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THE EFFECT OF LONGITUDINAL ARREST PATTERNS ON THE DEVELOPMMENT OF ROBBERY TRENDS AT THE NEIGHBORHOOD LEVEL

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ABSTRACT

Although the current resurgence of ecological deterrence research has addressed many of the methodological and theoretical problems of earlier studies, the question concerning the appropriate level of aggregation for such models has not been resolved. In this paper we argue that there is strong evidence in the criminological literature suggesting that the neighborhood might be the most meaningful level of aggregation for such studies. However, an analysis of robbery incidents and arrests over a 100 week period in five Oklahoma City neighborhoods fails to find any significant support for the deterrent effect of arrests on subsequent illegal behavior. We propose that the lack of such a relationship reflects periods of short-term equilibrium in the local community during which the levels of crime and arrests have relatively stabilized.

ESTIMATING THE DETERRENCE MODEL AT THE NEIGHBORHOOD LEVEL

INTRODUCTION

During the late 1960's and early 1970's, several important ecological studies concerning the relationship between legal sanctions and crime rates were published (see, for example, Ehrlich, 1973; Gibbs, 1968; Tittle, 1969; Tittle and Rowe, 1974). These studies usually were considered to provide evidence of the deterrent effect of legal sanctions since they rather consistently found an inverse relationship between the arrest rate and the crime rate at the state level. By the late 1970's, however, these findings were carefully being reconsidered in light of some serious methodological and logical shortcomings (see Nagin, 1978).

A new wave of ecological deterrence research appeared during the 1980's to address some of the problems of these earlier studies. However, while this more recent body of research has made significant improvements over previous formulations of the deterrence model, we feel that it has not resolved one of the most important criticisms made of the earlier work involving the issue of aggregation bias. In the following sections of this paper, we argue that the most appropriate level of analysis for ecological models of deterrence is the local neighborhood, rather than the city, county, SMSA, state or nation. The validity of this position is examined on the basis of calls for service received by the Oklahoma City Police Department between July, 1986 and June, 1988.

SELECTING AN APPROPRIATE LEVEL OF AGGREGATION

Although the degree to which official records of criminal behavior may validly be compared across police jurisdictions is not entirely clear (see the conflicting viewpoints of Seidman and Couzens, 1974, or McCleary et al., 1982, and Gove et al., 1985), there does appear to be a general agreement that the cross-jurisdictional comparability of clearance rates is questionable due to departmental differences in the manner by which an offense may be considered to be cleared (see the discussion of Nagin, 1978: 47; Wilson and Boland, 1978: 369).[1] This is an especially important consideration given the cross-sectional nature of much ecological deterrence research, for clearance rates often are used as the primary indicator of the threat of punishment.

Some researchers have attempted to "control" for such systematic sources of error by utilizing time series data to estimate the relationship between crime rates and the risk of punishment within a single city; such study designs can be found in Loftin and McDowall (1982; 1926-1977 Detroit data), Decker and Kohfeld (1985; 1948-1978 St. Louis data), and Bynum and Cordner (1980; 1952-1978 data from an unspecified city. This study is described in Decker and Kohfeld). However, the decision to utilize the city as the unit of analysis is not a trivial one, for as Bailey clearly has shown (1985), the empirical patterns which are observed may not be consistent at different levels of analysis. Strikingly contradictory results were produced in Bailey's analysis of a single dataset pertaining to economic inequality and crime depending on the level of aggregation imposed on those data (city, SMSA and state), including changes in both the magnitudes <u>and</u> the signs of the regression coefficients. Thus, he concludes that the selection of the appropriate level of aggregation is a central theoretical issue in aggregate studies of crime, rather than a decision that is made solely on the basis of data availability.

Greenberg and Kessler (1982; see also Greenberg et al., 1981) persuasively have argued that aggregates as large as counties, states and nations are unacceptable for the estimation of the effect of the threat of punishment on crime rates at the ecological level due to their internal heterogeneity. As they note, potentially criminal actors, <u>if</u> they are influenced at all, respond to the threat of punishment within much smaller ecological units. However, although they utilize city-level data in their related research, they never claim that cities are the appropriate ecological unit for deterrence research but only that they are more appropriate than states.

