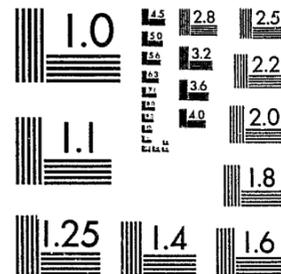


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DEVELOPING POLICY RELEVANT INFORMATION ON DETERRENCE:
AGGRESSIVE POLICING AND CRIME

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DEVELOPING POLICY RELEVANT INFORMATION ON DETERRENCE:
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Abstract

Some recent analyses of the relationship between crime and policing have focused on the deterrent effects of active or aggressive patrol. As is often the case in studies of the outcomes of public policies, the available evidence on this issue leads to no single conclusion. This paper extends discussion of the possible relationships between active patrol and crime and discusses the characteristics of policy relevant research on crime and the police. The empirical analysis based on a cross-sectional analysis of data from sixty urban neighborhoods, indicates that some forms of active or aggressive patrol may indeed deter certain types of criminal activity.

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DEVELOPING POLICY RELEVANT INFORMATION ON DETERRENCE:
AGGRESSIVE POLICING AND CRIME

Urban service delivery in America underwent a fundamental transformation during the last decades of the nineteenth and the first decades of this century. Urban police were not immune to this wave of reform; it stripped them of some welfare functions and changed police responsibilities for dealing with crime. Police were given (or assumed) the responsibility for preventing crime. Whether and how police can fulfill this mandate is a question about which scholars still disagree. Eric Monkonen finds the notion quaint but chimerical -- "doomed to failure" (1981:4). While many other researchers are equally critical, others suggest that police can prevent crime by implementing an aggressive patrol strategy.¹ That such a style of policing might affect crime is consistent with the implications of the deterrence doctrine and has long been an integral part of the folklore of police administration. As with many public policies, solid evidence of aggressive patrol's impact on crime, its target, is difficult to provide. In fact, only research efforts with certain characteristics can provide policy relevant information about aggressive patrol's effects.

EVALUATING PUBLIC PROGRAMS

Most public policies are not designed as ends in themselves: they are the means of bringing about some desired set of social conditions. The choice among policies, the evaluation of policies, and their continued justification requires assertions about their social impacts. If a policy is to be more than symbolic action, it must produce at least some of the consequences its designers intend.

Unfortunately, the connections between policy and social conditions are rarely well understood. In recent years, political scientists have

increasingly studied the effects of government programs on society. However, the complexity of the causal networks underlying such social conditions as crime or unemployment, for example, makes it difficult to isolate the effects of government programs, if indeed those programs have any effects. As Susan Hansen noted in a recent review of policy analyses by political scientists: "A sophisticated, multi-dimensional evaluation might well find that the problem at hand is due to forces beyond the control of the organizations being evaluated." (1983:29) Stressing the importance of understanding the connections between governmental action and social conditions, Hansen, in fact, defines policy analysis as "an explicitly focused, systematic analysis on [sic] the outputs of government and their effects on society." (1983:14)

Some investigators have concluded that there is little government programs can do to shape social conditions. For example, after studying the relationship between the content and administration of state workmen's compensation laws and the coverage and benefits provided under those laws, Joel Thompson concluded "policy outputs are not important determinants of policy outcomes." (1981:1151) The italics are his. Thompson obviously wanted to emphasize the general conclusion, although in a non-italicized clause he did qualify this generalization by saying "at least in regard to workingmen's compensation."

In other areas of public policy, evidence indicates that outputs do have quite noticeable social impacts. Hansen, for example, found that differences in abortion policy among the states were strongly related to differences in state abortion rates in the years following the Roe v. Wade decision (1980).

Whatever the policy area, however, the key characteristics of policy relevant analyses of public programs are quite similar. In order for assessments of state action to provide information that may be of use to policy makers, an assessment must:

1. precisely specify and measure the content of the policy under review (the behavior of government employees)
2. precisely specify and measure the particular social conditions that this behavior is expected to affect
3. precisely specify the postulated causal linkages between the policy and social conditions
4. estimate the effects of the policy in the context of a well-developed model of the social conditions, paying special attention to the possibility that social conditions generate policies.

A prescription as seemingly simple as that above is all too often difficult to implement. Fairly well-trodden areas of policy inquiry all too frequently fail to satisfy the minimal criteria for either policy or theoretical utility.

In this paper, hoping both to provide useful information on the relationship between policing and crime and to illustrate some of the generic problems in providing useful policy information, we evaluate the impact of an administrative response to street crime, an aggressive police patrol strategy. Analysts differ in their assessments of the deterrent effects of aggressive patrol. Some scholars have found evidence that aggressive police patrol deters crime (Roydstun, 1975; Wilson and Boland, 1978). Others dispute those findings and argue that there is no discernable relationship between rates of aggressive patrol and of crime (Jacob and Rich, 1980-1981). As uncertain as the outcome of this debate is, the debate itself has been very useful. It has focused on and emphasized some of the most important problems in the current policy research on deterrence.

In all policy areas, the precise specification of policy content is often fraught with difficulties. When we use authoritative statements of the policy, such as laws or regulations, we all too often find that the concrete behavior of public officials and agencies contradicts those abstract

statements. The content of public policy can probably be specified best by describing the behavior of those government agents who are charged with the policy's execution. In the case of police patrol policies, this is the behavior of the patrol officers. What then is "aggressive patrol?" As James O. Wilson and Barbara Boland indicate, aggressive patrol does not "mean that the officer is hostile or harsh but rather that he maximizes the number of interventions in and observations of the community." (1978,370) Even this definition may not be specific enough, however. Are all sorts of interventions likely to deter crime? Wilson and Boland use traffic citations as their indicator of patrol aggressiveness, arguing that citation rates are indicative of a style of patrol in which more interventions of all sorts occur. But, it may be that time an officer spends issuing traffic citations reduces the time available for other officer-initiated activities, and traffic citations have little face validity as a means of deterring crime. In the analysis that follows, we specify a set of activities that can be seen as part of an "aggressive patrol strategy."

Precision in measuring social conditions is also sometimes difficult to achieve. Terms like "crime" cover a wide array of human behaviors, however, and it is as unlikely that there is a single common cause or cure for crime as it is that there is a single cause or cure for "illness." In this analysis, we focus on specific sorts of crimes that may be deterred by specific sorts of police-initiated actions.

Precision in the identification of policies and the social conditions they affect is an integral part of achieving greater precision in the theories about how government programs help produce changes in society. By refining and accumulating detailed information about how police can deter which crimes we provide a more secure and a more instructive base from which to look for

the exact mechanisms through which the deterrent effects of police policies operate.

The validity of the measures and the mathematical models used to test relationships between policies and problems is also of prime concern. Measurement is always imperfect and the fit between theory and model is at best only approximate. Yet the more confidence we have that our measures capture the concepts of concern with minimal error and the better the fit between models and real world conditions, the surer we can be of our findings. The analysis that follows, for instance, recognizes the possibility that crime levels affect patrol strategy, while it attempts to determine whether patrol strategy affects crime.

