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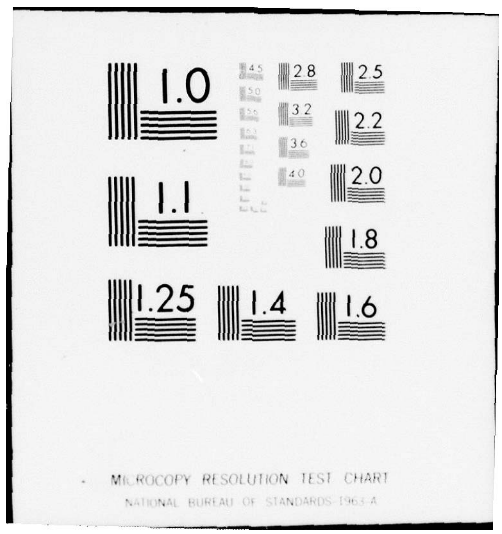
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(6) WHAT'S KNOWN ABOUT DETERRENT EFFECTS OF POLICE ACTIVITIES

(10) Jan M. Chaiken

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WHAT'S KNOWN ABOUT DETERRENT EFFECTS OF POLICE ACTIVITIES

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ABSTRACT

Several techniques have been used to estimate the effect of police activities on the incidence of crime, including: (1) cross-sectional analysis of reported crime rates in various jurisdictions as compared to resources devoted to the totality of police functions or certain police functions, (2) longitudinal analysis of a time series of crime incidence in several jurisdictions or in a single jurisdiction where police deployment or operations changed over time, and (3) experimental manipulation of the nature or amount of police activities. Nearly every study concerning deterrence has been subjected to criticism for one or more faults, such as failure to distinguish between true and reported crime rates, failure to specify or maintain the experimental conditions, apparent errors in the data, or confusion between cause and effect. This review indicates that most studies are consistent with the view that a substantial increase in police activity will reduce crime for a period of time, but in the real world increases in police manpower tend to follow increases in crime. The magnitude and duration of deterrence effects are essentially unknown.

Paper prepared for presentation at the Joint National Meeting of the Operations Research Society of America and The Institute of Management Sciences, Miami, November 3, 1976.

INTRODUCTION

Crime control is a primary mission of the police, as perceived by both the public and police administrators.[1] Although many police officers, especially those in the patrol force and those assigned to traffic duties, spend substantial amounts of their time on activities that are not crime related,[2] police departments would have a difficult time justifying their budgets, or especially increases in their budgets, in terms of the benefits to the public from such noncrime-related activities. For this reason, police administrators criticize vigorously any study that appears to show some time-honored police activity is ineffective in deterring crime.[3]

For the most part, such studies have not been intended as broad critiques of police effectiveness, but rather as guides to resource allocation. Since the police have numerous choices of activities that are believed to reduce crime, if some of them are shown to be ineffective, then attention can focus on the other ones. Even a brief list of activities that have been claimed to reduce crime will illustrate their diversity:

1. Foot patrol by uniformed officers. The presumed effect here is that persons who are contemplating a criminal act within sight of such an officer will be deterred because the risk of apprehension is too high. Secondarily, deterrence may occur out of the sight of the officer if the prospective criminal believes there is a good chance that an officer will appear before the completion of the criminal act. Actual apprehensions effected by such officers can in principle reduce crime in four ways: they may interrupt the crime before its completion (this is a *prevention* effect), they may dissuade the arrestee from subsequent criminal acts (this is a *special deterrence* effect), they may help persuade the general population that the risks of crime exceed the benefits (this is a *general deterrence* effect), and, for the period of time that the arrestee is in custody, they may effectively remove him

or her from the opportunity to commit further crimes* (this is an *incapacitation* effect).[4]

2. Patrol by uniformed officers in marked cars. This activity presumably has the same effects as foot patrol, but they are more diffused geographically.
3. Foot patrol or surveillance by nonuniformed or nonvisible officers. In this case the prospective criminal is supposed to realize that the victim of the crime, or a witness, may be a police officer. Actual apprehensions by such officers may also have the effects noted above.
4. Rapid response by patrol cars to reports of crimes in progress. This may reduce crime via the possibility of apprehending the offender.
5. Careful investigation of crimes whose perpetrators are known. This is intended to increase the probability that the full sanctions of the criminal law will be brought to bear on the offender by subsequent processing in the criminal justice system (i.e., by prosecutors, courts, and corrections authorities). The application of these sanctions may also have special and general deterrence effects, as well as incapacitative effects.
6. Investigation of crimes whose perpetrators are unknown. This activity is intended to increase the threat of apprehension. In addition, the offender may have direct knowledge that the crime is being investigated (e.g., by being interrogated or by reading the newspaper), a situation that might have special deterrence effects.
7. Counseling and assisting juveniles. The purpose of these activities is to ameliorate or remove those conditions in a juvenile's life that are conducive to criminal behavior and to divert juveniles from processing by the criminal justice system.[5] The intended effect is prevention.

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*Of course, crimes against fellow prisoners, prison guards, and court personnel may occur while the arrestee is in custody.

8. Arrest persons against whom warrants are outstanding. In many jurisdictions there is a substantial backlog of persons against whom arrest warrants have been issued (e.g., for failing to appear in court), but the police have not yet apprehended them. Executing the warrants presumably brings to bear the effects of apprehension noted above.[6]
9. Family crisis intervention.[7] Training police officers to respond properly to family disputes presumably leads not only to a successful resolution of the current incident but also to a reduced chance of future intrafamily crimes.
10. Encouraging community crime-prevention efforts. This includes a variety of activities such as sponsoring residential patrols,[8] block watches,[9] or property identification,[10] and learning about the community's perception of crime problems. The notion here is that crime control is not exclusively a function of the criminal justice system.
11. Data processing. Development of information systems and crime analysis capabilities is often viewed as having a deterrent value, presumably because it enhances the effectiveness of some other police activity.

