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Attacking Crime: Police and Crime Control

ABSTRACT

Growing experimental evidence suggests police actions can reduce crime, increase it, or make no difference, depending on a wide range of conditions. Growing epidemiological evidence suggests police can focus their crime-control efforts much more sharply on high-risk places, times, offenders, and (to a lesser extent) victims. These twin findings suggest the value of a more intensive and sustained program of research and development for testing current and innovative police efforts to control crime. Less than 3 percent of street addresses and 3 percent of the population in a city produce over half the crime and arrests. There has been little testing of alternative police tactics for addressing these high-risk targets. Improving police strategy and tactics for crime control requires much more empirical evidence to specify the conditions under which they succeed or fail. It also requires hard choices about resource allocation and more ideas for how to attack specific crime targets.

Can police efforts reduce crime? The answer is in the eye of the beholder. In the "get-tough" climate of the 1980s, victims' advocates and their lawsuits increasingly demanded more of the police actions they were certain would help reduce specific types of crime, such as more arrests for drunk driving (Jacobs 1988, p. 173) and domestic violence

(Lempert 1984; Sherman and Cohn 1989). At the same time, sociologists and social reformers reacted with increasing stridency about the inability of police to control crime in the face of the "root causes" of crime: family structure, unemployment, and poverty (Currie 1985; Gottfredson and Hirschi 1990). Police themselves partially embraced that view, placing renewed emphasis on the importance of voluntary action by citizens to supplement police efforts at crime control (Brown 1989). A 1989 Gallup poll reported 48 percent of the respondents confident and 50 percent not confident of the ability of the police to protect them from violent crime (Flanagan and Maguire 1990, p. 133).

Within this contentious historical context, police research on crime control has moved forward rapidly. Police have cooperated with unprecedented and powerful research methods, developed massive data bases revealing new insights, and brought research results more explicitly into policy-making. While the sum of the research is no more than a drop in an ocean of unanswered questions, it has provoked major strides in police thinking about crime control. Most important, it has fostered debate on major questions of police strategy, stimulating new ideas and innovations in police attempts to control crime.

The new research has made two specific contributions. One is to focus more attention on the *epidemiology* of specific crime problems, especially the concentrations of problems in small proportions of offenders, places, and victims. This has stimulated new ideas about strategies for setting police priorities among potential targets for crime-control efforts. The other contribution of the research is to probe more precisely the *results* of police work in relation to specific crime-control objectives. This has stimulated new debate about police tactics in reacting to crime and attacking crime problems.

What the research has not done is to settle the debate about the possibility of police efforts reducing crime. Those predisposed against that possibility read the mixed results of this research as evidence for their position. Those predisposed in support of it read the same results as proving the need for more policing. Those employed in doing the research, like most employees, argue that their work has value and should be continued, if only to discover when police work can *increase* crime as well as reduce it. But all would agree that the results so far are scant in relation to the complexity of the questions. If research is ever to make any contribution to crime control, it will come from a painfully slow process of accumulating and replicating results from hundreds of policy experiments and epidemiological studies, the way

of the tortoise and not the hare (Zimring 1976). For police policy re-
search after two decades, the race has only just begun.

This essay examines what has been learned so far about the epidemi-
ology of crime-control targets and the crime-control results of police
work. Section I considers the strengths and weaknesses of the primary
methods of police research on crime control, as viewed from the per-
spectives of opposing camps in the crime-control debate. Section II
examines the new choices of *strategy* in attacking crime raised by
crime-control research. It then considers those strategic options, with
appropriate tactics, in a crime-specific manner with four kinds of tar-
gets: offenders, places, times, and victims.[1] Section III examines police
efforts to control stranger violence, finding diverse evidence that field
interrogations and directed patrol may reduce robbery, but that place-
oriented and victim-oriented problem solving remains underdeveloped.
Section IV reviews the evidence on police control of soft crime, with
more specific findings about the conditions of deterrence versus escala-
tion in offender-oriented strategies. Section V reviews recent research
on the prediction and control of domestic violence. Early evidence of
the predictability of domestic homicide by place was seriously flawed;
and such homicides appear unpredictable from currently available po-
lice data. Minor domestic battery, in contrast, is highly predictable
among chronically violent couples, but reactive arrest has not deterred
it in most experiments and has even made unemployed suspects more
violent in one. Section VI provides briefer overviews of police efforts to
control street-level drug marketplaces, burglary, auto theft, and drunk
driving. Section VII concludes by suggesting how further research
could help identify harmful, helpful, and wasteful police practices in
their efforts to control crime.

I. Crime-Control Research: Methods and Perspectives

Three methods dominate modern crime-control research. One is essen-
tially epidemiological, examining the variations, distributions, and con-
centrations of crime problems in the population. A second is quasi-
experimental, examining the before-after differences in crime rates in
a target population subjected to a new policy intervention. The third
method is fully experimental, randomly assigning alternate tactics or

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[1] Police strategies for a fifth kind of target, communities, are reviewed in Sherman
(1986) and (1990a). The major crime-control evaluations of policing communities pub-
lished since the 1986 review are found in Rosenbaum (1986); Skogan (1988, 1990); Pate
(1989); and Uchida, Forst, and Sampson (1990).

sanctions across a large sample of equivalent units.[2] Each of these methods has different uses, strengths, and weaknesses. All of them make certain philosophical, empirical, and theoretical assumptions about the police role in crime control and what can realistically be expected from research at this stage in its development.

A. Methods—Epidemiology

The epidemiological method helps identify crime-control targets with the greatest potential yield, by showing where the risk of future crime is greatest. Many credit Wolfgang, Figlio, and Sellin (1972) with establishing the model for this kind of criminal epidemiology, by showing that a small proportion of all young males and a somewhat larger proportion of male offenders in a birth cohort produced the majority of all official police contacts. But the approach goes back at least two centuries, to Fielding (1751) and Colquhoun (1795), both of whom identified high-risk locations, victims, and offenders. Fielding stressed gambling houses and gin parlors and proposed to prevent crime more effectively by tighter regulation of those establishments. Colquhoun (p. 25) proposed the creation of a comprehensive registry of criminals as a tool for selecting the most dangerous for ongoing surveillance.

The strength of this method is that it helps distinguish between low-risk and high-risk units in the jurisdiction, whether people, places, or activities. It points, in theory, to targets yielding the "biggest bang for the buck," or the highest return on an investment of resources. It provides a more rational alternative to equal allocation of police resources across the community, without regard to extreme variations in risk. In a military analogy, it identifies key military targets for precision bombing as an alternative to indiscriminate bombing of an entire city.

One major difficulty with using epidemiological data for policy purposes is the risk of false positives, or incorrect predictions of future criminality. A National Academy of Sciences report on criminal careers, for example, shows the dangers of using this method to determine the length of prison sentences (Blumstein et al. 1986). Yet this

[2] A fourth method, cross-sectional correlations among large samples of cities, has been used very little in recent policy research, with the exceptions of Wilson and Boland (1978) and Sampson and Cohen (1988) discussed below (see Phillips and Votey 1972; Tittle and Rowe 1974; Logan 1975). Most of that research, however, fails to adjust for interactions between crime levels and measures of police activity that are hypothesized to affect crime levels (Nagin 1978; Sampson and Cohen 1988). Continuing difficulties of interpreting such models have limited their use and impact, and they are not considered further here.

problem is less severe where the costs of error are also less severe. It is remarkable, for example, how little difficulty false positives pose for public health policy. When the prediction is the basis for telling people to use condoms or to stop smoking cigarettes, few critics object that most unprotected sexual encounters do not transmit AIDS or that most smokers never contract "smoking-related" illnesses. When the prediction is the basis for sentencing people to longer prison terms, the problem of false positives becomes far more serious. Since most police strategies are far less intrusive than prison, however, it can be argued that false positives with crime-risk targets are not a serious problem. Performing surveillance and conducting investigations, for example, should not cause irreparable harm to individual liberty, as long as they are done legally and properly.

A more serious difficulty is that the crime distributions do not really identify targets very efficiently. High-risk units often cannot be identified in advance, especially those in the highest risk levels. This objection, where true, is sound, but the evidence presented below shows it is hardly universal. A second problem is that the distributions are not always skewed enough to make high-risk targeting strategies very profitable. If the top 2 percent of the units only account for 10 percent of the crime, then why bother? The answer, of course, is that a successful strategy could reduce crime by 10 percent, a substantial achievement by our current modest standards of success.

The most serious difficulty has been the cost of examining vast amounts of data on large populations. But recent technological advances in police record keeping have diminished this problem in most communities. Where offense and arrest reports were once kept on paper records and filed chronologically, they are now entered directly on computers. While few police departments have used these data sets to conduct their own epidemiological analyses, it is now relatively easy for them to do so.

B. Methods—Quasi Experiments

Police culture thrives on anecdotal evidence, with an epistemology driven by case-by-case analysis. Quasi experiments fit this mold perfectly but offer considerably more rigor for interpretations of cause and effect. Their major tool is a standard list of alternative hypotheses to examine whenever someone claims that "x" made crime go down. The archetype is the Campbell and Ross (1968) analysis of a Connecticut police crackdown on speeding. This analysis examined whether

the decline in traffic accidents immediately after the crackdown could have been due to regression to the mean, general changes in weather or road conditions, changes in methods of recording accident statistics, or other factors besides deterrence of speeding from stricter enforcement. These analyses can be made more rigorous by comparing the target jurisdiction to a similar jurisdiction subject to the same historical trends but lacking the specific policy intervention—such as other New England states.

The strengths of such quasi experiments include their relatively low cost and the speed with which they can indicate the success or failure of a new policy. Moreover, accumulations of similar cases over time provide a basis for beginning to draw more general policy conclusions, such as in the case of uniformed patrol crackdowns (Sherman 1990a). The limitations include their vulnerability to misinterpretation or misrepresentation. Klockars (1980), for example, shows how a quasi-experimental analysis misrepresented the effects of sting operations in reducing crime. Sherman (1991) suggests that failure to consider rival hypotheses in general will limit police efforts to solve crime problems, allowing self-deception about police effectiveness. Most important, because each quasi experiment is still essentially an anecdote, it is not possible to generalize very far from the results. Yet policymakers unschooled in the cautions required of such research are sorely tempted to draw fairly broad conclusions from a single anecdote, as the state of Florida is now considering doing with statewide regulation of convenience stores based on one quasi experiment in robbery preventions in a single city (Clifton 1987). Quasi experiments are not nearly as powerful a basis for drawing conclusions as are randomized experiments with large samples, yet they are often given equal weight in policy deliberations as having "proven" something does or does not work.

C. Methods—Large-Sample Randomized Experiments

This most powerful method in police research merits special consideration since it has provided more concrete answers about the effects of policing on crime than any other method. It assesses evidence of cause and effect from equal probability (random) assignment of alternative treatments to a large sample of target units (Pocock 1983). The logic is elegantly simple: equalizing most characteristics of two groups prior to giving them different treatments creates high *internal validity* (whether a change in one variable really caused a change in another) by eliminating most rival hypotheses about the cause of any differences

in outcomes (Farrington 1983, p. 260). In this respect the method stands far above quasi experiments, which may always suffer unknown and undetected validity threats from rival hypotheses (Cook and Campbell 1979).

Randomized experiments were invented for testing agricultural strategies and quickly spread to medicine (Fisher 1935). They were first applied to criminal justice policy in the Cambridge-Somerville project, which found (thirty years later) that assigning a "big brother" social worker to high-risk youth had many negative effects on their future lives (McCord 1978). Their first major policy impact came from the Vera Institute's release on recognizance experiment, the Manhattan Bail Project (Ares, Rankin, and Sturz 1963; Botein 1965), an unreplicated 730-case study that became the basis for national adoption of pretrial release without bail on the basis of community ties.

The bail experiment model heavily influenced the board of directors of the Police Foundation, established by the Ford Foundation in 1970 to foster innovation and improvement in American policing. After some initial battles over how best to accomplish that mission, the board decided to spend most of its initial funding of $30 million on a series of what it called policy experiments. The most visible of these was the Kansas City Preventive Patrol Experiment (Kelling et al. 1974), which found no difference in crime from assigning increased and reduced patrol to fifteen different patrol beats. But there were other Police Foundation policy experiments: the San Diego Field Interrogation Experiment (Boydstun 1975), the Cincinnati Team Policing Experiment (Schwartz and Clarren 1977), the San Diego one- versus two-officer patrol car experiment (Boydstun, Sherry, and Moelter 1977), and the Newark Foot Patrol Experiment (Police Foundation 1981). These experiments created a strong model for federal research funding, especially by the National Institute of Justice.

These experiments were not, however, large-sample randomized experiments (Farrington 1983). They were really small-sample quasi experiments, with some attempt to build randomization into the samples of two to fifteen units, usually patrol beats or districts. The randomization did little to improve on the internal validity problems from the many rival hypotheses associated with before-and-after case studies. Nor did it help to increase the weak statistical power associated with the small sample sizes. Not until the Minneapolis Domestic Violence Experiment (Sherman and Berk 1984) did the Police Foundation conduct a medical-style, large-sample randomized experiment in police

crime-control tactics, by randomly assigning arrest and no arrest in 314 cases of minor domestic assault. The finding that arrest produced the lowest recidivism was widely publicized, contributing to policy changes and support for further research (Sherman and Cohn 1989).

Randomized experiments in police crime control had actually been pioneered (Dennis 1988) in 1974, with National Institute of Mental Health funding, by Klein (1986); 306 moderately serious juvenile offenders were randomly assigned to postarrest release versus three other increasingly serious dispositions. That the experiment remained unpublished for so long limited its influence on police policy research, despite its striking finding that released juveniles had the lowest official recidivism rate (with no difference in self-reported delinquency). The lack of national publicity about the study may also have limited its impact on police policy in juvenile arrests and its likelihood of replication.

As the first randomized experiment in arrest, it is fortunate that the Minneapolis Domestic Violence Experiment was replicated in six other cities, with varying degrees of similarity to the original experiment. Three of those experiments have reported findings at this writing (Dunford, Huizinga, and Elliott 1990; Hirschel et al. 1990; Sherman et al. 1991). None of them found a deterrent effect of arrest in the main experimental analyses. This pattern raises major substantive questions about arrest policies addressed in Section V. Equally important is the effect of these mixed results on the future of police policy experiments.

The issue of replication constitutes the major difficulty with policy experiments. In most cases, results go unreplicated, which leaves unaddressed the question of *external validity*, or generalizability to other populations beyond the experimental sample. Randomized experiments are valued for strong *internal* validity; external validity is largely unexplored, even in medical research. For example, a drug found effective in treating AIDS among white males may not be effective among blacks and Hispanics (Kolata 1991). Similarly, arrests for domestic violence in Minneapolis appear to be more effective than arrests for domestic violence in Omaha.

The consequences of this first and only program of replications of a promising result in police crime-control research are still unknown. But the pattern of inconsistent replication results has predictably exposed the methods of policy experiments and crime-control research for police to some attack. These attacks derive not just from the method

but from more basic perspectives and assumptions about the roles of police and research in crime control.