PROBLEMS IN POLICY RESEARCH ON CRIME AND THE POLICE

Throughout the late 1960s and the 1970s both the number of studies analyzing the relationship between crime and policing and the statistical sophistication of these studies increased dramatically. The debate over the deterrent effects of aggressive patrol should be viewed in the context of a deterrence literature that generally lacks models of exactly which police policies affect which crimes and relies heavily on seriously flawed indicators of both police action and criminal activity. These problems are obviously intertwined; the lack of more diverse and meaningful indicators has thwarted the elaboration of our models, and barren models do not drive us toward more sophisticated measures (Wycoff, 1982 and Wycoff and Manning, 1983).

Specifying Social Conditions

Discussions of the reliability of official crime data are now legion (see Cook, 1977, 1980; Nagin, 1980; Wycoff, 1982 for summaries), though their gloomy conclusions are honored more in rhetoric than in research design. The basic alternative to official data is victimization data derived from citizen

surveys. While some field experimental analyses of deterrence use victimization data (e.g., Kelling, et al., 1974), few cross-sectional deterrence studies use them (Wilson and Boland, 1976). Victimization data, however, are to be preferred in studies involving a number of departments. This is because departments' reporting practices are a major source of systematic error in official crime data. That source of systematic bias in the data is removed in victimization survey data so that differences in departmental reporting practices do not masquerade as differences in criminal activity.

The validity issues surrounding the use of official crime statistics have not received as much consideration as the better known reliability issues. As Mary Ann Wycoff and Peter Manning indicate, in most studies of crime-focused policing "little regard has been given to the conceptual complexity of crime itself" (1983). Most studies simply deal with all reported (UCR) crime or all Index crimes (e.g., Allison, 1972; Carr-Hill and Stern, 1973; Ehrlich, 1973; Forst, 1976; Land and Felson, 1977; McPheters and Stronge, 1974; Morris and Tweeten, 1971; Orsagh, 1973; Phillips and Votey, 1972). Other researchers recognize a few conceptually compelling distinctions, such as that between property and personal crime rates (e.g., Avio and Clark, 1978; Chapman, 1974; Sjoquist, 1973; Swimmer, 1974; Thaler, 1977). There have been, however, some researchers who analyze specific offense types separately (e.g., Cloninger and Sartorius, 1979; Ehrlich, 1975; Mathieson and Passell, 1976; Phillips, et al., 1976; Wilson and Boland, 1976, 1978). These researchers recognize that populations of potential offenders, and their perceptions of risk and costs, may vary dramatically across different types of criminal activity. The "hedonistic calculus" of a potential robber may differ significantly from that of a potential vandal. Thus, it is important to sort out similar crimes in terms of their possible deterrents.

Specifying Policy Content

If analysts all too often think of crime as an undifferentiated set of phenomena, they also frequently conceptualize and operationalize police activities in an overly general fashion. For example, measures of municipal expenditures for police are quite often used as measures of police policy (e.g., Allison, 1972; Carr-Hill and Stern, 1973; Cloninger and Sartorius, 1979; Ehrlich, 1973; Forst, 1976; Land and Felson, 1977; McPheters and Stronge, 1974; Swimmer, 1974). Such analyses assume that expenditures are closely related to police activity on the street. But expenditure measures invariably contain "surplus construct irrelevancies" (Cook and Campbell, 1979); they include expenditures with no logical bearing on deterrence, as well as those that might deter crime.² To the extent that non-crime outlays account for variation in expenditures and vary with crime rates, the inverse relationship between expenditures and crime, which has heretofore been dubbed deterrence, is spurious (Morris and Tweeten, 1971).

Another service input measure commonly used as an indicator of police action is some rate based on the total number of police in a jurisdiction (Allison, 1972; Carr-Hill and Stern, 1973; Chapman, 1974; Cloninger and Sartorius, 1979; Morris and Tweeten, 1971; Mathieson and Passell, 1976; Orsagh, 1973; Phillips and Votey, 1976; Thaler, 1977). Although this type of measure seems clearly superior to expenditure data, "police on the payroll" may not imply "police on the street" (Wilson and Boland, 1978:370). One police department may, for example, require two officers in each car, and another may allow only one. These forces might have the same number of employees, but they would vary dramatically in their visibility in the community (Ostrom, 1983).

A number of other "natural variation" deterrence studies use data on police arrest or clearance rates as their measures of policy content (Avio and

Clark, 1978; Chapman, 1974; Cloninger and Sartorius, 1979; Ehrlich, 1975; Jacob and Rich, 1980-81; Mathieson and Passell, 1976; Phillips and Votey, 1972; Phillips, et al., 1976; Sjoquist, 1973; Thaler, 1977; Wilson and Boland, 1978). Such studies claims to internal and construct validity are far from compelling. All of these studies use official crime statistics and arrest data as indicators of levels of criminal and police behavior, even though the reliability of police department generated data is highly suspect in cross-sectional analyses (Sherman and Glick, 1982).

Though there are exceptions, most of these cross-sectional studies offer information of limited policy relevance. Studies based on input measures (i.e., personnel or expenditures) provide some useful information, but they give us no insight into the nature of the causal links between these input measures and crime rates. It is, after all, not simply the resources but how these resources are used that has an impact on the crime problem. Analyses of input measures do not identify effective agency technologies or indicate the usefulness of alternative allocation strategies given a fixed resource base. Even those analyses that analyze the effects of output measures (e.g., aggregate arrest or clearance rates) are of limited usefulness, leaving unanswered the important questions of how one increases arrests and failing to address issues as to exactly who should be arrested.

Finally, most quasi-experimental studies use indicators with somewhat higher face validity as measures of police activities. Many such studies involve analyses of the introduction of various patrol deployment strategies -- preventive auto patrol (Kelling, et al., 1974), foot patrol (Police Foundation, 1981), directed patrol (Cordner, 1981; Larson and Runcie, 1982), and team policing (Schwartz and Clarren, 1978).³ These studies are in some ways our best tests of the deterrent capability of police (see Cook,

1980). These studies provide our most useful results because of their much more precise specification of policy content and their strong claims to internal and construct validity (see Cook and Campbell, 1979, for a detailed discussion of the various forms of invalidity). However, the external validity of these field experimental studies is questionable. Any observed effects may vanish with only minor changes in the strength of the treatment, its implementation, personnel characteristics, or some other aspect of the test environment.

Linking Aggressive Patrol and Crime

As Figure I indicates, the relationship between aggressive patrol and crime might take a variety of forms:⁴

1. Aggressive patrol might lead to a higher arrest rate. Potential offenders would correctly perceive the higher likelihood of apprehension and would reduce their activity levels. Victimizations would fall, as would reported crime.
2. Aggressive patrol might have no discernible effect on arrest rates, but might simply make potential offenders think that their probability of apprehension had changed. They would reduce their activity, and victimizations and reported crime would decrease.
3. Aggressive patrol might increase the contact of police with citizens and increase the likelihood that citizens will report victimizations. While it would have no effect on victimizations, active patrol might thus increase reported crime.
4. Aggressive patrol might alienate citizens, causing them to lose allegiance to societal norms. These alienated citizens would then increase their levels of criminal activity, increasing victimization and reported crime.

5. Aggressive patrol might alienate citizens from the police, making them less likely to report crime and reducing the reported crime rate, while having no effect on victimization.

The first three of these five possible relationships have found some support in previous research. Neither the fourth nor fifth possibilities are supported by analysis of the data on which this research is based (Whitaker, et al., 1984).