MEASUREMENT PROBLEMS

From the research point of view, measuring the deterrent effects of police activities is extremely difficult. Primarily, this is because one is interested in detecting something that *did not happen* (namely, a deterred crime). Secondarily, it is because most research designs are incapable of separating the conceptually distinguishable crime control effects and attributing them to a particular activity. A design which considers the total number of crimes as a performance measure will necessarily capture all the effects together (that is, prevention, special and general deterrence, and incapacitation). Other designs, such as follow-up studies of the recidivism of offenders, will capture only one of the effects (in this case, special deterrence), leaving the other components unknown. To say that a specific police activity has no special deterrent effect, or a negative special deterrent effect, does not indicate that it is ineffective as a crime-control measure.

In addition to these broad difficulties, researchers concerned with deterrence are plagued with measurement problems that are never fully resolved in any one study.

Counting Crimes

The errors associated with crime counts tabulated by police departments are well known.[11] Some crimes occur but are not reported to the police, while other crimes are reported to the police but are ignored or reclassified as to time, location, or crime type. When the police officers know that the crime counts will or may be used to evaluate the effectiveness of a particular activity, the incentives for discretionary alteration of crime counts may be very great.

An example will illustrate the potential extent of this problem. A 1974 study of subway crime in New York City[12] attempted to determine the effects of uniformed foot patrol on crime rates. For nearly a decade, a large amount of patrol was conducted on subway trains and platforms only during certain hours (8 p.m. to 4 a.m.). Although some of the data used in this study were obtained from tabulations by the transit police, the data of greatest importance for the research were obtained directly from samples of crime reports filled out by uniformed officers. After the study was completed, the chief of the transit police, Robert Rapp, came under investigation for, among other things, having encouraged the alteration of crime and arrest reports, and he retired from his position.[13] The details of the alleged alterations are not publicly known, since no formal charges were brought against him. Nonetheless, it appears that for many years transit police officers were encouraged to record crimes that occurred between 8 p.m. and 4 a.m. as having occurred at other times and to downgrade the crime type if it was infeasible to alter the time (e.g., if an arrest were made).

Since counts of original crime reports confirmed the tabulations prepared by the transit police, any alterations must have been made not in the statistics office, but in the field. Thus, over a period of many years, both veteran officers and new recruits were apparently made aware of their chief's desire that they conform to a crime reporting policy that surely was not specified in writing. If it is indeed true that the chief and many officers were willing to take the risks of possible indictment and of failures in prosecuting arrestees whose crimes were incorrectly recorded, the strength of the desire for statistics to support the deterrence hypothesis is amply demonstrated by this example.

The authors of this study thought they observed a phenomenon in which criminals chose the hours just before 8 p.m. and just after 4 a.m. to commit their crimes, knowing their risks of apprehension were lower than during the high-patrol hours. Instead, the researchers were probably observing an artifact of data corruption.

Although victimization surveys have been introduced to avoid the perils of relying on police-reported crime figures,[14] these too are subject to bias. For one reason or another, some crimes are not reported to the interviewer in a victimization survey, whether or not they have been reported to the police.[15] In addition, cultural differences among subgroups of the population lead to certain types of incidents (e.g., assaults in a barrooom) being perceived as criminal events only by some victims.

In short, no matter what source of crime data a researcher uses, there will be measurement errors.

Probability of Apprehension

The police have as much discretion in recording arrests as they have in recording crimes, if not more discretion. Therefore, counts of arrests are subject to manipulation. If it wants to, a police department can increase its numbers of arrests in a given crime category by arresting on flimsy evidence or charging arrestees with a more serious crime than the prosecutor is likely to accept.

For purposes of estimating the probability of apprehension, one would like to know: Given that a person has committed a crime of Type A, what is the chance that he or she will be arrested? Dividing the number of Type A arrests by the number of crimes of Type A (however measured) is an unsuitable estimate of the desired figure. On the one hand, several individuals may be arrested for a single crime; on the other hand, a person may be arrested once and charged with several crimes. In short, arrest statistics count *people* while crime statistics count incidents, not (usually) offenders. These measurement errors are most apparent when small numbers are involved. For example, a city may report 14 homicides in a year, with 16 persons arrested for homicide.* A naive calculation then shows that the apprehension probability is 1.14, which is evidently preposterous.

Displacement

When, as a result of some police activity, potential offenders are deterred from committing crimes at the times or places where the activity is focused, they may instead commit crimes at other times or places. These displacement effects are difficult to detect unless the researcher has some hypothesis concerning where or when they occur.

Temporal Effects

Time Delays. The crime-reduction effects of a police activity may change with the passage of time. Some activities may be more effective a year after they start than in the first weeks. Other activities may wane in effectiveness as potential criminals become aware of the operation. Still others may be expected to show their influence only at some (possibly unknown) time in the future, whether the activities are continued or terminated.

Phantom Effects. A police activity can have deterrent effects at times and places where it is not operating. This phenomenon is hypothesized to occur because potential criminals have imperfect or false information about operational details,[16] perhaps engendered by deliberate deception by the police. While this is a genuine general deterrence effect, it may not be detected by data analysis that focuses on those times and places known to the researcher as the targets of the police activity.

Interaction Between Independent and Dependent Variables. In many instances a police activity is instituted in response to an increase

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*While the example is intended to illustrate the possibility that 16 persons committed 14 (or fewer) homicides, there can also be a disparity if some of the 16 were arrested for homicides committed last year, or in earlier years.

in crime incidence. After the apparent amount of crime decreases, the activity may be terminated or moved elsewhere. A correlational analysis may then reveal that the presence of the activity is associated with unusually high crime rate, i.e., the apparent opposite of a deterrent effect. Such a "finding" results from confusion between cause and effect.

In addition, it may be assumed that extremely high crime rates will overwhelm the police department's ability to process the crimes, so that even the arrest of some known offenders is foregone. This would be a *workload* effect. In this case, the crime rate is not high because the arrest probability is low; rather, the arrest probability is low because the crime workload is high.

