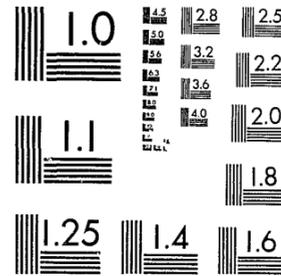


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Sell: Occupational Relocations

an & George: Measuring Role/Identity

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Morrissey: Comment on Lord & Falk

Lord & Falk: Reply to Morrissey

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The Effect of Arrests on Crime: A Multivariate Panel Analysis*

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ABSTRACT

We estimate multiwave panel models for the effect of clearance rates and a vector of socioeconomic control variables on index crimes, using a sample of 98 U.S. cities for the years 1964-70. No consistent evidence of a substantial effect is found.

The effect of police practices on rates of law violation is a matter that has a practical bearing on crime prevention strategies. It can also enhance our understanding of the contributions made by formal methods of social control to conformity. Despite the importance of these concerns, surprisingly little is known about the effects of criminal justice institutions on crime rates. Indeed, only in recent years have researchers begun to study the degree to which crime can be reduced by marginal changes in police employment, expenditure, patrolling strategies, or efficiency in solving crimes.¹

Statistical investigations of the relationship between aggregated rates of crimes known to the police and various indicators of police activity have employed various analytic strategies. Some researchers have analyzed cross-sectional data (Brown; Geerken and Gove; Sjoquist; Wilson and Bolland, a, b); others have analyzed time series (Cloninger and Sartorius; Phillips and Votey) or panel data (Greenberg et al., a; Logan). The present analysis has been designed to meet methodological criticism of this body of research.

Critics of research dealing with the effect of law enforcement on crime rates have pointed to two important statistical sources of bias: speci-

*Authors' names are in alphabetical order. We are grateful to Sheri L. Prupis and Barbara F. O'Meara for carrying out computer computations, and to Charles Logan for supplying data. Support for this research was received under Grants #79-NI-AX-0054 and #80-IJ-CX-0062 from the National Institute of Law Enforcement and Criminal Justice. Points of view are those of the authors and do not necessarily reflect the position of the U.S. Department of Justice. An earlier version of this paper was presented at the 1980 meeting of the American Society of Criminology.

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fication bias arising from the omission of relevant variables, and simultaneous equation bias (Fisher and Nagin; Greenberg, a, c; Nagin).

Specification Bias

Since crime rates are expected to be influenced by social, economic, and demographic variables, the effect of sanction levels on crime rates can be assessed only if these other variables are taken into account. Some researchers have ignored this issue altogether, reporting analyses based only on the relationships between crime rates and sanction levels (e.g. Brown; Geerken and Gove; Greenberg et al., a, b). Other investigators have attempted to control for such effects by introducing in their equations a set of variables believed to influence crime rates. However, should variables that influence both the crime rate and the sanction level be omitted from the analysis, parameter estimates for the effect of sanctions on crime will be biased, perhaps badly.

Simultaneous Equation Bias

Ordinary least-squares (OLS) methods for estimating regression equations assume that independent variables are unaffected by the dependent variable. In the present context, this means assuming that the crime rate has no influence on the predictor variables included in a regression equation for the crime rate. There are reasons for thinking that at times this assumption may be false. It has been suggested, for example, that high levels of crime could strain the limited resources of law enforcement agencies, reducing effective sanction levels. It may also be the case that when crime rates rise, the criminal justice system responds with harsher or more certain punishment. If these, or other processes in which crime rates affect law enforcement exist, failure to take them into account could lead to seriously biased estimates for the effect of law enforcement on crime.

Simultaneous equation methods permit the effects of crime on sanctions and sanctions on crime to be determined even when feedback effects of this sort are present. However, these methods can be applied to cross-sectional or time series data only if stringent assumptions are made about the effects of exogenous variables on the jointly dependent endogenous variables. To estimate the effect of a criminal justice sanction on crime one must be able to specify a priori the precise effect that one or more exogenous variables has on crime. Since criminological theory typically does not make predictions about the magnitude of effects that are expected to be present, this is usually done by specifying that these exogenous variables have no effect at all on crime.

It is rare in criminological research that assumptions of this kind can be made with much confidence. Too little is known about the effects on crime of a good many variables for us to be able to assert with conviction that they do not influence the crime rate. Yet if the researcher makes an

incorrect assumption about such effects, parameter estimates can be seriously biased. Statistical theory provides no method for discovering this bias from the parameter estimates themselves.

In their survey of the crime deterrence literature, Fisher and Nagin concluded that the assumptions made for the purposes of identifying simultaneous equation systems were often highly implausible on substantive grounds. Their conclusion that it will be difficult to arrive at an alternative set of more plausible assumptions underscores the desirability of using statistical techniques that do not entail such sharp limitations.

Researchers have begun to realize that panel data (collected for multiple units of analysis at a number of fixed times) can help to overcome the limitations of cross-sectional analyses. Thus Wilson and Boland (b) observe that information about changes in arrests and crime rates over a 5- or 10-year period would provide a surer basis for inferences about their reciprocal effects than the cross-sectional data they analyze; and Tittle, noting that the conclusions reached in his perceptual study of deviance deterrence are premised on assumptions about the causal order of variables that cannot be tested with his cross-sectional data, suggests that panel data would be helpful in this regard.

