

THE EFFECT OF THE POLICE ON CRIME

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The effect of police practices on the rate of robbery in 35 large American cities is estimated by a set of simultaneous equations. The measures of police resources (patrol units on the street) and police activity on the street (moving citations issued) are more precise than anything thus far available in studies of this kind and permit the use of identification restrictions that allow stronger inferences about the causal effect of arrests on crime rates than has heretofore been possible. Police resources and police activity independently affect the robbery rate after controlling for various socioeconomic factors. The political arrangements that lead to the use of aggressive patrol strategies are discussed and their effect estimated. The implications for, and limitations upon, policy are also discussed.

This is an attempt to estimate the effect of police practices on the rate of robbery in 35 large American cities and to set forth some reasons why those practices vary from city to city. Several previous studies have dealt with police effects on crime but all have been criticized: some for the measures used, others for the estimation procedures employed. Differences in police practices among cities have sometimes been explained, but usually as a result of differences in resources (expenditures on the police). We shall present data that are consistent with the view that police patrol strategies have an effect on the rate at which robberies are committed, that this effect is a causal one and not the result of a spurious statistical correlation, and that the existence of a given patrol strategy is affected by bureaucratic decisions as well as by levels of resources. We conjecture that these decisions about police practices can be altered independently of expenditures on the police, within certain unmeasured political constraints.

I. PRIOR STUDIES

A number of studies have found a strong negative correlation between the rate at which persons are arrested for an offense and the rate at which that offense occurs. The results of these studies are by no means unambiguous but they broadly

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suggest that police behavior, as measured by either clearance rates (the proportion of those offenses known to the police that are "cleared" by an arrest) or arrest rates (the ratio of arrests for a given offense to the number of such offenses reported), affects the crime rate independent of other social factors. Tittle and Rowe (1974), for example, found that among cities and counties in Florida, the higher the clearance rate the lower the total index crime rate,¹ at least for those jurisdictions in which 30 percent or more of the reported crimes are cleared by arrest. (The authors argued that there is a "tipping effect" such that differences in clearance rates only affect crime rates when the former exceed a certain minimum level.) Bailey (n.d.) re-analyzed the Florida data, looking at each major crime separately, and found that the negative relationship between clearance rates and crime rates persisted for certain offenses (primarily those committed for gain) but not for others. Block (1972) found a negative correlation between clearance rates and property crime rates among police precincts in Los Angeles; Phillips and Votey (1972) also found such a relationship using national data for the years 1952-67. Logan (1975) compared clearance rates with reported index offenses and found a negative relationship for property crimes using statewide data for five successive years.

There is a problem in employing the clearance rate in measuring police effect on crime: a crime "cleared by arrest" is whatever the police say it is. Some departments and officers conscientiously report offenses as cleared only when they have taken into custody a person against whom they have sufficient evidence to bring charges. Such arrested persons may, and often do, admit that they have committed offenses other than that for which they were arrested; the police are entitled to count these as having been cleared. But obviously the suspect will be influenced by many things in admitting other offenses: his memory and eagerness to boast, confess, or please his captors, and the interest of the police in eliciting additional admissions (Skolnick, 1966). Clearance rates may vary substantially among departments for reasons having nothing to do with the objective probability of getting caught.

A few studies of the police employ arrest ratios rather than clearance rates. Though there are difficulties with this index as well, at least it introduces less measurement error—an ar-

1. The total index crime rate, as defined by the FBI, is the total number of murders, rapes, robberies, aggravated assaults, burglaries, auto thefts, and larcenies reported to the police per 100,000 population.

rest is a more or less concrete, easily counted event not dependent on the vagaries of memory and interrogation. Sjoquist (1973) examined 53 middle-sized American cities in 1968 and found that the arrest ratios for property crimes (robberies, burglaries, and larcenies) were negatively related to the rates at which those crimes were reported, independent of various socioeconomic factors. Wilson and Boland (1976) compared arrest ratios and the rate at which serious robberies were recounted to Census Bureau interviewers in 26 large cities and found the same strong, negative association reported in other studies even though they used victimization surveys rather than Federal Bureau of Investigation (FBI) crime reports.