CROSS-SECTIONAL STUDIES

Numerous studies have been conducted recently showing the relationship between the crime rates in various states, cities, or other jurisdictions and the level of criminal justice sanctions in the same jurisdictions.[17] Measures of sanctions include arrests per crime, trials per crime, convictions per crime, imprisonments per crime, and severity of punishment (e.g., average duration of sentence or average time actually spent in prison per incarceration). In some of the studies, the crime rates have been controlled for the effects of external variables such as income (or income disparity), unemployment, fraction of the population in crime-prone age groups, population density, and migration rate. Frequently a strong regional variation is found, unexplained by the other variables, and this is controlled by use of dummy variables (for example, a variable that equals 1 if the jurisdiction is in the South, otherwise 0). See Fig. 1.

These studies are relevant to the deterrent effect of police activities if the variable arrests per crime was used as a sanction measure or if some overall measure of the intensity of police activity was used as a control variable (for example, number of police officers per capita or police budget expenditures per capita). Most, but not all, of these studies show a negative association between arrests per crime and crime rates and a positive association between the intensity of police activity and crime rates.

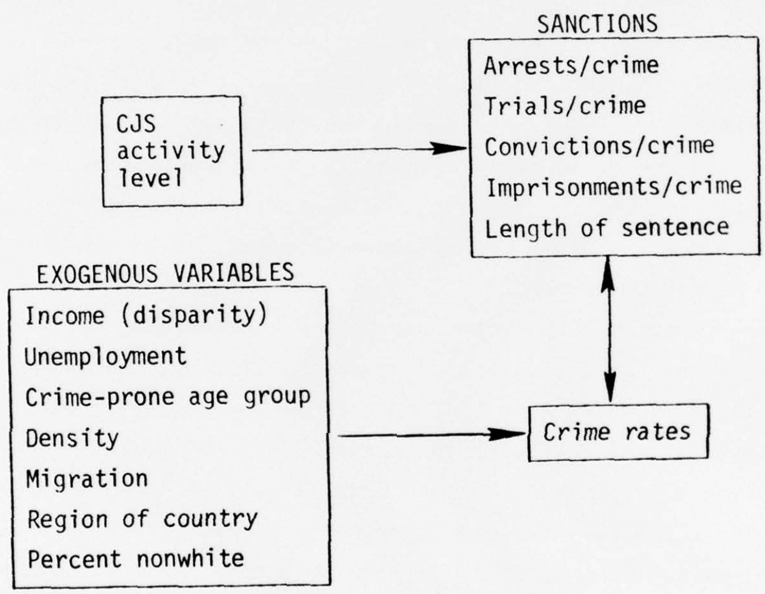

Fig. 1—Schematic representation of the design of cross-sectional studies

Variability in Findings

Part of the uncertainty in interpreting these results rests on the fact that not all researchers reach the same conclusion, with variations according to their choice of crime type, sample of jurisdictions, and control variables.

Measurement Errors

When the sanction variable is arrests per crime, there is the problem that the variable C = number of crimes appears in the denominator of the variable arrests per crime and in the numerator of the variable crimes per population. To the extent that there are any errors in measuring C, which we have argued above are very likely to occur, there is an automatic negative association between crime rate and the sanction variable.[18] The question then arises whether the automatic negative association can be comparable in size to the total association found in the study.

Cook[19] has given an example to show that the measurement error might indeed account for the entire observed association. He calculated the burglary rates in 26 cities that have had victimization surveys in two ways: first, assuming the survey gives the correct number of burglaries; second, by using Uniform Crime Reporting (UCR) data for the years in which the surveys were taken. The number of burglaries cleared by arrest is taken from UCR data in both cases, and the sanction variable is clearances per burglary (calculated in two ways). The simple correlation between burglary rate and the sanction variable is then significantly negative using UCR data but is not significantly different from zero using victimization data. The claim here is not that the victimization survey data are error-free, but rather that the automatic correlation due to measurement error is comparable in size to the total correlation.

By contrast, Wilson and Boland, in a somewhat more careful analysis,[20] report the opposite finding for the crime of robbery. Using victimization survey data for the same cities, and controlling for the variables "percent nonwhite" and population density, they find a significant negative association between robbery rate and arrests per robbery.

Interaction Among Variables

The question of whether crime workload could be affecting the arrest rate was explored in an analysis of 1972 UCR data for 300 police departments having 150 or more employees.[21] A significant negative simple correlation was found between the number of arrests per crime and crime workload for forcible rape, robbery, burglary, aggravated assault, and all crimes against persons taken together. The workload measure was crimes per police officer, where "crimes" was either total Part I crimes, total crimes against persons, or number of crimes of the type in question.

The correlation coefficient is, however, an inadequate descriptor of the functional relationship, which is nonlinear and may be described as follows.[22] Let A denote the number of arrests, C the number of crimes, N the number of police officers, $P = A/C$, $B = A/N$, and $W = C/N$. A simple regression showed that for values of W below a threshold, the equation

$$B = \alpha + \beta W$$

gives a good fit, with α significantly positive. For large values of W, the value of B did not increase significantly with W, and therefore was essentially a constant γ. Since $P = B/W$, we then have

$$P = \begin{cases} \frac{\alpha}{W} + \beta & \text{for W below a threshold} \\ \frac{\gamma}{W} & \text{for W above the threshold} \end{cases}$$

This nonlinear relationship yields the significant negative (linear) correlation between P and W. The essence of the correlation, however, arises in departments with very low workload or very high workload. Since α is not zero, the relationship indicates that a police department will make at least α arrests per police officer, even in the limit that a very small number of crimes are reported. Above the workload threshold, the department is essentially saturated from the point of view of arrest production.

By focusing on a sample of jurisdictions whose value of W is both below the threshold and also large enough that $\frac{\alpha}{W}$ is small compared to β, it is possible to assume that the workload effect is not present. For such jurisdictions one may hypothesize that workload is not affecting the arrest rate, and thereby to eliminate part of the problem of interaction between variables.[23] Any conclusions concerning the deterrent effect of arrests would nonetheless be of greatest interest to those departments whose workload is above the threshold.