The most comprehensive discussion of the relationship between aggressive patrol and crime is that offered by James Q. Wilson and Barbara Boland (1978). Wilson and Boland maintain that an active patrol strategy does deter crime. They argue that aggressive patrol yields a higher arrest rate -- "the police are more likely to find fugitives, detect contraband...and apprehend persons fleeing from the scene of a crime" (1978:373) -- which in turn deters crime. But they also discuss the possibility that aggressive patrol affects crime directly, "if it leads would-be offenders to believe that their chances of being arrested have increased, even though they have not" (1978:374).

Wilson and Boland analyze the effects of police practices on three types of crime (robbery, burglary, and auto theft) by estimating the parameters of a system of simultaneous equations. Their results suggest that aggressive patrol deters robbery, although in their data it affects neither burglary nor auto theft. This analysis, however, rests on a number of questionable assumptions: 1) that the rate at which traffic citations are issued is a valid indicator of aggressiveness, and 2) that the level of aggressiveness is unaffected by the level of crime in a community. Furthermore, the estimate of the effect of arrest rates on crime is predicated on the arguable assumption that aggressiveness has no direct effect on crime.

[Figure I About Here]

However troublesome these assumptions may be, Wilson and Boland's conclusions are consistent with an earlier analysis of the deterrent power of one form (arguably, the core) of aggressive patrol. John Boydston (1975) analyzed the effects of suspending "field interrogations" in one section of San Diego for nine months. He found that reported crime (noticeably malicious mischief/disturbance) increased significantly when the field interrogation ceased and then decreased (especially petty theft) when they resumed. Since few of the field interrogations (2%) resulted in an arrest and significant changes in arrests were not associated with significant changes in crime, Boydston concluded that the deterrent effect of active patrol derived "mainly from the field interrogation process itself" (1975:40).⁵

In response to Wilson and Boland's study, Herbert Jacob and Michael Rich (1980-81) examined longitudinal data from ten cities and found that "the relationship between moving violations and robbery arrests -- whether for the same year or whether lagged by one year -- varies greatly from city to city." (1980-81:113) For some cities the relationship between these two quantities was negative but for others the relationship was positive. This finding has one of two implications: 1) that traffic enforcement patterns are not an accurate reflection of the elements of an aggressive patrol strategy, as Jacob and Rich contend, or 2) that arrest rates are unaffected by aggressiveness. Jacob and Rich also present evidence suggesting that aggressive patrol has no deterrent effect. To the contrary, they argue, "in some cities police activity actually increases the recorded robbery rate" (1980-81:120) by inducing citizens to report such victimizations.

ELABORATING MODELS OF AGGRESSIVENESS AND CRIME

The current analysis differs markedly from earlier studies. Because of our data base we are able to develop more precise measures of both crime and

aggressive police action. We use neither cities nor sectors within a single city as our unit of analysis. Our data are sensitive to intra-city variations in police practices, yet cover a broad spectrum of urban and suburban residential areas served by twenty-four police departments. We use data on sixty residential neighborhoods in three metropolitan areas -- Rochester, NY; St. Louis, MO; and Tampa-St. Petersburg, FL. We estimate rates for four types of patrol aggressiveness through the direct observation of officers on patrol in these neighborhoods, and we measure criminal behavior by victimization surveys in these neighborhoods (see Appendix I for a discussion of the data base).

The richness of the data set allows us to consider the effects of four distinct forms of officer-initiated activities. (The exact operationalization of each of our variables appears in Appendix II.) The first of these, which has thus far received the greatest attention, is officer-initiated suspicion stops; we analyze the effect of officers' propensities to investigate suspicious individuals, vehicles, or circumstances. A second category includes officer-initiated investigatory activities such as warrantless searches, crime scene inspections, and the questioning of potential witnesses beyond the immediate scene. The rate of residential security checks is our third indicator. The rate at which proactive order maintenance interventions occur (with drunks, public nuisances, juveniles, and loiterers) in a neighborhood is our fourth measure.⁶ Although Wilson and Boland (1978) suggest that the four forms of proactivity that we study might be multiple indicators of a single organizational "ethos" of aggressiveness, none of the correlations among the four was great in our data (the largest was .16).

Although we expect that some sorts of aggressive behavior are more effective against certain crimes, we estimate the effects of these four police behaviors on the victimization rates for five different types of

crimes--robbery, burglary, motor vehicle theft, theft of goods from autos, and vandalism. While victim surveys are not without their faults, their validity and reliability are "well within the normal range of the instruments of social science" (Department of Justice, 1981: 22), and they are especially appropriate for comparisons across departments, since they are free of department based biases.

[Figure II About Here]

The Model

As Figure II indicates, we hypothesize that the relationship between patrol aggressiveness and victimization is direct and does not operate indirectly through changes in the arrest rate. This assumption is supported by two observations. First, both the San Diego experiment and our data indicate that suspicion stops rarely culminate in arrest -- only 2 percent in the San Diego experiment (Boydston, 1975: 40) and 1.9 percent of the stops in our data. Second, Boydston's study showed no consistent relationship between rate of interrogation and arrest rates (31-3).⁷

Figure II also indicates that our analysis may not be free from the simultaneity that plagues most deterrence studies. However, it is not clear exactly what sign any simultaneity bias might take. There are sound theoretical arguments both for a bias that diminishes or hides any deterrent effect and a bias that masquerades as a deterrent effect. Each of the victimization rates that we study will be positively correlated with the total victimization rate. Total victimizations will in turn be positively related to the reported crime rate. Reported crime may be related to aggressiveness either directly or indirectly. A higher crime rate in a neighborhood may directly make officers feel that an aggressive patrol policy is necessary. Such greater aggressiveness due to more reported crime would statistically

dampen any deterrent effect that might exist. Conversely, the crime rate should be positively related to an officer's workload, which should be inversely related to aggressiveness -- the higher the demands for reactive policing, the less time for officer-initiated actions. Thus a lower assigned work load might be mistaken for deterrence. (This form of feedback is minimized to the extent that a higher reported crime rate in an area leads to the allocation of more patrol units to that area, resulting in a lower workload for individual officers and more time to engage in aggressive patrol work.) Because either of these forms of reciprocity would introduce bias in ordinary least squares (OLS) regression estimation, where possible we estimate our model's parameters with both OLS and two-stage least squares (2SLS).

We anticipate that suspicion stops will be effective against all five of the crimes our data permits us to analyse (robbery, burglary, auto theft, theft from an auto, and vandalism). Each of these criminal activities typically involves some public exposure on the part of the would-be criminal. Such exposure might make him liable to a suspicion stop and hence more apprehensive about committing that crime in a place where police make such stops at higher rates. Officer-initiated investigations might similarly have a deterrent effect on all sorts of crime committed in public view. Much of the additional evidence police may collect in these investigations is that from eye-witnesses who are not on the scene when police arrive. Greenberg and his colleagues, found that most arrests result from witnesses' identification of suspects during the initial investigation of a reported crime (1975). Thus, this form of aggressive patrol may also be the one most closely related to increased arrests and indirectly deter crime in that way. Residential security checks, in contrast, should mainly deter burglary and vandalism. They might have some effect on motor vehicle theft and theft from vehicles, but presumably little impact on robbery, which, unlike crimes of stealth,

requires that the victim be present. Order maintenance interventions, however, may be expected to deter all five of the crimes studied. As James Q. Wilson and George Kelling argue, police activity of this kind reassures citizens and helps encourage a positive attitude toward police and the neighborhood which may lead to actions which reduce crime (1982).