D. Perspectives on Crime Control and Research

Police research on crime control has two basic premises. One is that it is a good thing for police to try to control crime. The other is that research results are in some sense portable from the setting in which they are produced to other settings where they can be applied. Both premises provoke considerable debate.

In recent years, many scholars and police executives have suggested that it is not, in fact, a good thing for police to try to control crime, as distinct from apprehension of offenders and provision of reactive peacekeeping services. They suggest that the police *can, do,* and *should do* little about reducing crime. Many crime victims and their advocates, however, claim that police have great powers, opportunity, and moral duty to prevent crime but often fail to do so out of negligence or ill-chosen priorities. These arguments, then, are respectively theoretical, empirical, and philosophical, with views on the replicability of crime-control programs as a corollary.

1. *Theoretical Perspectives.*   The theoretical argument against police controlling crime is that the causes of routine "street" crime—interpersonal violence, property offenses, and illegal vices—are far too complex for an agency like the police to address. More basic institutions of social bonding, such as the economy, family, schools, and churches, are the primary crime-control forces in society. The effects of police on crime are only marginal in relation to these master institutions, the argument suggests. These institutions, not punishment, control the "root causes" of crime (Currie 1985). This sensible assessment of the relative capacity of social and governmental institutions breaks down, however, when it gets to the subject of the police. It leads otherwise careful scholars into extreme and dogmatic claims about what "cannot be" (Klockars 1991, p. 542). As Gottfredson and Hirschi (1990, p. 270) conclude: "no evidence exists that augmentation of police forces or equipment, differential patrol strategies, or differential intensities of surveillance have an effect on crime rates." This claim is made without even discussing or citing the relevant literature (except to mis-cite Sherman [1983], which actually discusses three separate experiments showing police effects on crime control).

In making the same point, but with more emphasis on the "demysti-

fication" of crime control as mere police marketing rhetoric, Klockars (1991, p. 537) claims that "despite the fact that for the past 50 years police have been promoting themselves as crime fighters . . . the best evidence to date is that no matter what they do they can only make marginal differences in it." The use of the term "marginal" provides ample room for debate. A 10 percent reduction in robbery, for example, would be marginal to some, but substantial to others. Nonetheless, the thrust of the theoretical argument that police methods are essentially irrelevant to the crime rate is that research on such methods is pointless.

2. *Empirical Perspective.* Until recently, the preceding argument was bolstered by the empirical claim that police actually spend very little time on crime (Cumming, Cumming, and Edell 1965; Wilson 1968; Goldstein 1977). This claim arose from descriptions of police calls for service and observations of police activities, using categories that define much police work as unrelated to crime. This led many others to the conclusion that the "real" role of the police is provision of a wide range of community services rather than primarily the control of crime. An agency committed to helping people who lock their keys inside their cars cannot accurately claim to be engaged solely, or even primarily, in crime control. The unstated corollary was that that is how it is, so that is how it must be.

As Greene and Klockars (1991) observe, these descriptions were somewhat misleading and may now be very out of date. They were somewhat misleading because they only examined patrol work and not other police units. They also classified events that could potentially turn into violence, such as "disturbances," as not crime related. They are out of date because they preceded the adoption of 911 and computer-aided dispatch (CAD) systems around the United States, both of which appear to have expanded the volume of calls for service handled by patrol officers on a typical tour of duty. Analyzing CAD data for the city of Wilmington for a one year period ending May 31, 1986, Greene and Klockars (1991, p. 281) concluded that patrol officers "spend nearly half the time they are doing police work in dealing with criminal activity." Similarly, Sherman (1989) found over half the dispatched calls for service in Minneapolis in 1986 to be clearly crime related. While most of the work they actually do in relation to crime may have little connection to its control, the best evidence is that urban police actually do deal with crime, and not cats up in trees, most of the time.

3. *Philosophical Perspective.*   Underlying these theoretical and empirical arguments may be a more basic philosophical distaste for coercive institutions and punishment as a tool of social policy. Police policy research is inescapably linked to values and perspectives on core issues of authority, liberty, democracy, and order. The old joke about the greatest safeguard of American liberty being the incompetence of the police summarizes one position. The joke about America's internationally high crime rate making it the land of the brave, if not necessarily of the free, suggests another. The view that crime control and liberty (or at least legality) are mutually exclusive values has dominated much of the debate about the police. Packer (1968) is the most influential writer on that view, posing crime control and due process as polar opposites. Periodic scandals over police excesses tend to reinforce that view, such as the nationally broadcast videotape of California police brutally beating a restrained suspect in early 1991 (Mydans 1991).

This view leads to a preference not to develop research on more effective crime-control strategies, for fear that police may adopt them. This view is attractive to both left-wing and right-wing libertarians, who distrust the authority of the state. It may also be attractive to those who see the police as an important institution of social service and fear the effects of a crime-control focus in limiting police service activities (Goldstein 1977, 1990; Sparrow, Moore, and Kennedy 1990).

4. *Program Replication.*   These three arguments lead critics to oppose more police efforts at crime control and more research to evaluate those efforts. This conclusion easily leads to a second premise: that what works in one experiment is not very portable to other populations. The complexity of social life is too great for positivistic methods to discover any general laws of behavior, especially across the great diversity of American cities (Marx 1990). And if the external validity of police research is so poor, then policy experiments may be the wrong method to use. Clinical case studies of police practices seeking wisdom, not quantification (cf. Marx 1988), may be more appropriate. The failure of the Minneapolis replications to confirm the initial results seems to support this view.

A more sensitive position is that policy experiments are important but should be carefully replicated before their results are publicized (Lempert 1984, 1989). This position holds that, because the question of external validity is unanswered, it is better not to have police change what they are doing until at least some replications have been completed.

5. *Crime Victims' Perspective.*   Crime victim advocates, in contrast, offer many counterarguments. They take on faith the proposition that police can control crime if they want to and that they have many opportunities to do so. Advocates have a strong philosophical preference for policing and punishment as a tool of social policy, fusing retributivist demands for just deserts to offenders with empirical claims that punishment deters. Through legislation, litigation, and lobbying, they have pressed police for almost two decades to take a more punitive approach to crime or at least to make crime control a higher priority. Advocates for victims of drunk drivers and domestic violence have been particularly effective in redirecting police resources toward those offenses and in provoking more punitive action. They have also argued that the results of police experiments are highly portable, at least where they support the more punitive position. Some domestic violence victims' advocates, for example, opposed the replications of the Minneapolis experiment on the grounds that it was unethical to create new control groups after arrest had already been found to be effective in one site—a position consistent with much medical research practice (Sherman and Cohn 1989).

6. *Policy Research Perspective.*   Police policy research takes a different view on each of these arguments. *Theoretically*, it views the police as a central social institution, less powerful than the family and economy but perhaps as powerful as schools. While no reasonable theory would claim police alone determine crime rates, it is just as unreasonable to write police off as irrelevant without better evidence than we have now. *Empirically*, most work of urban police is already devoted to crime prevention and reaction, including the management of drunks (as potential robbery victims), noise and disputes (as potential assaults), and traffic violators (as potential suspects—see Wilson and Boland 1978, 1981–82). The nature of the work is all in the eyes of the beholder, and most of it can be justified as crime related. *Philosophically*, it is not necessary to like punishment or disvalue due process to value the development and testing of police crime-control strategies. Many of the most innovative police approaches to crime control (Goldstein 1990) employ methods other than punishment, and even police tactics implying the threat of punishment carry no obligation or inherent propensity to violate due process (but see Sherman 1990*a*, p. 41).

Most complex is the question of portability and replications. The research position on this is that it is indeed a matter for research. It is

not a matter to be resolved by theoretical fiat or philosophies of science. The extent to which findings from one jurisdiction, or one offense, or one type of offender can be generalized to others is simply unknown. Only hundreds of experiments and replications with diverse samples can even begin to answer that question. Where results differ from one type of sample (such as cities or offender types) to another, that is not reason to give up the effort to test police strategies. It is merely a challenge to discover what specific aspects of the differences in the samples (or methods) may account for differences in the results. When such characteristics can be specified, the results may appear to be more robust than they first appear. The proposition that "arrest deters spouse abusers in high-employment neighborhoods but not in low-employment neighborhoods," for example, may be far more robust than the less precise proposition that "arrest deters domestic violence."

The question of whether to publicize research results without replication is more difficult, given the concerns about external validity. Three factors argue in favor of doing so. One is that not announcing results creates a bias for the status quo in police practices, for which there is usually no empirical support; one experiment is arguably better than none. A second factor is that a steady stream of these results will only help make police more sophisticated consumers of research, rather than treating policymakers as if they cannot be trusted until findings have received a scientific seal of approval. Policymakers in all fields misuse research to comport with their biases, which is no argument to stop doing research. The most important factor, however, is that publicizing policy experiments helps to foster their replication, the essential but usually ignored step in the process. There is little reason to believe, for example, that the Minneapolis Domestic Violence Experiment (Sherman and Berk 1984) would have been replicated had it not received substantial publicity (Sherman and Cohn 1989).

The basic questions are what to expect from police crime-control research and how quickly it can be produced. For some, this research is little more than "rearranging the deck chairs on the Titanic" as center-city violence becomes more terrible (Marx 1990). For others, the central problem is that insufficient funds have been invested to speed the process along (U.S. Attorney General's Task Force on Violent Crime 1981, p. 73; Zimring 1990). Inconsistent results are an inherent part of scientific investigation. It is impossible to cite any field that made major breakthroughs in curing a social problem within two

decades after starting from scratch. From any perspective, however, research has clearly succeeded in stimulating police debate and innovations in crime-control strategy (Goldstein 1990), for better or worse.

II. Crime-Control Strategy: Hard Choices

The past two decades of research have sharpened several key questions. What is the optimal balance of proactive and reactive police effort? Should police strategies focus on specific crimes or on crime in general? Should police target offenders, victims, times, or places? Should priorities be set within or across types of crime? Every police department answers each of these questions on a daily basis. The answers are often shaped by organizational and political factors. Less often, perhaps, they are also shaped by a concern for crime-control effectiveness.

A. Balancing Proactive and Reactive Strategies

In the quarter-century since Reiss and Bordua (1967) first raised the question of who mobilizes police in any given action, the terms "reactive" and "proactive" have achieved widespread use. The language has helped clarify the hardest strategic choice police face. The choice concerns both philosophical questions of how police can best serve democratic values (Black 1973) and empirical questions about how police can most effectively control crime.

When police action is self-initiated, or proactive, police select their own targets. When police mobilization is initiated by a specific citizen demand, or reactive, police allow citizens to select police targets. All municipal police agencies use both forms of mobilization. The difficult choice is how to allocate personnel time between the two strategies.

Most police patrol time is devoted to reactive mobilizations (Reiss 1971). In recent years, however, there is increasing evidence of new emphasis on proactive strategies, such as decoy units to catch robbers (Wycoff, Susmilch, and Brown 1981), sting operations to catch burglars (Marx 1988), special units to watch repeat offenders (Martin and Sherman 1986), problem-solving strategies to attack high burglary locations (Eck and Spelman 1987), and intensified patrol crackdowns in retail drug marketplaces (Sherman 1990a). There is also more evidence that police agencies vary widely in the extent of their proactivity in patrol tactics generating citizen encounters (Wilson and Boland 1978; Sampson and Cohen 1988). Scholars are increasingly pressed to consider whether a far more proactive police would be good for both democracy and crime control.

1. *Democratic Philosophy.* The philosophical choice is deceptively simple, with reactive policing appearing to be far more democratic than proactive policing. What could be more egalitarian than to give all citizens an equal right to pick the targets of police crime control? Yet absent an equal willingness to use that right, reactive policing becomes anything but egalitarian.

Enormous "selection bias," as statisticians call it, afflicts every choice of police targets by citizens. Many crime victims never call the police, for example, for reasons ranging from fear of retaliation to lack of homeowner's insurance (Flanagan and Maguire 1990, p. 226). Other people falsely accuse enemies and relatives, using police as a tool for private disputes. Reactive policing is completely vulnerable to racial, class, religious, sexual, and ethnic prejudices in citizen decisions to complain about other citizens (Black 1973).

Proactive policing is equally vulnerable to such prejudices on the part of the police. It has the added disadvantage of potentially systematic discrimination against certain ethnic or political groups. Such biases, when they occur, are compounded by the imprimatur of state action. Yet unlike reactive policing, proactive policing has great potential for controlling such selection bias. Using objective criteria for target selection, proactive strategy can come far closer to egalitarian policing, giving all similarly situated individuals equal odds of being selected as targets. Whether using the principles of random selection from a list of congressmen for corruption investigations (Sherman 1983), rank-order targeting of street addresses by frequency of prior police problems (Sherman, Gartin, and Buerger 1989), or some other logic, police can attempt to reduce bias in target selection. While research shows some persistence of selection bias even with fairly objective targeting criteria (Martin and Sherman 1986; Sherman et al. 1989), the result is arguably less biased than purely reactive target selection.

More troubling to democratic philosophy may be any attempt to eliminate citizen power to mobilize police about certain problems. Even if proactive target selection is more egalitarian, it may do less to defuse citizen conflict that may cause crime. Shifting resources from reactive to proactive policing could foster increased vigilantism or "self-help" (Black 1983; Weisburd 1989), which can in turn cause a breakdown of democracy.

Democracy may also break down, however, with the anarchy of high crime rates. Crime control is merely another aspect of serving democratic values, rather than a contradiction of those values. While

control of "street" crime may appear to be easier in nondemocratic regimes, there is little systematic evidence supporting that proposition (but see Greenhouse 1990). Even dictatorships must decide how proactive police should be and answer the same questions about crime-control effectiveness.

2. *Crime-Control Effectiveness.*   Is proactive policing more effective than reactive policing in controlling crime? The available evidence permits no definitive answer. Each distinct type of crime may be more or less susceptible to proactive crime attacks, depending in large measure on how predictable in time and space the crime may be. Police have historically used proactive strategies most often with the most obviously predictable crimes, such as street prostitution, public intoxication, drunk driving, gambling, and (recently) street-level drug dealing. More recent analyses in the spirit of Goldstein's (1979) problem-oriented policing have revealed less obvious patterns of predictability in a wider range of offenses, such as burglary concentrated in a single apartment complex (Eck and Spelman 1987) and robbery concentrated in less than 2 percent of all addresses in a city (Sherman, Gartin, and Buerger 1989).

Predictability alone, however, does not necessarily make a proactive strategy more effective than a reactive one. It is also necessary to have tested and effective tactics available for attacking predictable crime. When confronted by 125 commercial addresses with highly predictable crime problems, for example, a specially selected group of five Minneapolis police officers (the Repeat Call Address Policing [RECAP] unit) was unable to develop effective ways to reduce repeat calls for service at those addresses (Sherman et al. 1989). They succeeded in closing down two high-crime taverns, reorganized security at a bus station, had fences built around a high-crime parking garage, and tried to install better access control at a YMCA. But overall, they could not reduce total calls compared to a control group. The team was more successful at reducing multifamily residential calls for service by working with the property managers on tenant problems (including evictions), but only temporarily; after six months of reduced calls, the effects of proactive intervention withered away.