There are several theoretical considerations that suggest that the use of a city as the unit of analysis may still be inappropriate. As Lavrakas et al. (1983: 463) have pointed out, most citizens within a particular city do not have access to the official statistics concerning the actual extent of crime and the related probabilities of arrest within that jurisdiction. Therefore, it has often been assumed that they depend on the reports of the mass media in the development of their subjective perceptions of risk. If this was in fact the case, the city might represent an appropriate unit of analysis; since all citizens could be assumed to be exposed to the same sets of information concerning the risk of arrest, heterogeneity within the city concerning the perceived threat of punishment would have fairly minor and unsystematic effects on the relationship between perceived risk and criminal activity.

However, Tyler and Cook (1984: 693) have shown that mass media presentations concerning crime and violence are generally unrelated to individual perceptions of the risk of victimization. Tyler (1984: 34) offers two explanations for the lack of such a relationship. First, most citizens do not find such crime-related news particularly informative because the less spectacular crimes (such as burglary and purse-snatching) for which citizens are at the greatest risk are significantly under-reported relative to their rate of incidence. Second, news presentations concerning serious crime tend to concentrate on activities within high crime areas, rather than providing a representative depiction of the distribution of crime within the entire geographic area. As a result, Tyler and Cook (1984: 694) argue that people develop their perceptions of the risk of victimization on the basis of their own experiences or, more importantly, those they learn about indirectly through their friends, co-workers or neighbors. Thus, victimizations that occur outside of one's extended network of relationships are unlikely to be given serious consideration when a person evaluates his or her own risk of victimization. Since such networks of association and information often develop within fairly localized sections of a city, these findings suggest that the city may be a higher level of aggregation than is appropriate.

Although the Tyler series of studies focus on the risk of victimization, similar processes also have been shown to exist in the development of perceived risks of punishment. Parker and Grasmick (1979) present evidence that subjective estimates of the certainty of arrest for burglary depend strongly on the experiences of oneself and one's friends. Although the respondents in their study overestimated the likelihood of arrest for burglary, their estimates were much more accurate than those which would have been obtained solely on the basis of mass media reports.[3] Such findings again suggest that a smaller unit of analysis than the city which is more likely to capture the distribution and variation in these networks is called for.

Given this evidence, we propose that the most appropriate level of aggregation for the ecological study of deterrence is the local neighborhood. Several considerations led us to this conclusion. First, it is well known that crime is not randomly distributed within a city (see Bursik, 1988). If the potential offenders in a given city are taking into account the arrest rates in the various neighborhoods within the city, then all else being equal, we would expect from classical deterrence theory that neighborhoods with high arrest rates relative to other neighborhoods in the city would have low crime rates; conversely, neighborhoods with low arrest rates would have high crime rates. Studies of criminal decision-making have shown that potential offenders often choose the area in which to commit a crime on the basis of the differential patterns of law enforcement in a city (see Carter, 1974; Carter and Hill, 1978; Rengert and Wasilchick, 1985). In addition, it has been noted that when police activities are increased in one area of a city, there is tendency for crime rates to increase in adjacent neighborhoods where the risks of apprehension are not so great (see McIver, 1981). These relationships would tend to counter-balance one another when such neighborhood patterns are aggregated to the city level, erroneously leading a researcher to reject the deterrence hypothesis.

Second, it is very dangerous to consider the risks of punishment to be relatively constant within large, differentiated urban areas. Traditional models of deterrence have been characterized by the implicit assumption that cities are under the jurisdiction of a single police department. Although technically this is true, most large urban areas are divided into precincts or districts, each with its own bureaucratic structure imbedded within the larger police hierarchy. The degree of variation in the risk of punishment among these subunits is an empirical question. Studies that examine the clearance or arrest rates of entire cities effectively assume that this variation does not exist. Yet such an assumption is inconsistent with the recent work of Smith (1986) which indicates that police behavior is related significantly to the neighborhood context in which a crime is reported.

Finally, our approach assumes that the primary sources of risk-related information are friendship networks within the local neighborhood. Of course, all persons have a variety of networks from which to choose their friends and many of these are based outside the boundaries of the area of residence (such as the places of work, worship or recreation. See the discussion of Huckfeldt, 1983: 653). Therefore, there will be variation in the degree to which perceptions of risk represent a mixture of internal and external sources of information. However, there is strong evidence that friendships <u>in general</u> have a marked geographic basis. Huckfeldt (1983: 659-660), for example, notes that between 36 and 41 percent of his respondents in Detroit had a majority of their friends living within the same neighborhood; less than one third reported that they had no friends within ten minutes of their homes. Similar friendship patterns have been reported in Seattle (Guest and Oropesa, 1986) and in Great Britain (Sampson, 1988).