Conclusions

Cross-sectional studies cannot separate out the various crime-control effects (e.g., deterrence from incapacitation) nor are they of much use, to date, in exploring the effects of particular police activities. However, the results indicate that the size of a police department is driven by crime rates (rather than the other way around), while increasing the probability of arrest may yet prove to decrease selected types of crimes. Resolving the question of the crime-control effect of apprehension probability is important because many police activities are evaluated in terms of their effect on apprehension probability. Knowing that an activity increases the apprehension probability leaves the merit of the activity in limbo until the effect of apprehension is determined.

LONGITUDINAL STUDIES

Studies that examine changes in crime rates in one or more jurisdictions over time suffer from the disability that reported crime rates in general have been increasing fairly steadily for over a decade. It is possible that changes in UCR crime rates are partially explained by changes in reporting rates rather than in actual amounts of crime, but for some crimes, such as homicide, most of the observed increase must be real. These increases are larger than what can be explained by changes in exogenous variables (such as income inequality, percent non-white, and percent in crime-prone ages), using the relationships found in cross-sectional studies. Therefore, there is a temporal effect that is not satisfactorily understood and that cannot even be studied carefully until victimization surveys have been continued for several years. Longitudinal studies must therefore be conducted within a framework of uncertainty about the interpretation of the data.

Levine[24] compared changes in police strength (police employees per capita) with changes in UCR robbery and murder rates over the period 1961-1971 in cities having population over 500,000. Both crimes increased by about the same percentage in cities having a large increase in police strength as in cities having a small increase or a decrease. The annual patterns suggest that police strength responded to the previous year's crime rate rather than crime rates responding to police strength. Controlling for changes in population characteristics strengthened rather than diluted this observation. Essentially similar findings have been reported by others,[25] so that the longitudinal studies support the cross-sectional ones.

Longitudinal studies in single cities have usually been *post hoc* evaluations of some change in police activity. These use nonexperimental designs of the "before and after" or "interrupted time series" variety.[26] The before-and-after design is limited by the fact that it cannot identify the nature of time-delay effects, if any. Moreover, to control for changes in the crime rate that may have nothing to do with the changed police activity, it is necessary to estimate what would have happened in the absence of the changed activity. This can be accomplished either by projecting past crime rates into the future period of interest or by selecting comparison geographical areas in which no change in activity occurred.

Increase in Police Manpower in New York

Two such studies in New York City, one in the 25th precinct and one in the 20th precinct (both in Manhattan), attempted to determine the effects of large infusions of police manpower. For four months in 1954 and early 1955, the number of police officers in the 25th precinct was doubled.[27] Many of the added officers were new recruits. The analysis, which was not very careful, compared crime counts during this period with the counts during the corresponding period of the preceding year. Thus it was a before-and-after design with no control or projection of crime rates.

The findings were that crime, especially "outside" crime, decreased substantially with the addition of manpower. However, no attempt was made to check whether displacement had occurred.

The duration of this experiment was too short for any implications to be drawn concerning the long-term effects of changes in the overall level of police activity. Since the number of arrests in the 25th precinct increased substantially during the experimental period, there is some question whether the same level of activity could be sustained for an extended period of time. For example, initially the new recruits did not lose any patrol time for court appearances; but later the arrests they had made would produce court-related workload for them. In addition, there is the possibility of a "roundup" effect--arrests can be made without adequate cause and yet some of the arrestees will nonetheless be incapacitated while awaiting their court appearances. Such an effect also cannot be sustained for extended periods.

The results of Operation 25 are not given much credence by many members of the New York Police Department, who believe that patrol officers were induced to report fewer crimes. However, such claims have not been documented by either an audit of crime reports or systematic interviews with officers who participated in the experiment.

About a decade later, the New York police conducted a similar experiment in the 20th precinct, which is not far from the 25th precinct but substantially different in population characteristics. During the period from October 18, 1966, through December 1967, patrol manpower was increased by an average of 40 percent in the 20th precinct, while other precincts experienced only small changes in manpower. This experiment was analyzed after the fact by Press.[28]

Press also used a before-and-after design, but the statistical analysis was much more sophisticated than in the earlier Operation 25. Weekly, rather than monthly, data were used, the data were adjusted for seasonal variations, displacement was explicitly considered, and an attempt was made to control for naturally occurring changes in crime rates. For each crime type, Press selected several precincts as controls for the 20th precinct. The criteria used in making these selections were as follows: (a) the number of crimes in a precinct had to be within 10 percent of the number in the 20th precinct during the period preceding the experiment, (b) changes in police manpower in a control precinct had to be under 15 percent during the experiment, and (c) the population of a control precinct had to be within 20 percent of the population in the 20th precinct. As a result, the particular precincts chosen as controls for the 20th precinct differed according to the type of crime in question. A similar method was used to select control precincts for those precincts that bordered the 20th precinct, except that Central Park forms one boundary of the 20th precinct, and there is no other comparable area in the city.

While conceptually one might prefer to have control precincts selected to match a study precinct in terms of crimes per population and social or demographic characteristics of the population, it is statistically much more difficult to analyze *percentage* changes in crime rates than *numerical* changes in crimes. Therefore, Press examined changes in the numbers of crimes. For example, in the 20th precinct the number of outside robberies increased from 4.52 per week before the experiment to 5.01 per week during the experiment (seasonally adjusted), while the average control precinct increased from 4.76 to 7.79 outside robberies per week. This was considered to be a *net reduction* of 2.54 outside robberies per week in the 20th precinct, which was found to be statistically significant.

The analysis showed that reported crimes visible from the street in the 20th precinct decreased significantly in comparison with control precincts. Inside crimes were not significantly affected, with the exception of robbery and grand larceny, which decreased. The number of arrests increased significantly. A possible displacement effect (into Central Park) was observed, but the increase in crimes in Central Park was not as large as the decrease in the 20th precinct.