The relationships outlined in Figure II imply responses over time. Our data are cross-sectional, but we assume the factors in the figure are parts of a stable system. Interviews with police officials in our cities indicate that this is a reasonable assumption. Part of the debate between Jacob and Rich (1980-81, 1981-82) and Wilson and Boland (1978, 1981-82) revolves around whether time series or cross-sectional data are more appropriate for analyses of the effects of aggressive patrol. Wilson and Boland (1981-82) are probably correct in arguing that the greater variation found in cross-sectional analyses allows for a better test of the general relationship between crime and aggressiveness. However, longitudinal analyses may reflect more accurately the practical utility of policy change; relationships identified in cross-sectional research do not necessarily imply that the manipulation of the independent variables will, in a real world environment, result in change in the dependent variable. Since we are using cross-sectional data, we most directly address the general relationship between crime and proactive patrol. We do not find this troublesome because the existence of such a general relationship has yet to be firmly established. Such a relationship is a necessary, if not sufficient, requirement for the deterrent effect of a policy of aggressive police patrolling.

Because factors other than police activity influence victimization rates and may confound the relationship between aggressive patrol and crime, several other variables are included in our models. First, we want to separate the

effects of aggressive patrol from those of the level of patrol. We do this by controlling for density of patrol in a neighborhood. Police density is operationalized as the amount of patrol time per square mile in a neighborhood, during an average 40 hour work week. Neighborhood and population characteristics also appear in our models: the amount of poverty in a neighborhood, neighborhood racial composition, the percentage of the neighborhood population which is male and between the ages of fourteen and twenty-three (i.e., arrest-prone), the level of residential instability in our neighborhoods, and the proportion of the households with a high income. Descriptive statistics and a zero order correlation matrix for all the variables included in our analysis appear in Appendix III.

The levels of aggressive patrol behavior in our neighborhoods are difficult to compare with those elsewhere. The only grossly comparable data deal with suspicion stops come from the San Diego experiment. While our neighborhoods averaged from 0 to 3.9 suspicion stops for each 40 hour period (roughly 0 to 78 per month), the test areas in San Diego averaged between 14 and 88 field interrogations per month. No comparative data are available for other forms of aggressive patrol. Officers in our 60 neighborhoods performed an average of 2.6 residential security checks in every 40 hours of observation, and they initiated investigative action in roughly half of their crime encounters. Order maintenance interventions occurred roughly once in every 80 hours of observation, on average.

Some of the victimization rates reported in our neighborhoods are much lower than the general urban rates reported in other citizen surveys (Hindelang, et al., 1977:406). The respondents report an average of only four robberies per 1,000 residents for 1976, while cities in the national crime survey report much higher rates for 1975 (e.g., Chicago, 34.7; Portland, 15.7; Dallas, 12.3; Atlanta, 17.6). Only in our worst neighborhood was the robbery

rate similar to that found in the national survey (i.e., 18.5). Our auto theft rates (13.5) are also lower than general urban rates (range for the 13 cities--23 to 73 per 1000 households). These lower rates are probably attributable to two factors. First, our neighborhoods do not include the very worst of the residential areas. Second, for our purposes, we are only interested in victimizations that occurred inside our neighborhoods. If one of our respondents was robbed elsewhere, that robbery was not included in our measure of neighborhood victimization. Or, if a resident's car was stolen outside the neighborhood, that was not counted in computing our measure. Conversely, victimizations occurring in the neighborhood but suffered by non-residents are not captured in our victimization data. Respondents in our survey report burglary rates (95 per 1000) much closer to those in other cities (range--77 to 174) and very similar to that for Philadelphia) (91). That the second factor discussed above may account for much of the difference between our robbery and auto theft rates and those of national surveys is supported by the greater congruence of our burglary rates with national survey burglary data. Data for comparisons are not available on thefts from autos or vandalism.

Our victimization survey requested information on victimizations in the neighborhood during the last year. Highly transient neighborhoods will thus have reported artificially low victimization rates. As an adjustment for this problem, we base our victimization rates only on those neighborhood respondents who had resided in the neighborhood for at least a year.⁸

We estimate separate equations for each of the five kinds of victimization. As the equation below indicates, each model contains all of the neighborhood characteristics and the four proactivity measures.

$$X_1, \dots, X_5 = b_6 X_6 + b_7 X_7 \dots + b_{15} X_{15} + e$$

where:

X_1 = robbery rate (per 1000 persons)

X_2 = burglary rate (per 1000 households)

X_3 = theft from auto rate (per 1000 households)

X_4 = auto theft rate (per 1000 households)

X_5 = vandalism rate (per 1000 households)

X_6 = suspicion stops (per 40 hours)

X_7 = percent of crime encounters with officer-initiated investigation

X_8 = residential security checks (per 40 hours)

X_9 = order maintenance interventions (per 40 hours)

X_{10} = patrol density (per 40 hours per square mile)

X_{11} = percent of neighborhood households with less than \$5000 income

X_{12} = percent minority residents in neighborhood

X_{13} = percent young males in neighborhood

X_{14} = percent residents in neighborhood less than one year

X_{15} = percent of neighborhood households with over \$25000 income

The Findings

The results of our OLS analysis appear in Table 1. During the course of the discussion, we will give some attention to those coefficients that are at least equal to their standard errors, but we will pay closest attention to those coefficients roughly twice their standard errors. The outcome for at least one of our measures of patrol aggressiveness or activity seems very promising for the deterrence argument. Suspicion stops show quite a strong inverse relationship with robbery victimization rates ($b = -1.27$, $B = -.25$). The effects of this variable on auto theft rates ($b = -3.42$, $B = -.20$) and vandalism rates ($b = -8.48$, $B = -.20$) are somewhat stronger but not as stable.

The coefficients for the two other offenses, although in the direction predicted by the deterrence argument, are quite small.

(Table 1 About Here)

The findings for the other proactivity measures are not at all as supportive of the deterrence argument. The rate at which officers carry out residential security checks shows a consistently negative relationship with the level of victimization in a neighborhood, but the size of the relationship is notable only with vandalism ($b = -2.27$, $B = -.26$). Neither order maintenance interventions nor investigative actions bear a consistent relationship to victimization. However, order maintenance interventions have a marginal effect on level of vandalism in a neighborhood ($b = -11.49$, $B = -.12$). With investigative action comes our only anomalous finding — it is positively related to the level of auto theft ($b = .22$, $B = .16$).⁹

The basic finding in the OLS analysis is that the level of suspicion stops is the most effective type of aggressive patrol; it has the most consistent, significant effect on crime. We also find that vandalism is the crime most sensitive to several forms of proactivity; three of the four activities have some negative effect on that victimization rate.¹⁰

The density of police patrol consistently shows a positive relationship with the level of victimization. This is attributable to the reciprocity in the model. Police administrators allocate resources to neighborhoods through a formula based, in part, on reported crime: victimization in our data is positively correlated with reported crime. Any deterrence or displacement that the police presence might generate, in these data, is overwhelmed by this feedback.

