

police officers are continually engaged in "battling" crime, as perpetuated by the media and perhaps even by the police themselves, was not substantiated in the reality of the situation as recorded by the observers. . . .

Findings [About Police Officer Attitudes Toward Routine Preventive Patrol]

1. The traditional concept of routine preventive patrol is not clearly defined nor disseminated to the officer in a standardized manner. This places the individual officer in the position of having to decide the value and effectiveness of routine patrol through personally developed criteria.
2. Two emphases were placed on the value and effectiveness of routine patrol: (1) it enhances citizen's feelings of security; and (2) patrol is somewhat effective in preventing crime.
3. Those officers interviewed were uncertain as to the degree of routine patrol effectiveness as it is now practiced.
4. Although almost all the officers interviewed accepted the traditional value of routine preventive patrol, a majority would make changes in the direction of less visibility. . . .

Note

1. In this report, routine preventive patrol is defined as those patrol activities employed by the Kansas City Police Department during the approximately 35 percent of patrol duty time in which officers are not responding to calls for service, attending court or otherwise unavailable for the self-initiated activities (the 35 percent figure was a pre-experimental estimate developed by the Kansas City Police Department for use in determining officer allocation). Information made available daily to patrol officers includes items such as who in their beats is wanted on a warrant, who is wanted for questioning by detectives, what criminals are active in their beats and types and locations of crimes which have occurred during the previous 24 hours. The officers are expected to be familiar with this information and use it during their noncommitted time. Accordingly, routine preventive patrol includes being guided by this information while observing from police cars, checking on premises and suspicious citizens, serving warrants, checking abandoned vehicles and executing other self-initiated police activities. Thus routine preventive patrol in Kansas City is informed activity based upon information gathered from a wide variety of sources. Whether Kansas City's method of preventive patrol is typical is hard to say with exactness. Clearly, some departments place more emphasis on pedestrian checks, car checks, and field interrogating than does Kansas City (experiments on some of these activities are now taking place elsewhere). Preventive patrol as practiced in Kansas City has some unique characteristics but for the most part is typical of preventive patrol in urban areas.

The Deterrent Effects of Arrest for Domestic Assault

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. . . We report here a study of the impact of punishment in a particular setting, for a particular offense, and for particular kinds of individuals. Over an eighteen-month period, police in Minneapolis applied one of three intervention strategies in incidents of misdemeanor domestic assault: arrest; ordering the offender from the premises; or some form of advice which could include mediation. The three interventions were assigned randomly to households, and a critical outcome was the rate of repeat incidents. The relative effect of arrest should hold special interest for the specific deterrence-labeling controversy.

Policing Domestic Assaults

Police have been typically reluctant to make arrests for domestic violence (Berk and Loseke, 1981), as well as for a wide range of other kinds of offenses, unless victims demand an arrest, the suspect insults the officer, or other factors are present (Sherman, 1980). Parnas's (1972) qualitative observations of the Chicago police found four categories of police action in these situations: negotiating or otherwise "talking out" the dispute; threatening the disputants and then leaving; asking one of the parties to leave the premises; or (very rarely) making an arrest.

The best available evidence on the frequency of arrest is the observations from the Black and Reiss study of Boston, Washington and Chicago police in 1966 (Black, 1980:182). Police responding to disputes in those cities made arrests in 27 percent of violent felonies and 17 percent of the violent misdemeanors. Among married couples (Black, 1980:158), they made arrests in 26 percent of the cases, but tried to remove one of the parties in 38 percent of the cases.

An apparent preference of many police for separating the parties rather than arresting the offender has been attacked from two directions over the last fifteen

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Written in collaboration with 42 Patrol officers of the Minneapolis Police Department, Nancy Wester, Donileen Loseke, David Rauma, Debra Morrow, Amy Curtis, Kay Gamble, Roy Roberts, Phyllis Newton, and Gayle Gubman.

years. The original critique came from clinical psychologists, who agreed that police should rarely make arrests (Potter, 1978:46; Fagin, 1978:123-24) in domestic assault cases, and argued that police should mediate the disputes responsible for the violence. . . .

By the mid-1970s, police practices were criticized from the opposite direction by feminist groups. Just as psychologists succeeded in having many police agencies respond to domestic violence as "half social work and half police work," feminists began to argue that police put "too much emphasis on the social work aspect and not enough on the criminal." . . .