The duration of this experiment was perhaps long enough to identify time lags, but the statistical design did not permit identification of such effects. The few graphs which are presented by Press suggest that the experiment may have been terminated when its effectiveness began to decline.

The results are difficult to interpret, primarily because of the multiplicity of control precincts and the identification of "significant net reductions" in crime when in fact increases occurred. In addition, we are not informed about the deployment of the added manpower. Was there more foot patrol during the experiment? Was there more preventive patrol in marked cars? Were the officers given any incentives to report fewer crimes? The answers to these questions will never be known because the analysts were called in after the experiment was completed.

Subway Robberies in New York

In 1965, the number of police officers on New York City's subway system was nearly doubled, from 1219 to over 3100 officers. The additional officers were to patrol every station and train in the system between 8 p.m. and 4 a.m. This change was analyzed after the fact by Chaiken, Lawless, and Stevenson.[29] Since this study was cited earlier as an example of an analysis distorted by data corruption, its findings must be reinterpreted with the benefit of hindsight.

The design of this study was an interrupted time series. The primary finding was that the number of reported subway felonies and misdemeanors decreased numerically immediately after the manning change and remained approximately constant for two years. Thereafter, they increased at about the same annual rate of increase as prior to the manning change. Reported subway robberies, which accounted for about 20 percent of the felonies, decreased numerically at the time of the manning change, but their annual rate of increase was unchecked, remaining approximately constant for a period of seven years.[30]

Since both misdemeanors and felonies decreased after the manning change, it is difficult to accept the hypothesis that transit officers lowered the reported number of felonies by downgrading them to misdemeanors. Moreover, the decrease in reported felonies was paralleled by decreases in reported robberies of token booths, a category of crime that seems peculiarly resistant to nonreporting. (How can the token clerk explain the missing cash if he or she was not robbed?) Thus, although we know there was data corruption, we don't know that it started immediately after the manning change, and it seems most unlikely that all of the observed decrease was artificial. It appears reasonable to conclude that there was a crime decrease, but its magnitude has been disguised.

The authors of this study also observed a phantom effect, namely, a decrease in crime at times of day when there were no changes in manning, and specifically raised the possibility that it was explained by a change in reporting practices. However, the phantom effect was temporarily very large and disappeared almost entirely after about eight months. For example, there were 22 token booth robberies reported during the "daytime" hours (4 a.m. to 8 p.m.) in the three months preceding the manning change, but only 2 during the same hours in the next three months. Five years later, even with data corruption, over 100 token booth robberies were being reported during the analogous period. So it does not appear that changes in reporting practices alone could have produced a reduction from 22 to 2 token booth robberies. The observed reduction in reported robberies during the "daytime" hours also does not permit an interpretation of the similar, but larger, reduction in reported nighttime robberies as having been produced by changing the times of robberies on the incident reports.

Later, however, an apparent displacement of nighttime to daytime robberies was observed. The study found a local maximum in the number of reported robberies in the hour between 7 and 8 p.m. and a local minimum in the hour from 8 to 9 p.m. As mentioned earlier, this must be at least partially an artifact of data corruption. This artifact can be eliminated by adjusting the number of reported robberies for the years 1970 and 1971 in the hours from 6 p.m. to 8 p.m. so that the *relative proportions* of daytime robberies in each hour follow the same patterns observed before the manning change. This adjustment causes 0.35 robberies per day that were identified in the study as daytime robberies in 1970 and 1971 to be attributed to the nighttime hours instead. Nonetheless, after the adjustment, the estimated number of robberies per hour in 1970 and 1971 is 2.1 times as large during daytime hours as during nighttime hours.* Before the manning change,

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*This estimate is approximately consistent with figures produced by the transit police after a new chief was installed and the data problems were reportedly corrected.

there were more robberies per hour at night than during the day. Therefore, a deterrent effect of the added manpower at night still appears to be present after the data are adjusted. Even if one assumes that some nighttime robberies were not recorded at all, it is difficult to believe that the entire difference between day and night could be explained thereby.

An unfortunate aspect of this story is that the transit police evidently manipulated data so as to demonstrate the presence of a deterrent effect that would have been observed even if the data were scrupulously correct. Only the estimated magnitude of the effect has been distorted.

Evaluation of Selected High-Impact Programs

A study by Dahmann reports on the effects of added police in high-crime areas of three cities (Denver, Cleveland, and St. Louis).[31] The nature and deployment of the officers differed substantially among the cities. In Denver, a team of patrol officers, detectives, and evidence technicians was targeted on problem neighborhoods. In Cleveland, added uniformed officers in marked vehicles were deployed to support the regular patrol officers in target areas, but with emphasis on answering calls for service involving criminal incidents. In St. Louis, the added officers were assigned to uniformed foot patrol.

The analysis followed a before-and-after design. Crime rate changes that might have occurred in the absence of added police were estimated by projection of past trends. Displacement was explicitly considered by dividing the cities into target areas, adjacent areas, and unaffected areas (the remainder of the city).

The results agreed basically with those of Press, in that reported outside crimes in target areas generally showed a net reduction in comparison with unaffected areas. In addition, some crimes in target areas had lower reported numbers of crimes than were projected from past trends. No one type of crime was lower than projected in all three cities. No strong indications of displacement were found, as the adjacent areas generally showed patterns similar to those in unaffected areas. However, a few instances of possible phantom effects were observed--adjacent areas showed crime rates lower than projected, as did the target areas, but the same was not true of unaffected areas. No conclusions were drawn concerning the relative effectiveness of the three different types of police operations.

This study was plagued by a variety of data problems in addition to the common problems noted for all the other longitudinal studies discussed above. First, only monthly data were available, rather than weekly, as in the Press study, for example. As a result, the data variance was unnecessarily large.* In addition, large inexplicable reductions in crime (below projected) sometimes occurred in all three types of areas--target, adjacent, and unaffected--at times of day when the operation was and was not in effect, and for crime types that would seem to be unlikely candidates for deterrence.