Variations in the distribution of local friendships are not random; Sampson (1988), for example, clearly shows that the number of friendships is strongly related to the residential stability of the neighborhood. This has important implications for the development of deterrence models at the neighborhood level. The social disorganization perspective argues that local rates of crime and delinquency are in part a function of the degree of interdependence and cohesion in an area, the density of local community networks and the range and content of communication resulting from these relationships. High rates of instability are assumed to have a detrimental effect on such networks because the development of primary relationships is less likely when the residential population is in a continual state of flux (see Bursik, 1988). In such a situation, the diffusion of information pertaining to the risk of punishment within the local community would tend to be fragmented and difficult. Therefore, we would expect that the deterrent effect of arrests on criminal behavior will be conditional on the extent of community organization.

SPECIFYING AN APPROPRIATE TIME LAG AT THE NEIGHBORHOOD LEVEL

Most longitudinal approaches to deterrence theory assume that arrest rates have both a long term (or "lagged") and short term (or "instantaneous") effect on the rates of crime (Freidman et al., 1989). Although some researchers have assumed that the deterrent effect of an arrest on crime is primarily instantaneous (Kessler and Greenberg, 1981: 34), the primary rationale for positing <u>any</u> lag is the possibility that some time is required for potential offenders to recognize any change which has occured in the threat of punishment. For example, the neighborhood network perspective that was proposed in the preceding section would assume that some unknown amount of time is necessary for crime/arrest-related information to filter its way through an information network so that it could have a deterrent effect.

Just as the appropriate level of aggregation should be determined by the nature of the dynamics assumed to underlie the processes of deterrence at the ecological level, the determination of the appropriate time lag between arrest rates and crime rates used to capture such long term effects should also be theoretically justified. Unfortunately, as Loftin and McDowall have observed (1982: 394), deterrence theory as it generally is formulated provides very little guidance in the selection of this lag. Therefore, many different assumptions have been used to justify the incorporation of a wide variety of lag structures into longitudinal models of deterrence.

Given the nature of the time series datasets that have been used to estimate ecological deterrence models, the minimum lag that can typically be examined is one year. However, there is some evidence from field studies that the lag is much shorter than this period. In his research on traffic law violations, Ross (1973; Ross, et al., 1981-2) concludes that well-publicized "crackdowns" have an almost immediate deterrent effect; however, these effects tend to dissipate over time, often after less than a year. "Crackdowns", of course, are a common form of change in the threat of punishment in an ecological unit; their deterrent effects, if any, are not likely to be manifest in analyses of variables lagged over periods as long as a year, let alone two or three years.

There is no direct evidence concerning the lag between changes in the threat of punishment in a neighborhood and subsequent changes in the local crime rate. In addition, just as we have argued that deterrent effects may be contingent on certain neighborhood characteristics, there is also reason to expect the length of the lag to be similarly conditional. Therefore, it is essential that the time series data have a structure that maximizes one's flexibility in the estimation of this lag. Unfortunately, most widely available datasets have been collected on a yearly basis, thereby inhibiting such estimation procedures. However, an alternative set of data is available in many jurisdictions that reports crime-related events on a continuous-time basis. Although such data have been rarely analyzed, their potential ability to overcome many of the methodological problems concerning the appropriate lag structure is enormous.

THE USE OF CALLS-FOR-SERVICE INFORMATION IN STUDIES OF DETERRENCE

The three most generally available sources of data typically used in deterrence research--official crime reports (typically the Uniform Crime Reports), victimization data, and self-reports--are each characterized by particular weaknesses that have been widely discussed in the literature (see the reviews of O'Brien, 1985; Gove et al., 1985). Recently, however, several studies have examined the viability of using records of citizen calls for service from local police agencies as an indicator of the spatial and temporal distribution of criminal activity, as well as an indicator of the incidence of such behavior (see Taylor et al., 1981; Pierce et al., 1984; Sherman et al., 1989). As Sherman et al. have noted (1989: 34), computers are increasingly being used to document the activities of centralized police dispatching systems and responses to 911 emergency calls. Although these data systems have been designed primarily for administerial purposes, they also represent the "widest ongoing data collection net for criminal events" in most cities, reflecting many criminal events that would not be reflected in official crime reports, victimization studies or self-report surveys (Sherman et al., p. 35).

One of the greatest strengths of such datasets is that many of the "gatekeeping" processes within police departments that are assumed to affect the validity of officially-recorded crime data are bypassed, since these systems maintain a continuous record of <u>all</u> transactions. Likewise, problems of respondent recall that characterize most victimization and self-report study designs are also eliminated. Finally, Taylor et al.'s (1981) comparison of the geographic distribution of such calls with the distributions of other indicators of reported crime suggests that calls-for-service data have a high degree of reliability.

