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Crime and Arrests: An Autoregressive Integrated Moving Average (ARIMA) Approach

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Various theoretical perspectives suggest that marginal changes in the quantity of crime and arrests are related to one another. Unfortunately, they provide little guidance as to the amount of time that is required for these effects to be realized. In this paper, autoregressive integrated moving average (ARIMA) time-series modeling techniques, which necessitate making minimal assumptions concerning the lag structure one expects to find, are utilized to examine the crime-arrest relationship. The bivariate ARIMA analyses of monthly crime and arrest data for Oklahoma City and Tulsa, Oklahoma, for robbery, burglary, larceny, and auto theft reveal little evidence of a lagged crime-arrest relationship.

KEY WORDS: autoregressive integrated moving average (ARIMA); deterrence; incapacitation; crime control.

1. INTRODUCTION

This paper is concerned with the relationship between aggregate measures of crime and arrests. Specifically, we are interested in identifying the amount of time that is necessary for current levels of crime to produce a change in the quantity of arrests and the amount of time that is necessary for current levels of arrests to produce a change in the quantity of crime. Knowledge about the proper lag structure between crime and arrests is especially critical for those who seek to examine the deterrent effect of legal sanctions on future law violations (e.g., Geerken and Gove, 1977), as well as for those who use crime as a statistical control in studies which attempt to evaluate hypotheses derived from the conflict perspective of social control (e.g., Williams and Drake, 1980; Liska and Chamlin, 1984). Misspecification

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of the crime–arrest relationship can lead to erroneous conclusions about the social processes under examination (Hanushek and Jackson, 1977).

2. THEORETICAL CONSIDERATIONS

The quantity of crime is believed to influence arrests in two ways. First, an increase in the amount of crime can "overload" the law enforcement capacities of criminal justice agencies. It has long been recognized that, in the short run, criminal justice resources are relatively inelastic. Thus, when the number of crimes increases, fewer investigatory personnel (especially police) can be allocated to each crime incident. Hence, the probability of an individual arrest, as well as the aggregate number of arrests, is likely to decrease. Second, a change in the amount of crime can also affect public perceptions of the crime problem. As the total number of crimes increases, communities may become desensitized and thereby more tolerant of criminal behavior. Over time, a more permissive public attitude can lead to less rigorous enforcement practices (Lemert, 1951; Greenberg *et al.*, 1979). On the other hand, an increase in the amount of crime could motivate citizens to apply more pressure on law enforcement officials to apprehend offenders aggressively and effectively. The net effect of public perceptions of the crime problem will depend on the relative strength of these countervailing processes.

Minimally, marginal changes in the level of arrests can be thought to affect crime rates in two ways. First, deterrence theory suggests that an increase in the certainty of punishment should lead to a decrease in future law violations. Although there is considerable leakage between an arrest and actual punishment, the importance of this initial deprivation of liberty is not trivial. An arrest, regardless of the eventual outcome, could stigmatize an individual and thereby restrict one's conventional opportunity structure. Therefore, it is likely that the perceived risk of being arrested enters into the calculation of the "costs" of engaging in illegal activities. Second, the "incapacitation" thesis also suggests that an increase in the certainty of punishment should lead to a decrease in future law violations. According to this view, a reduction in the quantity of reported crimes can be attributed, in part, to the removal of repeat offenders from the general population. Although most research emphasizes the incapacitation effects of imprisonment (e.g., Nagin, 1978), arrests can also result in the incapacitation of offenders. To the extent that those arrested cannot secure release (i.e., bail) while awaiting further criminal justice proceedings, the incidence of reported crime is likely to decrease.

In sum, there is sufficient reason to believe that there is a reciprocal relationship between aggregate measures of crime and arrests. What is less clear, however, is the expected lag structure of this relationship. For example, the deterrence thesis suggests that the lagged effect of arrests on crime should reflect the amount of time required to disseminate information about marginal changes in the likelihood of being arrested to potential offenders. However, there is little agreement concerning the actual amount of time that is needed to accomplish this task (cf. Greenberg *et al.*, 1979; Wilson and Boland, 1978). Similarly, while the overload and community tolerance theses anticipate that some time is likely to pass before a change in the level of crime results in a change in the level of arrests, neither perspective offers any guidance as to the expected length of time involved. In short, while there is considerable agreement among researchers that the relationship between crime and arrests is not likely to be instantaneous, there is little consensus about the lag structure one is likely to discover. Given the inability of theory to provide a priori assumptions about the potential lag structure between crimes and arrests, the estimation of the crime–arrest relationship becomes somewhat problematic.