We have pointed out elsewhere (Greenberg and Kessler; Kessler and Greenberg) that panel data do not automatically resolve all questions of causal inference. One must still make some a priori statistical assumptions in order to estimate the effects of variables on one another. Under specific circumstances, though, multiwave panel data make it possible to identify simultaneous equation systems with weaker assumptions than those required when working with cross-sectional or time-series data. In addition, panel data permit partial tests of these assumptions, and allow information about the possible contribution of omitted exogenous variables to be extracted from the data.

These features make panel analysis a methodology of choice for studying the relationship between crime rates and sanctions. However, the panel analyses of crime rates published to date (Greenberg et al., a; Logan; Pontell) fail to control for the effect of exogenous variables on crime and sanctions. As we noted above, this failure can result in spurious correlations being treated as evidence of causal effects, and hence lead to biased estimates of the effect of arrests on crime. The present study deals with this possibility by extending an earlier panel analysis of crime rates and clearance rates (Greenberg et al., a) through the introduction of a set of socioeconomic control variables. The earlier study found no evidence that higher clearance rates led to lower crime rates. In the present study we are able to determine whether this conclusion holds up when appropriate control variables are taken into account.

Data and Procedures

DATA

Our analysis is based on information about crimes, clearances (crimes solved by the police, usually but not always through arrest), and a number of control variables, for a stratified random sample of 98 U.S. cities with populations of more than 25,000, for the years 1964 through 1970.² We explore the relationship between per capita rates for the seven index crimes and total index crimes (as measured by offenses recorded by the police), and the clearance rates for these offenses, defined as the ratio of offenses cleared to the number of offenses known to the police for that offense in that year.³ Other influences on these variables are taken into account by introducing control variables believed on theoretical grounds to be relevant to crime causation and to police effectiveness in solving crimes.

The following variables are used in our analysis⁴:

1. *Population* (1967). A larger population implies a greater degree of anonymity, and hence weaker informal social controls. This in turn facilitates the emergence of a criminal subculture. In addition, police enforcement is less likely to be effective when victims do not know the identities of their victimizers.
2. *Population density* (1970). This variable is chosen for the same reason as *population*.
3. *Percent of the population below the age of 18* (1970). Arrest rates for teenagers are disproportionately high, probably because psychological and social stress associated with adolescence lead to higher rates of involvement in crime (Greenberg, b). Comparative lack of expertise in crime would be expected to increase the clearance rate of those in this age bracket.
4. *Percent of the labor force employed in manufacturing* (1970). We consider this variable to take account of the possible influence of labor force composition on crime. One might speculate that variability in the stress associated with different kinds of jobs is reflected in crime rates, and that blue collar parents socialize their children differently than white collar parents, in ways that have consequences for involvement in delinquency. In addition, Humphries and Wallace note that the ecological layout of manufacturing cities differs from that of cities with other kinds of economic activity in ways that can be expected to influence patterns of crime and law enforcement.
5. *Percent of the labor force that is unemployed* (1970). For offenses involving illegal acquisition, this variable measures one incentive to violate the law. Stress associated with unemployment may also be causally related to violent crimes. Individuals who are unemployed have, on the average, re-

duced prospects for future earnings, and thus risk less from an arrest than those who are presently employed. Reduced risk might be expected to result in higher crime rates.

6. *Median income* (1960). This is an index of potential victim stock for property crimes. To the extent that cultural evaluation of theft and violence are linked with socioeconomic status, median income will also control for cultural contributions to variation in crime rates. In addition, median income represents community resources available for law enforcement, and thus may indirectly influence the clearance rate.
7. *Percent of population with Spanish surnames* (1970). Minority group membership implies lower income and reduced prospects for future lawful achievement. Members of groups that have been victimized by discrimination are expected to accord less legitimacy to legal norms and law enforcement and, lacking internalized respect, less likely to conform to the law and its agents. In addition, the police may be less hesitant about arresting suspects who are members of minority groups.
8. *Percent of families headed by a female* (1970). Several delinquency theorists (Cohen; Miller) have argued that delinquency can originate in the reaction of adolescent males to a female-headed household. A household headed by a single parent may also possess fewer resources for supervising and controlling children.
9. *Skewness of income* (1970), as measured by the ratio of the standard deviation of income to median income.⁵ Anomie theory (Merton) suggests that skewness of income holds out to those in low income brackets the possibility of receiving incomes that are higher than can be attained through legitimate means. This may lead to crime as an illegitimate means of achieving material goals in the absence of internalized inhibitions against stealing. Cloward and Ohlin contend that prospective thieves who fail at illegal enterprises involving theft or organized crime may turn to violence in frustration. This variable is thus potentially relevant to the genesis of violent crimes as well as theft. Inclusion of this variable is also dictated by conflict-theoretical arguments that wealthy elites strengthen police forces to cope with the threats to the social order created by economic inequality (Jacobs). The particular indicator of inequality we use has the desirable properties of being scale-free, and of being smaller for a given spread of income when overall incomes are higher.
10. *Percent of the population that is black* (1970). The rationale for the inclusion of this variable is the same as that for variable 7. We treat the two variables separately rather than lumping blacks and Hispanics into a single minority category because cultural differences and differences in family structure between the two groups may be relevant to crime causation.

11. *Regional dummy variable for northern cities.*⁶ Region may be a proxy for cultural differences relevant to crime causation (e.g., subculture of violence) and law enforcement effectiveness, as well as for aspects of social structure that distinguish different regions of the U.S., but that are not fully taken into account by the other variables.

