variables are once-lagged. Heteroscedasticity-robust standard errors are in parentheses. Columns (1), (2), (4), and (5) are estimated using weighted least squares, with city populations as weights. Columns (3) and (6) are instrumental variables estimates using the once-lagged, logged number of firefighters as an instrument for the once-lagged number of police. With the exception of columns (1) and (4), other regressions include both city-fixed effects and year dummies. See the notes to Table 1 for further description of the variables and the data sources.

of −0.435 and −0.501, respectively. The instrumental variables are not particularly precise (although much more precise than can be obtained with electoral cycles), so both these coefficients are only of borderline statistical significance. The point estimates obtained using firefighters as instruments fall in the middle range among papers attempting to deal with the endogeneity of police: the values are in between the violent and property crime point estimates using elections as instruments, almost identical to the findings of Corman and Mocan (2000) for robbery and burglary, and slightly larger than the estimates in Marvell and Moody (1996).

The top panel of Table 4 presents a range of sensitivity analyses. Only the police coefficients are reported in the table, although the full set of covariates, including year dummies and city-fixed effects, are included in all specifications unless otherwise noted. The top row of the table presents the baseline results, which correspond to columns (2), (3), (5), and (6) of Table 3. The OLS estimates are not particularly sensitive to any of the changes in specification, so I focus the discussion on the instrumental variables estimates. Including a second lag of police (as done in Levitt, 1997) somewhat reduces the point estimate on violent crime, but does not greatly affect the property crime coefficient. Adding streets and highway workers as an instrument for police increases the elasticities for both crime categories, but also raises the standard errors because the sample size shrinks by 25 percent. In an alternative model, streets and highway workers might be more appropriate as a right-hand-side variable rather than an instrument. The estimates are little changed by its inclusion. The coefficients are similar across small and large cities in the sample, but appear to be larger in the later years of the sample.[7]

[7] Although results regarding city size and early, middle, and final years of the sample must be interpreted with some caution. The coefficients in the table are those obtained when I restrict the other variables in the regression to have the same parameters across the different subsets of the data. When those other parameters are freed up, the results become much more erratic.

TABLE 4—SENSITIVITY ANALYSIS OF THE ESTIMATED IMPACT OF POLICE ON CRIME

	Violent Crime		Property Crime	
Specification	OLS	IV	OLS	IV
Baseline	−0.076 (0.061)	−0.435 (0.231)	−0.218 (0.052)	−0.501 (0.235)
Include two lags of police	−0.143 (0.063)	−0.284 (0.230)	−0.251 (0.056)	−0.540 (0.261)
Instrument using both firefighters and streets and highway workers	−0.203 (0.076)	−0.661 (0.315)	−0.154 (0.058)	−0.530 (0.298)
Include streets and highway workers as a control variable	−0.069 (0.059)	−0.371 (0.231)	−0.216 (0.052)	−0.484 (0.236)
City population greater than 250,000	−0.094 (0.060)	−0.506 (0.238)	−0.219 (0.052)	−0.510 (0.242)
City population less than 250,000	−0.122 (0.061)	−0.540 (0.242)	−0.220 (0.053)	−0.515 (0.246)
Include region-year interactions	−0.015 (0.058)	−0.358 (0.305)	−0.188 (0.044)	−0.845 (0.434)
Estimation in first differences, not city-fixed effects	0.018 (0.043)	−0.270 (0.140)	−0.085 (0.031)	−0.378 (0.104)
Early years of sample	−0.051 (0.063)	−0.292 (0.336)	−0.264 (0.052)	−0.149 (0.394)
Middle years of sample	−0.097 (0.069)	−0.508 (0.268)	−0.209 (0.113)	−0.612 (0.255)
Final years of sample	−0.115 (0.068)	−0.938 (0.401)	−0.113 (0.061)	−0.726 (0.380)

Individual crime categories	OLS	IV
Murder	−0.408 (0.088)	−0.914 (0.332)
Rape	0.014 (0.079)	−0.034 (0.273)
Robbery	−0.434 (0.072)	−0.452 (0.255)
Aggravated assault	0.421 (0.106)	0.397 (0.348)
Burglary	−0.294 (0.066)	−0.195 (0.255)
Larceny	−0.136 (0.058)	−0.135 (0.198)
Auto theft	−0.317 (0.104)	−1.698 (0.570)

Notes: Each entry in the table represents the coefficient on the police variable from a separate regression. In all cases, the full set of covariates included earlier in the paper are also included in the regressions, but not reported in the table. The baseline results in the top row of the table correspond to columns (2), (3), (5), and (6) of Table 3. When two lags of police are included, the coefficient reported is the total effect of police, and two lags of firefighters are used as instruments. The estimates for the early, middle, and late part of the sample are done jointly, restricting the coefficients on the other variables in the regression to be constant across time periods. The cities with populations above and below 250,000 are also estimated jointly. Robust standard errors are in parentheses. The bottom panel of the table report results for individual crime categories.

Including region-year interactions for the nine census regions reduces the violent crime estimate but increases the property crime estimate. Estimating the model in first differences, rather than using city-fixed effects, yields smaller but more precisely estimated elasticities.

The bottom panel of Table 4 presents estimates for the seven individual crime categories tracked by the FBI. Interestingly, for only two of the crime categories (murder and auto theft), does instrumenting have a large impact on the point estimates. The large impact of police on murder parallels the findings in Levitt (1997) and Corman and Mocan (2000).

III. Conclusion

Estimating the causal impact of police and crime is a difficult task. As such, no one study to date provides definitive proof of the magnitude of that effect. Nonetheless, it is encouraging that four different approaches (elections as instruments, firefighters as instruments, using Granger causality, and using high-frequency time-series variation) have all obtained point estimates in the range of −0.30–0.70. The similarity in the results of these four studies is even more remarkable given the large previous literature that uniformly failed to find any evidence that police reduce crime—a result at odds with both the beliefs and the behavior of policymakers on the issue.

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