However, such data also have some serious limitations that must be noted (for a good review of these limitations, see Sherman et al., 1989). The most important drawback is that a citizen must report an incident for it to be entered into the database. For example, recent estimates indicate that while 73 percent of motor vehicle thefts come to the attention of law enforcement agencies, the proportion is much lower for other property offenses (Bureau of Justice Statistics, 1987: 4). Therefore, like other officially-based indicators of criminal activity, calls-for-service data represent an under-reporting of the actual level of such behavior.[4] However, unlike other types of official data, this under-reporting exclusively represents the likelihood that citizens will contact the police department, rather than the outcomes of decision-making practices of the department or patrol officers.[5]

In addition, as Sherman et al. point out (1989: 34-35), such data can also be subject to over-reporting. Such a situation is most likely to occur when citizens call in false reports, or when more than one call-for-service is made concerning a single incident. Some police departments, such as the one from which the data used in our study have been collected, have made efforts to minimize the effects of such over-reporting through a two stage process. First, the police who have been dispatched to the scene of the alleged incident reports whether the incident to which he or she is responding appears to have actually occured (i.e., is it "founded" or "unfounded"); this information is included in the general data file. Second, the calls-for service data are then restructured on the basis of discrete events, i.e., all calls concerning the same criminal incident are combined into a single incident report. Therefore, the levels of crime reflected in some calls-for-service systems are not confounded by the tendency of citizens to provide multiple calls for a single event.

While we feel that the calls-for-service systems provide a very rich set of data for the testing of the deterrence model, operationalizing the threat of punishment within their structures is somewhat problematic. Theoretically, it might be possible to trace the history of each of the calls to determine whether the incident was ever cleared through an arrest. Unfortunately, this is infeasible within many systems (including the one from which we collected our data) due to an inconsistency of the identification numbers that are given to each case at different stages in the investigation.

However, some systems do provide information concerning on-site arrests that are made when the police respond to the call, i.e., arrests that occur before the end of the field investigation. Most criminal activity has a very low probability of being cleared in such a manner. The Kansas City response time study, for example, reported that only 12 percent of the calls-for-service were cleared in on-site; the risk of arrest depended not only on the type of offense, but also on whether the citizen reporting the incident discovered the crime (or was involved in the event) while it was taking place, or whether it was detected after the crime had occured. Interestingly, on-site arrests that could be attributed to a rapid response by the police occured in only 2.9 percent of the cases (National Institute of Law Enforcement and Criminal Justice, 1978: 31). Similar arrest patterns have been observed in Jacksonville, San Diego, Peoria and Rochester (National Institute of Justice, 1983).

At first, the on-site arrest rate may seem to be a very limited indicator of the threat of punishment. However, such a formulation has some important benefits. First, it automatically imposes controls on the model for the celerity of punishment. Models that are based on temporally unrestricted arrest or clearance rates assume that the effects of such actions on the perception of risk in an area do not decay with elapsed time from the actual criminal incident. Second, the denominators and numerators used in the computation of arrest and clearance rates based on the Uniform Crime Reports are not temporally comparable. Such rates are computed relative to the number of crimes that have been reported in a specified period of time. However, arrests and clearances that are used in the calculation pertain not only to crimes reported during the period of the denominator, but also to crimes which were reported in previous periods but not cleared until the current one (Federal Bureau of Investigation, 1984: 48). Therefore, the numerators of the clearance and arrest rates refer to events that may have occured <u>any time</u> within the relevant statute of limitations of each offense and not solely within the time frame of the denominator.

Such an formulation introduces a significant amount of measurement error into the more traditional indicators of the risk of punishment, for it does not directly reflect the probability that a criminal event that occured within a period of time resulted in an arrest or clearance during that same period.

Rather, it confounds that probability with the probability that a criminal event from a preceding period was not cleared during the corresponding period of time. This makes the reliability of these measures of the risk of punishment in a given period very questionable; in addition, the implications of a lagged or instantaneous effect of that risk on crime in such a situation are not at all clear. Therefore, while the on-site arrest rate provides only a measure of the "instantaneous" risk of punishment, we feel that it is a much more reliable indicator than those available from the Uniform Crime Reports.