To the extent that experiments in proactive strategy employ a wide range of tactics (Wycoff, Susmilch, and Brown 1981; Martin and Sherman 1986; Sherman et al. 1989), they may obscure a key point. Effectiveness at crime control may depend more on specific tactics than on general strategy. There may be a substantial crime-control difference

between two proactive tactics, such as robbery stakeouts and target hardening with bulletproof enclosures. There may also be great differences between two reactive tactics, such as hidden robbery alarms or use of 911. Yet there is no difference between two ineffective tactics, regardless of whether they are reactive or proactive.

On empirical grounds, then, the balance between proactive and reactive strategies should depend on the availability of proven specific tactics. If more tactics are proven effective, then more proactive effort may become justified. And the evidence of effectiveness may become even clearer when specific tactics are linked to specific offenses.

B. Specific or General Strategies?

Police strategies are usually expressed in general terms. One strategy is often presumed to work for all offenses. The threat and delivery of apprehension and charging of criminals is implicitly applied across the board. Uniformed patrol and postcrime investigation are the master strategies—both reactive—for every crime from noise to homicide (Colquhoun 1795; Wilson 1963). Just as criminological theory makes few distinctions among types of crimes in explanations of crime (Wilson and Herrnstein 1985; Gottfredson and Hirschi 1990), police have historically made few distinctions among offenses in strategies for controlling crime.

To some, crime-general strategies may be justified by the apparent lack of offense specialization among offenders (Blumstein et al. 1986). To others, police could control crime better if they targeted the specific situations creating opportunities for specific offense types to occur (Clarke 1983). The judgment hinges in part on the choice between a "hydraulic" pressure model of crime causation or an opportunity model (Clarke and Mayhew 1988). Hydraulic models presume that the supply of offenses is determined largely by the supply of offenders and their innate rates of offending (e.g., Merton 1968, chap. 6). Opportunity models presume that the supply of offenders (given their predisposed rates of offending) is only one factor determining crime rates, with the supply of suitable victims and capable guardianship also necessary elements (Cohen and Felson 1979). Hydraulic models logically imply a need for general strategies. Opportunity models imply a need for specific strategies. This strategic choice relates, in turn, to the choice of targets for police strategy.

C. Offenders, Places, Times, or Victims as Targets

Reactive policing deals with incidents, which generally feature victims and offenders intersecting in time and place (Cohen and Felson 1979). It makes little strategic distinction among types of incidents, although this is changing. "Differential police responses" to calls for service are increasingly being used, such as taking a telephone report of a crime or making an appointment for a police car to visit the next day on a low-priority call. These responses explicitly vary by call type and are accepted by callers when they are clearly told what to expect (McEwen, Connors, and Cohen 1986). Such call-screening only delays or rechannels, however, a single strategy of response (investigation), regardless of the elements of the offense. Proactive policing, in contrast, can target any element of a given *pattern* of incidents, especially the most predictable ones (Eck and Spelman 1987). The question then becomes which elements are most predictable.

1. *Places.*   Police have long focused on offenders as the most predictable element in any incident. Yet the places—defined as street addresses or intersections—where crime occurs may be far more predictable than the people who commit crimes. In Minneapolis, for example, our analysis of 323,000 calls to the police in 1986 found that a small number of hot spots produced most of the crime in the city (Sherman, Gartin, and Buerger 1989). Only 3 percent of the places produced 50 percent of the calls to which police were dispatched. This concentration was even greater for the predatory crimes of robbery, criminal sexual conduct, and auto theft. Only 5 percent of the 115,000 street addresses and intersections in the city produced 100 percent of the calls for those usually stranger offenses. These findings have subsequently been replicated in Kansas City (Sherman, Rogan, and Velke 1991; see also Pierce, Spaar, and Briggs 1988).

One cause of that concentration, of course, is the small number of those crimes relative to the large number of places. Even without any repeat locations, for example, all of the robberies could only have occurred at 3.6 percent of all places. But the fact is that with repeat occurrences, they occurred at only 2.2 percent of all places, a 40 percent reduction from the hypothetical number with no repeat locations. If the analysis were restricted to commercial establishments or other high-risk places, the concentration would not be as great, but the identification of targets would still be just as clear. Only 297 addresses, for example, produced over half of all dispatched calls to the Minneapolis police in 1986.

Domestic violence is even more concentrated by place of occurrence

than robbery. While 21 percent of the places in Minneapolis could have had a domestic disturbance call if no place had more than one, only 8.6 percent of addresses actually produced one or more calls—a 59 percent reduction. While it is probably true that multifamily housing units comprised the bulk of those locations, and that much of the city (such as industrial areas) would be unlikely to have such calls, the identification of the 161 addresses with fifteen or more calls per year might stimulate some useful ideas for a proactive strategy against such violence.

Further examination reveals possible routine activities heavily implicated in causing hot spots (Sherman, Rogan, and Velke 1991). For example, analyses of taverns and surrounding areas in Milwaukee and Kansas City shows that, in 1986–89 in Milwaukee, 4 percent of homicides, 5 percent of aggravated assaults, 3 percent of robberies, and 3 percent of all serious violent offenses combined occurred in taverns—which constituted only 0.5 percent of all places in the city. Kansas City showed a similar pattern, with 3 percent of homicide, 6 percent of robbery, 4 percent of aggravated assaults, and 4 percent of total serious violence in its taverns, which constituted 0.3 percent of the places. These crime risks are also connected to violent crime on the block. In Milwaukee, *the location of a tavern on a block increases the relative risk of the block being violent (defined as 20 or more such offenses in four years) by a factor of 3*. Only 15 percent of 5,672 blocks without taverns met the criteria for violent blocks. Some 29 percent of the 625 blocks with nonviolent taverns were violent blocks, but 51 percent of the 170 blocks with at least one violent tavern (defined as four or more violent offenses in four years) were violent blocks. In Kansas City, the comparable ratios for the same time period were 11 percent for no-tavern blocks, 24 percent for nonviolent tavern blocks, and 44 percent for violent tavern blocks.

The fact that most taverns do not have these risks, however, shows the importance of analyzing all factors in each hot spot. Only 12 percent (132) of Milwaukee taverns produced over half of all 2,019 violent offenses in 1986–89, with 40 percent of taverns reporting no violence at all and 85 percent with one or less violent offense per year. The maximum was twenty-three violent offenses in four years. In Kansas City, only 10 percent of the taverns produced half of the 2,757 violent offenses in 1985–89, with 31 percent of the taverns reporting no violence and 68 percent with one or less violent offense annually. The maximum was seventy-five violent offenses in five years.

Police once gave substantial attention to high-crime bars, with fre-

quent field interrogations of customers. Current practices appear more limited to answering calls for service. The continuing connection of some taverns to concentrations of violent crime suggests they should be a central part of any proactive crime-control strategy.

2. *Offenders.*   Annual distributions similar to those for places can be found among all arrestees in a city as an indicator of the active offender population. In Kansas City, Missouri, for example, the 2.7 percent of the estimated 500,000 city-user (as distinct from resident only) population that was arrested two or more times in 1990 produced over 60 percent of all arrests that year (Sherman, Rogan, and Velke 1991). The most frequently arrested 642 persons produced over 10 percent of all 71,461 "body arrests," defined as each event of taking a person into custody regardless of the number of charges, victims, or co-offenders. One hundred persons were arrested fifteen or more times that year, and ten persons were arrested thirty-two times or more. The distributions for nontraffic arrests only are virtually identical.

More important is the predictability of these offenders' levels of activity, assuming that their frequency of arrests has something to do with their frequency of crime commission. Once a nontraffic offender was arrested three times in 1990 (as over 5,000 persons were), the conditional probability (odds) of yet another arrest hit 55 percent. After seven arrests, the 751 offenders who qualified had a 71 percent chance of a further arrest. The curve of the probability of recidivism rises to 93 percent at the nineteenth arrest that year, with an unstable plateau thereafter as the available time at risk to be arrested declines (because most of the year has been used up by the time one has been arrested nineteen times). On a year-to-year basis, the 426 (less than 1 percent of total) offenders who had ten or more arrests (7 percent of all arrests) in 1989 had, as a group, an 82 percent likelihood of being arrested at least once in 1990. Whether this is a high level of predictive accuracy or a high level of false positives depends, of course, on what kind of proactive policing program is being considered. Nonintrusive surveillance, for example, or listing the person on a "most active" list to be considered by investigators would seem to entail little or no cost for false positives or suffering needlessly inflicted on a person from an inaccurate prediction.

Epidemiological analysis of this type can produce startling results. In 1989, for example, Kansas City police arrested by citation one individual 345 times for false alarm violations. While one could discount this problem as a false indicator of repeat criminality, one can also ask

the question of why 345 separate citations were written without the problem being solved. The case is similar to a Philadelphia noise problem generating over 500 police calls in six months (Goldstein 1990, p. 81). Both examples indicate the inherent lack of coordination of reactive effort across the hundreds of patrol officers who can be assigned to each call and the value of proactive strategy based on epidemiological data solely from the standpoint of conserving police resources.

For all the emphasis police have placed on serious offenders, however, there have been remarkably few efforts to develop strategies targeting them. Incidents still drive police actions far more than any analysis of the offenders repeatedly involved in those incidents. Even a unit explicitly aimed at repeat offenders, like the Washington, D.C., Repeat Offender Project (ROP) was highly subjective, and minimally analytical, in its procedures for target selection (Martin and Sherman 1986). Prior to the Kansas City analysis cited above (Sherman, Rogan, and Velke 1991), no police agency ever (to my knowledge) rank ordered all offenders arrested in the past year by total or crime-specific arrest frequency. While such a ranking alone may not be sufficient as a targeting strategy, it would at least provide a check on the subjective tips and leads. This would help insure that targets selected for proactive investigation, or even extra reactive investigation, would be "worth" that effort in potential reduction of offenses.

3. *Times.*   Both places and offenders are highly time-sensitive targets. The concentration of all criminal events by time of day (Barr and Pease 1990, p. 296) may be even more marked within specific targets. For example, a recent analysis of some 1,200 violent crimes over four years in the Georgetown area of Washington, D.C., found that 65 percent of them occurred within a 1,000 foot radius of the "hot spot" intersection of Wisconsin and M Streets, a small portion of the total area. Of those crimes, however, fully half, or about one-third of the total violent crimes in the area, were reported to have occurred between midnight and 3:00 A.M. (Sherman et al. 1991c). Other hot spots, however, are not quite so time sensitive. A one-year analysis of all calls for service in 100 Minneapolis hot spot address clusters (with an average of over 100 calls for service each) found that they were all quite inactive between 3:00 A.M. and 11:00 A.M. Their prime times of high activity in the remaining sixteen hours, however, varied widely across locations.

Places may also become temporarily "hot," as in the immediate aftermath of a burglary. Canadian evidence cited by Barr and Pease (1990,

p. 297), for example, shows that the expected rate of burglary of an address during the first month after it has been burglarized is twelve times the rate without the first burglary. Bars and the immediate vicinity around them are typically hottest around closing time.

Time may also be important for specific offenders. Any offender with ten or more arrests can be analyzed for prime times of day for criminal activity. The times may vary widely across individuals, but that may be irrelevant once a specific person has been targeted for investigation.

The strategic question is the use to which police are willing or able to put these facts. The concentration of calls for service from 7:00 P.M. to 3:00 A.M. is widely known, for example, yet few police departments match that peak load period with peak assignments of personnel. The obvious inconvenience of these hours for police officers leads their unions to fight against making a standard shift to match them. Most agencies persist in using the traditional three eight-hour shifts starting at 7:00 or 8:00 A.M., many with equal numbers of personnel on each shift. This strategy would seem to be impossible to justify on a crime-control basis.

4. *Victims.* Compared to places and offenders, the individual victims of crime are substantially less predictable. In Kansas City in the five-year period 1985–89, for example, 3 percent of the city "user" population produced only 20 percent of all 231,714 victimizations of persons, as distinct from businesses (counting each person as victimized separately by each crime event, even with multiple victims, offenses, or offenders for a single event). All of the individuals with two or more victimizations in that period contributed only 38 percent of the total, in contrast to all persons with two or more *arrests* producing 62 percent of total arrests (Sherman, Rogan, and Velke 1991).

The fact that victims are not as predictable as arrestees does not rule them out as viable targets for crime prevention efforts, however. If the 3,452 people with an average of one or more Kansas City victimizations per year could be successfully "cured" of victimization, for example, it would reduce the number of victimizations by 7 percent. While this gain may appear "marginal" in percentage terms, it would still prevent 22,314 victimizations over a five-year period, or almost 4,500 victimizations per year. The 269 people with ten or more victimizations alone produced 3,372 of those problems, each of which may involve medical costs, lost days at work, and police investigative time (Sherman, Rogan, and Velke 1991).

We know little about how police might intervene effectively with repeat victims. But there is good reason to believe that victimizations are strongly related to "lifestyle." This perspective on routine activities (Hindelang, Gottfredson, and Garofalo 1978) suggests that some people manage their lives in ways that place them at much higher risk of being victimized. The number one victim in Kansas City in 1985–89, for example, suffered thirty-five offenses in 3.5 years: ten burglaries, ten larcenies, four robberies, two assaults, and nine other offenses, including attempted suicide. A white male in his 60s living in a poor black area, he had six home addresses and fourteen arrests (all minor) in the same time period. One of the arrests was for soliciting for prostitution, one for carrying a weapon, and several for violation of the animal leash law. This pattern may be consistent with any of the five reasons Sparks (1981) offered for multiple victimization: victim provocation, victim inability to defend, victim provision of criminal opportunities, victim attractiveness, or criminal impunity because victims are themselves breaking the law.

One of the helpful aspects of an official records repeat victim analysis (which police can now easily do), as distinct from national victimization surveys (which is all scholars have analyzed in the past), is the insight it offers into particular aspects of the victim's lifestyle. The overlap with the victim's arrest record is one example. A more surprising result is that eleven of the top fifty repeat victims in Kansas City for a five-year period were police officers, primarily victimized by assaults presumably committed by persons they were arresting. This may lead some to discount the value of the analysis. But if these officers are in fact suffering at least three (or more) assaults a year for five years, that should be an important flag for a personnel inquiry. Even in a high-crime area, this seems to suggest some problem in the officers' conduct (Toch 1975).

While there is no precedent for police targeting of high-risk victims, the computer systems needed to identify them are rapidly spreading throughout American policing. Once a citywide ranking of victims by frequency can be produced, as Kansas City has done, the only bar to effective intervention strategies will be inadequate ingenuity and lack of resources for testing them with controlled experiments.

D. Priorities Within or Across Crime Types?

The enormous overreach of the criminal law (Morris and Hawkins 1970) beyond the resources available for its enforcement always re-

quires some triage among offenses: discretion not to invoke the criminal justice process. But the conventional triage criterion of offense seriousness has substantial limitations. Police may well achieve better crime control by setting priorities within, rather than across, offense types.

