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NOTES

CRIME, DETERRENCE AND THE BUSINESS CYCLE IN NEW YORK CITY: A VAR APPROACH

Hope Corman, Theodore Joyce, and Norman Lovitch*

Abstract—We apply a VAR (vector autoregressive) technique to estimate the interrelationships between unemployment, arrests, police, demographics, and property-related felony crimes in New York City from 1970 to 1984. Despite the limitations of using a VAR, the technique provides a useful alternative to more standard models in analyzing what causes crime. The study concludes that arrests provide a strong deterrent to crimes. But, the reverse effect of crime on arrests, is extremely small. We find a brief and relatively weak effect of changes in unemployment rates on crime. Changes in demographics are associated with relatively stronger changes in crime rates

The issue of crime and its control has been extensively analyzed by economists since the seminal work by Becker (1968). The theoretical basis for most of the studies is straightforward. Crime is viewed as a labor force decision by the potential criminal who weighs the expected gains against the expected costs. Although the behavioral model is widely accepted, the empirical testing of the model has been criticized by a number of authors. One of the major problems discussed by Fisher and Nagin (1978) is that deterrence variables and the crime rate are mutually causal in some dynamic fashion which is extremely difficult to model using ordinary regression techniques. A second major problem, discussed by Freeman (1983), is that multicollinearity among the independent variables makes it difficult to assess the relative contributions of deterrence variables and employment variables.

Recent advances in econometrics, specifically the work by Sims (1980) and Litterman (1979) on vector autoregressions (VAR) provide an alternative approach to estimating the simultaneous relationship between crime, employment, and deterrence. As Sims makes clear, however, VAR are not a substitute for the estimation of structural models; rather they are a tool for empirically confirming or questioning the relevance of various hypothesized relationships. By applying VAR techniques to a time series of crime in New York City, we are able to approximate a dynamic structure in which all the relevant variables are endogenous. Thus, we circumvent the issue of ad hoc identification restrictions—a problem which has hindered the empirical estimation of the crime supply function. Moreover, contemporaneous correlations among the innovations of the variables in the model are a potentially serious problem. However, the various procedures for describing the system yield warnings when such "multicollinearity" may be obscuring an accurate accounting of each variable's relative impact.

I. The Empirical Test

We have selected five key measures for our universe of variables. The first three variables, the crime rate, the arrest rate and the size of the police force, refer to the criminal justice system. Our fourth variable is the New York City unemployment rate. For our final variable we include the percentage of the population which is male and between the ages of 16 and 24, the most crime prone cohort. There are 174 monthly observations for New York City beginning in January of 1970 and ending in June of 1984.

The crime rate refers to all property-related felonies per 1,000 population 16 and over in New York City. Property-related felonies include robbery, burglary, grand larceny and auto theft. For the arrest variable, we calculate arrests per 1,000 population 16 and over. The police variable measures the actual number of police officers per 1,000 population 16 and over.[1]

Because VAR require the estimation of many parameters, we are limited in the number of variables we can use in the analysis. We believe our five variables are the best possible representation of the criminal system, given the availability of data and our focus on "short-run" responses. The police and arrest variables

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[1] A description of data sources and a fuller presentation of results appears in a longer version of this paper, available on request from the authors.

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TABLE 1.—TESTS OF GRANGER CAUSALITY, *F*-STATISTICS, FIVE-VARIABLE, FOUR-LAG VAR 1970:1 THROUGH 1984:6

	Dependent Variable in Logs				
	Proportion of Young Males	Unemployment Rate	Police per Population	Crime Rate	Arrest Rate
Four lags of...					
Proportion of young males (mean value = 0.082)	431.07 (0.0)	0.82 (0.51)	0.22 (0.92)	3.76 (0.01)	0.89 (0.47)
Unemployment rate (mean value = 8.54%)	1.48 (0.21)	25.30 (0.0)	0.60 (0.66)	0.73 (0.57)	1.52 (0.20)
Police per 1,000 population (mean = 4.7)	0.40 (0.81)	2.65 (0.04)	139.02 (0.0)	0.81 (0.52)	0.83 (0.50)
Crime rate per 1,000 population (mean = 13.3)	0.73 (0.57)	1.54 (0.19)	0.18 (0.95)	37.40 (0.0)	1.36 (0.25)
Arrests per 1,000 population (mean = 0.838)	0.04 (0.99)	0.78 (0.54)	1.22 (0.30)	6.84 (0.0)	34.45 (0.0)