In sum, there is a good deal of evidence that supports the assertion that differences in the risk of arrest are associated with differences in the rate of crime. But there are at least two difficulties that stand in the way of drawing any policy conclusions from this observed association.

The first is the familiar problem known to economists as "simultaneity." Crime rates and arrest probabilities may be negatively associated either because high arrest rates cause lower crime rates or because higher crime rates cause lower arrest rates. The latter would be the case if a high and rising level of reported crime swamped police resources so that the rate at which criminals were arrested went down. Crime and arrest rates influence each other; they are "simultaneously determined." The observed negative association in the studies mentioned may mean either that arrests deter crime or that crime "deters" arrests.

The second problem arises from the fact that changes in police practices may affect the crime rate through causal linkages that do not increase the chances of being arrested. It may be difficult or impossible to increase this probability because the police have little or no control over such decisive factors as the willingness of citizens to report a crime, offer information to the police, or identify suspects. But the police may be able to do other things that affect the crime rate and affect it only indirectly, or not at all, by altering the chances of being arrested. These direct deterrence effects depend on the number and activity of patrol officers. An offender may alter the rate at which he commits crime not because the *actual* chance of being caught has increased but because he *perceives* that it has, perhaps because he sees more officers than usual or more activity among them.

Thus far efforts to see if crime rates are affected by differences in police presence—usually measured by the number of police per capita—have proved unavailing. Cross-sectional statistical studies have found either no relationship between police per capita and crime rates or a perverse one: the greater the police presence, the higher the rate of crime. But such studies are clearly defective in that they do not measure how many police are *on the street*, only how many are on the payroll and, as we shall see, there is only a weak relationship between the size of a police force and the number of officers it deploys on patrol. Studies like the Kansas City Preventive Patrol experiment suggest that simply increasing the number of patrol units in a given beat will not have any effect on crime rates (Kelling *et al.*, 1974). Moreover, some of the studies that purport to discern a perverse relationship between police per capita and crime rates (e.g., Pressman and Carol, 1971; Allison, 1972) fail to take into account the obvious fact that cities hire more officers because their crime rates are high; in other words, the causal direction of the relationship may be reversed.

But the police may affect crime rates less by *how many* of them are on patrol than by *what* they do there. What they do includes many things in addition to, and perhaps more important than, making arrests. Though patrol behavior is complex, we can distinguish two extreme strategies: “aggressive” and “passive.” By an aggressive strategy we do not mean that the officer is hostile or harsh but rather that he maximizes the number of interventions in and observations of the community. In another study, Wilson (1968) has referred to this as the “legalistic” police style. An officer follows a passive strategy when he rarely stops motor vehicles to issue citations for moving violations or to check for stolen cars or wanted fugitives, rarely stops to question suspicious persons, and does not employ “decoy” or stake-out procedures in areas with high crime rates. When an officer acts in the opposite manner, he is employing an “aggressive” strategy.

In San Diego, a well-designed experiment showed that one component of an aggressive patrol strategy—field interrogations or “street stops”—is associated with a significant decline in certain kinds of crime. In one area of the city, field interrogations were eliminated, whereupon the number of “suppressible” crimes rose by about a third; when field interrogations were resumed the number of such crimes dropped. (“Suppressible” crimes were defined as robbery, burglary, theft, auto theft, assault, sex crimes, malicious mischief, and disturbances.)

There was no change in the frequency of suppressible crimes in control areas where field interrogation practices continued unchanged. The presence or absence of field interrogations did not, however, affect the number of arrests in the experimental areas (Boydston, 1975). So far as we know, there is no study that attempts to see whether other components of aggressive or passive patrol strategies are associated with changes in the level of reported crime.

To achieve an aggressive patrol strategy a police executive will recruit certain kinds of officers, train them in certain ways, and devise requirements and reward systems (traffic ticket quotas, field interrogation obligations, promotional opportunities) to encourage them to follow the intended strategy. This used to be, and for many officers still is, the core of the concept of "police professionalism." Some police chiefs have developed a new theory of professionalism that emphasizes community service, but the choice of an aggressive patrol strategy is still quite common.