12. *Regional dummy variable for southern cities.*⁷ This variable is included for the same reason as variable 11. Cities not classified as either northern or southern are western.

Comparison with the control variables employed in studies conducted by economists shows that our list taps dimensions of social life that they have not included.⁸

PROCEDURES

Because there is reason to think that the lagged and instantaneous effect of crime on clearances might be of opposite sign, we estimated models that included both lagged and instantaneous effects.⁹ As theory does not tell us precisely what time-lag to expect, we estimated models with lags of one, two, and three years. We found that the autocorrelations among crime rates a year apart were extremely high—so high, in fact, that the correlation matrix could not be inverted. This means that the effect of other variables on these rates is too low over the space of a year for this effect to be distinguished statistically, given our sample size. For this reason, we examined models involving lags of two and three years.

All the models considered were variations on the model shown in Figure 1, a three-wave model involving per capita crime rates (C), clearance rates (A), and a set of exogenous control variables (Z) assumed to influence C and A . To avoid complicating the figure, the exogenous variables are omitted, and correlations among errors are not shown; however, the models estimated did take into consideration the possible existence of cross-sectional and serial correlations among error terms and higher-order autoregressive terms (e.g., the effect of A_1 on A_3 , where the subscripts label the waves of observation). Models with two-year lags were estimated with data for the years 1964, 1966, 1968 and 1970; models with three-year lags, with data for 1964, 1967 and 1970.

The computer program LISREL IV (Jöreskog and Sörbom) was used to estimate all the models discussed below. We began our analysis by estimating four-wave models with a two-year time span between each wave, for each index offense separately, and for total index offenses. In these models we assumed that high-order autoregressive terms were absent, and that all correlations among errors were zero. When correlations among variables were too high to permit inversion of the correlation matrix, we considered three-wave, three-year lag models instead.

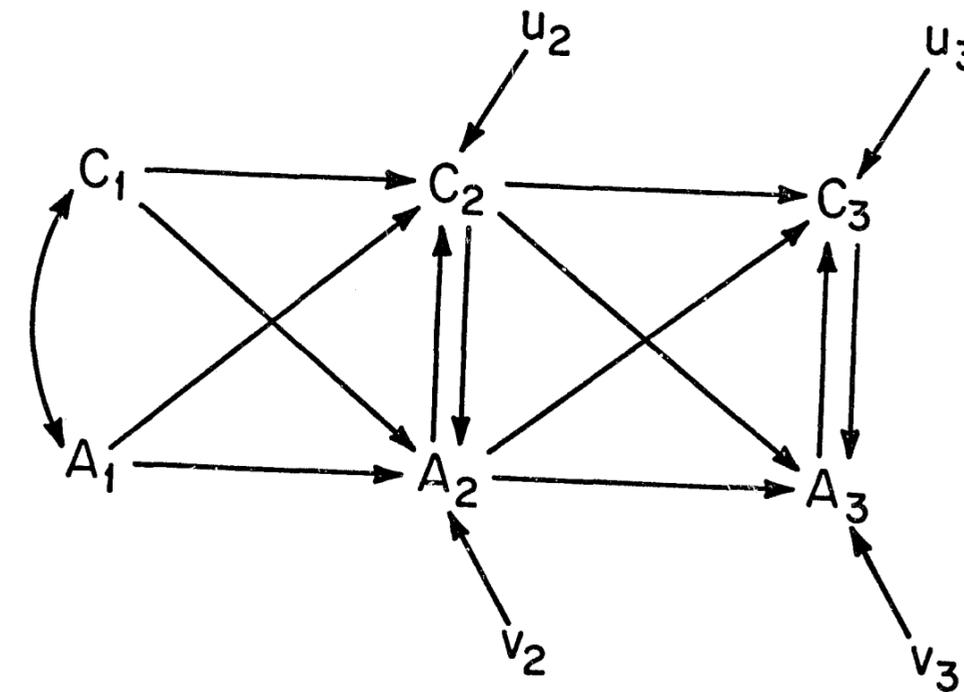


Figure 1. THREE-WAVE PANEL MODEL FOR CRIME RATES, ARREST RATES, AND EXOGENOUS CONTROL VARIABLES

In estimating the four-wave models we assumed that instantaneous standardized regression coefficients (e.g., for the influence of A_t on C_t) remain constant over time. Cross-lagged effects linking time 1 with time 2 were unconstrained, but those linking time 2 with time 3 were assumed to equal those linking time 3 with time 4. A comparison of the estimates linking times 1 and 2 with those linking 2 and 3 (or 3 and 4) then permits a partial test of the constancy assumption. No constraints were imposed on the first-order autoregressive terms, but cross-lagged effects lagged by two or more waves were assumed to be absent.

Equilibrium Conditions

Provided the crime rates–clearance rates system is not in equilibrium, these models are overidentified, with every parameter either just-identified or overidentified, even though no assumptions have been made about the magnitude of the effect of any exogenous variable on crime or on clearances. In equilibrium, on the other hand, the normal equations used to estimate model parameters are redundant, so that insufficient information is available to identify the model. Intuitively, this happens because in equilibrium multiple waves of data do not provide the additional information

needed to identify equations; in a sense, one is seeing the same thing in each wave, so that the over-time information of panel data does not add to the information contained cross-sectionally.¹⁰

The assumption that the relationship between crime rates and clearance rates was not in equilibrium between 1964 and 1970 is reasonable on substantive grounds. Crime patterns began to change markedly in the early sixties. For instance, homicide rates, which had been declining for three decades, began to rise in 1963, and continued to increase through the early seventies. Victims were more likely than before to be unknown to perpetrators, slain in connection with street robberies; and were proportionally less often than before spouses or friends of the perpetrator. Clearance rates began to decline in the same period.