THE DATA

The neighborhood-level data were collected over a one hundred week period beginning June 1, 1986 from the Oklahoma City Police Department computerized Calls-For-Service system. Since these records have been re-organized by the department to reflect discrete incidents rather than the raw calls for service, the dataset has been corrected for multiple calls that may be received for a criminal event. In addition, all reports that have been determined to be unfounded have been deleted. During this period, the police department received calls pertaining to 584,440 incidents; for each incident, the nature of the call, the month, day, year and time of the call, and the address to which the police were dispatched were recorded. As can be seen in Table 1, these data are similar to those analyzed by Gilsinan (1989) in that many of the requests for service pertained to non-criminal matters.

(Table 1 about here)

In this paper, we are restricting our analysis to robberies that were reported during this period. As a baseline against which to evaluate neighborhood variation in the deterrence process, we first recoded the data into weekly periods and examined the city-wide trends in reported robberies and on-site clearances of those robberies through an ARIMA model. A (0,1,1) process provided a good fit to the number of reported crimes, i.e., first differences made the trend stationary and random shocks tended to persist for a one week period. Surprisingly, the on-site clearance time series was fit exceptionally well by a (0,0,0) process, indicating that the temporal distribution of these clearances is essentially random. In addition, we examined the relationship between the development of these two series through the cross-correlation function. Just as Loftin and McDowall (1982) found in Detroit using yearly data, there is no evidence of relationship between the number of reported robberies and the number of on-site clearances. Thus, at the city level, the deterrence model receives no support.

To examine the degree to which the city-level model may mask neighborhood-specific variation in the deterrence process, we selected five neighborhoods of Oklahoma City with sufficient numbers of reported robberies to facilitate such an analysis. Each of these neighborhoods is defined in terms of a particular police "beat", so that variation in the risk of punishment associated with different jurisdictions of the Oklahoma City Police Department can be controlled. As Table 2 indicates, this is an important consideration since the on-site clearance rate is more than twice as high in Neighborhood IV than in

(Table 2 about here)

the other four areas. A comparison of this rate with the aggregated rate for the other neighborhoods indicates that the difference is significant at less than the .05 level. This provides strong evidence that studies using entire cities as the unit of analysis may confound significant variation in the risk of punishment that exists at the smaller, neighborhood level.

ARIMA models were estimated within each of the neighborhoods. In all five areas, the robbery calls and the on-site clearance series were both characterized by random, white noise development over time; likewise, the cross-correlation function did not produce any evidence of a relationship between the series in any neighborhood. Thus, the deterrence process is not initially supported in any of the five communities. However, there is an important technical issue in the estimation of ARIMA models at the neighborhood level that makes such findings problematic. As the length of the time period becomes progressively smaller, the expected value of a robbery call and on-site clearance in a particular time period in a given neighborhood rapidly approaches zero; in the limiting situation, time series models would always depict the processes as random walks. Therefore, the ARIMA parameters only have theoretical relevance if one can assume that a time period of appropriate length has been used in the analysis. As noted earlier in this paper, deterrence theory provides no guidance concerning this issue.

However, when using calls-for-service data, researchers are not forced to arbitrarily divide the time series data into a set of discrete temporal categories. Rather, since the date of the call is available for analysis, it is possible to compute the elapsed time between each reported robbery (see Figure 1). Such an approach enables one to utilize event history models to examine the effect that an on-site arrest has on the hazard of a future reported robbery. A significant negative effect would indicate that an arrest decreased the rate at which such reports occur (see the discussion of Allison, 1984: 23).

Due to its great flexibility concerning the assumed distribution of the

(Figure 1 about here)

hazard function, we utilized the proportional hazards model developed by Cox (1972). The interval used to compute the hazard function reflects the time period (in days) between robbery report i and i-1. A dummy variable (ON-SITE) was created that equals 1 if the i-1st report resulted in an on-site arrest. Therefore, the model is used to determine whether an arrest has a significant effect on the elapsed time to the next report. However, it is also possible that the effect of such on-site arrests is cumulative, i.e., the decision to engage in a robbery reflects a consideration of the overall pattern of arrests. Therefore, we also created a variable (TOTCLEAR) that represents all on-site arrests that occured prior to the i-1st report.

As shown in Table 3, the event history approach to the time series data still fails to provide support for the deterrence hypothesis. In each of the neighborhoods under consideration, the instantaneous and the cumulative on-site arrest measures were generally unrelated to the hazard of a robbery report.