Note: Each entry is an *F*-statistic based on the null hypothesis that the set of lags in the explanatory variable is equal to zero The marginal significance level is in parentheses

are the only criminal justice variables relevant exclusively to New York City. Courts and prisons are administered at the state level. Also, police and arrests can respond to changes more quickly than courts, where the conviction and sentencing process often takes over a year.[2]

Vector autoregressions require other restrictive assumptions which may affect our results. First, we limit the number of lags. Second, all of the variables must be lagged for the same number of periods, a restriction which may not be appropriate. Finally, we must assume that during the time period under study, there were no major structural shifts in the crime system. Altogether, our results must be interpreted with these restrictions in mind.

II. Results

All of the variables are expressed as logarithms. None of the variables is seasonally adjusted; rather, each equation contains a set of eleven seasonal dummies, a dummy variable for the City's fiscal crisis, a constant and a time trend. There are 174 observations covering the period from January 1970 through June 1984. To increase the degrees of freedom a system with 4 lags was tested as a restriction on a system with 6 lags. The test is based on the difference in the log determinant of the system-wide variance-covariance matrix of the restricted versus the unrestricted equations (Sims, 1980). The null hypothesis of no difference was accepted. The χ^2 (with 50 degrees of freedom) was 55.45 with a marginal significance level of 0.28.[3]

Our results section concentrates on two important issues. The first is to ascertain which variables appear exogenously determined and which variables appear to be influenced by the others. The second is to examine the magnitudes of the response of each variable to unexpected changes in the others variables. These simulations are referred to as the impulse response functions (Sims, 1980).

A. *Tests of Exogeneity*

To determine exogeneity, several tests are performed. The first such test is an *F*-test on the set of lags from the autoregressive system referred to as a test of "Granger causality." Stated informally, a test of whether y is Granger (1969) caused by x is equivalent to regressing y on lagged values of itself and lagged values of x and testing whether the set of coefficients on x are different from zero.

Table 1 presents the *F*-statistics for each set of lags in each equation. The results generally conform to expectations. Surprisingly, however, lagged values in the number of police per capita have a significant effect on the unemployment rate. Given the circumstances surrounding the New York City fiscal crisis, it is plausible that municipal layoffs, as captured by size of the police

[2] Other omitted variables relate to the socio-economic status of the population. We believe SES variables are important. But, the socio-economic structure of New York City did not vary greatly over our sample period.

[3] This is the modified χ^2 proposed by Sims (1980).

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TABLE 2.—VARIANCE DECOMPOSITIONS, FIVE-VARIABLE, FOUR-LAG VAR, 36 MONTHS AHEAD

	Dependent Variable in Logs				
	Proportion of Young Males	Unemployment Rate	Police per Population	Arrest Rate	Crime Rate
Percentage of variance accounted for by...					
Proportion of young males	82 (82)	8 (8)	22 (22)	1 (1)	13 (13)
Unemployment rate	0 (0)	66 (65)	2 (2)	3 (3)	2 (2)
Police per 1,000 population	1 (1)	12 (12)	40 (39)	2 (2)	2 (2)
Arrest rate per 1,000 population	3 (10)	2 (1)	32 (26)	90 (79)	57 (41)
Crime rate per 1,000 population	13 (7)	12 (12)	3 (9)	4 (15)	25 (40)

Note: Entries are the percentage of forecast error variance accounted for by innovations in the explanatory variables The first entry is for causal ordering A and the figure in parentheses is for causal ordering B. Causal ordering A is as follows: young males, unemployment rate, police, arrest, and crime. Causal ordering B is the same as A except arrest and crime are reversed.

force, serve as a leading indicator for the New York City unemployment rate. Of particular interest is the apparent exogeneity of the arrest rate with respect to crime. Lagged values in the arrest rate explain current values of the crime rate whereas the reverse is not true.

A second means of gauging the exogeneity of each variable is based on a decomposition of the variance of the forecast error at various time horizons (Sims, 1980, 1981). If a variable is exogenous to the other components, then its own innovations should account for a major portion of the variance in its forecast error when shocks to that same variable are simulated. A major criticism of such simulations is that the innovations among the variables in the system must be uncorrelated contemporaneously to insure an accurate accounting (Gordon and King, 1982).