The main limitation with using only a seriousness criterion is that it can virtually legalize less serious offenses. Such de facto legalization can have serious consequences, possibly undermining any general deterrence of the problem. The maintenance of at least token levels for every type of offense may help to make some deterrence a part of a rational choice to commit the offense. How much enforcement is sufficient to accomplish that goal is an empirical question. But it is clearly possible to provide some enforcement for both homicide and minor assaults, speeding and parking violations, shootings and unscooped dog feces, and burglary and jaywalking. With limited enforcement efforts for the less serious offenses, the question becomes how to set priorities within those offense types.

Seriousness alone may also be a bad guide to setting priorities within offense types. Consider the value-of-goods-lost criterion for burglary investigations, which some departments employ. The not unprecedented threshold of $5,000 value may have the effect of decriminalizing burglary in poorer areas. Yet the arrest of a burglar for a $5,000 theft may have no better crime-control effect than arresting a burglar for a $50 theft. If burglary rates are higher in poorer areas already, then a crime-control perspective might concentrate burglary investigation among low-value theft losses, where greater crime control may be possible from each arrest.

Discretion to make arrests—or not—both within and across offense types has been left largely to street officers as individuals, with little structuring of that discretion by management. But there is no reason why guidelines could not be used to set criteria for high- and low-priority arrests. The same logic could even be used to *limit* arrests, especially where arrests compete with patrol presence. Until we know that arrests are better for crime control than police patrol presence in hot spots, it may be quite important to seek nonarrest alternatives to curbing minor disorder. In agencies with substantial time costs for booking each offender, too many arrests can deplete the street of patrol and possibly encourage more offenses than the arrests deter. The evidence reviewed below suggests all the more reason to have management

attempt to structure discretion far more clearly, using criteria based on crime-control effectiveness as well as offense seriousness.

Police managers could, for example, establish an annual arrest "budget," projecting and limiting the number of arrests to be made for each offense type in each area on each watch (Sherman 1990*b*). Officers would then be given goals, limits, and standards for arrests to use whenever there is an evidentiary basis for making an arrest. Such budgeting would help to reveal tradeoffs between offense types, as well as resource choices between preventive patrol and enforcement. Patrol officers, of course, would certainly resist such attempts to restrict their discretion, but they might come to support the idea if they were given some power in the planning process.

III. Controlling Stranger Violence

There is good reason to apply these strategic perspectives first to stranger violence. Citizen rankings of crime severity (Rossi et al. 1974) suggest that stranger violence is the top priority for crime control. It is also the problem police may be least able to control. It is far rarer than other kinds of crimes and therefore much less predictable. The specific chains of causation are harder to identify, and vulnerable links are harder to find. While stranger violence offenders may be deemed most worthy of long-term incapacitation, very few offenders can actually be controlled by that strategy.

Stranger violence encompasses some, but not all, of the legal categories of homicide, rape, robbery, assault, and arson. Applying the estimates of the proportions of each offense committed by strangers (Flanagan and Maguire 1990, p. 247) to the FBI data on the relative frequency of these offenses (Federal Bureau of Investigation 1989), we can estimate the proportion of stranger violence each offense type produces (table 1). The majority of the problem is aggravated assault, which is fairly difficult to predict by place of occurrence, victims, or offenders. Only 24 percent of all 1989 assaults in Kansas City occurred at the 336 addresses with more than one assault, for example. Almost half of all stranger violence is robbery, which is somewhat (but not much) more predictable by place of occurrence. Almost half (44 percent) of the 1989 Kansas City robberies occurred at the 503 addresses (0.31 percent of all addresses) with two or more robberies. These epidemiological data suggest the potential, if modest, value of proactive police strategies aimed at places as targets. Other data suggest the potential

TABLE 1

Distribution of Stranger Violence by Offense Type, 1988

Offense	Total	Crime by Stranger (in Percent)	Stranger Violence (in Percent)
Robbery	542,968	77	40
Rape	92,486	42	4
Aggravated assault	910,092	63	55
Homicide	20,675	12	1
Total	1,566,221		

Sources.—Federal Bureau of Investigation (1989); Flanagan and Maguire (1990).

value of field interrogations aimed generally at all "suspicious" persons on the streets. Perhaps the most effective strategy would be a combination of the two, with strict attention paid to proper methods of field interrogation.

A. Offender-oriented Strategies

A major issue in criminology is the extent to which offenders specialize. Whatever the evidence over a criminal career, it is relatively hard for police to identify currently active robbers from current year arrest data. Only seventy-nine persons had two or more robbery arrests in Kansas City in 1990. Those seventy-nine, in contrast, accounted for 20 percent (173) of all 879 robbery arrests and may have accounted for even more than that proportion of all robberies (for which the clearance rate is only about 10 percent). The question is what effects on crime would result from giving *legal* special attention to such offenders. Previous efforts have not always met that standard.

In the 1930s, when automobiles were still relatively rare, New York police operated "Strong Arm Squads" against violent criminals (Behan 1990), just as other jurisdictions operated "Goon Squads." The target "goons," or offenders, were probably contract workers who committed stranger violence against people who had broken business deals or failed to pay extortion money. They were a very different type of offender from the modern robber or rapist, if only because they were more readily identifiable through criminal networks. Their known faces allowed police squads to roam the streets by the carful, stopping

to jump out if they passed by a known goon. The preventive action consisted of beating the goon up, taking his gun away, and telling him to leave town or at least watch his step. By some accounts, this was an effective way to attack stranger violence.

Detroit maintained a similar tradition well into the 1960s with the "Cruiser" unit (Reiss 1990), also a car full of uniformed officers. Its demise was followed shortly by the creation of the STRESS unit (Stop the Robberies—Enjoy Safe Streets), a group of plainclothes officers that attempted to put stress onto robbery suspects. Its record of killing black suspects (and nonsuspects), if not of reducing crime, made the unit a central issue in the next mayoral campaign (Milton et al. 1977). The results included the election of Detroit's first black mayor and abolition of the unit.

Contemporary attacks on suspected active stranger assailants suggest they are relatively hard to find. The seventy-officer Washington, D.C., Repeat Offender Project began with the goal of focusing constant surveillance on stranger robbers but could not identify enough of them to stay busy. The ones they did identify had the unfortunate habit of going home at night and staying there for 12 to 16 hours, which made surveillance extremely expensive and very boring. ROP officers wound up making more "serendipitous" arrests while watching their targets than actual arrests of the targets. Citing the theory of frequent offense switching among repeat offenders, the unit eventually concentrated on making arrests of reportedly active property offenders, on any charges they could find. The target selection was based almost entirely on tips, with no systematic analysis of repeat arrest patterns like those described for Kansas City. Using informants to lure offenders into burglarizing locations where the officers were waiting, or raiding a home full of stolen property, the Washington ROP unit arrested about half of its targets within one week of being targeted (Martin and Sherman 1986). Whether the active offenders they arrested for property crimes were also the highest risk offenders for robbery and rape, however, is unknown.

One major problem with offender-oriented strategies may be the failure to recognize important patterns of co-offending. While the lone offender model may be generally correct for adult robbers, juvenile robberies are more often committed by groups of two or more (Reiss 1988). Locking up one offender who participates in one or several groups may do nothing to reduce the total number of crimes, given the continuation of the co-offending groups. The one offender, one

crime assumption is clearly inadequate as a causal model for stranger violence.

Both juveniles and adults may also be vulnerable to the suggestive influence of "Typhoid Marys," or people who accumulate high numbers of co-offenders (Reiss 1988). These "carriers" are not ringleaders of an ongoing group as much as idea men in a social network, people whose presence in any particular group may tip the balance of action toward committing a violent offense. While the evidence for the existence of these spreaders of criminality is not strong, it can be readily assembled in any police agency with computer systems linking individual rap sheets to arrest reports naming co-arrestees. Rank ordering of all rap sheet subjects by total numbers of co-offenders could reveal enormous disparity, with a small number of offenders being linked to over half of all other offenders. Such persons could just as easily be the unlucky ones who are always talked into crimes resulting in arrests by smarter ringleaders who do not get caught. But the possibility of their being true recruiters warrants closer examination.

If a police department could identify Typhoid Marys of stranger violence in this fashion, they would clearly become a high priority target for proactive investigation. Given the same number of offenses by two different offenders, one a lone wolf and the other a Typhoid Mary, there could be far greater crime reduction from incarcerating the latter. This could occur from the spinoff effects of the carrier's recruitment of new offenders into a first offense of stranger violence. Given that experience, the co-offender could go on to commit other offenses without the carrier, with or without other co-offenders. The lone wolf, in contrast, does not seem to spread crime around, making less contribution to the total volume of stranger violence.

Field Interrogations.   Not all offender-oriented strategies that reduce robbery are focused on specific names or even on that specific crime of stranger violence. Indeed, there is substantial evidence that a police department's level of proactive traffic and disorderly conduct enforcement can affect its robbery rate. Wilson and Boland (1978), using a simultaneous equation model across thirty-five U.S. cities, first reported an inverse relation between the number of moving violations per officer and the rate of robbery victimizations per capita. They theorized that aggressive traffic enforcement was an indicator of the general level of active surveillance with which police patrolled the streets in any given city, which in turn was a function of municipal culture and predictable by various characteristics of local government.

They hypothesized that more intensive watchfulness by police on the streets was an effective deterrent to robbery, either indirectly by apprehending more actual or would-be robbers, or directly by affecting community perceptions of the likelihood of arrest. While admitting their evidence was not as conclusive as a randomized experiment, Wilson and Boland still found strong support for the theory.

Further research has refined and extended the conclusion. Jacob and Rich (1981) challenged the conclusion on the grounds that it could not be replicated with longitudinal data *within* cities on traffic citations and robbery rates. Wilson and Boland (1981–82) replied that, among other things, changes over time within cities did not approximate the differences across cities in patrol style and that Jacob and Rich failed to use the traffic citations per officer as a measure of proactive patrol. More important was the replication and extension of Wilson and Boland by Sampson and Cohen (1988), using a more complex design with the much larger sample of 171 cities. They combined traffic enforcement and disorderly conduct enforcement per officer as a single indicator of "aggressive policing," defined as a tendency to invoke the law even for minor offenses. They also employed elaborate controls on the urban social structural characteristics most highly correlated with street crime rates—the only cross-sectional police and crime study to do so. Police aggressiveness was inversely related to both robbery offenses per capita (but not burglary) and the prevalence of robbery offenders in the cities. Police behavior was almost as powerful an influence on crime as the city's divorce rate.

Sampson and Cohen (1988) also found complex effects of police aggressiveness on robbery offending rates by age and race. Citywide aggressiveness had more powerful effects in reducing black robbery rates than white rates, for both juveniles and adults. Yet further analysis showed that police aggressiveness toward whites (drunk driving and disorder enforcement rates per capita whites), who comprise the majority population in most of the cities, had an independent effect in reducing both white and black adult robbery offending rates. Similarly, police aggressiveness toward blacks had an independent negative effect on adult robbery offending rates among whites and blacks. But black adult robbery rates are more strongly associated with proactive policing of *whites* than of blacks. Thus police aggressiveness has a "pervasive effect" in reducing robbery rates, at least among adults. The effects are generally weaker among juveniles but show more impact on black juveniles than on whites.

The troublesome aspect of these findings is that they run directly contrary to the "friendly policing" thrust of current police innovations (Skolnick and Bayley 1986), designed in part to counteract the kind of aggressive policing Sampson and Cohen find to be so effective in reducing robbery. Proactive field encounters with suspicious persons received very bad press in the 1960s and early 1970s, in large part because of overtones and evidence of racial discrimination and harassment (Rossi, Berk, and Eidson 1974). Field interrogations and traffic stops clearly have a high potential for officer rudeness and racial slurs. Even worse, the police culture associated with proactive patrol has been blamed for causing extreme cases of brutality (Nazario and Jefferson 1991).

Yet there is no reason why legalistic traffic and disorder enforcement needs to be offensive or insensitive to persons stopped and questioned. With proper training and supervision in a community relations–conscious police culture, field interrogations need not provoke community hostility, as the San Diego Field Interrogation Experiment suggests (Boydstun 1975, pp. 54–55). This 1973–74 quasi experiment compared three patrol areas: one area where all field interrogations were suspended for nine months and then reinstated, one area where field interrogations were only conducted by officers specially trained to reduce friction with citizens they stop, and one area where there was no change in field interrogation practices only. Total reported crime was virtually unchanged in the two areas retaining field interrogation, but it rose significantly from seventy-five "suppressible" crimes a month in the baseline period to 104 per month in the experimental period, dropping again to eighty-one crimes per month in the follow-up period. Suppressible crimes were defined as those in theory most sensitive to field stops: robbery, burglary, theft and auto theft, assault, sex crimes, malicious mischief, and disturbances. Citizen surveys in the three areas found that reducing field interrogations had no effect on community attitudes toward police. Interviews with interrogation subjects found most favorable attitudes toward officers who had been given special training in field interrogations, although observations of trained and untrained officers found no differences in their behavior.

The results suggest that, with proper organizational development, it should be possible to do proactive field stops without alienating those persons stopped, with the benefit of controlling suppressible crime by a margin of about 20 percent. The cost of doing so is about 5 percent of a patrol officer's time. The difference across areas in dosage, as

reported in police logs, was 22 minutes of field interrogations per offi-
cer per shift in the control area compared to 0 minutes in the experi-
mental area. There may also be community relations costs in such a
practice. The U.S. Supreme Court's subsequent ruling (*Kolender v.
Lawson*, 46 U.S. 352 [1983]) that the San Diego Police had collectively
and discriminatorily harassed a black man with a "dreadlock" hair style
who liked to walk around town suggests the potential difficulty with
any proactive policy aimed at "suspicious" persons as targets. But this
is again a problem that might be solved with more specific guidelines,
especially if they are race-neutral in design and effect.

An unmeasured aspect of the experiment was the kinds of places in
which the field interrogations were conducted. Any guidelines for the
selection of a "suspicious" person must entail a judgment about persons
in relation to the places where they are found. In that sense, field
interrogations may also be seen as a place-oriented strategy for crime
control.

B. Place-oriented Strategies

Some stranger robberies are highly predictable by place over a one-
year period, but most robbery locations have had no recent prior rob-
beries. Even where robbery is predictable, it is impossible to predict
when, in the short run, robbery will occur, absent an informant's tip.
Of the 161 addresses with three to six robberies in Kansas City in
1988, for example, 116 of them (72 percent) had at least one robbery
in 1989. This compares, however, to 1,947 addresses robbed in 1989
that had no robberies in 1988. While the latter group was only 1
percent of the places with no 1988 robbery, it was also 77 percent of
all 1989 robbery locations. Thus, only 5 percent of those locations
were highly predictable as robbery targets.

The most common place-oriented antirobbery strategy has been
stakeout units, which were discredited in many cities for the high rate
of shootouts with and deaths of robbers they produced (Milton et al.
1977). They also seem fairly inefficient in their consumption of police
time, even with informant tips, although this question has not been
examined systematically; epidemiological data alone are clearly inade-
quate, with a one-year time period covered by the prediction. One
unintended consequence of stakeout units was to reveal an unknown
portion of the robbery rate as fraudulent cover-up of employee theft.
On several occasions in Kansas City in the 1970s, for example, off-site
stakeouts of commercial premises were conducted without notification

of the store clerks. While plainclothes police sat observing no one go into the store, the clerk phoned in a robbery at the premises. When the uniformed officers responded to investigate, the stakeout squad came in. Searches of the clerks revealed the money missing from the register (Joiner 1990). Similar discoveries were made in Washington, D.C., with robberies of both convenience stores and dry-cleaning delivery trucks (Wilson 1990a).

