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DOES PATROL PREVENT CRIME?

ACQUISITIONS THE MINNEAPOLIS HOT SPOTS EXPERIMENT¹

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A colleague of mine recently remarked that "much social science consists of taking things my grandmother could have told you and determining whether they are true or not." Perhaps the most obvious thing my grandmother could have told us about urban crime prevention is that having more police on patrol will prevent more crime. Why else would human societies around the world have used that strategy for urban crime prevention for so many hundreds of years?

Until recently, the available evidence was all on grandmother's side, at least in the West. The evidence came first from the police strikes in Liverpool (Sellwood, 1978) and Boston (Russell, 1975) in 1919, in which uniformed police presence was reduced by about 90%. The strikes were associated with massive looting, disorder, and increases in violent crime. Two decades later, when the Nazis arrested the entire police force of

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Copenhagen in World War II, there was a ten-fold increase in both robberies and property crime as measured by insurance reports (Andenaes, 1974, p. 51). In 1969 the Montreal police strike resulted in a sharp rise in serious crime, including an 13-fold increase in burglaries, a 50-fold increase in bank robberies, and other predatory crimes (Clark, 1969). The 1976 police strike in Finland, perhaps the best evaluated of any police strike, also showed significant increases in store robberies, injuries from assault, public disorder and larceny (Makinen and Takala, 1980).

Similarly, a whole series of sudden, short-term increases in police patrol in the United States have been shown to have immediate crime reduction effects. In fifteen of eighteen case studies of these police "crackdowns," an initial deterrent effect was reported (Sherman, 1990). These crackdowns covered a wide range of offenses, from drunk driving to narcotics dealing to prostitution and robbery. Although the initial deterrent effects almost always decayed after a while, an interesting pattern of continued or "residual deterrence" was also found if police withdrew their extra patrols while the initial deterrent effect was still present. Early withdrawal seemed to produce a "free bonus" of deterrence until potential offenders learned the police presence had decreased. Based on these findings, in fact, we have recommended that scarce police resources be allocated through a series of rotating crackdowns, shifting patrols from target to target in order to obtain the greatest "free bonus" effect.

None of this would have surprised grandmother. What would have surprised her is the fact that big city Mayors, police chiefs, and leading scholars in the U.S. disagree with her. She would be even more surprised to learn that the police chiefs' views were derived not from their street

experience, but from a scientific experiment. Perhaps if they had consulted grandmother, she would have told them not to put all their faith in the results of one experiment that was never repeated or replicated.

THE KANSAS CITY PREVENTIVE PATROL EXPERIMENT

In 1974, the Kansas City Preventive Patrol Experiment (Kelling, et al, 1974a) shook the theoretical foundations of American policing. The year-long study found that experimentally manipulated variations in the amount of police patrol had virtually no effects on street crime. Then-Kansas City Police Chief Joseph McNamara concluded that "routine preventive patrol in marked police cars has little value in preventing crime or making citizens feel safe" (Kelling et al, 1974a: vi).

This finding has dominated American police thinking about patrol strategies ever since. It has convinced scholars like Professor Carl Klockars (1983:130), for example, who has written that "it makes about as much sense to have police patrol routinely in cars to fight crime as it does to have firemen patrol routinely in firetrucks to fight fire." Distinguished professors like Jerome Skolnick and David H. Bayley (1986:4) have concluded that "random motor patrolling neither reduces crime nor improves chances of catching suspects." In 1989, Minneapolis Mayor Donald Fraser opposed hiring more police for patrol purposes, arguing from the Kansas City experiment that their presence on the street would be wasted.

Such strong conclusions are premature. The Kansas City experiment was a pathbreaking achievement, a major event in the history of policing. But as Zimring (1978:144) observes, what should have been a pilot study, "repeated in Kansas City and replicated in other settings, instead has

been cited as a definitive work." Moreover, two decades of debate have revealed substantial statistical, measurement and conceptual problems with its research design.