Some commentators have attributed changes in crime patterns in the early sixties to the disillusionment with the failure of the civil rights movement to achieve more rapid gains in the status of blacks, and declining clearance rates to restrictions imposed on the police by the U.S. Supreme Court in these years. For purposes of the present analysis, it is immaterial what the reasons were for these changes. What is important is that an external shock or constraint of some sort generated disequilibrium among our variables, at least for a time. Internal evidence from our estimates suggests that the assumption of disequilibrium is not unreasonable.

Model Revision

The models on the basis of which we draw our inferences were arrived at through a series of analyses. We first estimated the initial model described above for each offense. Where the initial model fit the data poorly, the model was revised by adding serial correlation of errors, higher-order autoregressive effects, or causal effects linking crime rates and clearance rates separated by more than one wave. The technical output provided by the LISREL program provided guidance as to which additional effects would prove helpful in improving the fit. Once an acceptable fit was obtained, effects that were small and consistent with zero at the 0.05 significance level were fixed at zero. These revised models were then reestimated. This process was repeated until a good fit was achieved *parsimoniously*. Comparison of the chi-square statistics for good-fitting models in which the effect of clearances on crime were estimated, with identical models in which these effects were fixed at zero, enabled us to determine whether a significant crime-prevention effect was present.

There is a danger that in fitting and trimming a model in this way, one will overfit the data. To protect against fitting what are essentially sampling fluctuations, a useful rule-of-thumb is to incorporate only effects that are fairly consistent over time. Some of the models arrived at by our procedure violate this principle. In some of the models, for example, we found that an effect linking time 1 and time 2 variables proved to be

significant, while the corresponding effects linking time 2 with time 3 and time 3 with time 4 were small and insignificant. We decided to consider such models despite the danger of over-fitting, since the alternative was to accept models that fit the data poorly. In that case, interpretation of parameter estimates would be uncertain, and we would be faced with substantial ambiguity in model specification as well. It remains true, though, that where a given effect is found for one pair of waves but not for other pairs in the panel, our confidence that it represents a genuine effect is greatly reduced.

Findings

The best-fitting models for each offense are summarized in Table 1, which contains parameter estimates for the models with 2-year lags, and Table 2, which contains parameter estimates for the models with 3-year lags.¹¹ Where more than one model provided an acceptable fit, all are shown in the table. We discuss for each offense the effect of arrests on crime, the effect of crime on arrests, and the effect of exogenous control variables on arrests and crime. Stability coefficients are given in a table in the appendix, as they are of less interest.

THE EFFECT OF ARRESTS ON CRIME

If arrests at time t reduce crime rates at time t' (where t' may or may not be the same as t), the estimated standardized coefficient $A_t C_{t'}$ should be negative and statistically significant. This proves to be true in only one of the two models that provide an acceptable fit to the murder data (Table 1). In model (a), one lagged effect is negative and statistically significant ($A_1 C_2 = -.356$), but the other lagged effects (for $A_2 C_3$ and $A_3 C_4$) and the instantaneous effects are all consistent with zero. In model (b), all parameters for the effect of clearances on crime are fixed at zero. Model (a) fits the data significantly better than (b), but both models provide quite good fits. Evidence for a possible crime-prevention effect here is clearly very limited. The lack of consistency in the estimates for model (a), and the good fit obtained for the null model (b) lead us to conclude that we have no persuasive evidence in our data for the existence of a crime-prevention effect for murder.

For burglary, we find good fits with two different models. In model (a), the lagged effect $A_1 C_2$ is estimated while all other lagged effects are fixed at zero; and the instantaneous effects $A_t C_t$ are constrained to be equal. The lagged effect is extremely small, providing a check on the assumption that the parameters $A_2 C_3$ and $A_3 C_4$ are zero. The instantaneous effect is negative and statistically significant, but quite small in magnitude.

Table 1. ACCEPTABLE MODELS FOR CRIME RATES (C) AND CLEARANCE RATES (A) WITH TWO-YEAR LAGS^a

Parameter	Offense					
	Murder (Model I)	Murder (Model II)	Rape	Burglary (Model I)	Burglary (Model II)	Grand Larceny
A → C						
A ₁ C ₂	-.356*			.017		
A ₂ C ₃						
A ₃ C ₄						
A ₂ C ₂				-.085*		
A ₃ C ₃				-.085*		
A ₄ C ₄				-.085*		
C → A						
C ₁ A ₂	-.365*			-.096		
C ₂ A ₃						
C ₃ A ₄						
C ₁ A ₄	-.081					
C ₂ A ₄			-.329*			
C ₂ A ₂			-.291*	-.034		
C ₃ A ₃				-.034		
C ₄ A ₄				-.034		
C ↔ A						
C ₁ A ₁	-.173	-.173	-.105	-.255*	-.255*	-.451*
C ₂ A ₂	.065	.074			-.074*	
C ₃ A ₃	-.193*	-.193*			-.052*	
C ₄ A ₄	-.019	-.023			-.038	
Fit Statistics						
χ ²	12.47	23.14	17.46	29.06	28.13	21.81
d.f.	14	17	16	16	17	17
probability level	.50	.10	.30	.02	.02	.10
largest S-Σ ^b				.068	.091	

^aX_iY_j represents the standardized regression coefficient for the effect of X_i on Y_j.