This phenomenon is important to interpreting the effects of more recent innovations in robbery control through situational crime prevention. Cash control devices in stores, bulletproof barriers for lone gas station attendants, and the requirement of two clerks after dark in certain stores may all defeat *fraudulent* robbery as well as real robbery. Where unwitnessed robbery is at stake, it will be difficult to determine how much of it is real or fraudulent. But where environmental design makes it much harder to lie credibly about the occurrence of a robbery, any reduction in reported robbery should not be uncritically accepted as evidence of control of stranger violence.

One of the most influential recent place-oriented strategies to reduce robbery (Goldstein 1990, p. 80; Moore, in this volume) falls victim to this kind of interpretive problem, among others. In Gainesville, Florida, police attacked a rapid rise in convenience store robberies in the mid-1980s by a thorough analysis of the epidemiology of the problem, including its prevalence and frequency across all stores in the community. They analyzed the situational features of the crime and concluded that robbers prefer to wait until they are alone with a clerk before committing the robbery. This conclusion, of course, was based on lone clerks' accounts of how the robberies occurred. It was also supported by a University of Florida survey of imprisoned convenience store robbers the police department commissioned, which found that robbers took the number of clerks (as well as their size and gender, even if alone) to be very important factors in their decision to rob a store.

The police chief used this extensive analysis (Clifton 1987) to support a recommendation to the city council for requiring convenience stores to employ two clerks in each store open after dark. After it was passed, the department claimed a 65 percent reduction in convenience store robberies was attributable to the law. Problem-oriented police officers in other cities, such as Minneapolis, accepted the results as valid, and the *New York Times* cited it on the editorial page as a success story (Anderson 1990). The Florida Legislature was scheduled in 1991 to consider adopting a statewide two-clerk requirement, and other juris-

dictions have considered adopting the law based on the success of the Gainesville results (Richman 1990).

Unfortunately, closer inspection by a former Washington police chief retained by the National Association of Convenience Stores (Wilson 1990*b*) found little compelling evidence for the effectiveness of a two-clerk rule, which took effect four months *after* the robberies had already dropped precipitously. He also found several plausible rival hypotheses explaining the reduction in convenience store robberies. Chief among these is an almost identical pattern of rise and fall in convenience store robberies in the Alachua County Sheriff's jurisdiction surrounding Gainesville, even though the county passed no requirement for using two clerks. County police do report, however, that three convenience store robbers, highly active in both the city and county, were apprehended and continuously incarcerated four months prior to and long after the two-clerk rule took effect. The date of their arrests corresponds exactly to the sudden drop in convenience store robberies in both jurisdictions. The robberies merely continued at the same low rate in both areas after the two-clerk rule took effect, with no additional drop in rate.

Additional rival theories for the drop include legislative requirements for a set of additional security features, such as cameras and cash control, which went into effect the same month the robbers were apprehended (by stakeout methods) and robberies declined. The difficulty of concocting a fraudulent robbery claim with a co-employee present is another rival theory, as is simple regression to the mean after a sudden eighteen-month peak (Campbell and Ross 1968).

A more effective place-oriented approach to robbery reduction may be bulletproof booths, which have been widely adopted in gas stations, subway change booths, banks, and liquor stores. In 1987, the Maryland legislature enacted a law requiring their use at all gas stations. The governor vetoed the law, however, and appointed a task force on retail security to study the matter. The task force could find no evidence that the booths were effective and did uncover evidence of pouring and igniting gasoline in the cash hole as a technique for injuring clerks. Examples of robbers taking customers hostage have also been found. The question awaits careful experimentation for resolution.

1. *Preventive Patrol.*   The most basic police strategy against public stranger violence has been preventive patrol. This strategy has been given little credence since the Kansas City Preventive Patrol Experiment found increases in patrol ineffective at reducing crime (Kelling

et al. 1974). This quasi experiment in a relatively low-crime area at-
tempted to double patrol car presence in five patrol beats, eliminate it
altogether (except for answering calls) in another five, and hold it con-
stant in a third group of five beats. No *statistically significant* changes
were observed in crime rates, although there were ample differences
of magnitude that the small sample size lacked statistical power to test
(Sherman 1986). Moreover, Larson (1976) has suggested that the vol-
ume of calls answered in the no-patrol area gave it virtually as much
patrol presence as the regular patrol area. Hence the frequent conclu-
sion 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 fire
trucks to fight fire" (Klockars and Mastrofski 1991) is not supported
by the Kansas City experiment and is contradicted by numerous other
quasi-experimental results (Sherman 1990a).

Perhaps the most compelling evidence for the preventive effects of
patrol on stranger violence is the increase in such crime during the
major police strikes for which good data are available. The mayhem
during the Liverpool (Sellwood 1978) and Boston (Russell 1975) police
strikes in the late summer of 1919 included looting, armed robbery,
and (in Boston) numerous rapes and gang rapes (Russell 1975, p. 137).

Admittedly, this increase might be written off to the general political
instability of that period rather than to a loss of deterrence from police
patrol. Yet, the political instability argument is weaker when used to
explain away the crime increase during the seventeen-hour Montreal
police strike of October 7, 1969 (Clark 1969): 13.5 times the normal
hourly rate of burglaries, 50 times the normal hourly rate of bank
robberies, and widespread looting by "ordinarily disciplined, peaceful
citizens," although there were no reported rapes (Clark 1969, p.176).

The political instability argument completely fails to explain away
the 50 percent increase in store robberies and 42 percent increase in
hospital admissions for violent injuries at a large Helsinki clinic during
the seventeen-day Finnish police strike of February 1976 (Makinen and
Takala 1980). The Finnish "experiment," in an era of long-term politi-
cal stability, is even more compelling because of very cold weather, a
major national fixation on the televised Winter Olympics, and the fact
that the data evaluating crime rate changes came from sources indepen-
dent of the police. Even without the looting and collective behavior of
the more famous strikes, Finland's loss of 86 percent of its police force
and virtually all of its uniformed patrol force was accompanied by a

clearly documented increase in the rate of violence and stranger vio-
lence. Similarly, the Nazi arrest of the entire Copenhagen police force
in 1944 was followed by a tenfold increase in both robberies and larce-
nies reported to insurance companies (Andenaes 1974, p. 51).

Drawing conclusions from Kansas City is also inappropriate because
traditional preventive patrol in automobiles has been widely dispersed,
even along the main commercial arteries police prefer to frequent. Yet
as I noted earlier, stranger violence and crime in general are highly
concentrated in a small number of addresses (Sherman, Gartin, and
Buerger 1989). The odds of a widely dispersed police patrol encoun-
tering stranger violence in progress are so low that it appears unreason-
able 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).

2. *Hot Spots Patrol.*   Given the concentration of stranger violence in
hot spots of crime, the same dosage of patrol can be applied much
more intensely where it may do the most good. 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, 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.

The Minneapolis Hot Spots Patrol Experiment (Sherman and
Weisburd 1990) begins to fill that gap. From December 1988 through
November 1989, the Minneapolis Police Department, Crime Control
Institute, and Rutgers School of Criminal Justice conducted a random-
ized experiment in directed patrol in marked automobiles by uniformed
police at 110 specially selected hot spots of crime. These locations were
clusters of an average of fifteen 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 that were highly active two years in succession were
eligible, to minimize regression to the mean. The average number of
calls about both types of crime in the baseline year before the experi-
ment was 188, or about one every forty-six 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 multiple dwellings. All of them were visually independent of the others, so that a police car in one hot spot could not be seen in another.

The 110 address clusters were randomly assigned to two groups of fifty-five (in five statistical "blocks" based on call frequency), one of which was designated to receive increased patrol. 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 actual dosage over the year was about 2.5 hours per day according to official police logs. Some 6,500 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 cars were present in the hot spots for 12.8 percent of the observation time of the experimental group, but only 4.5 percent in the control group. This 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 percent of the addresses overall receiving dosage levels close to the mean of the other 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.

David Weisburd's preliminary analysis of the call data for stranger violence suggests that directed patrol had a modest deterrent effect on robbery in the hot spots, although there was no significant deterrence of hard crime calls generally (Sherman and Weisburd 1990). Subsequent analyses will explore the extent to which any crime reduction in the experimental hot spots was attributable to displacement to other locations (Cornish and Clarke 1987; Barr and Pease 1990). Scholars will also need time to reflect on the findings and their implications, just as they have on past experiments. But if displacement and other problems are found to be minor, then directed public (though perhaps not private) police patrol in hot spots may be a viable, if expensive, robbery-control strategy.

C. Victim-oriented Strategies

Police have attempted relatively few victim-oriented strategies against stranger violence, apparently with good reason. There is very little repeat victimization for robbery (including larceny purse snatch) found in official crime reports. Over the period 1985–89 in Kansas City, only 2.72 percent of the robbery victimizations were accounted for by the 127 victims who were robbed three times or more. However, those people had seven times the risk of further robberies of people who had not been robbed before. While it makes sense to devote some attention to such a high-risk group, the crime-control payoff citywide would be very marginal indeed.

The most common victim-oriented strategy for stranger violence is police lectures to concerned groups. Subjects include how to prevent crimes through simple precautions, many of which reflect common sense (such as looking in the back of a car before entering it, and always locking a parked car). More problematic topics include how to respond to a criminal attack once it begins: what to do if a rapist attacks, or if you hear a burglar in your home, or if a gun is pointed at you.

One problem with such lectures is that they are based on question-able beliefs about the consequences of taking the various options rec-ommended or described. Victims of would-be rapists are more likely to escape if they offer nonforceful resistance, for example, but only if they live to answer the survey questionnaire (Skogan and Block 1983). The relative rate of death for resistance and cooperation remains un-known. The consequences of forcefully resisting armed robbers, how-ever, are somewhat clearer, in that it increases risks of physical attack and injury (Skogan and Block 1986), and standard police advice is to cooperate. More subtle points about tone of voice, eye contact, speed of movements, and so on are also made in these lectures, but with questionable empirical foundation.

The major problem with this line of strategy is that it remains virtu-ally unevaluated. No one has any idea whether these lessons reduce injury rates or increase them, and both possibilities are quite plausible. The major obstacle to evaluation, fortunately, is that the base rates of stranger violence victimization after receiving this instruction are so low. Reliable estimates of the impact of such instruction, even with a randomized design, would require a very large sample to be observed for many years.

More controversial, but also better evaluated, is the practice of police in some communities of recommending that potential victims—

especially women—buy guns. Recent public health research (reviewed in Cook 1991) suggests that the presence of guns in a home is a strong factor in firearms deaths, including accidents and suicides. But this does not alter the view many hold about the need for guns in self-protection, as well as the evidence some researchers have mustered to support the hypothesis (see Cook 1991). While police executives are increasingly in favor of regulating gun sales to bar access to criminals, many are of mixed minds about the virtues of honest citizens possessing guns to fight criminals.

As long as there is no strong message from police professionals against keeping guns for self-defense, purchases for that reason seem likely to increase. And as gun density grows, the overall homicide rate goes up, according to increasing evidence (Cook 1991). It should follow that police could fight serious violence by actively discouraging most private ownership of guns as merely throwing oil on the fire, even if the effect is that some crimes cannot be rebuffed by armed citizens.

IV. Controlling Soft Crime

Stranger violence dominates the headlines, but the far more frequently occurring "soft" or minor crimes (Reiss 1985) dominate police workloads. They also have a dominant role in generating public fear of crime and contributing to residential and large corporate flight from center cities (Skogan and Maxfield 1981). Soft crime may also attract hard crime, by communicating to potential assailants that an area is "out of control" (Wilson and Kelling 1982). One of the strongest correlates of calls about robbery in Minneapolis hot spots is calls about drunks (Weisburd, Maher, and Sherman 1991).

Soft crime embraces a wide range of behavior with similar community consequences. Car break-ins leave shattered glass for all passersby to see. Drunks use foul language, urinate in public, or collapse on the sidewalk. Teenagers use "boom boxes" to disturb a quiet residential block as they walk by late at night. A man beats his wife in a parking lot. Each of these events suggests that anarchy is around the corner and underlines the continuing threat of crime in the local environment.

Police strategies against soft crime are often not explicit because most of the offense categories at stake are not serious enough for priority treatment. Soft crimes are often good examples of how some offenses have been effectively decriminalized, with virtually no police attention. Yet there are some things police do already that can have effects on soft crime.

A. Offender-oriented Strategies

Field interrogations, for example, appear to have substantial impact on soft crime. Boydstun (1975, p. 33) found significant increases in a range of soft crime in the San Diego quasi-experimental comparison of one area where field interrogations were suspended for nine months, compared to two comparison areas where they were continued. The soft crimes deterred during the baseline and follow-up periods, but that increased when interrogations were suspended, included petty theft, grand theft, sex crimes, malicious mischief, and disturbances.

Antiloitering "sweeps," invoking ordinances against obstructing pedestrian traffic, produced a decline in recorded crime (but not surveyed victimization) in an experimental area in Newark, New Jersey (Pate et al. 1986). The procedure of ordering young males to clear the sidewalk, then arresting them minutes later if they fail to leave, has been attacked as unconstitutional by scholarly observers (Skolnick and Bayley 1986, p. 199) but not by the courts. Other police procedures employed in this area during the experimental year, which may also have contributed to the measured crime-reduction effect, included automobile checkpoints, random police inspections of public transit buses, and foot patrol. Which of these, if any, was more important than the others in producing the effect is impossible to tell from the design. But it is instructive that the same crime-reduction effect was achieved with much more positive citizen attitudes in a similar area that received a "community policing" program of a storefront, door-to-door police visits, and a neighborhood cleanup as well as street sweeps and other intensive enforcement (Skogan 1990, p. 119).

Newark was also the site of an unevaluated police effort to combat predominantly soft youth crime through truancy enforcement (Williams 1983). Working in the early 1980s with specially powered truancy officers and a school bus, police officers cruised the city during school hours looking for school-aged children. Those picked up were taken to a special detention center–study hall and kept until school hours ended. Parents were also contacted, although without much optimism. The primary objective was to incapacitate the truants from daytime burglary and shoplifting.

Another youth-focused but unevaluated strategy against soft crime was the Minneapolis RECAP unit's enforcement of the state curfew law. Plagued by frequent soft crime calls at 7-Eleven's and other youthful gathering places, the RECAP unit organized a "sweep" of the entire neighborhood surrounding these locations looking for underage people

after 10:00 P.M. While it is not clear that the sweeps reduced calls for service at the repeat call locations, over two-thirds of the apprehended juveniles had prior arrests. It seems plausible that sustained curfew enforcement pressure, where legal, could incapacitate the youths from being out at night, perhaps reducing a wide range of soft crime as well as drug abuse and other evening activities (Buerger 1991).