The one-year Kansas City quasi-experiment in a relatively low-crime area attempted to double patrol car presence in 5 patrol beats, eliminate it altogether (except for answering calls) in another 5, and hold it constant in a third group of five beats. No statistically significant changes were observed in crime rates. What few observers have recalled from introductory statistics, however, is that statistical significance is highly dependent upon sample size. Although there were ample differences in the magnitude of crime in different patrol beats, the small sample size of fifteen beats lacked statistical power to find the differences in crime rates unlikely to be chance effects (Feinberg, Larntz and Reiss, 1976; Sherman 1986). The experiment found (Kelling, et al 1974b: 96), for example, that a 300 percent increase in the average reported outside robberies in the reduced patrol areas was not significant, because the large relative difference reflected an absolute difference of less than one outside robbery per month. The observed difference in robbery in Kansas City might have been significant with a sample size of hundreds of patrol beats. But few big American cities have even fifty patrol beats, let alone hundreds.

Professor Larson (1976) has suggested a second major problem with the Kansas City experiment: that the volume of calls answered in the no-patrol area gave it virtually as much patrol presence as the regular patrol area. Because there was no independent measurement of patrol presence in the three treatment groups, we have no idea how much difference actually

occurred between them.

Finally, a conclusion that patrol does not prevent crime was inappropriate because traditional preventive patrol in automobiles has been widely dispersed, even along the main commercial arteries police prefer to frequent. Yet crime and disorder are not dispersed, at least not in U.S. cities with their typically lower population density than is usually found in European or Asian cities. In Minneapolis, for example, only 3% of the addresses generate over 50% of all crime and police calls for service (Sherman, Gartin and Buerger, 1989). These "hot spots" may experience hundreds of crimes a year, sometimes more than one a day. The odds of a widely dispersed police patrol encountering stranger violence in progress are so low that it appears unreasonable to expect it to have much deterrent effect. Over 6,000 hours of evening observations of high crime intersections in Minneapolis, for example, found a mean frequency of patrol cars driving by only once in every 23 hours (Sherman and Weisburd 1990).

Given the concentration of crime in "hot spot" street corners or commercial establishments, the same dosage of patrol can be applied there much more intensely where it may do the most good. American police have increasingly employed such a "directed patrol" strategy over the past two decades, with open air drug markets providing a wealth of targets in recent years. Privately owned premises have also expanded their use of such patrols by off-duty police officers, as well as private security officers, in such locations as shopping center parking lots, fast food restaurants, and garden apartment complexes. Until recently, however, there has been little systematic evidence on the effects of such focused

patrols at deterring stranger violence, or any other kind of crime.

In summary, the Kansas City experiment suffered three major problems in design:

1. inadequate sample size
2. no measurement of patrol intensity
3. dispersal of patrol over too broad a low-crime area.

THE MINNEAPOLIS HOT SPOTS PATROL EXPERIMENT

The Minneapolis Hot Spots Patrol Experiment (Sherman and Weisburd, 1990) attempted to address all three problems. First, it used a sample size of 110, with 55 hot spots in each treatment group. Second, it measured patrol presence in each hot spot by two independent methods: records kept by police officers, and over 6500 hours of systematic observation by our trained civilian research staff assigned to count minutes of police presence in each of the hot spots. And third, it concentrated the experiment in the highest crime hot spots in the city.

Sample. The 110 hot spots were clusters of an average of 15 street addresses selected on the basis of high frequencies of calls for police service for "hard," or predatory, crimes, as well as high volumes of calls about "soft" crime and disorder. Only hot spots which were highly active two years in succession were eligible, to minimize statistical fluctuations. The average number of calls per hot spot in the year before the experiment was 188, or about one every 46 hours. The typical hot spot extended for several addresses, or up to half a block, in all four directions from an intersection, while others were centered on large

apartment buildings. All of them were visually independent of the others, so that a police car in one hot spot could not be seen in another.

Patrol Presence and Measurement. The 110 address clusters were randomly assigned to two groups of 55. From December of 1988 through November of 1989, the Minneapolis Police Department and Crime Control Institute randomly assigned 55 hot spots to receive "extra" patrol, and 55 to receive "normal" patrol, primarily answering citizen calls for service. Almost all "extra" patrol was performed in marked automobiles by uniformed police, who sometimes got out of the car but often sat in the cars. The "extra" patrol group was intended to receive about two to three times as much as normal patrol presence. The goal was to provide three hours per day of intermittent patrol presence between 11:00 a.m. and 3:00 a.m., the highest crime period. Officers left the hot spots to answer radio calls, but returned at unpredictable intervals to write reports, talk with pedestrians, or just (in their words) "sit on the hot spot."