^bS-Σ is the difference between the observed and predicted covariance matrix. Cell entry is the largest element in the difference matrix. Entries are given only for models in which the probability level is greater than 0.05.

*Statistically significant at the 0.05 level.

Table 2. ACCEPTABLE MODELS FOR CRIME RATES (C) AND CLEARANCE RATES (A) WITH THREE-YEAR LAGS^a

Parameter	Offense			
	Aggravated Assault	Robbery	Auto Theft	Total Index
A → C				
A ₁ C ₂	.077			
A ₂ C ₃	-.143*			
C → A				
C ₁ A ₃			.239*	
C ↔ A				
C ₁ A ₁	-.177*	-.215*	.456*	-.346*
C ₂ A ₂	.053			-.012
C ₃ A ₃	.055			-.100*
Fit Statistics				
χ ²	11.09	13.82	39.61	7.37
d.f.	5	9		8
probability level	.02	.30	<.001	.30
largest S-Σ ^b	.127		.070	

^aX_iY_j represents the standardized regression coefficient for the effect of X_i on Y_j.

^bS-Σ is the difference between observed and predicted covariance matrix. Cell entry is the largest element in the difference matrix. Entries are given only for models in which the probability level is greater than 0.05.

*Statistically significant at the 0.05 level.

In model (b), all effects in which A influences C are fixed at zero, but cross-sectional correlations among errors are estimated (in model (a) they were fixed at zero). The fit here is also good, but not significantly better than in model (a). Thus the evidence for a crime prevention effect here is ambiguous, and the effect is small in any event.

For aggravated assault (Table 2), our best-fitting model differs significantly from the observed correlation matrix; but since the fit is substantially quite good, we proceed to interpret parameter estimates. We find a statistically significant estimate $A_2C_3 = -.143$, consistent with a crime-prevention effect. However, the estimate for A_1C_2 is smaller, positive and not statistically significant (.077). The lack of consistency here makes us re-

luctant to interpret the estimate for A_2C_3 as evidence for a crime-prevention effect.

In all other offense categories, good fits to the data were obtained for models in which crime prevention effects were small and not statistically significant. When these effects were then fixed at zero and the models reestimated, good fits were still obtained; subtraction of the chi-square statistics for the two models showed the crime prevention effects to be statistically insignificant.

THE EFFECT OF CRIME ON CLEARANCE RATES

Estimates of the parameter C_tA_t , provide evidence regarding the effect of crime rates on clearance rates. For several of the models in Tables 1 and 2, we find isolated estimates for C_tA_t , parameters that are negative and statistically significant; for example, in model (a) for murder (Table 1), the parameter C_tA_2 is $-.365$, statistically significant. But the other seven parameters are either not significant ($C_tA_4 = -.081$) or consistent with zero. Thus there is no consistent evidence for a saturation effect, or for a process in which rising crime rates enhance the efficiency of law enforcement efforts. In other models such as murder (b), burglary (b), and grand larceny (Table 1), and aggravated assault, robbery, and total index offenses (Table 2), all parameters are consistent with zero.

THE EFFECT OF EXOGENOUS VARIABLES

The effect of exogenous variables can be studied in our models in two ways: through parameter estimates for the influence of control variables on our endogenous variables, and through the correlations of errors in models where these were estimated. Since the focus of this paper is on the impact of law enforcement on crime, we report our findings for the effect of the control variables on crime rates and clearance rates somewhat cursorily.¹²

With only a few exceptions, the effects of the control variables on crime rates and clearance rates were neither large nor consistent from one year to the next. A few of the variables did show consistent effects that were more than miniscule in magnitude (statistically significant at the 0.05 level or having an absolute value of at least .10); we confine our discussion to these contributions.

The effect of city population on murder, rape, and robbery was positive, but no such effect was found for the other offenses. The effect of population density on robbery was also positive, while its effect on clearances for murder, assault, robbery, larceny, and total index offenses was negative. Contrary to expectation, the proportion of the population below the age of 18 had no effect on crime rates. It tended to reduce the clearance rate for assault, but to increase it for auto theft.

Proportion of the labor force employed in manufacturing was found to have no consistent, substantial effect on crime rates. It did increase the clearance rate for assaults, but reduced it for auto thefts. Percent of the labor force that is unemployed had a positive effect on murder, but not on any of the other offense rates. Median income had a negative effect on rape, a positive effect on burglary, but no consistent effect on the other crime rates. The effect of this variable on clearances for rape and auto theft was negative.

Percent of the population with Spanish surnames was positively related to rape and assault; it tended to reduce clearance rates for rape, but to increase them for assaults. Percent of the population that is black was positively associated with murder, assault, robbery and auto theft, as well as with clearances for assault and robbery.

Percent of families headed by a female had no consistent effects on crime rates, but had a positive effect on clearances for robbery and larceny. Skewness of income had a negative effect on rape and no consistent effect on other crime rates. It did tend to reduce clearances for larceny and auto theft. Cities in the North had higher robbery rates and clearance rates for robbery, but lower clearances for auto theft; cities in the South had lower rates of rape and clearances for rape.