While the analysis was not designed to detect changes in effectiveness of the police activity over time, the raw data provided in the appendix suggest that at least in Cleveland crime reductions in target areas lessened with the passage of time.

Conclusions

By using reported crime rates, the studies described in this section have left themselves open to varying interpretations. It seems undeniable that practically any kind of increase in police manpower can increase the number of arrests made by the police. Very large increases in the amount of patrol, it appears, can produce detectable reductions in crime in the target area. The magnitude of these changes and the degree to which they are diluted in value by displacement effects appear to be serious open questions. I do not believe that additional studies of this type can help resolve the questions; rather, careful experiments using victimization surveys as well as police data will be needed.

KANSAS CITY PREVENTIVE PATROL EXPERIMENT

The Kansas City Preventive Patrol Experiment[32] is unique among tests

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*For example, generally fewer crimes are reported in February than in January, because February has fewer days. Also, some months have five weekends while others have four.

crimes. In short, arrest statistics count *people* while crime statistics

of the deterrent effects of police activities in that the analysts participated strongly in the design of the experiment, reasonable attempts were made to randomize the choices of experimental and control areas, and a vast array of different measurements were applied to determine the outcomes. The results, by now well known, were that neither crime rates nor citizens' perceptions of crime or safety were significantly affected by the changed operations in Kansas City. Crime rates were measured by both police reports and victimization surveys.

Larson,[33] in a careful review, has questioned just what the changed operations were and whether they could have reasonably been expected to produce any changes in crime rates or citizens' perceptions. Basically, the designers of the experiment intended to have some parts of the study area (proactive beats) with more preventive patrol than previously, some (control beats) with the same as before, and others (reactive) with none. In this context it is important to distinguish between "uncommitted time" (i.e., time during which the patrol car is available to respond to calls for service) and "preventive patrol time." There is no way to reduce uncommitted time without affecting other characteristics of the patrol system, such as response time to crime calls. The Kansas City experiment attempted to manipulate the amount of preventive patrol without changing uncommitted time substantially, and therefore cannot be viewed as having any implications for the effects of an overall manpower reduction. In fact, manpower levels in the study division increased somewhat during the experiment.

Larson questions whether the amount of preventive patrol was in fact reduced in the reactive beats. To this end he cites five arguments. First, from simple analytical models one can determine that when patrol cars respond from the periphery of their beat to calls for service inside the beat and then return to the periphery, they travel over many street-miles in the course of their tour--certainly more than they would travel while responding from inside the beat. Second, the number of self-initiated activities by patrol cars in reactive beats was higher during the experiment than before. Third, it frequently happened that two or more units responded to calls in reactive beats--more frequently than in proactive beats and presumably more frequently than before the experiment in reactive beats. Fourth, the patrol units used sirens and lights in reactive beats more frequently than in proactive beats--also presumably more than before the experiment in reactive beats. Finally, specialized police units which are visible to the public but are not regular patrol cars operated in the reactive beats.

In my view, all of these arguments except the second one are relevant to only one aspect of preventive patrol, namely, "visible police presence." Another important aspect of preventive patrol is the "mental set" of the patrolling officer, in which he is specifically looking for suspicious circumstances, crimes in progress, etc. An officer responding to a call of a robbery in progress might quite reasonably fail to stop if he sees a fight on the sidewalk or a person peering into a parked car, whereas an officer on preventive patrol might well pay attention to such events. The responding officer will also not enter the license numbers of cars he passes into his computer terminal to see whether they might be stolen. Thus, an officer on preventive patrol perceives himself as engaged in a certain kind of activity that extends beyond "being there" or "passing by." Except for the peculiar data concerning self-initiated activities, it seems reasonable to believe that there was much less of this activity in reactive beats than in proactive beats. The findings from the experiment then tell us something--neither the public nor criminals make the distinction I have just explained, and this special mental set of "preventive patrol" appears to have no effect.

As a result, the experiment opens up the possibility that uncommitted patrol officers might profitably do something other than preventive patrol, as long as it leaves them uncommitted. The challenge is to determine what alternative activity would be more effective.

Another point made by Larson is that the amount of preventive patrol was not manipulated over a large range in Kansas City. He correctly notes that the highest level of preventive patrol achieved during the experiment "does not adequately reflect routine levels of patrol experienced in other cities."[34] Therefore, the study's results are not inconsistent with the hypothesis that a large increase in police manpower will reduce true crime rates.

RESPONSE-TIME STUDIES

A study of response time currently under way (also in Kansas City) may shed some light on the possible deterrent effects of fast responses by patrol cars.[35] Prior to the completion of this study, however, we can point to two studies that yielded remarkably similar results. One was conducted by Isaacs[36] using 1966 data from Los Angeles, the other by Clawson and Chang[37] using 1974-75 data from Seattle. Both showed that the fraction of patrol car responses that resulted in an arrest is a constant plus an exponentially decreasing function of response time. The Seattle study considered only on-scene arrests, while the Los Angeles study considered both on-scene and follow-up arrests. Similar studies in other cities[38] have not revealed the same pattern, perhaps because the curves are essentially flat for response times larger than 3 minutes, and therefore a substantial amount of data for responses under 3 minutes is needed to observe any effect.

The difficulty in interpreting such studies, which is acknowledged by their authors, is the intertwining of cause and effect. It may well be that the response time is short because the officers know the offender is still at the scene or perhaps is even being detained at the scene by a civilian. Thus the known high arrest probability produces the fast response time, rather than the fast response time producing an arrest. Only by controlling for the information provided to the responding officer can the relationship be properly analyzed.

SO WHAT?

In my view, the notion that police activities have no deterrent effect cannot be seriously entertained. Certainly in the limit of eliminating all police departments crime would increase, while in the limit of shoulder-to-shoulder foot patrol on every street crime would decrease. The question being posed by research on deterrence is not whether deterrence exists. The real questions are these: Within realistically achievable ranges of police activity, how large are the variations in deterrence? Do deterrence effects decay with the passage of time? Among different possible police activities, which ones produce the largest deterrent effect per dollar spent? It seems entirely possible that reducing average response times of patrol cars from 8 to 5 minutes would have a negligible deterrent effect, while reducing them from 5 to 2 minutes, which is much more expensive, would have a noticeable effect but only for a limited period of time. This is the kind of information that police administrators need for resource allocation.