The measurement showed that the goal of differences in patrol presence was accomplished. The actual dosage of "extra" patrol over the year was about 2.5 hours per day according to official police logs. Some 6500 hours of independent observations in both the experimental and control groups during the evening hours (7:00 p.m. to 2:30 a.m.) showed that police car minutes in the hot spots were equal to 12.8% of the observation time of the experimental group, but only 4.5% in the control group. This observed patrol dosage ratio of 2.83 to 1 does not count police car drive-throughs, the addition of which drops the ratio slightly to 2.6 to 1.

Patrol time was fairly evenly distributed within each group, with only 9% of the addresses overall receiving dosage levels close to the mean of the opposite treatment group. The greater inconsistency was across groups over seasons. When total calls were down, there was less (reactive) patrol presence in the control group and more (proactive) presence in the experimental group. When calls were up, the pattern reversed. The ratio of observed police presence time between the experimental and control group varied from almost 6 to 1 in March to 1.2 to 1 in August, but exceeded 2 to 1 in all months but August.

The major departure from the experimental design was a reduction in extra patrol from June 15 to September 1, due to a 50% increase city-wide in calls for service during the summer months. This was further complicated by a change in the computerized telephone calltaking system in October, which altered the system of identification of addresses. Thus the experiment is best analyzed in two ways: the 6.5 months when it was implemented properly, and the full year including implementation failures.

PRELIMINARY RESULTS

Our analysis of the results so far shows, not surprisingly, much stronger effects for the "correct implementation" period, on which what follows will concentrate. Our analysis to date has been limited to two measures. One is the effects of patrol on crimes reported by citizens by telephone, unscreened by police investigation. The other is the effects of patrol on disorders observed by the civilian research staff during the 6500 hours of observation. Future plans include examination of official crime reports and possible displacement effects of crime to other

locations.

"Soft Crime". The majority (76%) of all telephoned crime reports about the 110 hot spots in the experimental year concerned what Professor Reiss has labeled "soft crime:" disputes, fights, vandalism, car break-ins, prostitution, drug-dealing and other minor offenses. The increase in patrol presence at the 55 experimental hot spots produced a 13% reduction (or displacement) in total calls for service about crime, and a 16% reduction for soft crime only. Both of these comparisons are relative to the treatment group, for the first six and a half months. This effect even lasted for six weeks beyond the end of the full implementation period, consistent with earlier findings of "free bonus" residual deterrent effects (Sherman, 1990).

Two interesting observations stand out from these preliminary results. One is that the effects of directed patrol were largely consistent each month, at 100 crime calls deterred per month, as long as the observed patrol time ratio between experimental and control spots remained in excess of 2 to 1. As soon as this ratio dropped (after August 1, 1989), the deterrent (or displacement) effect disappeared. The other striking finding, consistent with the theory of residual deterrence from police crackdowns (Sherman 1990), is that the deterrent effect lasted for six weeks after the directed patrol time in the experimental group was officially cut back by 33%, and actually cut even further according to our observations. From the perspective of the department, this was a free bonus of crime control without the full price of extra patrol.

The price of patrol, of course, is a key issue in using directed

patrol against soft crime. It is often said that directed patrol is effective but too expensive to use on a wide scale (e.g., Schnelle, et al 1977). The Hot Spots experiment provides the first experimentally based estimates of crime control costs per crime, at \$1,000 per crime.² The price of \$1,000 per prevented (assuming no displacement) crime is relatively high, at least compared to the price of responding to a call after a crime occurs. The value of preventing the crime, of course, may be worth the higher price. Depending on how the medical costs, lost wages and property losses from soft crimes are estimated, the cost of each crime prevented could exceed the \$1,000 prevention cost. This analysis depends heavily on the assumption of no displacement, an assumption which still awaits testing. But even assuming partial displacement of those crimes to other locations (Barr and Pease, 1990), the cost of crime may still exceed the price of prevention.