Given that the correlations of control variables with crime rates and clearances are neither negligible nor inconsistent, this failure to find stronger and more consistent effects may seem surprising. The reason for this finding, however, can readily be seen by considering the unstandardized structural equation for the crime rate:

$$C_t = a + b_1C_{t-1} + b_2A_t + b_3A_{t-1} + b_4Z_t + u_t. \quad (1)$$

For simplicity, the equation includes only a single control variable Z , but the argument will not be affected if additional control variables are present. If we subtract the quantity C_{t-1} from left-hand and right-hand members of the equation, we have

$$\Delta C_t = C_t - C_{t-1} = a + (b_1 - 1)C_{t-1} + b_2A_t + b_3A_{t-1} + b_4Z_t + u_t. \quad (2)$$

We see that the coefficient $(b_1 - 1)$ measures the effect of the level of C on change in C , while the remaining coefficients are direct measures of the effect of A_t , A_{t-1} and Z_t on change in C . When we estimate equation (1), then, we are not estimating the effect of Z on C , but the effect of Z on change in C . Once this is understood, the paucity of statistically significant coefficients for the effect of our control variables on crime rates is less mysterious. We might expect, for example, that a certain level of unemployment would generate a corresponding level of crime in a city. But we would not expect a fixed level of unemployment to lead to steadily increasing levels of crime. The same is true for our other control variables.

Even where the control variables did contribute significantly to the

regressions, their contributions were almost always quite modest. Hardly any of the standardized effects were as large as .30 in magnitude. Comparing regressions with all control variables deleted from the regressions (reported in Greenberg et al., a) with those reported here, we find increments in the variance explained of roughly 5 percent, not a large amount. Moreover, our conclusions about the effect of arrests on crime are not substantially changed by the inclusion of these variables. In the earlier study, good fits were obtained for all offenses with models in which the effect of clearances on crime rates was fixed at zero. Here, such effects are either consistent with zero or quite small in magnitude.

The insensitivity of these conclusions to the inclusion or exclusion of the twelve control variables used here gives us confidence that our findings are unlikely to be badly biased by the omission of additional causes of crime.¹³ Our confidence that this is so is strengthened by our estimates of the correlations among contemporaneous error terms of crime rates and clearance rates. Good fits for rape, burglary (a), grand larceny, robbery, and auto theft were obtained for models in which these correlations of errors were fixed at zero; in models for murder (a), murder (b), burglary (b), aggravated assault and total index offenses these correlations were estimated; but as seen from the entries in Tables 1 and 2, the estimates proved to be invariably small. In no case did they exceed .20 in magnitude, and in most instances they were considerably smaller. The amount of bias in estimates for the parameters $A_i C_i$, that could be present as a result of the omission of variables responsible for these correlations of errors could not be large.

Discussion

Our analysis finds no consistent evidence for the proposition that higher arrest clearance rates result in substantially lower index crime rates. Where parameter estimates for the effect of arrests on crime are consistent (as in the model for aggravated assault), they are quite small. When effects are larger in magnitude (as in one of the two models for murder), they are not consistent over time. For most of our models we did not even find inconsistent evidence for a crime-prevention effect of arrests.¹⁴

Our failure to find evidence for a crime-prevention effect contrasts with the econometric studies based on cross-sectional or time-series data, which have found evidence consistent with a crime-prevention effect. We attribute these discrepant findings to the dubious assumptions made in the econometric analyses. Cloninger and Sartorius assume that crime rates are entirely uninfluenced by socio-economic variables, and thus that law enforcement variables *alone* influence crime rates. As noted earlier, Phillips

and Votey control for labor force participation alone, neglecting all other social influences on crime. Sjoquist considers a wider range of control variables, but neglects the possible reciprocal effect of crime on arrests.

In the most careful cross-sectional analysis to date, one which finds that arrests reduce crime for robbery but not for burglary, and only inconsistently for auto theft, Wilson and Boland (b) assume that police patrol strategy is affected by a city's political climate but not by its crime rate; and that patrol strategy affects the crime rate only by changing the probability of an arrest, not directly. They frankly concede that their analysis depends on the validity of these assumptions, and while they find them plausible, they cannot demonstrate that they are true. We think Wilson and Boland's assumptions are at least debatable. Crime has become a hot political issue over the last fifteen years. In this atmosphere, a city's political climate and police patrol strategies may well have been influenced by its crime pattern. Police can deter crime by appearing on the street, even if they make no arrests; indeed, it is a common experience that highway drivers will slow down when they see a squad car, even if they do not see any cars being ticketed.

Our approach, based on panel data, has obviated the need for assumptions as stringent as those made in earlier work, solely for reasons of statistical convenience. This is not to say that the present approach is entirely free from assumptions. The method we outline here assumes that parameters are stationary, and our parameter estimates are not entirely consistent with it. Fortunately, this assumption is not needed in the null models. We have assumed also that lagged effects are felt within the space of a few years; were the correct time-lag closer to a century, we would be unlikely to see any signs of a causal effect. Here our everyday knowledge about the duration of social processes comes to our aid to exclude such possibilities.