Foot patrol officers have long used a variety of techniques for managing panhandlers, drunks, the homeless, and the mentally ill on their beats. As Bittner (1970) and Wilson and Kelling (1982) have shown, police effectiveness in controlling the potential for soft crime by these marginal populations depends heavily on knowing them as persons. With verbal orders or persuasion based on trial and error with each local "character" in the officer's repertoire, a great deal can be prevented with minimal effort and very rare use of force or arrest. The conventional strategy of wide-ranging automobile patrol, however, greatly limits the capacity of officers to gain this depth of experience with specific individuals.

The use of arrest and prosecution for soft crime may have different consequences than patrol does and different consequences for individuals than for communities. While Boydstun (1975) and Sampson and Cohen (1988) find deterrent community effects from legalistic policing of soft crime, Klein (1986) found crime-*increasing* effects from legalistic treatment of juveniles arrested for primarily soft crimes. This three-month experiment in 1974 was conducted in nine of the eighteen police stations in an unnamed West Coast metropolis, randomly assigning 306 arrested juveniles to four conditions in the juvenile division office: release with no further action, referral to social services, referral with subsidies to cover the costs of social services, and petition (prosecution) to juvenile court. Initial cheating by police on the random assignment procedures was detected and apparently overcome. Follow-up interviews with the juveniles were conducted nine months later with a 59 percent completion rate. Official data on repeat crime were collected for 100 percent of the sample for twenty-seven months after random assignment.

The more complete data set showed the more startling results. While there were no significant differences across the groups in subsequent self-reported crime, there were significant differences in official recidivism. The prevalence of recidivism at fifteen months varied directly with the formality of the treatment, from 37 percent among those juveniles released to 63 percent among those prosecuted. While this

experiment does not show that the *arrests* had a crime-escalating effect, it did show that police decisions to treat juveniles legalistically had a clear effect of increasing crime.

Clearly much more needs to be learned about the effects of offender-oriented patrol and arrest strategies on soft crime, preferably through additional randomized experiments. The possibility that police may be increasing crime or hurting community relations with these methods is too serious to ignore.

B. Place-oriented Strategies

Just as there is more soft than hard crime overall, the frequencies of soft crime in specific locations are much higher than frequencies of hard crime. Targeting police resources at hot spots of soft crime is thus even more efficient than targeting at hot spots of hard crime, although they often turn out to be the same places. Two of the most popular place-oriented strategies are problem solving and directed patrol (Moore, in this volume).

1. *Problem Solving.*   Eck and Spelman (1987) describe the Newport News experience with the general approach of "problem-oriented policing" that has been used in many cities to deal with disorderly places (Goldstein 1990). Based on the concepts of Goldstein (1979), and using a strategy similar to the RECAP model (Sherman 1986), problem-oriented policing tries to diagnose the causes of soft crime concentrations at specific locations, do something about them, and then follow up to see if the strategy has been effective.

In Newport News, for example, the approach was used with prostitution as a cause of the problem of robbery in a four block area of downtown. Diagnosis showed that about half the robberies were linked to the persistent prostitution problem with twenty-eight "regulars" in the same area. The multiple-tactic solution began with showing the local judges the connection between prostitution and robbery and obtaining their agreement to increase the sentence for prostitution to two months. In addition, they agreed to suspend ten more months with the condition that the prostitutes stay away from that area or else go back to jail. Police also put enforcement pressure on area bars and rooming houses, using regulatory statutes aimed at control of prostitution. After six months, only six of the twenty-eight regulars were still found in the area, and robberies (of an unspecified frequency) had dropped by 43 percent (Eck and Spelman 1987, p. 80).

In Houston, problem-oriented policing led to the diagnosis of a park

as an open-air drug market due to lax supervision at night (Brown 1989). Mobilizing sufficient resources to close down the park at night, police were able to dry up the local drug market.

In Minneapolis, problem-oriented policing led the RECAP unit to a diagnosis of two taverns as disorderly from serving intoxicated patrons, as well as encouraging drug sales. The solution was to have their liquor licenses suspended; both were later torn down for urban renewal, with no new liquor license granted for another location (Buerger 1991).

All of these examples constitute quasi experiments in problem solving for soft crime. Unfortunately, they suffer from strong selection biases, both in picking the problems to begin with and then in reporting success in the literature. Buerger (1991) reports detailed case studies of about 100 attempts by the Minneapolis RECAP unit to solve soft crime problems at specific addresses selected solely on the basis of high total call volume. There are as many failures as successes in the casebook, with opportunities to reflect on what might have been done differently to insure greater success. Perhaps the leading critique is that there were too many diverse problems and individuals at each address, and too many addresses for each officer (about sixty over one year) to take on successfully, even as a full-time assignment (Buerger 1991).

Whatever the reason, the first controlled experiment in problem solving at high-call locations showed no effect on calls in the experimental group for commercial addresses. This conclusion from the RECAP experiment suggests that "problem solving" as a strategy is probably too general for a meaningful experimental test. More useful may be controlled experiments on homogeneous kinds of places with similar problems, testing identical tactics at all experimental sites. The RECAP experiment in strategy, in contrast, targeted widely diverse kinds of places with widely diverse tactics and levels of police attention. The statistical power of any experiment under such conditions of heterogeneity is understandably quite weak. More important, perhaps, is the weakness of the knowledge base from which police can draw to diagnose and solve soft crime problems. Until there is a better literature to guide them in seeking chains of causation, problem-solving officers may be better off selecting problems subjectively on the criterion of a sound hypothesis for solving the problem.

2. *Directed Patrol.*   The evidence on directed patrol shows consistent effects in reducing soft crime. The Oakland experience of the

early 1980s (Reiss 1985), for example, is a striking quasi-experimental success in police control of soft crime through directed uniformed patrol presence. In committing themselves to a complete rebuilding of downtown Oakland, a group of developers wanted to insure that its skid row atmosphere and problems would disappear with the old pawnshops and flophouses. Their solution was to offer $1 million per year to the Oakland Police Department (OPD) for additional patrol officers to be directed to the several block "new" downtown, patrolling on foot, horseback, and scooters. The department accepted the offer on certain conditions guaranteeing total control over the officers to the OPD. The additional presence of some twenty-five police officers was multiplied by fairly close collaboration with many more office building security guards.

The destruction of the skid row physical environment, of course, may have been sufficient to displace large portions of the potential soft crime offenders. Adding the directed patrol at the same time as the environmental change makes it impossible to assess directly the effects of police alone. But the joint result insured the economic viability of the new office center. To the extent that reported crime statistics are a valid indicator of the change, they support the conclusion of a reduction in soft crime (Reiss 1985). Considering the substantial population increase in the area as an increase in the supply of potential victims, soft crime could have increased dramatically, but did not.

The conclusion that uniformed police presence can deter soft crime is strongly supported by the experience of police strikes. In the Boston police strike of 1919, for example, the first widespread lawbreaking to start and the last to end was public dice games (Russell 1975, p. 131). In Finland in 1976, systematic observation by researchers independent of police showed clear increases in public drinking and the size of groups of young males (Makinen and Takala 1980, p. 103), as well as a 50 percent increase in phone booth coin burglaries compared to the periods before and after the strike. To the extent that smashing store windows for looting constitutes "soft" crime, the Liverpool, Boston, Montreal, and Baltimore (1974) strikes all experienced that phenomenon (Russell 1975, p. 242; Sellwood 1978), although some other police strikes have not.

The most systematic evidence for this proposition comes from the Minneapolis Hot Spots Patrol Experiment (Sherman and Weisburd 1990). David Weisburd's preliminary analysis of that experiment found that a 250 percent increase in patrol presence at target hot spots pro-

duced a 13 percent reduction (or displacement) in total calls for service about crime, most of which were soft crimes. This effect lasted from the beginning of the experiment on December 1, 1988, until August 1, 1989, six weeks after directed patrol was reduced for the summer. In 300,000 minutes of observations of the hot spots during that period, the controls had disorderly events during 4 percent of the time and the experimentals had disorder for only 2 percent of the time. Measurement thereafter was unfortunately confounded by a change in the CAD system on October 1.

Two interesting observations stand out from Weisburd's preliminary results. One is that the effects of directed patrol were largely consistent 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, the deterrent (or displacement) effect disappeared. The other striking finding, consistent with the theory of residual deterrence from police crackdowns (Sherman 1990a), is that the deterrent effect lasted for six weeks after the directed patrol time in the experimental group was officially cut back by 33 percent. From the perspective of the department, this was a free bonus of crime control without the full price of 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. The price of $1,000 per prevented crime (assuming no displacement) is relatively high, at least compared to responding to a call after a crime occurs.[3] The value of preventing

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[3] Assuming no displacement, hot spots patrol deterred 101 crime calls per month through July 31. At 2.5 hours per day × 55 hot spots × 30 days per month, the cost of this patrol in one officer cars is 4,125 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 30–60 minutes of officer time saved. Every hour on hot spots patrol saves (or displaces) 1.3–8.8 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 1,200 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 2,080 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.

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, however, on the assumption of no displacement, an assumption that still awaits testing.

C. Victim-oriented Strategies

Virtually nothing has been done to identify patterns of repeat victimization in soft crime. The area is ripe for computerized records analysis and a problem-solving strategy (Goldstein 1990). Some repeat victim problems may be alcohol related, while others may be chronic and intractable in other respects. But there may be obvious solutions suggested by other patterns, from improved physical security to changing jobs. The first step is to identify and interview any high rate repeat victims.

V. Controlling Domestic Violence

While police can do more to control hard and soft crime than many believe, they can probably do less to control domestic violence. They can certainly do less than many victims' advocates and attorneys have claimed. The irony is that minor domestic violence is among the more predictable offenses, by places, offenders, and victims. The problem is that serious domestic violence, particularly homicide, is virtually unpredictable, despite preliminary findings suggesting the contrary.

Police may be able to do more proactively than they have done before, although our political culture makes it unlikely. Reactive arrests of on-scene offenders, however, may not be as effective as many had concluded from the Minneapolis Domestic Violence Experiment (Sherman and Berk 1984).

A. Reactive Strategies

Given our cultural preference for reactive policing of domestic violence, it is not surprising that we have embraced a reactive strategy so readily. In the period 1970 to 1980, several states loosened the evidentiary requirements for police to make arrests in cases of minor domestic violence they had not witnessed. From 1984 to 1986, there was a fourfold increase in the proportion of urban police agencies encouraging police to make such arrests; several states made such arrests mandatory (Sherman and Cohn 1989). At least part of this increase may have been due to the findings of the Minneapolis Domestic Violence Experiment (Sherman and Berk 1984).

The 1981–82 randomized experiment in Minneapolis encompassed misdemeanor domestic violence cases within four hours of occurrence in three high-crime areas of the city. Forty-four officers were given prerandomized forms for determining which of three treatments to follow: arrest, ordering the suspect to leave the home for eight hours on pain of arrest, or "advising"—essentially doing neither of the other two responses. Of the 314 valid cases, 82 percent were treated as randomized. The other 18 percent received a different treatment from the random assignment, usually arrest, under procedures agreed to at the outset for such reasons as imminent threat of violence, assault on a police officer, or the suspect's refusal to leave the premises. Follow-up measures included official crime reports and interviews with victims, although the response rate to the interviews was very poor; fewer than a third of victims completed the planned six months of interviews.

The analysis consistently found deterrent effects from arrest using a variety of different models (Sherman and Berk 1984; Berk and Sherman 1988). Both official records and victim interviews indicated that arrest had the lowest prevalence of repeat violence for the same couple over a six-month follow-up period, although the rank order of the two nonarrest treatments was different for the two measures. Linear and logistic regression and time-to-failure models all confirmed the deterrent effect of arrest. The results were the same when analyzed by treatment as randomly assigned as well as in an adjusted model of treatment as actually received (Berk and Sherman 1988).

Numerous questions have been raised about the experiment concerning both its external and internal validity. As Sherman and Berk (1984) and Sherman and Cohn (1989) point out, among other things, the sample size was too small to examine interaction effects: the question of whether the rates of repeat violence varied by suspect or victim characteristics. They also note the lack of control on the screening of cases for eligibility, with the officers knowing what the treatment would be at the time they made the eligibility decision. Finally, they suggest that the effects of arrest could be quite different in cities with different cultures or employment rates.

A central recommendation of the final report, however, was that the experiment should be replicated in other sites, in order to address these questions. A second recommendation was that neither police nor state legislatures should adopt mandatory arrest policies, given the potential for diverse individual reaction to arrest (Sherman and Berk 1984). If arrest would increase violence among some persons, even while deter-

ring it among others, it would make little sense to order arrest in all cases where police discretion had previously employed arrest very rarely.

The second recommendation was widely ignored, in part because of the publicity about the experiment generated by the principal investigator (Sherman and Cohn 1989). But the National Institute of Justice accepted the recommendation to replicate the experiment. Police departments and state legislatures adopted mandatory arrest policies in many states, thereby making replication of the Minneapolis experiment impossible in some locales. But the National Institute of Justice did fund six replications, three of which have now been completed. The replications address the separate questions of reactive strategy when the offender is present at the scene and when the offender has departed on police arrival.

1. *Offender Present.* None of the three replications to date has reported a deterrent effect, but one (Milwaukee) has reported significant interaction effects. These results begin to show how the effects of arrest may be highly conditioned on the ecological context or offender characteristics, with arrest increasing violence under some conditions and not under others. The Omaha replication (Dunford, Huizinga, and Elliott 1990) was the most similar of all six replications to the original Minneapolis design. Its sample size (330), eligibility, treatments, and measurement closely followed Minneapolis, with improvements in randomization procedures. All patrol officers in the department were asked to screen for eligible cases without knowing the treatment that would be assigned. When they had an eligible case, they called the dispatcher for a randomly assigned disposition generated on the spot by a computer program. The three treatments were then implemented as assigned in 92 percent of the cases, in part because the more troublesome cases were not subjected to random assignment. Follow-up measures included initial and six-month victim interviews with a 73 percent completion rate overall. Both official and victim data were restricted to same-couple violence, as opposed to violence outside the relationship.

The Omaha experiment found no specific deterrent effect of on-scene arrest and no differences at all across the three treatments. This result was robust across the same range of models as in the Minneapolis analysis. A longer-term follow up, with one-year victim interviews, is still in progress.

The Charlotte replication (Hirschel et al. 1990) also found no deter-

rent effect of arrest, although with a different design and sample. Unlike the Minneapolis and Omaha experiments, the Charlotte sample had a strong majority (73 percent) of blacks. Unlike the Milwaukee sample, which also had a majority (75 percent) of blacks, the Charlotte sample had a majority of employed suspects. Its 686 cases were randomly assigned to three treatments, with 83 percent compliance with the design: arrest, arrest by citation without taking into custody, and no arrest. Six-month victim interviews were completed for 47 percent of the sample. No deterrent effects of arrest were found with any of the now standard models of analysis.