The feminist critique was bolstered by a study (Police Foundation, 1976) showing that for 85 percent of a sample of spousal homicides, police had intervened at least once in the preceding two years. For 54 percent of the homicides, police had intervened five or more times. But it was impossible to determine from the cross-sectional data whether making more or fewer arrests would have reduced the homicide rate.

In sum, police officers confronting a domestic assault suspect face at least three conflicting options, urged on them by different groups with different theories. The officers' colleagues might recommend forced separation as a means of achieving short-term peace. Alternatively, the officers' trainers might recommend mediation as a means of getting to the underlying cause of the "dispute" (in which both parties are implicitly assumed to be at fault). Finally, the local women's organizations may recommend that the officer protect the victim (whose "fault," if any, is legally irrelevant) and enforce the law to deter such acts in the future.

Research Design

In response to these conflicting recommendations, the Police Foundation and the Minneapolis Police Department agreed to conduct a randomized experiment. The design called for random assignment of arrest, separation, and some form of advice which could include mediation at the officer's discretion. In addition, there was to be a six-month follow-up period to measure the frequency and seriousness of domestic violence after each police intervention. . . .

The design only applied to simple (misdemeanor) domestic assaults, where both the suspect and the victim were present when the police arrived. Thus, the experiment included only those cases in which police were empowered (but not required) to make arrests under a recently liberalized Minnesota state law; the police officer must have probable cause to believe that a cohabitant or spouse had assaulted the victim within the last four hours (but police need not have witnessed the assault). Cases of life-threatening or severe injury, usually labeled as a felony (aggravated assault), were excluded from the design for ethical reasons.

The design called for each officer to carry a pad of report forms, color coded for the three different police actions. Each time the officers encountered a situation that fit the experiment's criteria, they were to take whatever action was indicated by the report form on the top of the pad. We numbered the forms and arranged them in random order for each officer. The integrity of the random assignment was to be monitored by research staff observers riding on patrol for a sample of evenings. . . .

Anticipating something of the victims' background, a predominantly minority,

female research staff was employed to contact the victims for a detailed face-to-face interview, to be followed by telephone follow-up interviews every two weeks for 24 weeks. The interviews were designed primarily to measure the frequency and seriousness of victimizations caused by the suspect after the police intervention.¹ The research staff also collected criminal justice reports that mentioned the suspect's name during the six-month follow-up period.

Conduct of the Experiment

. . . . Ninety-nine percent of the suspects targeted for arrest actually were arrested, while only 78 percent of those to receive advice did, and only 73 percent of those to be sent out of the residence for eight hours were actually sent. One explanation for this pattern, consistent with the experimental guidelines, is that mediating and sending were more difficult ways for police to control the situation, with a greater likelihood that officers might resort to arrest as a fallback position. When the assigned treatment is arrest, there is no need for a fallback position. For example, some offenders may have refused to comply with an order to leave the premises.

Such differential attrition would potentially bias estimates of the relative effectiveness of arrest by removing uncooperative and difficult offenders from the mediation and separation treatments. Any deterrent effect could be underestimated and, in the extreme, artifactual support for deviance amplification could be found. That is, the arrest group would have too many "bad guys" relative to the other treatments. . . .

We also found that five other variables had a statistically significant effect on "upgrading" the separation and advice treatments to arrests: whether police reported the suspect was rude; whether police reported the suspect tried to assault one (or both) of the police officers; whether police reported weapons were involved; whether the victim persistently demanded a citizen's arrest; and whether a restraining order was being violated. We found no evidence that the background or characteristics of the suspect or victim (e.g., race) affected the treatment received. . . .

We were less fortunate with the interviews of the victims; only 205 (of 330, counting the few repeat victims twice) could be located and initial interviews obtained, a 62 percent completion rate. . . .

The response rate to the bi-weekly follow-up interviews was even lower than for the initial interview, as in much research on women crime victims. After the first interview, for which the victims were paid \$20, there was a gradual falloff in completed interviews with each successive wave; only 161 victims provided all 12 follow-up interviews over the six months, a completion rate of 49 percent. . . .

There is absolutely no evidence that the experimental treatment assigned to the offender affected the victim's decision to grant initial interviews. . . .