(Table 3 about here)

There is some indication, however, that the effect of these measures are not consistent in all of the neighborhoods. Although none of the effects are significantly different from zero, the difference between the ON-SITE beta weights in two of the communities is significant, i.e., an arrest reduces the rate of robbery reports significantly more in Neighborhood II than in Neighborhood V.

In addition, the proportional hazard model assumes that the ratio of the hazards for any two individuals remains constant over time, regardless of the length of the interval (Allison, 1984: 34). Thus, the ratio associated with the rate of reports following on-site arrests and the rate of reports following the absence of such arrests should be constant regardless of the length of the interval between events. SAS PHGLM provides a test of this assumption, which can be interpreted as a normal deviate (SAS Institute, 1986: 443); this is represented in Table 3 as Z:PH.

As noted in the description of the SAS PHGLM procedure, a positive Z score indicates that "the ratio of hazards for high values of the covariate to low values of the covariate is increasing over time"; when it is decreasing over time, the statistic is negative (SAS Institute, 1986: 443). For example, a positive value associated with ON-SITE would imply that the likelihood of a robbery report proportionately increases more rapidly with the length of the interval when the preceding event was characterized by an arrest; such is the case in Neighborhood I. Likewise, this is the case in Neighborhood II when the cumulative number of on-site arrests is relatively high; the opposite is the case in Neighborhood V.

Nevertheless, although there is some relatively weak evidence of significant neighborhood differences in the deterrence process, the general conclusion that must be reached is that regardless of the neighborhood context, on-site arrests do not have a deterrent effect on reported robberies.

DISCUSSION

The apparent inability of on-site arrests to deter subsequent levels of robbery at the neighborhood level has some important implications for the future development of deterrence models. Perhaps the most basic of these concerns the difficulties of modelling criminal activities that are extremely rare events in any given community. As shown in Table 1, robberies accounted for less than 1 percent of all calls for service to the Oklahoma City Police Department. It would be possible to aggregate different offense types into a single offense "rate" to make such analyses more feasible, but this makes the very strong assumption that the underlying processes of deterrence are the same for each of those offenses (see Tittle, 1969).

We resolved this problem by restricting our analyses to the five areas of Oklahoma City that had at least 100 robbery-related calls during the period under examination, i.e., an expected value of at least one robbery a week. The decision reflected an effort to ensure a sufficient degree of variation in the temporal distribution of robbery. However, this restriction resulted in a very limited base of neighborhood comparison; it is impossible to determine if the lack of a deterrence effect that characterizes these areas is the same pattern that would be found in communities with much lower levels of robbery. That is, we may have been trying to decompose primarily within-group rather than between-group variation. Therefore, our findings concerning the effect of on-site arrests can only be considered tentative.

Unfortunately, as noted earlier, models which are generally used for the analysis of ecological time series data are not appropriate to the examination of such rare events and preclude the inclusion of all neighborhoods into an analysis. Thus, the examination of deterrence processes at the neighborhood level will necessitate the development of new and innovative analytic techniques.

Perhaps, however, it simply is the case that on-site arrest rates do not deter robberies at the local level. Two possible explanations might be offered for this lack of a relationship. The most apparent one concerns the very low risk of on-site arrest that characterize these areas of Oklahoma City. Tittle (1980) has argued that perceptions of risk are only pertinent in decisions to engage in crime after they exceed certain thresholds. Such a consideration may also account for the lack of deterrent effects that have been observed in many aggregate models. That is, the risk of an on-site arrest may be too small for the potential criminal to consider pertinent to his or her decision to engage in a robbery.

Models can easily be specified in such a manner that the existence of such thresholds can be examined. Unfortunately, our second proposition is not nearly as easy to resolve in aggregate time series models. There has been a long debate in the literature concerning the relative merits of cross-sectional and longitudinal study designs for the analysis of the deterrence hypothesis (see, for example, Friedman et al., 1989). Yet, one limitation of longitudinal ecological approaches that rarely has been discussed is that the temporal boundaries of such studies represent only a small segment of the entire period of time in which rates of criminal behavior and aggregate risks of punishment mutually develop within a group. Especially in aggregates as small as the local neighborhood, it is possible that this relationship attains a state of equilibrium fairly rapidly; after the point at which arrest and crime rates adjust to each other, they may develop over time with relatively random fluctuation around this position of balance. Recall, for example, that in each of our five neighborhoods, the use of ARIMA models led to the conclusion that changes in both of these components over time were essentially random. Therefore, the deterrent processes would only be observable prior to the reaching of equilibrium, or if particular social dynamics gave rise to dramatic changes in the rates of crime or arrest during the period of observation (which was not the case in cur study).