3. METHODOLOGICAL CONSIDERATIONS

Recently, Greenberg and Kessler (1982, p. 773) have argued that multiwave panel analysis ". . . is the methodology of choice for studying the relationship between crime rates and sanctions . . . ." Their contention rests on the following observations: (1) cross-sectional and econometric time-series techniques (e.g., 2SLS) require the researcher to make rather strong and often implausible assumptions to meet identification restrictions of the models; and (2) since theory and research are rather vague and contradictory as to the lag structure one might expect to find, panel analysis is preferred because it permits the researcher to make weaker assumptions to identify the model (some of which may be relaxed or altered to produce a better fit) and allows the data to play a greater role in reaching a final solution. In short, Greenberg and his associates argue that multiwave panel analyses are better than simultaneous equation and econometric time-series analyses because the data are better (Greenberg *et al.*, 1979; Greenberg and Kessler, 1982).

The data employed by Greenberg and his associates, however, are not without their limitations. Both the 1979 and the 1982 panel analyses of the crime–arrest (clearances) relationship are performed on yearly data. Insofar as the actual relationship between crime and arrests is characterized by yearly lags, this is no problem. Unfortunately, as Greenberg and Kessler (1982, p. 776) readily admit, "[sociological] . . . theory does not tell us what time-lag to expect." Hence, to the extent that the actual time lag between the effects of crime on arrest and the effects of arrest on crime varies appreciably from a year, multiwave panel models are likely to misrepresent the crime–arrest relationship. Given that Greenberg *et al.* (1979) and Greenberg and Kessler (1982) find virtually no systematic association between crime and arrests (clearance rates), this potential deficiency becomes more troublesome.

We agree with Greenberg and others (e.g., Loftin and McDowall, 1982) that current theory is too imprecise to allow one to make firm a priori assumptions about the potential lag structure of the relationship between crime and crime control variables (including arrests). Analytical strategies which permit the data to play the greatest role in determining the lag structure between crime and arrest are preferable. For this reason, we contend that autoregressive integrated moving average (ARIMA) time-series models may be well suited for the purpose of estimating the crime–arrest relationship.

ARIMA time-series models have a number of advantages over panel models, most of which accrue from the quantity of data required to estimate the ARIMA model. Specifically, multivariate ARIMA models can more precisely identify the lag structure between variables, account for seasonal variation in a systematic manner, and estimate model parameters with a higher degree of reliability (McCleary *et al.*, 1980, pp. 270–272). This is not to say that ARIMA models are inherently "superior" to panel models. Indeed, ARIMA models have a number of limitations. First, ARIMA techniques do not allow one to estimate the instantaneous relationship between two series (Jenkins, 1979). However, insofar as our present concern is with the lagged relationship between crimes and arrests, this deficiency is not fatal. Second, ARIMA tests for causation between two series have been criticized as being too conservative. However, the debate about this issue is far from closed (Pierce, 1977; Granger and Newbold, 1986). Third, multivariate ARIMA models can require the utilization of rather long time series to produce reliable parameter estimates (McCleary *et al.*, 1980), whereas panel models more readily accommodate many independent variables. In short, while ARIMA techniques are not above criticism, they are well suited to the task of estimating the lagged relationship between variables, especially when theoretical guidance as to the expected lag structure is minimal. Moreover, given the substantive interest in the crime–arrest relationship, it seems reasonable to find out whether ARIMA modeling techniques, which allow the reseacher to examine one jurisdiction over many points in time, yield conclusions which are comparable to those produced by examining many jurisdictions over a few points in time (i.e., panel modeling).

In sum, the implications of previous attempts to identify the crime–arrest relationship are clear. Given the present advancement of sociological and economic theory, it is necessary to locate and analyze data which permit one to employ methods that require minimal a priori assumptions about the time lag between crime and arrests. The research effort attempts to do just that.

4. DATA

The present study examines the causal relationship between the number of crimes and the number of arrests occurring within Oklahoma City and Tulsa, Oklahoma, for the following offense categories: robbery, burglary, grand larceny, and auto theft. The data were ascertained from the Uniform Crime Reports: National Time Series Community-Level Database, 1967–1980 (ICPSR 8214). These data include monthly counts of the number of crimes and arrests reported to the FBI from January 1967 to November 1980. Each time series consists of 167 observations. Hence, these realizations are long enough to allow one to obtain reliable parameter estimates of the lag structure between series with ARIMA models (McCleary *et al.*, 1980).