Since the assumptions we make are less stringent and more plausible than those made in earlier studies, we are inclined to attribute the discrepant findings to model misspecification in the earlier work. We were able to verify in our own data that an inappropriate cross-sectional analysis could lead to biased parameter estimates consistent with a crime-prevention effect. We did this by estimating multiple regression equations for the crime rates in 1970 using 1970 clearance rates and the 12 control variables as predictors. Here lagged endogenous variables are not included among the predictors, and no attempt is made to model the short-run effect of crime on clearances.¹⁵ In these models, the estimated standardized regression coefficients for the effect of clearances on crime rates were: murder, $-.004$; rape, $-.128$; assault, $-.177^*$; robbery, $-.232^*$; burglary, $-.213^*$; auto, $-.142$; grand larceny, $-.319^*$; and total index offenses, $-.232^*$ (starred parameters are significant at the 0.05 level). All eight estimates

are negative, and five of them are statistically significant. This contrasts with our failure to find consistent negative estimates for these parameters in the panel analysis.

Although we find no evidence that marginal changes in clearance rates have an appreciable impact on crime rates, we are far from claiming that police practices have no effect on crime. Part of the appeal of the deterrence doctrine in its current revival at the hands of economists is that we all know from introspection and from everyday observation, that people can be made to conform by threatening them with undesirable consequences. It may be that we fail to see evidence of this in the present data because the clearance rate is too poor an indicator of the probability of an arrest for us to detect a prevention effect. Another possibility is that marginal changes in risk are not communicated very well to prospective offenders. Since systematic information about risks is not at all easy for prospective offenders to obtain, this may well be the case. Where information about changes in penalties is not communicated, we can hardly expect those changes to result in changed deterrence.

Once we acknowledge that information about penalties and how they change can be highly inaccurate, our failure to find substantial and consistent evidence that clearances reduce crime becomes understandable. It can still be the case, however, that crime is deterred by mistaken beliefs about penalties. There is an old joke about the small hamlet that purchased a sign reading "Radar used to apprehend speeders"—but couldn't afford the radar equipment. Our findings cannot say whether that investment was a sound one. A sign in a store warning that "shoplifters will be prosecuted" may reduce theft even if the store owner never apprehends or prosecutes shoplifters.

What we can say is that given the sort of publicity about law enforcement practices that now prevails, marginal increases in clearance rates—those that are on the order of the differences observed among the clearance rates of the cities in this sample—are unlikely to lead to measurable reduction in index crimes. More substantial changes in clearance rates, or in the sanctions imposed at later stages of the criminal justice system (or propaganda campaigns about the risk of crime, whether empirically founded or not) might well have a greater effect.

Notes

1. Research dealing with the effect of law enforcement on crime rates is often advertised as "deterrence research." In fact, most of this work does not study the deterrent effect of sanctions alone, but the net effect of law enforcement on crime, regardless of the process by which enforcement creates this effect. The possible contributions of rehabilitation and incapacitation (restraint), reinforcement of the collective conscience, and so forth, are not eliminated in analyses of the aggregate relationships between crime rates and sanction levels.

2. For details of the sampling procedure, see Greenberg et al. (a). On the basis of a separate analysis of data for all 50 states as well as the data for the sample of cities analyzed here, we

argue elsewhere (Greenberg et al., b) that data for cities are less likely to be subject to aggregation bias.

3. Roland Chilton has recently argued that ambiguities in the definition of a clearance make it less satisfactory than the ratio of arrests to crimes (although an arrest is the most common way the police clear a crime, there are other ways as well). However, the latter ratio is not entirely satisfactory either. Since a single crime can be cleared by the arrest of more than one individual, the ratio of arrests to crimes is not identical to the theoretically relevant probability that a crime will be followed by an arrest. We consider it an open question at this time which indicator is the most appropriate in studying the impact of arrests on crime.

4. Although we had initially planned to include indices for poverty and for high income, these variables proved to be highly correlated with our other control variables and were consequently excluded from the analysis. Data for some of the control variables included were available for both 1960 and 1970. However, correlations among observations at the two times proved to be quite high (ranging from 0.70 to over 0.90). To avoid problems of multicollinearity, data for only one of the two time points were used. We employed 1970 data for all variables except median income; 1960 values were used for this variable because they were not as highly correlated as the 1970 values with other variables. Data for all the control variables were drawn from the Bureau of the Census.

5. These quantities are computed on the basis of the proportions of families earning less than \$3,000, between \$3,000 and \$4,999, between \$5,000 and \$6,999, between \$7,000 and \$9,999, between \$10,000 and \$14,999, between \$15,000 and \$24,999, and over \$25,000. For computing purposes, the upper limit of the highest-income category was taken to be \$35,000.

6. Cities located in the following states were classified as northern: Connecticut, Illinois, Indiana, Iowa, Massachusetts, Michigan, Minnesota, Missouri, New Hampshire, New Jersey, New York, Ohio, Pennsylvania, Vermont and Wisconsin.

7. Cities were classified as southern if located in Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Virginia. Cities were classified as western (neither northern nor southern) if located in California, Nebraska, Nevada, New Mexico, North Dakota, Oklahoma, Utah and Washington.

8. For example, Phillips and Votey introduce only a single control variable, an indicator of labor force participation; Sjoquist fails to include indicators of such theoretically relevant variables as age distribution of the population, the degree of affluence in a community, inequality of income, occupational distribution, broken homes, and region of the United States.