The neighborhood characteristics in our victimization equations serve only as control variables, but a few of the findings related to these measures

are worthy of note. The percent of the neighborhood's population that is male and between fourteen and twenty-three years of age seems to be an important determinant of victimization levels. This is consistent with earlier findings using analogous indicators (Avio and Clark, 1978; Land and Felson, 1977; Morris and Tweeten, 1971; Orsagh, 1973; Thaler, 1977). Interestingly, the magnitude of this variable's effect varies by the type of victimization. Its impact is greatest on thefts from autos ($b = 10.63$, $B = .62$) and burglaries ($b = 10.29$, $B = .44$) and smallest on robberies ($b = .23$, $B = .10$). The effect of percent minority also depends on the type of victimization under consideration: for robbery its effect is positive and marginally significant; for vandalism its effect is strongly negative; for the remaining three types of victimizations it has no discernible effect. The level of residential instability displays, for four of our victimization types, an inverse relationship with crime level. This relationship is rather puzzling; it is the opposite of what one expects. Possibly, lower crime neighborhoods attract new residents.

Ideally, we would now present the results for a 2SLS analysis, which would give us unbiased parameter estimates for our patrol density measure and all our aggressiveness measures--i.e., purified of any reciprocity with victimization. In our attempts to develop purified aggressive patrol estimates, we used four variables which, according to theory, bear no direct relationship with victimization levels in a neighborhood: 1) the average number of years of police experience for the patrol officers in a department, 2) the size of an officer's department, 3) the percent of officers in the department who believe that politics has no effect on departmental policies, 4) the average number of contacts that an officer has with a supervisor during a single shift. Interestingly, neither these nor other department or officer characteristics proved useful in modeling the level of investigative

activities, order maintenance interventions, or residential security checks. For suspicion stops, however, these four variables work quite well, and we generate a satisfactory estimate of suspicion stops.

The results of the two stage analysis for suspicion stops (see Table 2) suggests that our earlier results may be minimal estimates of its crime prevention effects. The bias generated by simultaneity, at least for these data and this measure, seems to diminish suspicion stops' deterrent effect; higher victimization rates seem to generate more suspicion stops. In our two stage model, the coefficients for our suspicion stop indicator remain negative and become larger. For four of the five offenses (robbery, burglary, auto theft, and vandalism), the raw coefficient now exceeds an amount double its standard error. Only with theft from an auto does the coefficient hover near its standard error.

Similar 2SLS with our other aggressiveness or activity measures might lead to similar results; the effects of these measures might become more negative and significant. However, such a result is not a certainty. Higher victimization levels may not lead to higher levels of order maintenance interventions, investigative activity, or residential security checks.

Lest we mistake a deterrent effect for a displacement effect, we analyzed the deviations of the actual victimization rates from the rates we predicted with the OLS model. If high levels of suspicion stops merely displace rather than deter crime, we would expect to find that neighborhoods that are adjacent to neighborhoods with appreciably higher rates of suspicion stops would have a higher victimization rate than we would otherwise predict. We would expect, in other words, their residuals to be positive. Not all of our neighborhoods are contiguous; thirty-four bordered at least one other study neighborhood, in thirteen of those the suspicion stop rate was at least one standard deviation

(.92) lower than the rate in an adjacent neighborhood. While our test of this hypothesis is only suggestive, as Table 3 shows, there is no evidence that suspicion stops displace crime. For each victimization type, the actual level of crime in those neighborhoods with lower rates of suspicion stops than contiguous neighborhoods is, more often than not, lower than we predicted.

[Table 3 About Here]

CONCLUSIONS

This analysis indicates that some aggressive patrol activities in a neighborhood may reduce victimizations in that neighborhood. The simplicity of that statement may, however, be deceiving. Wilson and Boland argue that aggressive patrol is a complex of behaviors that derives from a departmental "ethos" instilled by organizational and political leaders. When we look at a variety of proactive behaviors that should all fall under the rubric of "aggressive patrol," however, we find that they display quite low intercorrelations. In part, that might be expected. Residential security checks will probably not occur in the same neighborhoods where conditions are conducive to high levels of suspicion stops. However, one would, if such an ethos is operative, expect such behaviors as suspicion stops, order maintenance interventions, and investigative action to occur within the same areas. But in our data they do not, and we must ask if there is, in fact, some complex of behavior that derives from an aggressive, professional ethic.

In the same way that we discover that "aggressiveness" may not be of a single piece, we find that the effects of various proactive patrol activities may vary dramatically. Suspicion stops appear to deter several types of crime, but the effects of other forms of active patrol are not as clear. The deterrent effects of the other aspects of proactive patrol may have been underestimated in our OLS analysis, but we cannot be sure.

Our analysis cannot clarify one important question: What is the mechanism through which suspicion stops work to generate deterrence? Is it through arrest rates or is its effect direct? We assume that the effect is direct, and we can marshal some support for that assumption. Few of our stops resulted in arrests, and that form of proactivity that should be of most help in generating arrest (investigative action) had no effect on victimizations. Our assumption, however, must still be counted as simply that. So few arrests were observed in our data that we cannot analyze the effects of arrests on crime. This uncertainty clouds the policy implications of our findings. If suspicion stops have a direct effect, then we know quite clearly what technology must be applied: suspicion stop rates should be increased. However, if the effect is indirect through arrest, then we could consider a broad set of reforms directed at increased arrest rates. Aggressive patrol is only one possible technology within that set.

The general policy implications of our effort are also far from clear. Whether what we discover in a cross-sectional analysis has any implications for the implementation of a policy change in a single jurisdiction is debatable. However, the San Diego experiment indicates that temporal changes in field interrogations did affect crime rates in one city. That study, unfortunately, offers no evidence on the probable results of a long-term strategy of high suspicion stop rates. Nor does our analysis address the issue of implementation. What types of organizational arrangements or incentives might induce patrol officers to change their styles of patrol? Another policy caveat concerns our limited analysis of displacement. While one department might not mind simply displacing crime into another department's jurisdiction, this represents a poor crime control strategy for the society as a whole. Until we can get some better evidence on possible crime displacement caused by increased suspicion stops, this is a rather risky technology to adopt.

Furthermore, we have little idea what negative side effects might accompany an increase in suspicion stops or other forms of aggressive patrol. Such issues must be addressed before we can support the use of suspicion stops as an effective crime fighting technology.¹¹

The ambiguity of the policy implications of our analysis are not terribly disturbing, because research on aggressive patrol is still in its preliminary stages. Our findings offer some hope that proactive patrol may deter crime, but we agree with Jacob and Rich that "we need to look further before we announce that aggressive patrol deters crime" (1980-81:121).