We choose to focus on these data for a number of reasons. First, since extant theory yields rather vague and contradictory predictions concerning the lagged effects that we may expect to find, it seems prudent to examine repeated observations which are aggregated over a relatively short period of time (i.e., months). The inability of previous studies (Greenberg *et al.*, 1979; Greenberg and Kessler, 1982) to find a reciprocal relationship between crime and arrests may reflect, in part, the decision to analyze yearly data. Second, deterrence theory assumes that potential criminals weigh the costs and benefits before engaging in illegal activities. If, as has been suggested, arrests serve to highlight the "costs" of committing crimes and thereby reduce future law violations, then they should have the greatest effect on crimes in which offenders can make accurate, a priori appraisals of the potential costs and benefits of their behavior. Robbery, burglary, grand larceny, and auto theft have long been recognized as the most "profit-maximizing" of the FBI index crimes (Gibbs, 1975). Third, since one of our concerns is with the implications of the lag structure of the crime–arrest relationship for cross-sectional research, it is important to discover if observed time lags differ substantially across jurisdictions. To the extent that they do, cross-sectional analyses become extremely problematic. Given the exploratory nature of this study, we examine only two jurisdictions.

5. PROCEDURES

Bivariate ARIMA model building is an iterative procedure. First, univariate models are constructed for both series. Second, the univariate models are inverted and applied to each series, respectively (prewhitening). Third, the cross-correlation function (CCF), a measure of correlation between two time series, is inspected and a tentative model is identified and estimated. Fourth, the initial model is subjected to a number of diagnostic checks, and if it is found to be inadequate a new model must be estimated. This procedure continues until a statistically adequate model is constructed. The computer program JENASYS (Jenkins, 1982) is used to estimate the univariate and bivariate ARIMA models.

6. RESULTS

Before we begin our discussion of the univariate ARIMA models, it may prove useful to provide a brief description of some selected characteristics of Oklahoma City and Tulsa, Oklahoma, for the period of time under consideration. Table I presents the population size, the percentage of blacks, the percentage of families below the poverty level, and the police-force size for Oklahoma City and Tulsa, for the years 1970 and 1980, respectively. For purposes of comparison, the mean values for these variables for the

Table I. Selected City Characteristics for Oklahoma City, Tulsa, and the Sorenson *et al.* (1975) Sample of 109 U.S. Cities[a]

	1970	1980	Net change
Oklahoma City			
Population size in 1000s	367	403	+36
Percentage black	13.7	14.6	+0.09
Percentage of families below the poverty level	10.6	9.3	−1.3
Police-force size per 1000 population	1.7	2.3	+0.6
Tulsa			
Population size in 1000s	332	361	+29
Percentage black	10.6	11.8	+1.2
Percentage of families below the poverty level	9.0	7.4	−1.6
Police-force size per 1000 population	1.6	2.2	+0.6
Mean values for the Sorenson *et al.* sample			
Population size in 1000s	430	406	−24
Percentage black	25.1	30.5	+5.4
Percentage of families below the poverty level	12.3	14.2	+1.9
Police size per 1000 population	2.6	3.0	+0.4

[a] Population size, percentage black, and percentage of families below the poverty level were ascertained from the *County and City Data Book* (1972, 1983). Police-force size was ascertained from the *Uniform Crime Reports* (1971, 1981).

Sorenson *et al.* sample of 109 U.S. cities [which has been used in a number of published works (e.g., Liska and Chamlin, 1984)] are also included for the years 1970 and 1980, respectively. Both Oklahoma City and Tulsa evidence similar patterns of change during the period from 1970 to 1980. Each city experienced increases in the population size, percentage of blacks, and police-force size and decreases in the percentage of families below the poverty level. Further inspection of Table I indicates that the pattern of change for Oklahoma City and Tulsa differs from that for the 109-city sample with respect to the population size and the percentage of families below the poverty level. These divergencies may be explained in part by the economic growth experienced in Oklahoma as a result of the rapid growth in crude oil prices which began in the mid-1970s.

Almost invariably, two raw time-series realizations will be correlated due to common sources of trend, drift, and autocorrelation (Granger and Newbold, 1986). Hence, prior to the estimation of bivariate models it is necessary to prewhiten each of the original raw series. Prewhitening entails (1) identifying and estimating an appropriate ARIMA model for each series and (2) inverting and applying the final ARIMA model for each series to that same series. If the models are satisfactory, the residuals of each series should be uncorrelated (i.e., "white noise").