Problems of data quality, interactions among variables, and uncontrollable changes in the real world have made research on deterrence extremely difficult and costly. The prospects for unambiguously resolving the major research questions in the next few years appear quite dim. For example, one might ask why not repeat the Kansas City Preventive Patrol Experiment in some other city, but with a much larger variation in the amount of patrol and better experimental controls over what the officers do? The answer to this question is another question: Who would do it? Probably the researchers who conducted the study in Kansas City would not enjoy repeating the experience, and most competent researchers would not feel they could advance their careers by replicating someone else's work. Locating a source of funding for a replication would also be difficult, because the Kansas City Experiment cost over $1 million. Finally, where is the police chief who would welcome the chance to have his department be the host for such an experiment?

We should not anticipate that any single study will definitively settle current questions about deterrence. Rather, research on deterrence must be recognized as important and requiring a long-term effort.

nificant negative association between robbery rate and arrests per robbery.

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FOOTNOTES

1. See, for example, Advisory Group on Productivity in Law Enforcement, *Opportunities for Improving Productivity in Police Services*, National Commission on Productivity, Washington, D.C., 1973.

2. About half of calls for service to the police involve noncrime incidents; see, for example, Tony Pate, et al., *Police Response Time*, Police Foundation, Washington, D.C., 1976. During the time when patrol officers are not engaged in handling calls for service, they also undertake various noncrime-related activities; see, for example, Richard Larson, *Measuring the Response Patterns of New York City Police Patrol Cars*, The Rand Corporation, R-673-NYC/HUD, Santa Monica, 1971, and Chapter XI of George Kelling, et al., *The Kansas City Preventive Patrol Experiment*, Police Foundation, Washington, D.C., 1974.

3. See, for example, Edward M. Davis and Lyle Knowles, "A Critique of the Report: An Evaluation of the Kansas City Preventive Patrol Experiment," *Police Chief*, Vol. 42, June 1975, p. 22, and Daryl F. Gates and Lyle Knowles, "An Evaluation of the Rand Corporation's Analysis of the Criminal Investigation Process," *Police Chief*, Vol. 43, July 1976, p. 20. The Rand study evaluated in the latter paper did not, in fact, reach any conclusions about the deterrent effects of police investigative activities.

4. For a more general conceptual discussion of these categories of crime control, see Franklin E. Zimring and Gordon J. Hawkins, *Deterrence*, The University of Chicago Press, 1973.

5. See, for example, Alan R. Coffey, *The Prevention of Crime and Delinquency*, Prentice Hall, Englewood Cliffs, 1975.

6. The strength of the assumption that apprehension has a deterrent effect is illustrated by the paper by Melvin D. Platt, "Apprehension and Prosecution: A Deterrent to Crime," *Police Chief*, June 1976, pp. 52,53, which does not discuss deterrence at all, but simply indicates how an increased number of apprehensions can be effected.

7. Richard W. Kobetz (ed.), *Crisis Intervention and the Police*, International Association of Chiefs of Police, Gaithersburg, Md., 1974; Morton Bard, *Family Crisis Intervention*, U.S. Government Printing Office, 1974.

8. Robert K. Yin, et al., *Patrolling the Neighborhood Beat*, The Rand Corporation, R-1912-DOJ, Santa Monica, 1976.

9. George J. Washnis, *Citizen Involvement in Crime Prevention*, Lexington Books, Lexington, Mass., 1976.

10. Nelson B. Heller, et al., *Operation Identification Projects: Assessment of Effectiveness*, The Institute for Public Program Analysis, St. Louis, 1975.

11. See, for example, Albert D. Biderman and Albert J. Reiss, Jr., "On Exploring the 'Dark Figure' of Crime," *The Annals of The American*

Academy of Political and Social Science, Vol. 374, November 1967, pp. 1-15; Donald R. Cressey, "The State of Criminal Statistics," *National Probation and Parole Association Journal*, Vol. 3, 1957, pp. 230-241; Gloria Countvan Manen, "Use of Official Data in the Evaluation of Crime Control Policies and Programs," in Emilio Viano (ed.), *Criminal Justice Research*, D. C. Heath, Lexington, 1975; Peter Lejins, "Uniform Crime Reports," in Simon Dinitz and Walter Reckless, *Critical Issues in the Study of Crime*, Little Brown and Company, Boston, 1968; Peter Lejins, "National Crime Data Reporting System: Proposal for a Model," Appendix C in *Task Force Report: Crime and Its Impact--An Assessment*, President's Commission on Law Enforcement and Administration of Justice, Washington, D.C., 1967; Michael D. Maltz, "Crime Statistics: A Mathematical Perspective," *Journal of Criminal Justice*, Vol. 3, 1975, pp. 177-194; Elinor Ostrom, "The Need for Multiple Indicators in Measuring the Output of Public Agencies," in Frank Scioli, Jr., and Thomas Cook (eds.), *Methodologies for Analyzing Public Policies*, Lexington Books, Lexington, 1975; Wesley G. Skogan, "Measurement Problems in Official and Survey Crime Rates," *Journal of Criminal Justice*, Vol. 3, 1975, pp. 17-32; and Marvin E. Wolfgang, "Uniform Crime Reports: A Critical Appraisal," *University of Pennsylvania Law Review*, Vol. 11, 1963, pp. 708-38.

12. Jan M. Chaiken, Michael Lawless, and Keith Stevenson, *The Impact of Police Activity on Crime: Robberies on the New York City Subway System*, The Rand Corporation, R-1424-NYC, Santa Monica, 1974. Abbreviated version in *Urban Analysis*, Vol. 3, 1975, pp. 173-205.