9. We have shown elsewhere (Greenberg and Kessler) that parameter estimates may have the wrong sign in models that have incorrectly specified lags. In the present context, there is reason for thinking that lagged and instantaneous effects of crime on sanctions may be of opposite sign, since saturation effects, if they exist, should be short-run (instantaneous), while pressure to increase the efficiency of policing in response to rising crime rates is expected to be effective only gradually, over a period of time. If these two opposite-sign effects are lumped together in a model that included only an instantaneous effect or only a lagged effect, the two processes would tend to cancel one another.

10. As the system of equations approaches equilibrium, the standard errors of parameter estimates increase rapidly, so that even moderately large parameters fail to achieve statistical significance in large samples. We did not encounter this in our estimates. At the point where equilibrium is reached, our models become underidentified. Intuitively, this happens because in equilibrium, multiple waves of data do not provide the additional information needed to identify equations. For a more mathematical treatment of approach to equilibrium in multi-wave panel models, see Kessler and Greenberg.

11. Models involving lags of three years were estimated only when the correlation matrix with variables lagged at two-year intervals could not be inverted.

12. We do not provide estimates for the effects of control variables on crime rates or clearance rates here (there are several dozen such estimates for each offense), but restrict our discussion to the most noteworthy features of the estimates. The first-named author will provide tables of these estimates upon request.

13. Given this argument one may wonder whether our use of panel models renders our analysis equally insensitive to the effect of *arrests* on crime. The answer is no. If the true structural equation governing crime rates is $C_t = a + b_1A_t + b_2Z_t + e_t$, where e is an error term, and

one subtracts from this the same equation lagged by one time unit and rearranges terms, one obtains the equation

$$C_t = C_{t-1} + b_1(A_t - A_{t-1}) + b_2(Z_t - Z_{t-1}) + (e_t - e_{t-1}).$$

This equation tells us that the coefficients of A_t and Z_t represent the respective effects of change in A and Z on change in C . If Z_t and Z_{t-1} are correlated and only Z_t is included in the analysis, its contribution will be suppressed by the factor

$$(1 - r_{ZZ_{t-1}}).$$

Where the correlation between Z_t and Z_{t-1} is high, as it is in our data, this suppression is high. Since A_t and A_{t-1} were not highly correlated in our data, both were included in our initial models, and one or the other deleted only when its contribution was negligible. Here the low correlation between lagged and instantaneous values reduces the suppression effect.

14. We also examined models involving offense rates for individual offenses and arrest clearance rates for total offenses. The rationale for considering these models was the possibility that an arrest for one offense might deter someone contemplating the commission of another offense. Moreover, if criminals switch crime categories (and there is reason to believe that some do), the arrest and incarceration of someone charged with a given offense may eliminate offenses in other crime categories through the restraining (incapacitative) function of imprisonment. None of these models yielded evidence suggesting the existence of a crime-prevention effect for arrests.

15. Our panel analyses suggest that no serious error is likely to result from the neglect of reciprocal causation. Any discrepancy between the cross-sectional analysis and the panel analysis, therefore, must arise from the inclusion of the lagged endogenous variable in the latter analysis.

To see the circumstances under which the two approaches will yield the same conclusions about the effect of predictor variables on the criterion, consider the panel equation $y_2 - y_1 = a_1y_1 + a_2x + a_3z$. If the variables have reached equilibrium, so that $y_1 = y_2$, we can set the left hand member equal to zero, and solve the resulting equation for y_1 , obtaining $y_1 = -(a_2/a_1)x - (a_3/a_1)z$. The coefficients for x and z are in the correct proportion, although their magnitudes will in general be biased. Thus it is only in equilibrium that the static and dynamic approaches yield comparable results.

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Appendix

The stability coefficients for the two-year lag models summarized in Table 1 are given in Table A.1; those for the three-year lag models summarized in Table 2, in Table A.2.

Table A.1. STABILITY COEFFICIENTS FOR TWO-YEAR LAG MODELS FOR CRIME RATES (C) AND CLEARANCE RATES (A)^a

Parameter	Offense					
	Murder (Model I)	Murder (Model II)	Rape	Burglary (Model I)	Burglary (Model II)	Grand Larceny
C ₁ C ₂	.334	.382	.267	.840	.856	.927
C ₂ C ₃	.368	.368	.213	.854	.893	.796
C ₃ C ₄	.273	.274	.621	.621	.650	.904
C ₁ C ₃			.236			
A ₁ A ₂	.356	-.288	.173	.568	.606	.568
A ₂ A ₃	.237	.237	.069	.594	.608	.334
A ₃ A ₄	.183	.189	.218	.670	.673	.687
A ₁ A ₃		.427		.301	.303	.386
A ₁ A ₄			-.206			
A ₂ A ₄			.166			

^aX_iY_j represents the standardized regression coefficient for the effect of X_i on Y_j.

Table A.2. STABILITY COEFFICIENTS FOR THREE-YEAR LAG MODELS FOR CRIME RATES (C) AND CLEARANCE RATES (A)^a

Parameter	Offense			
	Aggravated Assault	Robbery	Auto Theft	Total Index
C ₁ C ₂	.760	.771	.912	.847
C ₂ C ₃	.660	.689	.986	.825
A ₁ A ₂	.467	.256	.638	.606
A ₂ A ₃	.557	.196	.713	.513
A ₁ A ₃	.269	.140	1.015	

^aX_iY_j represents the standardized regression coefficient for the effect of X_i on Y_j.

END