Table II presents the final univariate ARIMA models for each of the series. This table contains information concerning the form, as well as the statistical adequacy, of the final univariate ARIMA models. The general form of the ARIMA model is $(p, d, q)(P, D, Q)$, where p = the order of the autoregressive process, d = the degree of nonseasonal differencing, q = the order of the moving average process, P = the order of the seasonal

Table II. Univariate ARIMA Models of Crime and Arrests for Oklahoma City and Tulsa

	Crime		Arrests	
Offense	Model	Q statistic	Model	Q statistic
Robbery	$\ln(2,1,0)(2,1,0)_{12}$	$Q = 23.2$, df = 21	$\ln(0,1,1)(0,1,1)_{12}$	$Q = 24.5$, df = 23
Burglary	$(1,1,0)(1,0,0)_{12}$	$Q = 19.3$, df = 23	$(3,0,0)$	$Q = 10.7$, df = 22
Larceny	$\ln(0,1,1)(0,1,1)_{12}$	$Q = 14.0$, df = 23	$\ln(0,1,1)$	$Q = 11.9$, df = 24
Auto theft	$(0,1,2)(0,0,1)_{12}$	$Q = 16.9$, df = 22	$(0,0,0)$	$Q = 29.7$, df = 24
		Tulsa		
Robbery	$\ln(2,1,0)$	$Q = 25.1$, df = 23	$\ln(0,0,1)$	$Q = 23.9$, df = 24
Burglary	$(2,1,0)(2,0,0)_{12}$	$Q = 24.4$, df = 21	$(0,0,0)(2,0,0)_{12}$	$Q = 15.9$, df = 23
Larceny	$(2,1,0)(2,1,0)_{12}$	$Q = 12.8$, df = 21	$(0,0,1)(0,0,2)_{12}$	$Q = 25.2$, df = 22
Auto theft	$\ln(0,1,1)(0,0,2)_{12}$	$Q = 23.7$, df = 22	$(0,1,1)$	$Q = 21.8$, df = 24

autoregressive process, D = the degree of seasonal differencing, and Q = the order of the seasonal moving average process. One of the necessary conditions of an ARIMA model is that it be stationary in its variance. Inspection of a plot of the raw time series reveals whether or not the series is stationary in its variance. Fortunately, a series which is not stationary in its variance may be made so by performing a natural logarithm transformation of the series. As indicated in Table II, several of the series have been transformed to their natural logarithms to make them stationary in their variances. As noted above, the residuals of statistically adequate models are distributed as white noise. The Q statistic, also reported in Table II, tests whether the model residuals as a whole are different from what would be expected of a white-noise process. All of the models reported in Table II meet this diagnostic criterion (i.e., all the autocorrelation functions are insignificant at the 0.05 level).

In brief, Table II reveals that the univariate models vary dramatically in complexity. For example, the model for Oklahoma City robbery offenses requires a natural logarithm transformation of the raw series, as well as nonseasonal and seasonal differencing. It also indicates that both nonseasonal and seasonal first- and second-order autoregressive processes are present. In contrast, the model for Tulsa auto theft arrests indicates that the residuals from the raw series do not differ significantly from white noise.

Table III. Bivariate ARIMA Analyses of the Crime–Arrest Relationship for Oklahoma City and Tulsa

Offense	C → A	Q_{12}	A → C	Q_{21}
		Oklahoma City		
Robbery	No relationship[a]	$Q = 15.7$, df = 25	$\theta = -0.453$* (SE = 0.105)	$Q = 29.3$, df = 25
Burglary	No relationship	$Q = 12.7$, df = 25	No relationship	$Q = 17.9$, df = 25
Larceny	No relationship	$Q = 27.8$, df = 25	No relationship	$Q = 21.0$, df = 25
Auto theft	No relationship	$Q = 24.3$, df = 25	No relationship	$Q = 16.7$, df = 25
		Tulsa		
Robbery	No relationship	$Q = 26.2$, df = 25	$\theta = -0.575$* (SE = 0.096)	$Q = 22.3$, df = 25
Burglary	No relationship	$Q = 19.9$, df = 25	No relationship	$Q = 28.8$, df = 25
Larceny	No relationship	$Q = 22.9$, df = 25	No relationship	$Q = 27.4$, df = 25
Auto theft	No relationship	$Q = 28.2$, df = 25	No relationship	$Q = 30.9$, df = 25

[a] No relationship means that there are no significant autoregressive or moving average processes at the 0.05 level.

*Statistically significant at the 0.05 level.

More importantly, Table II clearly reveals that this is considerable within-series variation, suggesting that failure to prewhiten the series would seriously confound the bivariate analyses.