To insure orthogonality, Sims suggests applying a transformation to triangularize the variance-covariance matrix of residuals to obtain a block recursive system among the errors. However, this imposition of an a priori ordering would seem to beg the question of exogeneity. Practitioners of VAR respond by trying several orderings based on theory and the degree of correlation among the errors (for example, see Litterman and Weiss, 1985). By careful use of the variance decompositions, insights with respect to exogeneity can be gleaned from the data.

Table 2 presents the variance decompositions for a 36-month horizon under two alternative causal orderings. Overall, the results support the conclusions with respect to exogeneity obtained from the autoregressive specification. Specifically, the arrest rate is again found to be exogenous. It accounts for 90% of the variance in its own forecast error when its innovations are orthogonal to innovations in the crime rate, and 79% when this ordering is reversed. Furthermore, it explains more of the variation in the crime rate than innovations in the crime rate itself. This holds regardless of the ordering.

Also noteworthy is the lack of a relationship running from police per capita to either the arrest rate or the crime rate. As surprising as this result may seem, Brecher and Horton (1985) report that although the total number of arrests in New York City fell by 20% between 1975 and 1979, arrests for felony crimes increased by 11%. These figures imply that police responded to cut-backs in personnel by allocating scarce resources away from the prevention of misdemeanors and violation activities and towards the prevention of felonies.

Innovations in the arrest rate explain at least 26% (depending on the ordering) of the variance in the number of police per capita. Except for the police variable itself, no other variable accounts for as much of the variance in the forecast error of police per capita as does the arrest rate. We take this as support for the inclusion of the arrest rate as a determinant of the demand for police protection. Specifically, increases in the arrest rate lower the price of protection, thereby prompting greater demand for police protection.

B. Impulse Response Functions

The most useful way to characterize the dynamic relationships among the components of the system is to simulate their responses to unanticipated shocks in each of the variables. However, as with the variance decompositions, contemporaneous correlations among

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innovations may obscure the true impact of a shock in one variable on the others. The solution is the same as with the variance decompositions—a transformation is applied to the variance-covariance matrix of residuals such that the resulting matrix is block recursive. The imposition of a causal ordering among the contemporaneous innovations only matters when their correlations are substantial. In our system, two relationships appear problematic: The correlations between the proportion of young males and police per capita ($p = -.40$) and between the arrest rate and the crime rate ($p = -.22$).[4] Because the latter relationship is of more interest and potentially more consequential, we present alternative causal orderings for these two variables. We initially permit contemporaneous shocks in the crime rate to be influenced by shocks in the other four variables in the following order: teens, unemployment, police and arrests (causal ordering A). For causal ordering B, we permit contemporaneous innovations in the crime rate to impact on arrests.

The impulse responses generated from causal orderings A and B are presented in figure 1a–g.[5] To make the results comparable, each response is expressed as a proportion of the standard deviation of its residuals. Figure 1a presents the responses by the crime rate, the arrest rate and police per capita to a one standard deviation shock in the proportion of young males. The changes in the crime rate are as expected. After an initial decline in the first period, the crime rate remains above its initial level for the next 35 periods. The arrest rate is below its initial level in 18 of the first 20 months after the shock. Although such a shock is unlikely, it confirms the results obtained from the autoregressive specification that demographics are an important determinant of crime.

A jump in the unemployment rate (figure 1b) precipitates a rise in the crime rate that is quickly overwhelmed by the deterrent effect of an increased arrest rate. This accords with Freeman's (1983) summary of the literature—the impact of unemployment on the crime rate is far weaker than the effect of criminal sanctions. The response of police per capita to a shock in unemployment conforms to expectations. The manpower measure falls below its initial level three months after the jump in unemployment. Note that the