13. See Marcia Chambers, "Target of Nadjari Investigation Retires as Transit Police Chief," *The New York Times*, February 14, 1975, page 1; Marcia Chambers, "Rapp Admits Transit Authority Misconduct Charges," *The New York Times*, March 7, 1975, page 40; Joseph Kiernan and Edward Kirkman, "Former TA Top Cop Admits Lying to Probers," *The News* (New York), March 7, 1975.

14. U.S. Department of Justice, *Criminal Victimization Surveys in the Nation's Five Largest Cities*, Law Enforcement Assistance Administration, National Criminal Justice Information and Statistics Service, Washington, D.C., April 1975; U.S. Department of Justice, *Crime in Eight American Cities*, Law Enforcement Assistance Administration, National Criminal Justice Information and Statistics Service, Washington, D.C., July 1974; U.S. Department of Justice, *Criminal Victimization Surveys in 13 American Cities*, Law Enforcement Assistance Administration, National Criminal Justice Information and Statistics Service, Washington, D.C., June 1975; Wesley G. Skogan, "The Victims of Crime: Some National Panel Data," in Anthony Guenther (ed.), *Criminal Behavior and Social Systems*, Rand McNally, Chicago, 1976; and Wesley G. Skogan (ed.), *Sample Surveys of the Victims of Crime*, Ballinger, Cambridge, Mass., 1976.

15. C. B. Kalish, *Crimes and Victims: A Report on the Dayton-San Jose Pilot Survey of Victimization*, National Criminal Justice Information and Statistics Service, Washington, D.C., 1974; Michael D. Maltz, "Crime Statistics: A Mathematical Perspective," *J. Criminal Justice*, Vol. 3, 1975, pp. 177-193; A. G. Turner, *The San Jose Methods Test for Known Crime Victims*, U.S. Government Printing Office, 1972.

16. Chaiken, Lawless, and Stevenson, cited above (fn. 12).

17. Nineteen such studies have been reviewed by Daniel Nagin, "General Deterrence: A Review of the Empirical Evidence," *Management Science*, to appear, 1977. Of these, the best known study supporting deterrence is Isaac Ehrlich, "Participation in Illegitimate Activities: An Economic Analysis," *J. Political Economy*, Vol. 81, 1973, pp. 521-565; and the most careful study finding no indication of deterrence is Brian Forst, "Participation in Illegitimate Activities: Further Empirical Findings," *Policy Analysis*, Vol. 2, 1976, pp. 477-492.

18. It may be reasonably argued that the crimes most likely to be unrecorded by the police are those for which an arrest has not been made and is unlikely to be made, so that the arrest rate is artificially inflated. However, such an observation is unnecessary to the argument, since the spurious negative correlation occurs whether the error in measuring C is positive or negative.

19. Philip J. Cook, "Current Findings Concerning the Preventive Effects of Deterrence," *Law and Contemporary Problems*, to appear, 1977.

20. James Q. Wilson and Barbara Boland, "Crime," Chapter 4 in William Gorham and Nathan Glazer (eds.), *The Urban Predicament*, The Urban Institute, Washington, D.C., 1976.

21. Jan M. Chaiken, *The Criminal Investigation Process. Volume II: Survey of Municipal and County Police Departments*, The Rand Corporation, R-1777-DOJ, Santa Monica, 1975.

22. The general form of the relationship between arrest rate and workload had been earlier hypothesized on the basis of the principle of decreasing marginal productivity. See, for example, Ehrlich, cited above (fn. 17).

23. Work in progress by Boland, private communication.

24. James P. Levine, "The Ineffectiveness of Adding Police to Prevent Crime," *Public Policy*, Vol. 23, 1975, pp. 523-545.

25. E. Terrence Jones, "Evaluating Everyday Policies: Police Activity and Crime Incidence," *Urban Affairs Quarterly*, Vol. 8, 1973, pp. 267-279; Thomas F. Pogue, "Police Expenditures and Crime Rates," *Public Finance Quarterly*, 1975, pp. 14-44.

26. Donald T. Campbell and Julian C. Stanley, *Experimental and Quasi-Experimental Designs for Research*, Rand McNally and Company, Chicago, 1966.

27. *Operation 25*, New York City Police Department. See also, James Q. Wilson, *Thinking About Crime*, Basic Books, New York, 1975.

28. S. James Press, *Some Effects of an Increase in Police Manpower in the 20th Precinct of New York City*, The Rand Corporation, R-704-NYC, Santa Monica, 1971.

29. Chaiken, Lawless, and Stevenson, cited above (fn. 12).

30. An exception to this general increase in 1969 is explained by a temporary displacement of subway robberies to robberies on buses.

31. Judith S. Dahmann, *Examination of Police Patrol Effectiveness*, The MITRE Corporation, McLean, Virginia, 1975.

32. Kelling, et al., cited above (fn. 2).

33. Richard C. Larson, "What Happened to Patrol Operations in Kansas City? A Review of the Kansas City Preventive Patrol Experiment," *J. Criminal Justice*, Vol. 3, 1975, pp. 267-98. See also Franklin Zimring, "Policy Experiments in General Deterrence," mimeograph, 1976.

34. Larson, "What Happened...," p. 290.

35. For a preliminary report, see Deborah K. Bertram and Alexander Vargo, "Response Time Analysis Study: Preliminary Findings on Robbery in Kansas City," *Police Chief*, Vol. 43, May 1976, pp. 74-77.

36. Herbert Isaacs, "A Study of Communications, Crimes, and Arrests in a Metropolitan Police Department," *Task Force Report: Science and Technology*, A Report to the President's Commission on Law Enforcement and Administration of Justice, Washington, D.C., 1967.

37. Calvin Clawson and Samson Chang, "Impact of Response Delays on Arrest Rates," Seattle Police Department, unpublished, 1975.

38. William Brown, "Evaluation of Police Patrol Operations," unpublished M.A. thesis, University of Ottawa, Ontario, 1974; Leo P. Holliday, "A Methodology for Radio Car Planning," The New York City-Rand Institute, unpublished, 1974.