The major problem in testing a theory that relates crime rates and police strategies is to find valid and systematic measures of an aggressive strategy and the procedural arrangements that sustain it. These data are not readily available and efforts to gather them may be hindered by the fact that many departments are unaware of, or reluctant to comment on, the relationship between reward systems and patrol strategies. We have selected a measure we believe is a reliable, though not complete, proxy for the existence of an aggressive strategy: the number of citations for moving traffic violations issued per sworn officer.

In an earlier study, one of the present authors (Wilson, 1968) found that where one element of a "legalistic" (or aggressive) patrol style was present, other elements were likely to be present as well. From our experience and that of others whom we have consulted, departments in our sample with high rates of traffic citations are generally thought by both their members and outside observers to display a strong commitment to "police professionalism" as conventionally defined. It is possible, of course, that the police in some cities may be aggressive about enforcing the traffic laws but lax about making street stops, checking suspicious persons, or employing other specialized patrol techniques. We think this unlikely, and we believe our statistical results, taken as a whole, suggest that this is not generally the case, but the reader should be aware that this is a possible limitation on the study.

II. THE MODELS

We shall investigate in two ways the possible effect of police practices on crime. First, we shall ask whether patrol strategies affect the probability of arrest for robbery and whether this probability in turn affects the rate at which robberies are committed. (We selected robbery for examination because we, and probably most citizens, think it an especially serious and fearsome offense.) In this first line of inquiry we assume that the probability of an arrest for any given robber is affected by, among other things, the level of police resources (measured by the number of patrol units on the street per capita) and the degree of aggressiveness of those units (measured by the number of moving traffic citations issued per patrol unit). Because the number of arrests that can be made will be constrained by the number of patrol units available, it is possible that any negative association between robbery rates and arrest rates can have either of two meanings: high rates of robbery have "swamped" police resources or high rates of arrest have lowered the robbery rate. We therefore estimate the effect of police strategies on arrests and of arrests on crime by means of simultaneous equations that explicitly take police resources into account. The model we estimate contains four separate equations: a crime rate equation that measures the effect of arrest rates on crime rates; an arrest productivity equation that measures the effect of an aggressive strategy, resources, and crime levels on arrest rates; a police deployment equation that explains the number of patrol units (per 100,000 population) on the street; and a resource equation that explains the number of police employees. Like most economic models of police behavior, this one assumes that communities respond to increased crime rates by hiring more police in the expectation that such hiring will lead to an increase in the arrest rates and a concomitant reduction in crime. According to the economic view of arrest productivity, what actually happens depends on how fast and to what extent police resources are increased relative to the crime rate. If the increase in the number of police is not proportional to the increase in the crime rate, or the hiring and training of additional police officers take a significant amount of time to implement, an increase in crime results in an overload on the police and a decline in arrest rates and deterrence, at least temporarily. In this model, departmental choice of a patrol strategy is viewed not as a response to high or rising crime rates or to community demands that the police do something about crime, but rather

as an organizational decision influenced by factors unrelated to crime; these latter will be investigated in a later section.

In estimating models of this sort where two variables, X and Y, are thought to affect each other mutually it becomes necessary to make defensible assumptions that there are additional factors that will affect one of the variables but not the other. These assumptions, called "identification restrictions," ensure that the estimating techniques can separate the effect of X on Y from the effect of Y on X. In the present case, since we are particularly interested in identifying the deterrent effect of arrests on crime, the critical identification restriction is a justifiable assumption that there are one or more factors that affect the arrest rate but not the crime rate (Nagin, 1978; Fisher and Nagin, 1978). If such factors can be found, we can develop logically and empirically separate equations to estimate the deterrent effect of arrests. We are then entitled to say that the observed negative association between the crime rate and the arrest rate cannot be wholly the result of crimes overloading the police because other factors, independent of the crime rate, also determine the arrest rate.