FOOTNOTES

1. Throughout the text we will use the terms aggressive patrol and proactive patrol interchangeably. They both refer to the general manner of policing discussed by Wilson and Boland (1978).
2. Elinor Ostrom, for example, argues that differences in expenditure levels for police may, in part, reflect differences in municipal union strength rather than varying levels of crime-focused police activity (Ostrom, 1983).
3. Other field experimental studies are simply analyses of the effects of substantial changes in police patrol levels (see Chaiken, 1978 or Zimring, 1978 for a review of these experiments).
4. These relationships are expressed in phrasing suggestive of experimental manipulation of the level of proactive patrol. They could as easily be phrased to suggest a cross-sectional relationship between crime and aggressive patrol.
5. Gary Cordner's (1981) analysis of the Pontiac directed patrol experiment is another quasi-experiment that touches on the effects of proactive patrol. The treatment in this field experiment, however, is multi-faceted, and it is impossible to determine whether it was changes in the levels of directed patrol, field interrogations, public relations contacts, or arrests that generated the changes in the crime level.
6. We omit traffic citations (Wilson and Boland's measure [1978]) from our analysis because we consider our measures much more theoretically sound indicators of proactive, crime-seeking behavior. Interestingly, though, in these data the neighborhood level correlation between suspicion stops and traffic citations per officer is quite high ($r = .67$).
7. Statistically, this assumption is of little importance. If our assumption is incorrect, our estimate for the effects of active patrol is

simply the product of two paths (active patrol-arrest-victimization) rather than the parameter for a simple path. The real importance of the assumption is substantive.

8. When we run the model with all victimizations, the results for our police variables do not differ dramatically. Our estimates also failed to change when we added dummy variables representing the three SMSAs from which our data were gathered.
9. Our measure of officer-initiated investigations may involve considerable measurement error since it is calculated as a percentage of crime-related encounters and those are quite infrequent in some study neighborhoods.
10. Our suspicion stop, order maintenance intervention and security check indicators reflect the behavioral propensities of the average officer or unit in a neighborhood. These measures do not reflect the frequency with which such behaviors occur in a neighborhood. The absolute frequency of each behavior depends on both officers' propensities and the number of units operating in a neighborhood. That it may be frequency, rather than propensity, which affects victimizations is a possibility we investigated by analyzing the effects of an interaction term derived by multiplying the number of units in a neighborhood by the neighborhood average for each type of aggressive behavior. The results of this investigation do not vary greatly from results based solely on the average aggressiveness of the officers.
11. We are pursuing some of these issues in other efforts. For a preliminary analysis of the determinant of patrol proactivity, see [deleted]. For an analysis of the effects of aggressive patrol on citizens' attitudes, see [deleted].

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Appendix I: The Data -- Components and Collection Procedures

The data were provided by the Police Services Study, a research project conducted jointly by the Workshop in Political Theory and Policy Analysis at Indiana University in Bloomington and the Center for Urban and Regional Studies at the University of North Carolina at Chapel Hill between 1974 and 1980. Part of the project consisted of intensive data collection in twenty-four local police departments. On-site data collection was conducted in the summer of 1977 by research teams assigned to three metropolitan areas in which the departments were located: Rochester, New York; St. Louis, Missouri; and Tampa-St. Petersburg, Florida. Departments were selected in each SMSA to produce a sample which would reflect a rough cross-section of organizational arrangements and service conditions for urban policing in the United States. The sample is thus not representative of the entire population of police departments in the United States, but includes a wide array of police in urban and suburban residential areas.

Although some data instruments relied upon agency records, most techniques were researcher-intensive -- conducted independently of agency supplied data. Research focused on patrol service to the sixty residential neighborhoods used in this study. These neighborhoods were selected to reflect a cross-section of the residential service conditions with which each department had to deal. Ethnicity and family income of residents served as the principal selection criteria, most neighborhoods being either predominantly white or predominantly non-white.

Two major data sets from the Police Services Study were used in the construction of the variables in this study. The first, observation of patrol officers, involved 7200 hours of in-person observation by trained researchers of more than 500 patrol officers in a matched sample (for day of

week and time of day) of 15 shifts for each of the 60 neighborhoods. During this time period, 5688 police-citizen encounters involving more than 10,000 citizens were observed. Detailed coding of each encounter covered 650 variables, including how the encounter was initiated and what the officer did. A summary of the non-encounter events on each shift was also coded. Our indicators of police activities are neighborhood-level aggregations of these data, each representing a rate of activity per patrol per eight hour shift in a neighborhood.

The source of our victimization and demographic data was a survey of a random sample of neighborhood residents. Approximately 200 residents per neighborhood were interviewed by telephone. There were 172 items per interview, including respondent characteristics and household victimization. These data were aggregated to comprise our crime and socioeconomic variables.

The organizational characteristics used in estimating Predicted Suspicion Stops for the two-stage analysis were constructed from agency records, surveys of officers in each department, and the patrol observation data.

APPENDIX II: Operationalization of Variables

VICTIMIZATION RATES

(All victimization rates are based on data for only those respondents who had resided in the neighborhood for at least one year. All victimizations occurred in the study neighborhood and within one year of the interview.)

Burglary (per 1000 households) includes burglaries, attempted burglaries and break-ins.

Robbery (per 1000 residents) includes robberies and attempted robberies.

Auto theft (per 1000 households) includes motor vehicle theft and attempted motor vehicle theft.

Theft from Auto (per 1000 households) includes theft from motor vehicle; attempted theft from motor vehicle; break-in into motor vehicle; and attempted break-in into motor vehicle.

PROACTIVE BEHAVIORS

Suspicion stops are expressed as rate per 40 hours of observed time, and include the following types of encounters: suspicious persons; prowler; suspected violator; person wanted by police; unauthorized entry; trespassing (residential and commercial), suspicious motor vehicle; open door or window; miscellaneous stops of juveniles.

Investigative actions are expressed as percentage of crime encounters in which an officer performed any one (or more) of the following activities: searched premises or car without a warrant; looked around crime area or car; questioned persons outside of the immediate scene.

Security checks are residential security checks per 40 hours of observation.

Order interventions (expressed as a rate per 40 hours of observation) include the following problem types: public nuisance; drunk; vagrancy;

loitering; obscene activity; noise disturbance; peddling; begging; gambling; prostitution; curfew violation; juvenile problem; harassment; missing person; juvenile runaway; miscellaneous juvenile problems.

CONTROL VARIABLES

Patrol density is the mean unit-hours of nonadministrative time per square mile per 40 hours of observation.

Minority is the percentage of neighborhood residents who are not white.

Young males is the percent of residents who are male and between the ages of 14 and 23 (inclusive).

New residents is the percent of residents who have resided in the neighborhood for less than one year.

Poverty is the percent of households with income less than \$5000.

High Income is the percent of households with income over \$25000.

VARIABLES USED IN CONSTRUCTION OF INSTRUMENT FOR 2SLS

Patrol experience is the mean length of service (in years) of all patrol officers in the department.

No politics is the percent of all interviewed officers (of any rank) that disagreed with the following statement: "Local politicians have too much influence over the police department."

Employees is the natural log of the total number of full-time sworn and civilian personnel.

Supervisor contacts is the mean number of contacts with supervisors -- in the field, face-to-face or by radio -- per 8 hours of observation, for all observed officers in each department, weighted by time observed.