The Milwaukee Domestic Violence Experiment (Sherman et al. 1991*b*) greatly expanded the sample size (1,200 cases); altered the treatments (arrest with three hours in jail, arrest with overnight in jail unless bond is posted, and a warning of arrest if more trouble occurs); and provided direct experimental control over the randomization procedures by Crime Control Institute staff who were always on duty while the experiment was in operation. The misassignment rate was 2 percent, the six-month victim interview rate was 79 percent, and a systematic measure of prevalence and frequency of violence covered periods both before and after the randomized intervention. The sample was predominantly black males with very high unemployment rates, living in high unemployment neighborhoods.

The experiment found no consistent differences in recidivism across the three randomized treatments. There was some evidence, however, of an initial deterrent effect followed by a long-term escalation in the frequency of violence among those assigned to short custody arrest (Sherman et al. 1991*b*). More important, there was very powerful and consistent evidence of an interaction effect with suspects' unemployment (Sherman and Smith 1991). For employed suspects, arrest reduced the frequency rate of repeat violence by 16 percent. But for unemployed suspects, arrest *increased* the frequency of repeat violence by 44 percent. It is not clear, however, whether this effect is due to individual characteristics or the ecology of their neighborhoods and the differential shame or stigma those neighborhoods attach to arrest (Braithwaite 1989).

These mixed results suggest that arrest is certainly no proven panacea and that mandatory arrest may not be justifiable on the grounds of crime control. This may not disturb its proponents, who may be more persuaded by a policy argument in favor of vigorous state action in response to alleged domestic violence incidents. But it should con-

cern police agencies not required by state law to make arrests when probable cause is found. Mandatory arrest policies for underclass areas with chronic unemployment, similar to Milwaukee's, may be increasing violent crime and should probably be abandoned (Sherman 1992).

Many issues must be addressed in drawing conclusions across sites, a task best left until the completion of all six replications. A central issue in comparing results across sites, however, is the threshold of seriousness necessary to have a case qualify for the experiment. Observations in different experimenting cities suggest very different interpretations of this question by experimenting officers. If that is the case, the differences in results may reflect different effects of arrest on different kinds of offenses or offenders. If that is not the case, a likely explanation for the differences is that similar police actions have different effects in different cities due to differences in demographics, local culture, and the local context of crime and criminal justice. These are all factors that must be addressed in developing systematic knowledge about the effects of the police on crime.

Moreover, these experiments are limited to the individual effects of arrest on those arrested. No analysis to date has addressed the *general* deterrent effects of a mandatory arrest policy in a jurisdiction. No quasi-experimental analyses of domestic assault reports, for example, have been performed to determine any before-after impact. Nor has the most powerful design of all, a random assignment of mandatory arrest to some cities or areas and not others, even been contemplated. But without such analyses, our understanding of the effects of arrest on minor domestic violence will be incomplete.

2. *Offender Absent.*   An additional strategy for improving knowledge about this problem is the development and testing of innovative responses. One extension of reactive strategy tested in the Omaha experiment is the seeking of an arrest warrant when the offender is absent. A 247 case randomized experiment in use of this tactic, backed by a very high rate of issuance and prosecution, produced a clear deterrent effect on repeat violence (Dunford, Huizinga, and Elliott 1989). It is curious that a warrant should have more effect than actual incarceration (from arrest) in the same city. But if that result is replicated elsewhere, it would address a large portion of all domestic violence cases—up to half in some jurisdictions. Its value is enhanced by its relatively low consumption of police time.

Police time poses a larger question about policing domestic violence, however, in the relative priority it merits in relation to the cost. Manda-

tory arrest laws have been extraordinarily costly to some cities. In Milwaukee, for example, there were over 7,000 reports a year under a mandatory arrest policy. Under a 1989 state law mandating arrest, the net was widened to include intimidation, and reports almost doubled to around 14,000. The arrests consume at least three to four hours of police time (using two officer cars), or as much time as it would take to prevent (or displace) 1,200 crime calls in hot spots of crime.

This is a prime example of how an arrest "budget" could limit the amount of resources expended on each crime, while still avoiding decriminalization of the offense. If, for example, mandatory arrest were employed on selected days on a random sequence basis, the number of arrests could be cut substantially without, perhaps, losing the general deterrent effects of such arrests. The residual deterrence of arrests from one day carrying over to other days may be another consequence of rotating crackdowns (Sherman 1990*b*).

B. Proactive Strategies

All domestic violence control by U.S. police is now reactive. Nowhere, to my knowledge, do police identify high-risk couples and undertake proactive interventions. Such a strategy could pay off in the reduction of minor domestic violence. But it is unlikely to succeed in preventing domestic homicide, as many scholars had long hoped. It is also unlikely that any American police agency would develop proactive strategies to detect hidden domestic violence, especially among the middle and upper classes.

1. *Predicting Domestic Homicide.*   The epidemiological analysis of domestic homicide is a case study in the methodological pitfalls such analysis entails. One of the most influential studies of the police role in crime control is the Police Foundation–sponsored Kansas City Police Department analysis of the precursors of domestic homicide and aggravated assault (Breedlove et al. 1977). The widely cited study, which sparked the design of the Minneapolis Domestic Violence Experiment, reported that in about 90 percent of the domestic homicides police had responded to at least one call for service at the address of the domestic homicide victim or suspect in the two years preceding the homicide and to five or more calls in about 50 percent of the cases (Breedlove et al. 1977, p. 23). Similar findings were reported for domestic aggravated assault. Wilson (1977, p. iv) concluded from these results that, at least in Kansas City, "the police can obtain some early warning of assaults

and homicides" since "any given homicide arrest is likely to be the culmination of a series of police interventions."

A follow-up study conducted by the Kansas City Police Department under a National Institute of Mental Health grant reached a similar conclusion about assaults, if not homicides: "The premise that we may have some kind of 'early warning system' embedded in the relationship of disturbances and assaults is substantiated" (Meyer and Lorimor 1977, p. V-2). No data on risk levels in relation to prior frequency of calls, however, were displayed.

Taken in conjunction with the Kansas City homicide findings, these data have suggested a pattern of escalating frequency of police interventions in domestic violence that might describe a high-risk profile for homicide. The findings raised the possibility of proactive police interventions for preventing domestic homicide, which accounts for anywhere from 8 to 21 percent of all murders nationally (Federal Bureau of Investigation 1989), and even more in specific cities. They also raised expectations about police ability to prevent domestic homicide, including numerous lawsuits when police failed to do so.

These expectations were unrealistic, however, given the major limitations of the research: units of analysis and sample sizes and selection. On the units of analysis, the research confused the criminology of *places* with the criminology of *people*. None of the findings cited above measures the prior behavior of the persons involved in the homicide. All of them merely involve police CAD records of police cars dispatched to events at certain addresses, without any record of the identities of the individuals involved. The first Kansas City study was somewhat sensitive to this issue and therefore decided to omit homicides occurring in "apartment buildings with many tenants" (Breedlove et al. 1977, p. 23). This exclusion implies, however, that two- and three-family houses, not uncommon in Kansas City, were left in the analysis. The question then becomes what percentage of the prior calls involved one of the parties involved in the subsequent homicide. Given the nameless CAD data, there was no way to tell.

On the issue of sample size, it is striking that the published version of the Kansas City study, apparently based on 1970–71 homicides (Police Foundation 1977, p. 5), did not report the exact numbers of homicides included in the analysis. We can, however, estimate the number at less than fifty and no more than seventy-three (Sherman et al. 1991*a*). The small number of homicides suggests that there are

probably far more *buildings* with high frequency of domestic calls than there are domestic *homicides*. In Minneapolis, for example, a city then of two-thirds Kansas City's population, there were 1,197 buildings with five or more disturbance calls in 1986. If we assume there were twice as many buildings with that many calls in Kansas City, with no more than one homicide in any one building over two years, we have a predictive ratio of only 73 homicides in 2,394, or only 3 percent of high-risk buildings with a homicide. Predicting domestic homicide on the basis of five or more calls would therefore lead to a 97 percent false positive rate, and that is only at the level of buildings. If we multiply the number of buildings by the number of couples in them, the false positive rate would substantially exceed 99 percent. When cases (and buildings) deleted for multiple tenancy are taken into account, the error rate could be even higher.

A similar analysis in Minneapolis (Sherman et al. 1991*a*) shows that knowing the prior number of domestic calls at an address provides little increase in ability to predict where domestic homicides will occur, even if it does reduce the number of candidates to about 1,000 buildings. Attempts to intervene for prevention of those homicides would clearly produce substantial overprediction and much wasted effort.

Several of the problems described above can be overcome with data from the Milwaukee Domestic Violence Experiment (Sherman et al. 1991*b*), one of the six replications of the Minneapolis experiment funded by the National Institute of Justice. These data show that out of 15,537 police reports of domestic battery among named couples citywide from April 7, 1987, to February 8, 1989, only in one couple did a homicide later occur (Sherman et al. 1991*a*). Even more surprising was that thirty-two of the thirty-three domestic homicide victims during that period had no prior police record of domestic battery. Of the 110 batteries involving guns and threat of death, none led to any serious injury.

It thus appears unlikely that homicides are more likely to occur at the extreme end of a distribution of repeated reports to police of less serious violence, falsifying the "escalating violence" theory of prediction at least in terms of official data if not undetected events. The evidence against this thesis is even more compelling with the 1,113 couples in the experiment, with a twenty-two month surveillance period and a mean follow-up period of 15.8 months from the first report. Seventy couples had five or more reported batteries, but no homicides.

In the absence of the "early warning system" ability to make accurate predictions, there seems to be little hope for a proactive police strategy against domestic homicide.

2. *Controlling High-Risk Couples.*   The ability to predict which couples will have additional minor violence is greater, but still limited. In Minneapolis over a one-year period, 9.1 percent of the addresses with any domestic calls accounted for 39.5 percent of all domestic calls, while *9 percent of all addresses accounted for 100 percent of all domestic disturbance calls* (Sherman et al. 1991*a*). The Minneapolis analysis of crime hot spot addresses also found that domestic disturbance calls had the greatest concentration by address of any of six types of calls examined (including robbery, auto theft, burglary, assault, and criminal sexual conduct). Compared to the number of addresses at which domestic calls would be expected to occur without any repeat call addresses, the actual number of addresses with domestic calls was 59 percent lower than expected due to repeat calls—the largest reduction among call types examined (Sherman, Gartin, and Buerger 1989, p. 41). The building addresses at which domestic disturbance calls occur are fairly predictable, with better than two-thirds accuracy of predicting an additional call at some time within the year once there have already been three such calls.

Nonetheless, police predictions of specific couples likely to experience more minor batteries, or the places likely to have more domestic calls, must suffer some false positives (Sherman et al. 1991*a*). The best prediction Milwaukee Police could make about repeat batteries over an almost three-year period, for example, is that couples with seven or more prior reports will have another one during that time period. This prediction will be wrong in one out of four cases. While this error rate is low for purposes of scientific analysis, it may be too high for intrusive (as distinct from more passive) policing. Depending on the intrusiveness of any prevention measure suggested, it could be fiscally or ethically unacceptable to impose a measure that turns out to be unnecessary in such a large proportion of the cases.

If some powerful "inoculation" against domestic violence were possible, it could certainly be applied to the seven-or-more police-report couples. The problem is imagining what could work. A prosecutorial threat to invoke more serious penalties for recurrence sent by mail or delivered by police is one possibility and could be tested experimentally on this high-risk population. Other low cost ideas might also be

tried. The point is to continue to address a pervasive problem in a spirit of trial and error rather than concluding it is "impossible" for police to do anything constructive.

VI. Policing Other Crimes
The development of police research has hardly been systematic enough to test the control of all types of crime. This section considers several of the many remaining types of crime that offer either important research results or promising strategies for crime control: drug markets, burglary, auto theft, and drunk driving.

A. *Policing Drug Marketplaces*
Policing drug crime has been one of the most innovative areas of American law enforcement in the last decade. It is also one of the most confused areas of strategy, with entirely unclear objectives. The longstanding concern with reducing supply has gradually been supplemented with a desire to restrict demand. In the wake of the crack epidemic, however, both goals took second place to reduction of violence and disorder in the immediate vicinity of drug marketplaces.

The central strategic choice in drug enforcement has been between wholesale and retail level dealers. Through most of the 1970s and 1980s, police focused drug enforcement on drug wholesalers. Drug enforcement was limited to special units conducting fairly complex undercover investigations. But the advent of crack created highly visible street markets and crack houses, putting tremendous pressure on police to control retail-level dealers.

From 1985 to 1989, the national arrest rate for homicide doubled among persons under age eighteen (Federal Bureau of Investigation 1986–90; U.S. Bureau of the Census 1986–90). Much of that increase undoubtedly occurred in center cities near retail drug marketplaces, as it did in Washington, D.C., where the homicide rate rose by over 300 percent (Federal Bureau of Investigation 1986–90). The sounds of gunfire became a daily occurrence in some neighborhoods, and the quality of life deteriorated rapidly (Kotlowitz 1991).

Police responded with crackdowns (Chaiken 1988; Sherman 1990*b*) of massive numbers of uniformed officers on patrol, raids of crack houses, and innovative tactics: "jump-out" squads intercepting observed drug transactions, condemnation of buildings and landlord fines over drug deals in residential settings, and even using front-end loaders to assault a well-fortified crack house. Kansas City police in early 1989

began raiding about one crack house every day, and local taxpayers approved a referendum for an increase in the sales tax to hire more police and prosecutors dedicated solely to drug enforcement.

Evaluation results on drug crackdowns to date have been mixed (Kleiman and Smith 1990). A Lynn, Massachusetts, crackdown on an open-air drug market reportedly reduced the robbery and burglary rates for two years, without discernible displacement (Kleiman 1988). The Lynn crackdown's use of observation-of-sale arrests was apparently more effective than the warrant arrests made in Lawrence, Massachusetts, where the crackdown resulted in no reduced drug use and an *increase* in violent crime. A massive crackdown in New York's lower east side, "Operation Pressure Point," succeeded in reducing drug use, robbery, and homicide, but the reductions decayed after the first year—a typical pattern found in most crackdowns (Sherman 1990*a*, p. 21).

Uchida, Forst, and Annan (1990) compared intensive drug enforcement alone to intensive drug enforcement plus community oriented "door-to-door" police visits to residences in Birmingham, Alabama, and Oakland, California. The intensive enforcement consisted of street corner buy-and-bust tactics, as well as raids and sweeps. In both cities, the door-to-door tactics were not fully implemented as planned, due to police resistance. But in both cities the only reductions in violent crime were found in the areas where door-to-door visits supplemented the intensive enforcement. Combined with the positive results of door-to-door visits in Houston and Newark (Pate et al. 1986; Skogan 1990), these results suggest that visits may be a far more powerful strategy than most police imagine.

On balance, we still know very little about dealing with the quality of life and soft crime problems surrounding drug marketplaces. But the continuing war on drugs creates both opportunities and a demand for more evaluations. To make further progress, the next stage of drug enforcement evaluations will require large-sample randomized experiments using marketplaces as the unit of analysis.