The obvious alternative is to return to the traditional cross-sectional analyses that characterized the early deterrence literature, under the assumption that equilibrium becomes established at different levels of crime in response to different risks of punishment. With such an approach, we might have noted, for example, that the neighborhood with the highest risk of punishment (Neighborhood IV) also had the lowest number of reported robberies. However, the existence of equilibrium is a very strong assumption and in certain situations can lead to conclusions that are at best misleading and at worst, completely wrong (see Friedman et al., 1989). In addition, it is very difficult to reliably estimate the nonrecursive relationship between crime and the risk of punishment with cross-sectional data (Nagin, 1979).

Therefore, we propose that the resolution of the deterrence issue may be impossible with solely cross-sectional or longitudinal data. Rather, data similar in spirit to those utilized in pooled cross- sectional times series will be necessary to capture both between-aggregate variation that exists at a particular point in time, as well as the within- aggregate variation over time as the crime and punishment rates adjust themselves to one another. Until such data become generally available, we feel that the findings of deterrence research will be incomplete, contradictory, and unable to fully and finally resolve one of the longest-standing issues in the area of criminology.

FOOTNOTES

1. It is interesting that Gove et al.'s (1985) spirited and extensive defense of the Uniform Crime Reports does not consider the validity and comparability of the clearance rates reported by different jurisdictions.

2. However, as the studies of McCleary et al. (1982) and DeFleur (1975) indicate, the findings can also be misleading when such time series reflect an extended period of time due to changes in police policies and administrations that may occur during that period.

3. Parker and Grasmick's content analysis of the newspaper in the community in which their study was conducted indicated that the arrest rate that would be derived from newspaper reports (60 percent) was nearly four and a half times the actual arrest rate (13 percent).

4. Sherman et al. (1989: 35) also note that such under-reporting can represent the intentional misreporting of an incident by a citizen, such as when people refuse to report an incident (or lie about its location) when such reports might jeopardize licenses to provide certain services.

5. Gilsinan (1989) has shown that the police call-takers on 911 systems play an important gatekeeping role to the extent that they must make sense of the callers' reports so that they can be recorded in a manner that is consistent with the classification schemes of a police department. However, these interpretive dynamics characterize all forms of criminal data collection and are not unique to calls-for-service data. Police officers at the scene of the incident must interpret the activity in order to file the reports reflected in more traditional official data. Likewise, respondents in self-report and victimization studies must interpret the "true" meaning of the offense description and fit their experiences into the proper categories.

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TABLE 1

RELATIVE DISTRIBUTION OF SELECTED CALLS FOR SERVICE-OKLAHOMA CITY

OFFENSE	PERCENT	ON-SITE CLEARANCE RATE
ASSAULT WITH WEAPON	0.8	6.0
ROBBERY	0.8	5.5
RAPE	0.2	6.4
BURGLARY	13.1	2.4
GRAND THEFT	4.0	1.5
AUTO THEFT	3.5	2.3
SIMPLE ASSAULT	1.5	2.1
FRAUD	1.2	1.6
CHILD MOLESTING	0.1	4.8
OTHER SEX OFFENSE	0.6	2.8
DOMESTIC DISTURBANCE	5.9	4.5
DISORDERLY CONDUCT	10.6	4.5
PUBLIC DRUNK	1.9	2.7
VICE AND DRUGS	0.4	1.9
PETIT LARCENY	3.2	0.7
SHOPLIFTING	0.9	1.3
KIDNAPPING	0.1	10.5
SUSPICIOUS ACTIVITY	7.7	6.8
RESPOND TO ALARM	12.2	44.2
RUNAWAY	1.1	3.7
VANDALISM	2.0	1.8
OTHER	28.2	4.0
TOTAL CALLS	584,440	