APPENDIX III: Descriptive Statistics

	<u>MEAN</u>	<u>ST. DEV.</u>	<u>MIN.</u>	<u>MAX.</u>
Burglary	94.67	50.80	0	221.67
Robbery	3.75	4.80	0	18.46
Auto theft	13.45	15.90	0	71.09
Theft from auto	70.46	37.00	15.08	197.04
Vandalism	69.81	39.13	5.35	201.09
Suspicion stops	1.30	.92	0	3.90
Investigative actions	49.03	11.54	20.00	73.68
Security checks	2.61	4.41	0	21.15
Order interventions	.42	.40	0	1.39
Patrol presence	10.13	9.15	.90	48.73
% Poverty	21.64	15.44	0	58.40
% Minority	35.10	37.83	0	100.00
% Young males	10.17	2.15	5.05	13.92
% New residents	2.55	2.98	0	15.25
% High income	15.75	7.82	4.50	40.93
<u>Other Variables</u>				
Patrol experience	6.07	1.38	3.77	11.00
% No politics	40.56	20.11	5.00	90.47
# Employees - log	5.55	1.59	2.64	7.88
Supervisor contacts	2.45	1.06	.75	5.58

APPENDIX III continued: Correlation Matrix

	X ₁	X ₂	X ₃	X ₄	X ₅	X ₆	X ₇	X ₈	X ₉	X ₁₀	X ₁₁	X ₁₂	X ₁₃	X ₁₄	X ₁₅
X ₁ Robbery	--														
X ₂ Burglary	.61														
X ₃ Theft from auto	.45	.45	.49	--											
X ₄ Auto theft	.73	.51	--												
X ₅ Vandalism	-.12	-.04	.03	.30	--										
X ₆ Suspicion stops	-.32	-.17	-.29	-.23	-.31	--									
X ₇ Investigative actions	.06	.09	.21	.07	.00	-.05	--								
X ₈ Security checks	-.08	-.01	-.04	-.05	-.16	.13	-.07	--							
X ₉ Order interventions	-.01	-.00	.01	-.01	-.19	.16	-.11	-.10	--						
X ₁₀ Patrol presence	.45	.44	.46	.24	-.18	-.09	.09	.15	.10	--					
X ₁₁ % Poverty	.43	.34	.28	.11	-.34	-.09	.05	-.21	.07	.35	--				
X ₁₂ % Minority	.47	.45	.33	.16	-.48	.06	.10	-.04	.45	.47	.62	--			
X ₁₃ % Young males	.35	.56	.38	.57	.14	-.29	.13	.01	-.08	.27	.22	.34	--		
X ₁₄ % New residents	-.25	-.27	-.08	-.18	.08	.10	.13	-.11	.01	.13	.06	-.10	-.23	--	
X ₁₅ % High income	-.29	-.05	-.15	.00	.27	-.07	.04	.27	-.14	-.15	-.42	-.13	-.42	-.38	--

TABLE 1: OLS Results for Victimization Rates and Aggressiveness (N=60)

	<u>ROBBERY</u>	<u>BURGLARY</u>	<u>THEFT FROM AUTO</u>	<u>AUTO THEFT</u>	<u>VANDALISM</u>
SUSPICION STOPS	-1.27/-.25* (.58)	-.72/-.01 (6.11)	-.64/-.04 (4.95)	-3.42/-.20 (2.14)	-8.48/-.20 (4.87)
INVESTIGATIVE ACTIVITY	.01/.03 (.04)	.19/.04 (.46)	.04/.01 (.37)	.22/.16 (.16)	-.21/-.06 (.36)
RESID. SECURITY CHECKS	-.05/-.04 (.12)	-.08/-.01 (.30)	-.28/-.03 (1.04)	-.02/-.01 (.45)	-2.27/-.26 (1.02)
ORDER INTERVENTIONS	-.63/-.05 (1.26)	-3.15/-.02 (13.40)	.68/.01 (10.80)	.12/.003 (4.66)	-11.49/-.12 (10.64)
PATROL DENSITY	.03/.29 (.01)	.30/.27 (.14)	.12/.14 (.11)	.11/.33 (.05)	.06/.08 (.11)
% POVERTY	.02/.05 (.05)	-.05/-.01 (.48)	-.31/-.13 (.39)	-.07/-.07 (.17)	-.56/-.22 (.39)
% MINORITY	.03/.21 (.02)	.17/.13 (.20)	-.09/-.09 (.16)	.04/.08 (.07)	-.42/-.40 (.16)
% YOUNG MALES	.23/.10 (.27)	10.29/.43 (2.90)	10.63/.62 (2.37)	1.71/.23 (1.02)	4.08/.22 (2.34)
% NEW RESIDENTS	-.55/-.34 (.18)	-4.88/-.29 (1.95)	-1.89/-.15 (1.58)	-.87/-.16 (.68)	1.85/.14 (1.58)
% HIGH INCOME	-.27/-.28 (.08)	-1.51/-.23 (.84)	-1.23/-.26 (.69)	-.53/-.26 (.30)	.80/.16 (.68)
Intercept	4.16	-.52	-9.02	-5.27	66.58
R ²	.52	.51	.40	.39	.48

*raw coefficient/standardized coefficient
(standard error)

TABLE 2: 2SLS for Victimization Rates, Using Estimated Values for Suspicion Stops[§]

	Robbery	Burglary	Theft From Auto	Auto Theft	Vandalism
Suspicion Stops (OLS)	-1.36/-0.26* (.55)	-1.09/-0.02 (5.80)	-1.82/-0.05 (4.69)	-3.55/-0.21 (2.06)	-10.78/-0.25 (4.87)
Predicted Suspicion Stops (2SLS)	-3.46/-0.71 (1.03)	-22.97/-0.45 (10.87)	-9.21/-0.26 (7.97)	-7.53/-0.44 (3.54)	-18.84/-0.41 (8.31)
R ²	.48	.47	.39	.36	.41

Predicted
Suspicion Stops = 3.65 + .007 % POVERTY + .002 % MINORITY - .11 % YOUNG MALE - .02 TRANSIENCE
(.01) (.004) (.05) (.04)

+ .002 HIGH INCOME - .22 AVG. YRS. OF EXPERIENCE FOR PATROL OFFICERS
(.02) (.78)

- .15 NO. OF EMPLOYEES -- LOG + .01 % NO POLITICS + .19 CONTACTS WITH SUPERVISORS
(.07) (.006) (.11)

R² = .45

§ same equation as in Table 1 except that other proactivity measures were deleted -- other coefficients not displayed. The OLS parameters are taken from an OLS model using the same variables as the 2SLS.

* b/D
(std. error)

Table 3

OLS Residuals for Neighborhoods with Lower Levels of Suspicion Stops
than an Adjacent Neighborhood

Victimization Type	Positive Residuals	Negative Residuals
Robbery	2	11
Burglary	5	8
Auto theft	3	10
Theft from auto	3	10
Vandalism	5	8