FIGURE 1a.—SHOCK IN TEENS—CAUSAL ORDERING A

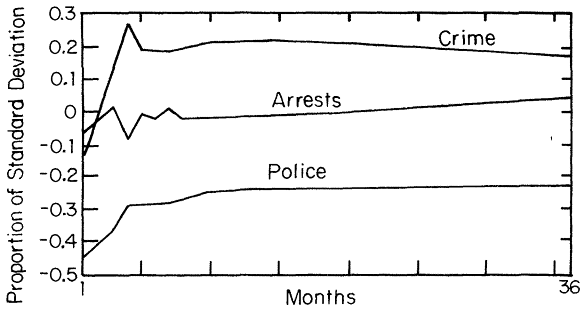


FIGURE 1b.—SHOCK IN UNEMPLOYMENT—CAUSAL ORDERING A

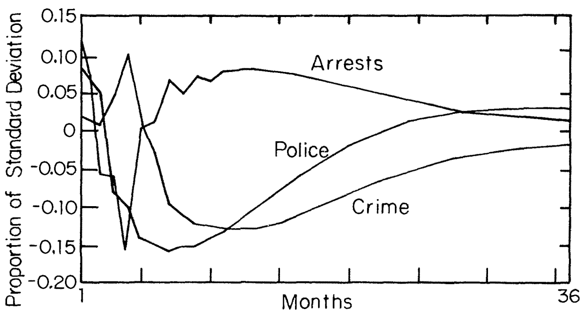
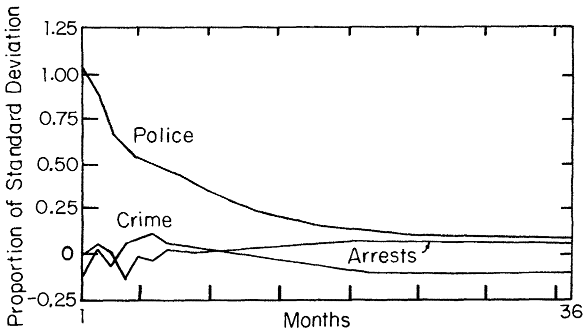


FIGURE 1c.—SHOCK IN POLICE—CAUSAL ORDERING A



unemployment rate returns to its preshock level 10 months after its initial jump.

Unanticipated changes in the number of police per capita (figure 1c) impact on the arrest rate. However, it takes nine months before increases in manpower are translated into greater arrest rates and thirteen months before these changes result in a declining crime rate. Whether the decrease in the crime rate is a direct result of increased presence of police on the street or the response to a greater probability of being arrested cannot be determined with certainty.

[4] Changing the causal ordering to allow innovations in the manpower measure to affect the proportion of young males in the same time period lowered the percentage of forecast error in the proportion of young males accounted for by its own innovations from 82% to 62%. No other relationships were altered.

[5] We present both causal orderings only when there are perceptible differences in the results—for shocks in arrests and crime.

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FIGURE 1d.—SHOCK IN ARRESTS—CAUSAL ORDERING A

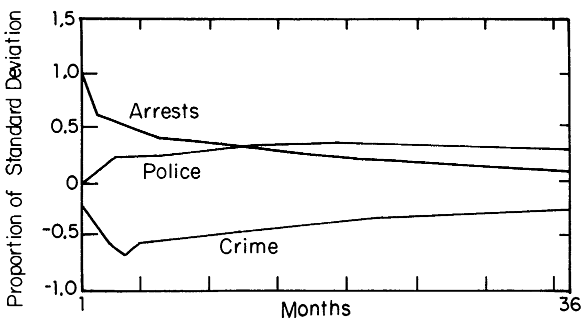


FIGURE 1f.—SHOCK IN ARRESTS—CAUSAL ORDERING B

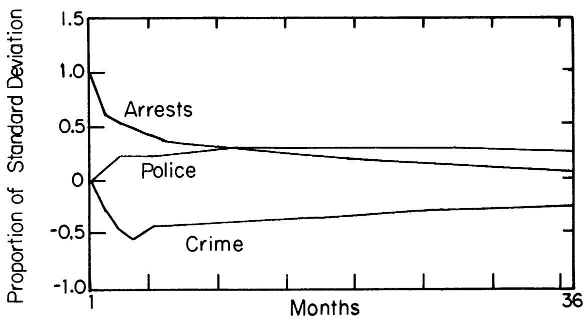


FIGURE 1e.—SHOCK IN CRIME—CAUSAL ORDERING A

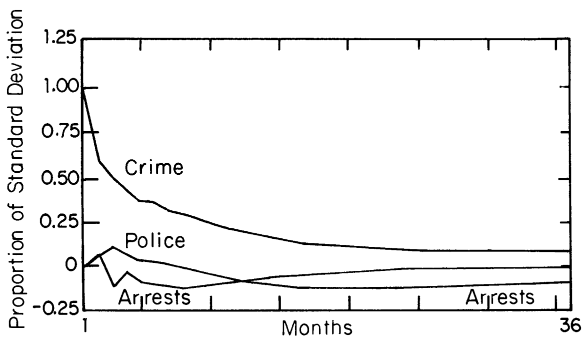
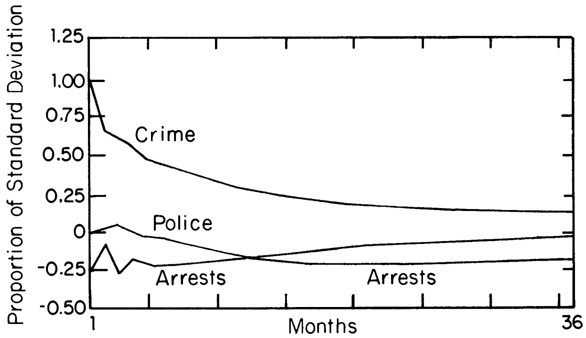