B. Controlling Burglary

Police strategies for controlling burglary have been highly burglar oriented, minimally victim oriented, and rarely fence oriented (Shover 1991). The burglar-oriented strategies include traditional use of informants and witnesses, as well as two newer strategies. The early 1970s Robert Redford movie, "The Sting," apparently prompted similar

methods against burglars. Police set up fake fencing operations and bought stolen goods from burglars to get evidence for prosecution. Despite some favorable evaluations, however, Klockars (1980) found no evidence of a crime-reduction effect. Marx (1988, p. 126) offers strong suggestive arguments that stings can simply increase the supply of burglars by raising the level of demand for stolen goods.

If Marx is correct, then Reiss (1990) may correctly hypothesize that a fence-oriented strategy could do more to reduce burglary than a burglar-oriented strategy. Police have rarely incapacitated major fences, in part because of their skill in avoiding an evidentiary trail. But a thorough diagnosis of the local market structure for stolen goods (which may vary widely by type of goods, from art to computers) could reveal a number of potential fences as targets for proactive, undercover enforcement.

A more valuable burglar-oriented strategy may be police monitoring of previously convicted burglars, in cooperation with correctional authorities. Under a National Institute of Justice grant, for example, Indianapolis police are participating in an experiment to supplement electronically monitored house arrests of juvenile burglars. One comparison is between burglars on house arrest without police surveillance and burglars visited daily at unpredictable times by the local beat car. The theory is that burglars will be less likely to cheat on their house arrest (by going out to commit burglaries, among other things) if they know police may catch them—a testament to the weak technology and correctional follow up of electronic monitoring. An added virtue of this program should be building up police information networks among burglars, which could help to identify more fences.

The advent of automatic fingerprint identification systems may also dramatically increase police capacity to apprehend, and perhaps to deter, burglars. In the past, police had to have a suspect in mind in order to check fingerprint records. In recent years, however, many states have implemented a computerized search process requiring only one good print to produce a match with any prints already in the local system. This development has produced many anecdotal accounts of improved apprehension rates, but no systematic evaluations.

One reason burglar-oriented strategies may not work, however, is the low rate of repeat burglary arrests in any given year, at least in Kansas City (Sherman, Rogan, and Velke 1991). The maximum number of repeat burglary arrests in 1990 was only four. In contrast, only 134 persons had two or more burglary arrests, collectively generating

29 percent of all burglary arrests. Assuming that they commit many burglaries besides those for which they are arrested, it would seem that those people bear watching.

The victim-oriented strategy of the 1970s focused on target hardening. Police became virtual public relations agents for lock manufacturers, alarm companies, and hardware stores. But the evidence that hardware reduced burglary risks was quite mixed. Waller and Okihiro (1978) found no evidence that hardware made a difference, although having someone in the home did. Yet hardware-encouraging community-organizing programs in both Seattle (Lindsay and McGillis 1986) and Portland (Schneider 1986) reported substantial crime-reduction effects. One reason they may have been so successful, however, is the selection bias built into the choice of neighborhoods. Attempting to mount similar neighborhood watch programs in Chicago and Minneapolis neighborhoods that were less eager to implement the programs met with no measured crime-reduction effect (Skogan 1990, chap. 6).

Marking identification on personal property, or "Operation Identification," also shows mixed results. While it has not been found to increase the likelihood of property being recovered, both U.S. (Schneider 1986) and Welsh (Laycock 1986) evaluations have found that it reduces the rate at which homes are burglarized. It is possible, of course, that the act of marking goods creates some greater level of watchfulness by families or that visible stickers indicating participation in the program displace burglars to other locations. But in the absence of any better evidence, the police effort expended in getting the property marked appears to be cost-effective (Laycock 1986).

High-risk victims could also be the focus of special police programs, with a potentially greater crime-reduction effect. In Kansas City in 1985–89, only 1 percent of the population (5,423 people) produced 26 percent of the burglary victimizations (Sherman, Rogan, and Velke 1991). There were 751 people burglarized at the rate of one per year or higher, and one victim reported thirty-six burglaries in five years. These people could be targeted for problem-solving efforts to diagnose the causes of the repeat burglaries and the possible solutions to each individual set of circumstances.

C. Controlling Auto Theft

Big-city auto theft enforcement is often focused on professional "chop shops" for breaking up cars and on professional gangs of car

thieves. And in Kansas City, a small number of auto thieves—who may or may not be very "professional"— are repeatedly arrested. In 1990, 209 persons accounted for 502 auto theft arrests, or 32 percent of all such arrests (Sherman, Rogan, and Velke 1991). But the offender-oriented focus has been problematic in the late 1980s, as increased auto theft rates appear to be connected to drug shipments and subsequent abandonment of cars. The problem might be better controlled by increased technology and potential fraud analysis.

The primary new technological solution is the Lo-Jack car locator system, which has been tested in Massachusetts since 1986 (Grable 1991). The commercially marketed system, based in Needham, Massachusetts, works by having a radio transmitter installed in a hidden location in the car by company personnel. If the owner reports the car stolen, duly equipped police agencies are then able to pinpoint the location of the car within a twenty-five mile square, after they activate the transmission device from police headquarters. In the first five years of operation, Massachusetts police recovered 95 percent of the vehicles reported stolen. The average recovery time is two hours, and the fastest was seven minutes. Arrest rates average 20 percent, compared to the national auto theft clearance rate of 5 percent.

The cost of the system to private citizens is about $600. A more expensive system ($1,500) activates a central station alarm if the car is moved without proper authorization codes. The Michigan-based Code-Alarm system avoids the possible delay in reporting associated with Lo-Jack since the owner does not need to discover the theft for the system to be activated. Code-Alarm also gives police a device that enables them to turn off the engine of the stolen car once they are in pursuit of it, avoiding the possible damage of a high-speed chase (Grable 1991).

If either commercial system were universally adopted by police agencies, police could track and arrest a car thief at any time until the car is disassembled and the locator destroyed. This capacity would not create a deterrent effect, however, until the prevalence of the system was so high that thieves would calculate the risk to be unattractive. This could happen more readily with mandatory installation in all new cars, which in the long run could make a big dent in car theft rates. In the short run, however, it would probably just displace theft on to older cars made before the new requirements. That is exactly what happened in England, for example, after the introduction of steering column locks on all new cars (Mayhew et al. 1976). In Germany in

1963, however, auto theft went down 60 percent across the board when all cars—old and new—were required to install steering column locks (Mayhew et al. 1976).

A mandatory locator system would also help to deter insurance fraud in car theft reporting, although smart con artists might still find ways around it. What will be harder to escape is a regional or national registry of car theft victims, which would identify repeat victims by social security number or drivers' licenses. Unusually high levels of car theft would either expose victims as frauds or restrict their access to further car insurance as plain bad risks. In Kansas City, for example, in 1985–89, there were 386 people who reported three or more car thefts in five years, and eighteen people who reported five or more (Sherman, Rogan, and Velke 1991). Whether careless or fraudulent, these people are worth some special attention.

D. Controlling Drunk Driving

Police control of drunk driving has probably been more successful than critics have claimed. H. Laurence Ross (1982), the leading student of drunk driving enforcement, has found fairly consistent evidence of actual or threatened police crackdowns producing immediate deterrent effects on single-vehicle nighttime fatal accidents. But he also finds these effects to wear off, sooner or later. He therefore concludes pessimistically that "deterrence-based policies are questionable in the long run" (Ross 1982, p. 111). As Jacobs (1988, p. 212) points out, this conclusion may be unjustified by the data, which may tap only a small subset of the potential drunk driving population. It also unduly discounts the value of the short-term deterrent effects he so consistently observes in different nations, in different decades, with different police enforcement methods.

As Sherman (1990a) suggests, Ross's consistent finding can be put to more strategic use. The key is the common finding of the short-term crackdowns that the measured deterrent effects persisted after the police efforts had ended. This "residual" deterrent effect lasted in some cases as long as the crackdown itself. In order to obtain twice the deterrence for the same amount of personnel time, police might continually alter targets and tactics of drunk driving crackdowns. Since there are more areas in most jurisdictions than police can cover intensively, constant shifts in geographic targets makes sense in any case.

The difficulty in evaluating some of the newer tactics, like checkpoints, is that few state laws require alcohol testing of drivers involved

in accidents, even fatal ones. Most drunk-driving data are based on dead drivers or inferences from overall accident rates. Until wider testing of drivers in fatal accidents is required, even the U.S. Department of Transportation's national FARS (Fatal Accident Reporting System) data on trends in alcohol-related deaths will remain highly speculative. At the local level, it will be difficult to examine the effects of any geographically focused enforcement efforts on drunk driving in the immediate vicinity of those efforts.

These strategies are primarily across-the-board, general deterrent strategies. Relatively little problem-oriented policing has been focused on the more persistent problem of chronic recidivists at high risk of injuring themselves or their passengers in an accident. The situational epidemiology of DWI-related accidents, for example, has not been widely discussed. But a few police agencies have recognized the high prevalence of drunk drivers who have just come from a tavern or pool hall, over 50 percent in one California study of DWI arrestees (Yoder and Moore 1973). Some agencies have responded to this by staking out bars and following cars as they depart, watching for erratic driving. Shaw (1989) suggests that police lobby for barring convicted drunk drivers from even visiting public drinking places as a condition of probation or parole.

Reactive strategies for drunk driving enforcement take on new dimensions with the car phone. While it was once almost impossible for good citizens to call police with the exact whereabouts of a drunk driver, car phones allow citizens to tail the driver until police come. Public relations campaigns to encourage such actions could stimulate them, even with the predictable cost of some rate of false alarms.

VII. Future Prospects

What can police do about crime? One answer to the question might be that we do not know because they never really tried. That judgment would be overly harsh, for despite the preoccupation with answering 911 calls there is much that police do that has some effect on crime. The important question is *what effect* since police efforts can sometimes increase crime as well as control it. A related question is *what else* could they do that they are not now doing, and should it be substituted for some current activities? As little as we know about the effects of current activities, there are probably more legal, constitutional, and morally proper tactics that have not been tried than have been. Therein lies the lesson for future prospects.

It is unlikely that police will achieve anything like medicine's spectacular successes in virtually eliminating certain diseases. There have been some such precedents in crime control: robberies of buses eliminated by eliminating cash, domestic skyjackings almost eliminated by metal detectors. But the police have not yet directly accomplished such results. Nor, given the intractability of most crime problems, are they likely to.

What seems more likely to work is a steady accumulation of results, searching for some with modest success. While this process is necessarily plagued by a "theory gap" in frameworks for organizing and understanding diverse results (Zimring 1978), that problem seems likely to be solved more quickly with more data rather than more dataless theory. Police are more than willing to step up the pace of experimentation with specific tactics against specific crime problems. The major problem is a lack of trained and willing social scientists available to work with them, a shortage linked directly to the dearth of federal and foundation support for research and development for crime control.

This pace will remain sluggish if it is wholly or primarily dependent on federal sources of funding. While police chiefs have called for more research and development on drug enforcement, Congress has ignored their pleas. What may be more feasible is a combination of city and foundation funds for conducting local experiments, with technical support from the national research organizations. The New York City Police, for example, have funded research by the Vera Institute of Justice. But American foundations in the 1990s are almost uniformly uninterested in crime, and the continuing decline of most big cities makes city funding unlikely.

No matter how research is funded, the key resource will still be imagination. Without creative ideas for diagnosing and treating a crime problem with policing, there will be nothing to evaluate. Academics tend to scorn the process of idea generation and to respect rigorous tests. But rigorous tests of bad ideas will make little progress. Police professionals should be emboldened by their own detailed knowledge of the crime problems they face and encouraged to gather even more systematic data on them. There are only 500 active research criminologists in the United States and over 500,000 police. The odds should support more ideas from the rank and file. Anything national and local organizations can do to encourage the "suggestion box" for crime control will increase the stockpile of hypotheses.

Innovative crime-control tests could also revive the office of chief of

police, a position now slowly being strangled by police unions, red tape, and 911 calls. Modern police chiefs have relatively little opportunity to exercise leadership in crime control except for bold initiatives, which can be even more valuable if properly evaluated. For despite the quasi-military symbolism of police rank structure, the jurisprudence of policing assumes more of a hospital model. Officers, like doctors, deal with cases and take the primary responsibility for the decisions they make. The police department, like the hospital, is merely a source of organizational support and personnel resources for those decisions, but not the decision maker itself. Police chiefs have become hospital administrators rather than generals.

This essay's reliance on the military concepts of strategy and tactics may seem inappropriate for the hospital model of police work. But it is the hospital model itself that is inappropriate to crime control. Hospitals are reactive organizations, relying on people to come to them when they are sick. The goal of the hospital is to heal the sick, not to control disease. Public health organizations, by contrast, take no patients, but seek to control disease. They employ many doctors, but decision making is an organizational, not individual, responsibility. The strategies and tactics for fighting epidemics like AIDS, or chronic sources of mortality like heart disease, are determined in truly quasi-military fashion. It is no accident that the federal Centers for Disease Control is an arm of the U.S. Public Health Service, which is a uniformed service commanded by a (surgeon) general.

A police chief as "surgeon general" would be far more concerned with crime analysis, especially chains of causation and emerging epidemiological trends. A crime-control chief would strike at those causal chains wherever there is a vulnerable link. Just as the surgeon general speaks out against smoking, a police chief could speak out against divorce and other individual choices that may contribute to community crime rates—even while respecting the rights of people to make such choices. And just as the local health department closes down restaurants for operating unclean kitchens, a crime-control police department can close down taverns or entertainment facilities for operating high-crime establishments.

Police departments cannot escape their responsibilities as "hospitals," of course. But they already do refuse to deal with many kinds of cases, through the exercise of officer discretion. This precedent opens the door to the addition of the public health model to the traditional hospital model of police organization, paying for "disease con-

trol" by cutting back on the costs of "patient care" (McEwen, Connors, and Cohen 1986). Indeed, the merger of the two approaches may allow joint strategies that are unheard of in disease control. Hospitals play relatively little role in providing preventive medicine, despite the enormous opportunity they have through direct contact with the sick. Police can use their contacts with victims, places, and offenders to attack causal chains as well as to treat the specific cases.

Given this perspective on the possible future—and to some extent, current—directions for police departments, there is every reason to talk about strategic and tactical choices for crime control. Police chiefs can become both surgeons general and hospital administrators. There is no major legal obstacle to clearer organizational direction of officers' time, including discretion to arrest. This does not mean that the direction must be top-down since policies can be made just as well through bottom-up initiatives and participation. But it does mean that the organization can make coordinated and systematic choices rather than letting the choices emerge topsy-turvy from the individual decisions of each officer.

Approaching strategic choices in this manner can also breathe new life into the concept of community policing (Skolnick and Bayley 1986). In making explicit choices about priorities, strategy, and tactics, police agencies can seek citizen comment before the choices become final. They can also make different choices for different communities, in reflection of local preferences. Interest group pressure can also be dealt with more rationally through clear choices. Rather than seeing the police department as a bottomless well, interest groups could be educated to see it as an organization of finite resources. If there is to be increased attention to one problem, attention to something else must be cut. Getting interest groups to see the broader public interest, rather than making unilateral assaults on police executives, would be much better for democratic policing. It might even foster more effective crime control.

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