FIGURE I. POSSIBLE RELATIONSHIPS BETWEEN VICTIMIZATION, REPORTED CRIME, AND AGGRESSIVE PATROL

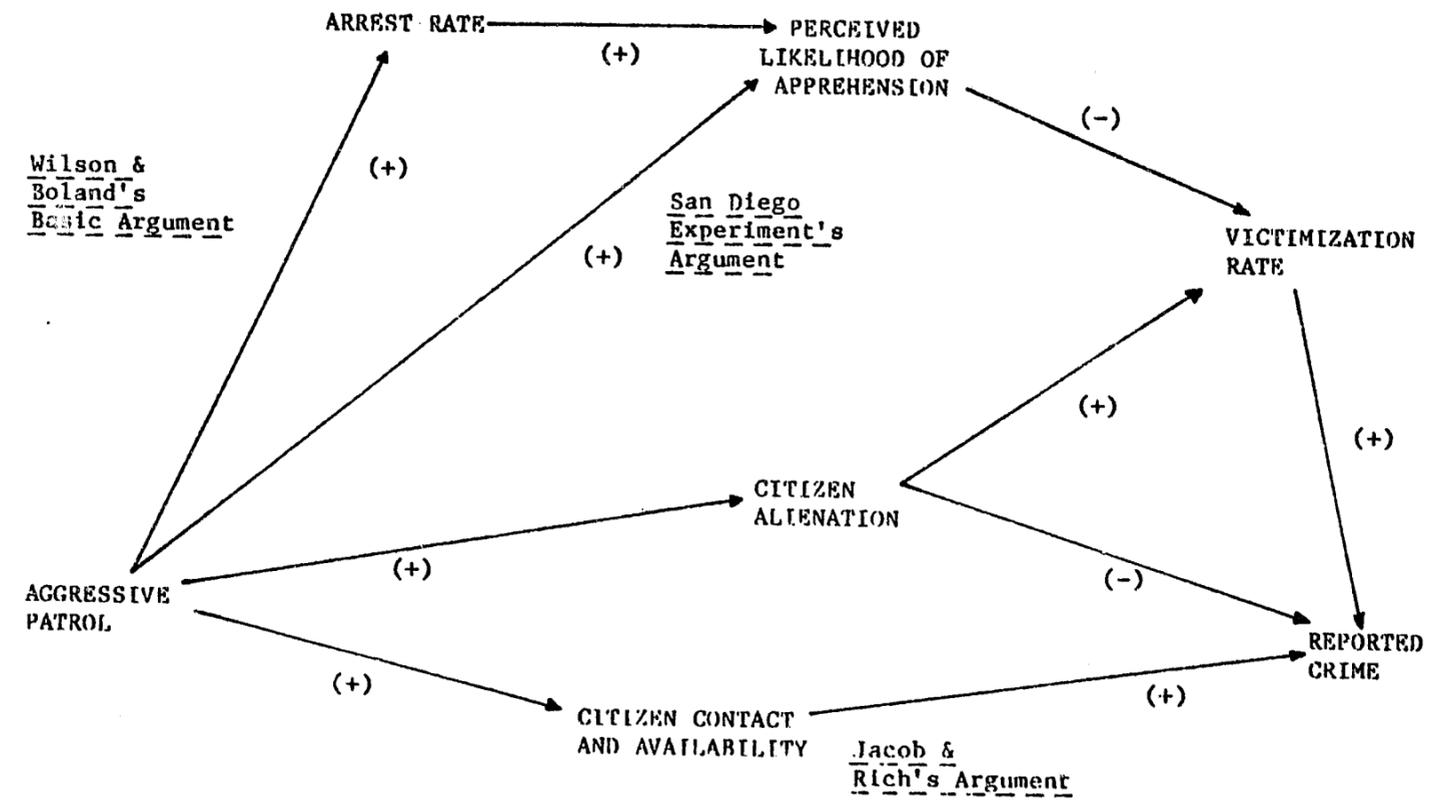
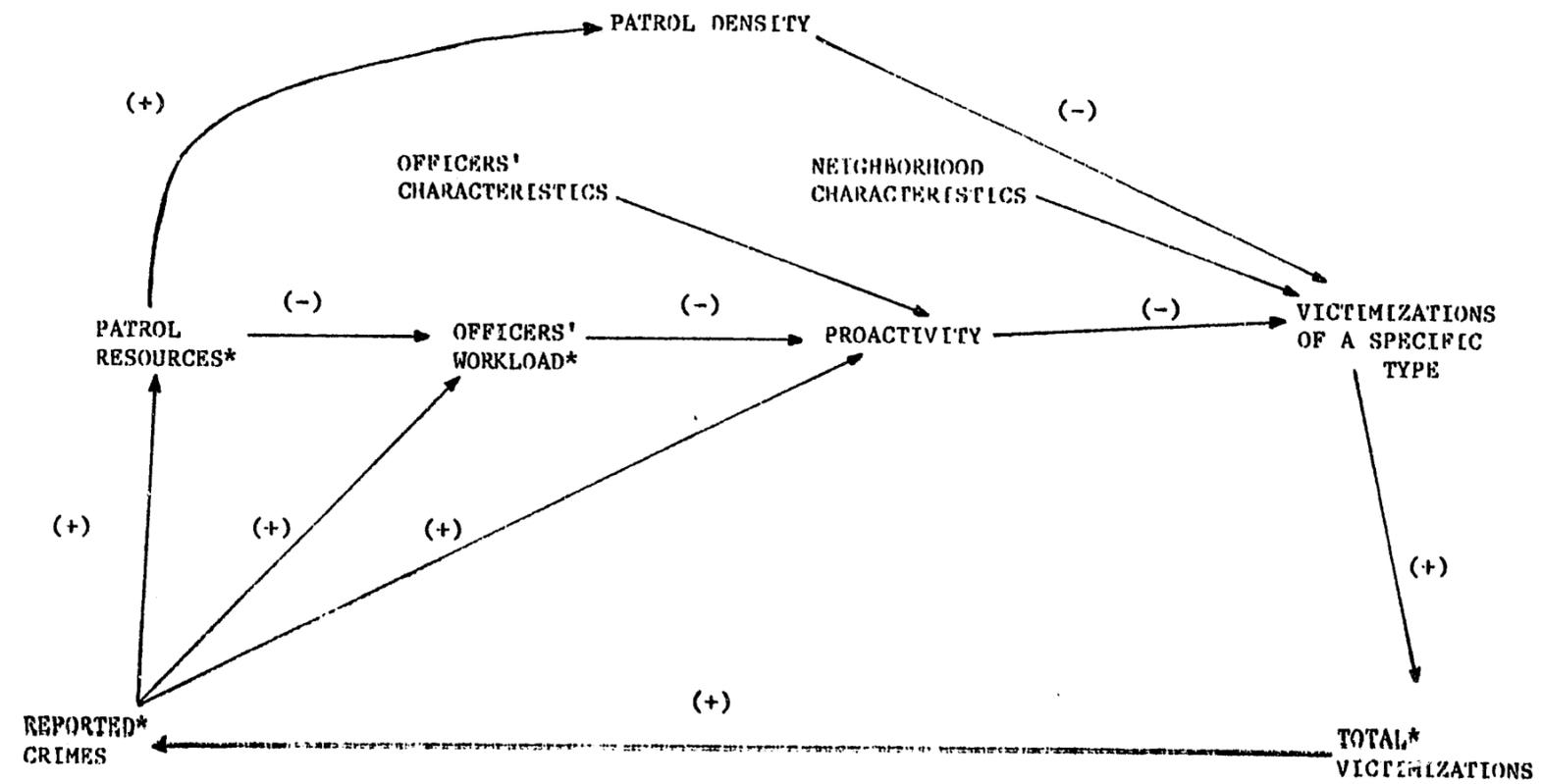


FIGURE II: A MODEL OF ACTIVE PATROL AND ITS RELATIONSHIP TO VICTIMIZATION



*These variables are not included in our analysis. They appear here simply to illustrate the way feedback might occur.

END