Drawing on our analysis in the preceding section, we suggest that the arrest ratio is affected by the choice of a patrol strategy, which is itself the result of various organizational and political decisions that are independent of the crime rate. We do not mean that police administrators create and sustain an aggressive patrol strategy without regard to its effect on crime—they will certainly justify it on that ground, among others. Rather, we argue that a city's patrol strategy cannot be predicted from its crime rate but can be explained by the political arrangements (governmental structure and animating ethos) within which police decisions are made. In an earlier study, we crudely approximated these independent "police strategy" factors by a subjective measure of "police effectiveness" as judged by outside observers and found that it was significantly, and negatively, correlated with the crime rate (Wilson and Boland, 1976).

We assume, in estimating this model, that patrol strategy affects the robbery rate only by changing the probability that an arrest will be made. It is easy to see how this might occur. By stopping, questioning, and otherwise closely observing citizens, especially suspicious ones, the police are more likely to find fugitives, detect contraband (such as stolen property or concealed weapons), and apprehend persons fleeing from the scene of a crime. But it is also possible that an aggressive pa-

rol strategy will affect the crime rate directly, and not through its effect on the arrest rate, if it leads would-be offenders to believe that their chances of being arrested have increased, even though they have not. If this occurs, then patrol strategy cannot be used to identify the crime rate by excluding it from the crime-rate equations, and thus we will not be able to say whether a higher arrest rate will drive down the robbery rate or a higher robbery rate will drive down the arrest rate. We know of no way to demonstrate the validity of our assumption, though we believe it is reasonable, for we have no information on the perceptions of would-be offenders and find it difficult to imagine how one would acquire such information.

To deal with this problem, we shall develop a second line of inquiry based on the assumption that patrol strategies may affect crime rates indirectly or directly—that aggressive patrolling reduces robberies either by increasing the actual chances of being caught or by misleading would-be offenders into believing that this has happened. In designing this model, we shall argue that the effect of patrol strategy on crime does not face the problem of simultaneity and thus can properly be estimated using ordinary least squares.

The first model is a set of four equations estimated using 1975 data gathered from 35 large American cities. The sources of the published data are given in the Appendix. The unpublished data (patrol units on the street per capita and the number of moving traffic citations issued per patrol unit) were gathered by the authors from the cities in the sample. The results, estimated using two-stage least-squares procedures, are presented in Table 1.

Equation 1(a) states that the rate at which robbery occurs is a function of sanctions (the arrest ratio for robbery), demography (the proportion of males ages 15-29 in the population, the proportion of the population nonwhite, and the unemployment rate), and opportunity (population density). Equation 1(b) states that the arrest ratio used in the preceding equation is a function of aggressive patrol (moving violations cited per patrol unit), the level of police resources relative to crime or workload (crimes per patrol unit) and demography (proportion of population nonwhite). Equation 1(c) states that the number of patrol units on the street (per 100,000 people) is a function of the level of police resources (number of sworn officers per 100,000 people), the proportion of officers assigned to two-officer cars, the housing density of the city, and the population. Equation 1(d) states that the level of police resources is a function of the

TABLE 1
ESTIMATING ROBBERY-ARREST-POLICE RELATIONSHIPS

Dependent Variables	Constant	Independent Variables				R ² (Corrected) ^a	
		Robbery Arrest Ratio	Age/Sex	Percent Nonwhite	Unemployment Rate		
(a) Robbery Crime Rate Coefficient (t-ratio) ^b	730.82 (1.49)	-11.6 (-2.02)	-39.3 (-1.62)	12.7 (5.74)	28.6 (1.42)	.024 (2.90)	.74
(b) Robbery Arrest Ratio Coefficient (t-ratio) ^b	12.3 (.80)	Moving Citations Per Patrol Unit (log) 6.54 (2.43)	Crimes Per Patrol Unit -1.0 (-3.74)	Percent Nonwhite .04 (.51)	West 1.17 (.34)		.25
(c) Patrol Units Per Capita Coefficient (t-ratio) ^b	38.4 (1.65)	Sworn Officers Per Capita .17 (3.91)	Percent Two-Officer Patrol Cars -46 (-5.04)	Housing Density -0.16 (-.72)	Population -0.005 (-2.63)		.72
(d) Sworn Officers Per Capita Coefficient (t-ratio) ^b	113.70 (.45)	Property Crime Rate -0.018 (-1.03)	Personal Crime Rate .169 (5.43)	Per Capita Tax Base .010 (2.42)	Officers' Salary .002 (.19)	Northeast 58.0 (1.18)	.50

Data: 35 largest cities in 1975 (see Appendix).