In sum, despite the practical difficulties of controlling an experiment and interviewing crime victims in an emotionally charged and violent social context, the experiment succeeded in producing a promising sample of 314 cases with complete official outcome measures and an apparently unbiased sample of responses from the victims in those cases.

Results

... Two kinds of outcome measures will be considered. One is a *police-recorded* "failure" of the offender to survive the six-month follow-up period without having police generate a written report on the suspect for domestic violence, either through an offense or an arrest report written by any officer in the department, or through a subsequent report to the project research staff of a randomized (or other) intervention by officers participating in the experiment. A second kind of measure comes from the *interviews with victims*, in which victims were asked if there had been a repeat incident with the same suspect, broadly defined to include an actual assault, threatened assault, or property damage.

Overall, the police data indicate that the separation treatment produces the highest recidivism, arrest produces the lowest, with the impact of "advise" (from doing nothing to mediation) indistinguishable from the other two effects.

When self-report data are used: A "failure" is defined as a new assault, property destruction or a threatened assault. (Almost identical results follow from a definition including only a new assault.) These results suggest a different ordering of the effects, with arrest still producing the lowest recidivism rate (at 19%), but with advice producing the highest (37%).

Overall, 28.9 percent of the suspects "failed." Still, the results are much the same as found for the official failure measure.

An obvious rival hypothesis to the deterrent effect of arrest is that arrest incapacitates. If the arrested suspects spend a large portion of the next six months in jail, they would be expected to have lower recidivism rates. But the initial interview data show this is not the case: of those arrested, 43 percent were released within one day, 86 percent were released within one week, and only 14 percent were released after one week or had not yet been released at the time of the *initial* victim interview. Clearly, there was very little incapacitation, especially in the context of a six-month follow-up. Indeed, virtually all those arrested were released before the first follow-up interview. Nevertheless, we introduced the length of the initial stay in jail as a control variable. Consistent with expectations, the story was virtually unchanged.

Another perspective on the incapacitation issue can be obtained by looking at repeat violence which occurred shortly after the police intervened. If incapacitation were at work, a dramatic effect should be found in households experiencing arrest, especially compared to the households experiencing advice. . . . It is apparent that *all* of the police interventions effectively stopped the violence for a 24-hour period after the couples were reunited. Even the renewed quarrels were few, at least with our relatively small sample size. Hence, there is again no evidence for an incapacitation effect. There is also no evidence for the reverse: that arrested offenders would take it out on the victim when the offender returned home.

Discussion and Conclusions

... We have found no support for the deviance amplification point of view. The arrest intervention certainly did not make things worse and may well have made things better. There are, of course, many rejoinders. In particular, over 80 percent of offenders had assaulted the victims in the previous six months, and in over 60 percent of the households the police had intervened during that interval. Almost 60 percent of the suspects had previously been arrested for something. Thus, the

counterproductive consequences of police sanction, if any, may for many offenders have already been felt. In labeling theory terms, secondary deviation may already have been established, producing a ceiling for the amplification effects of formal sanctioning. However, were this the case, the arrest treatment probably should be less effective in households experiencing recent police interventions. No such interaction effects were found.

There are, of course, many versions of labeling theory. For those who theorize that a metamorphosis of self occurs in response to official sanctions over a long period of time, our six-month follow-up is not a relevant test. For those who argue that the development of a criminal self-concept is particularly likely to occur during a lengthy prison stay or extensive contact with criminal justice officials, the dosage of labeling employed in this experiment is not sufficient to falsify that hypothesis. . . . The absolute strength of the dosage is irrelevant to this hypothesis, as long as some variation in dosage is present. While the experiment does not falsify all possible "labeling theory" hypotheses, it does at least seem to falsify this one.

The apparent support for deterrence is perhaps more clear. While we certainly have no evidence that deterrence will work in general, we do have findings that swift imposition of a sanction of temporary incarceration may deter male offenders in domestic assault cases. And we have produced this evidence from an unusually strong research design based on random assignment to treatments. In short, criminal justice sanctions seem to matter for this offense in this setting with this group of experienced offenders.

Notes

1. The protocols were based heavily on instruments designed for an NIMH-funded study of spousal violence conducted by Richard A. Berk, Sarah Fenstermaker Berk, and Ann D. Witte (Center for Studies of Crime and Delinquency, Grant #MH-341616-01). A similar protocol was developed for the suspects, but only twenty-five of them agreed to be interviewed.

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