Table III presents the results of the bivariate analyses of crimes and arrests for Oklahoma City and Tulsa. For each city, column 1 reports the offense category. Columns 2 and 4 report the effects of crimes on arrests, and arrests on crime, respectively. Columns 3 and 5 report the Q statistics (with appropriate degrees of freedom) for the model residuals for the effects of crimes on arrests and of arrests on crimes, respectively. The Q statistics indicate that the overall pattern of the residuals for each of the models does not differ significantly (at the 0.05 level) from white noise. Hence, it is fair to conclude that the models are statistically adequate.

The results are clear. The bivariate ARIMA analyses reveal no statistically significant lagged effects of crimes on arrests. For the crime categories of burglary, larceny, and auto theft, the lagged effects of arrests on crimes are identical to those reported for crimes on arrests. Only for robbery do we find a lagged relationship between arrests and crime. Oklahoma City ($\theta = -0.453$, $P < 0.05$) and Tulsa ($\theta = -0.575$, $P < 0.05$) robbery arrests each have a negative effect (1-month lag) on robbery offenses. In short, our findings, not unlike those reported by Greenberg and his associates (1979; Greenberg and Kessler, 1982), provide little evidence to support the contention that marginal changes in quantity of crimes and arrests are related to one another.

7. DISCUSSION

Employing ARIMA modeling techniques, this paper examines the lagged reciprocal relationship between crime and arrests for two Oklahoma cities. Consistent with the deterrence and incapacitation theses, we find that robbery arrests have a negative effect on robbery offenses for both Oklahoma City and Tulsa. Yet contrary to expectations, we find no other significant relationships between crimes and arrests. Given the overall pattern of the results, it seems reasonable to question, as do Greenberg *et al.* (1979), the efficacy of cross-sectional analyses of the crime-arrest relationship.

Indubitably, there are numerous post hoc explanations that one may invoke to "explain away" the negative findings reported in this paper. For example, Greenberg and his associates (1979; Greenberg and Kessler, 1982) argue that arrest is not a pure sanction and, therefore, may have a limited impact on a potential offender's calculations of the costs of engaging in illegal behavior. They also contend that current levels of crime may be unaffected by marginal changes in the arrests because potential offenders may be ignorant of the changes in the probabilities of being apprehended for engaging in illegal behavior. If potential offenders are unaware of the increased risks associated with criminal activities, they are not likely to be deterred.

A possible explanation for the insensitivity of arrests to marginal changes in the amount of crime may have to do with the role of victims. Most crimes come to the attention of police because of citizens' complaints (Wilson, 1967; Reiss, 1971). Similarly, most arrests depend upon the assistance (i.e., instigation) of citizens (Reiss, 1971). In short, citizen behavior may have a large impact on the actual relationship between crimes and arrests. To the extent that members of less powerful groups and strata are disproportionately victimized, there may be less pressure on police to make arrests, regardless of marginal changes in the quantity of crime (Turk, 1969; Black, 1976).

Part of the impetus for researchers to provide explanations for the inability to find a crime-arrest relationship using longitudinal data techniques rests with the fact that several cross-sectional studies (e.g., Tittle and Rowe, 1974; Geerken and Gove, 1977) report strong negative correlations between measures of crime and arrest. However, the appeal to other factors to explain contradictory findings that arise from the utilization of alternative data analysis techniques raises an interesting question. Specifically, why are cross-sectional analyses seemingly unaffected by social processes which apparently call into question the validity of longitudinal studies (both panel and ARIMA designs)? It seems reasonable to assume that if a lack of knowledge about marginal changes in the chances of being arrested is affecting panel analyses of the crime-arrest relationship (Greenberg *et al.*, 1979; Greenberg and Kessler, 1982), it should also affect cross-sectional examinations of the arrest-crime relationship.

Cross-sectional studies of the reciprocal relationship between crimes and arrests have repeatedly been criticized on methodological grounds (e.g., Nagin, 1978; Greenberg and Kessler, 1982). To be sure, longitudinal techniques also have their limitations. ARIMA analyses examine the relationship among variables within a single jurisdiction over time. The present study examines the crime-arrest relationship in two jurisdictions. Hence, our ability to generalize to other U.S. cities is circumscribed. The above notwithstanding, these analyses add to a growing body of research which raises serious doubts about the efficacy of cross-sectional studies of the crime-arrest relationship. Minimally, our results indicate that all economic crimes are not affected by the behavior of police. Rather it would seem, as Wilson and Boland (1978) suggest, that robbery may respond more readily to increased police effectiveness than other economic index crimes. Maximally, our results suggest that potential criminals are not as "rational" as deterrence theorists would have us believe. Clearly, more longitudinal research, both panel and ARIMA modeling, is needed to clarify this debate.

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