FIGURE 1g.—SHOCK IN CRIME—CAUSAL ORDERING B



The most dramatic result is the strong and persistent deterrent effect of an unanticipated increase in the arrest rate on property crime. The results are unchanged qualitatively by altering the causal ordering of the contemporaneous innovations (compare figures 1d and 1f). As can be seen in figure 1d, the arrest rate remains above its initial level over the entire 35 months following the shock, although it slowly approaches its initial level. The crime rate, however, shows the greatest decline 4 months after the unanticipated change in the arrest rate. From that point on, the crime rate stays well below its initial level. A shock in arrests also raises the number of police per capita above its initial level for the entire 36 months.

The result of a one-standard deviation increase in the crime rate is presented in figures 1e and 1g. As is evident, crime remains higher although it slowly moves back towards its initial level. The arrest rate, after an initial jump upwards, remains somewhat below its initial level. The response by law enforcement officials to a surge in the crime rate is an initial increase in police per capita that lasts for only six months before declining modestly over the following 30 months. Note that although there are some quantitative differences between the causal orderings, the results are qualitatively similar. Comparison of the results in figures 1d and 1f with results in figures 1e and 1g indicates that the proportion of the error in the crime equation attributable to innovations in the arrest rate far exceeds the proportion of the error in the arrest equation that is attributable to innovations in the crime rate.[6] In contrast, changing the

[6] These results imply that arrest rates are unaffected by contemporaneous changes in the crime rate. For example, under causal ordering A we postulate the following error structure:

$$e_{at} = u_t$$

and

$$e_{ct} = Bu_t + v_t$$

where e_{at} and e_{ct} are the innovations in the arrest rate and the crime rate, respectively, at time t. A shock in the arrest rate implies a proportional shock in the crime rate. Under causal ordering B the reverse holds: a shock in the crime rate implies a proportional shock in the arrest rate. The result that innovations in the arrest rate are not affected by the causal ordering whereas innovations in the crime rate are, suggests that v_t has little impact on e_{at}. As a further check, we ran impulse response functions without a causal ordering and found no qualitative difference in results.

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causal ordering does not alter the average response of the arrest rate to an unanticipated change in the arrest rate itself. These results imply that the probability of arrest affects the crime rate contemporaneously whereas the arrest rate is unaffected by contemporaneous changes in the crime rate.

Conclusion

Estimation of the supply of crime function has remained a difficulty for economists because of the unknown dynamic interrelationships between the demand for police protection, the production of protection, and the actual supply of crime functions. Standard time-series modeling of these relationships has been justly criticized since results are only as good as the identification restrictions. Because the VAR in essence relaxes these restrictions, it allows an econometric analysis more consistent with a dynamic behavioral model.

Our results strongly confirm the often reported finding that arrests do deter crimes. Moreover, criminal behavior is more sensitive to changes in sanctions than law enforcement agencies are to changes in crime. With respect to unemployment, we find a brief and relatively weak increase in the crime rate to unanticipated jumps in the unemployment rate. These are important findings for they corroborate the conclusions of other researchers with a new, and potentially fruitful, statistical approach.

At the same time, our results underscore some of the shortcomings of VAR in this context. Foremost, the limited specification permits rather unrefined conclusions as to various causal relationships. For example, increases in police per capita have no impact on either the crime rate or the arrest rate. We take this as evidence that law enforcement agencies in New York City responded to decreases in manpower by redistributing resources towards the prevention of more serious crimes. Yet, we are unable to test this hypothesis formally. To do so would require that we expand the empirical model to include crimes and arrests for both misdemeanors as well as non-property felonies. However, even if we had such data, which we do not, the exponential loss of degrees of freedom makes the estimation of such a model impossible without many more observations. Another example relates to the unemployment rate. More refined data on unemployment by age and race might yield a stronger relationship between crime and the business cycle. Again, for reasons just described, the inclusion of various labor market measures is not possible. Despite these limitations, VAR permit a rich set of interactions which can serve as a complementary analysis to more traditional structural models.

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