². Assuming no displacement, hot spots patrol deterred 101 crime calls per month through July 31. At 2.5 hours per day X 55 hot spots X 30 days per month, the cost of this patrol in one officer cars is 4125 patrol car/police officer hours. The gross cost per crime call deterred (or displaced) is therefore 41 car/officer hours. The cost of answering 101 crime calls, with an average of two cars per call (a low estimate, given usual backup patterns) for 15 minutes per call (also low) is at least 50 hours. Assuming twice as many officers per call answered, or twice the average length of time involved (more realistic for arrests), the cost to answer 101 calls could be 100 officer hours.

For every 41 hours invested in hot spot patrol, the benefit to the city was therefore 1 crime call prevented (or displaced) and 30 to 60 minutes of officer time saved. Every hour on hot spots patrol saves (or displaces) 1.3 to 8.8 minutes in patrol officer time. There was no net cost increase per crime prevented (or displaced) by directing existing patrol personnel to target hot spots, with an annualized crime reduction benefit of 1200 crime calls. The cost per crime call prevented of hiring additional officers to perform hot spots patrol, at \$25 per hour cost to the city, is \$1,000. Put another way, each officer working 2080 hours per year would be expected to prevent 51 crime calls per year if permanently assigned to directed patrol at hot spots, and if--a very big if--there was no displacement.

Hard Crime. The hard crimes of rape, robbery, burglary, auto theft, stabbings and shootings were reduced in the 55 experimental hot spots by a modest 5%, a difference that was not statistically significant. A separate analysis of robbery reports, however, found a much larger reduction, of over 20%.

Observed Disorder. The most powerful effect of the extra patrol presence was on disorder events observed by our civilian research staff. These events included some criminal behaviors (like fights, drug-dealing and soliciting for prostitution), as well as many non-criminal behaviors that are nonetheless quite harmful to the quality of urban life (Skogan, 1990): persons urinating in public, begging, falling down drunk on the street, playing very loud music from portable radios, loud shouting matches, and anything else that could appear threatening or challenging to a bystander.³

In 300,000 minutes of observations of the hot spots, the 55 "control group" hot spots had disorderly events during 4% of the observed time. In contrast, and the 55 experimental "extra patrol" hot spots had observed disorder for only 2% of the time. While this is an absolute difference of only 2% of the time, it is a relative decrease of 50%. The absolute difference of 2% equals some 29 minutes per day, or over 175 hours (7.3) days of disorder each year. As my grandmother could have told you, she would rather have 7 days of disorder on her block each year than 14 days.

³. These data were analyzed for the period from the beginning of the experiment on December 1, 1988, until August 1, 1989, six weeks after directed patrol was reduced for the summer.

The important point about the reduction in observed disorder--what my grandmother would not have told you--is that it is not attributable just to the time when the police are present. University of Maryland graduate student Christopher Koper's analysis of the data for his M.A. thesis shows that, when all observations are taken together, there is no difference in the disorder rate when the police are present and when the police are absent. Analysis in progress at this writing is testing to see whether that result holds up even when the disorders in progress at the time the police arrive are eliminated from the analysis, and whether the rate of disorder initiations varies. But in at least one way of looking at the data, the 50% relative reduction in observed disorder is due to the "residual" effects of extra police patrol after the police leave, rather than to the extra time when police are present on the scene.

CONCLUSIONS

What can we conclude from one experiment in a very snowy, cold city, with a climate like Moscow's and a crime rate like London's? The properly modest conclusions reflect the role of each single experiment as merely one data point in a much larger distribution of potential observations. If criminology was a physical science, we would hold off on any strong conclusions until the experiment had been repeated in our own laboratory and those of several independent scholars. We would also want to vary the dosage levels of police patrol, to see what the dosage response curve looks like. Would four hours a day produce a 20% crime reduction? Five hours a 30% reduction? Ten hours a 60% reduction? These additional experiments are of special relevance to private property managers, who

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ications, however, we can conclude several things:

There is a difference in the amount of crime occurring in

As my grandmother could have told you, there
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Patrol levels may only produce a modest

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3. Patrol increases can prevent crime and disorder from occurring in hot spots even after police are gone, as a kind of "residual" deterrence.

Where resources for patrol are scarce, it may be more cost-effective to rotate patrol back and forth between different hot spots, rather than concentrating continuous patrol at a smaller number of hot spots.

Additional conclusions, also modest, may arise from further analysis of the data. Finally, they will also result from further experiments. For as my mother could have told you, don't put all your eggs in one basket.

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