TABLE 2

NEIGHBORHOOD-SPECIFIC ON-SITE CLEARANCE RATES

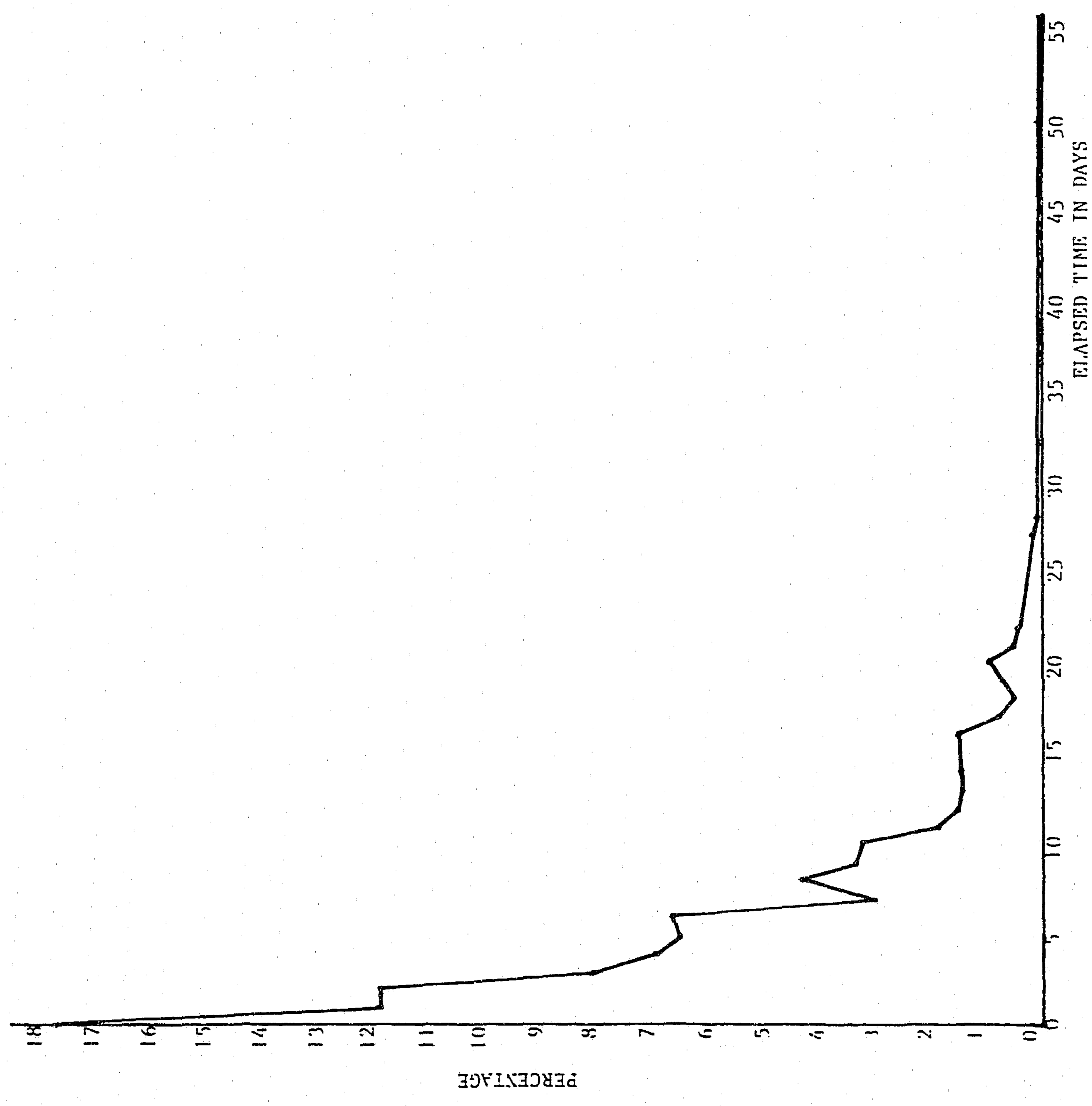
		Neighborhood				
		I	II	III	IV	V
Cleared On-Site	No	95.4	94.9	94.8	89.1	94.8
	Yes	4.6	5.1	5.2	10.9	5.2
	N	109	137	135	101	135

TABLE 3

EFFECTS OF ON-SITE ARRESTS ON THE HAZARD OF A ROBBERY REPORT

		Neighborhood				
		I	II	III	IV	V
ON-SITE						
	Beta	-.077	-.549	-.039	.167	-.128
	S.E.	.462	.394	.397	.328	.390
	p	.868	.162	.921	.611	.742
	Z:PH	2.02	0.42	1.23	-.097	0.38
TOTCLEAR						
	Beta	.008	-.059	-.048	.001	.046
	S.E.	.053	.041	.049	.035	.041
	p	.875	.155	.329	.974	.258
	Z:PH	1.33	3.55	0.37	0.55	-3.46

FIGURE 1


PERCENTAGE
18
17
16
15
14
13
12
11
10
9
8
7
6
5
4
3
2
1
0
ELAPSED TIME IN DAYS
5
10
15
20
25
30
35
40
45
50
55