Method: Two-stage least squares.

a. R²s are not a valid measure of goodness of fit in simultaneous equation models estimated by two-stage least squares. They are reported as an item of interest to many readers.

b. t-ratio: Ratio of estimated coefficients to asymptotic standard errors.

rates of property and personal crime, available municipal resources (equalized property tax base per capita), the cost of adding additional police manpower (the starting salary of sworn officers), and a northeast regional dummy variable to control for the frequently observed tendency of large cities located in the northeast to spend more on municipal services.

The results are consistent with expectations. Controlling for demographic and opportunity variables, the robbery crime rate is strongly and negatively correlated with the robbery arrest ratio. This, in turn, is affected by both the deployment measures and the workload variable. The number of patrol units on the street is affected by the number of officers, the proportion in two-officer cars, and the population of the city. Police resources are determined by the rate of personal crime (but not by the rate of property crime) and by the available tax base.

Alternative versions of the crime equation 1(a) were estimated using different socioeconomic variables, including measures of income distribution, poverty, proportion of families on welfare, and the like. These other variables were not statistically significant and did not increase the explanatory power of the model. The unemployment variable is included in equation 1(a) though it is not significant; other economic variables do even less well (to our surprise).

Many of the cities with an aggressive patrol strategy are located in the western part of the United States. Cultural factors associated with region may have an effect on arrest ratios independent of an aggressive patrol strategy. To ensure that the moving citations variable is not just a proxy for these other factors, we inserted in equation 1(b) a regional dummy variable. This regional control variable is not significant.

We have repeated this analysis for two other crimes, burglary and auto theft, but without obtaining significant results for 1975 data. The poor results for burglary do not surprise us: it is a crime of stealth, rarely has eye-witnesses, leaves few clues, and is not visible to police patrol. The results for auto theft are harder to explain, since we had expected this to be a patrol-suppressible crime. The same model was estimated using 1970 crime and arrest data for auto theft, and the results were as predicted and quite robust: the auto theft rate was strongly and negatively associated with the auto arrest ratio, which was strongly and positively associated with the rate at which moving citations were issued. We do not know why high arrest ratios lowered the auto theft rate in 1970 but not in 1975,

nor why aggressive patrolling increased the arrest ratio in 1970 but not in 1975. One possibility is that by the early 1970s an increasing proportion of cars on the road were equipped with steering locks and other antitheft devices. These devices will not defeat a professional auto thief but they will frustrate, or at least discourage, casual auto thieves, mostly juveniles seeking joy-rides rather than income (Zimring, 1975). The proportion of juveniles among persons arrested for auto theft began to decline in the early 1970s, probably as a result of their decreased participation in the crime. This pattern may have proceeded unevenly, with a different mix of professional and casual auto thieves in different cities. Such a trend could explain the decline between 1970 and 1975 in the sensitivity of auto theft rates to arrest ratios and patrol methods.

Equation 1(b) suggests that the arrest ratio is influenced by both the level of resources (crimes per patrol unit) and how those resources are deployed (moving citations per sworn officer). Defining police resources in terms of patrol units on the street eliminates the paradoxical findings of other studies that use police expenditures or total police personnel as indices (cf. Greenwood and Wadycki, 1973; Swimmer, 1974). The reason some of these latter studies reach the implausible conclusion that police resources either have no effect on crime rates or a perverse effect is that the correlation in our sample between the total number of sworn officers and the number of patrol units on the street at a given time is only .48; if two extreme cases are eliminated (Boston and Washington, D.C.), the correlation falls to .24.

Equation 1(c) suggests that the number of patrol units per capita will depend on the number of sworn officers per capita, the proportion of the force assigned to two-officer rather than one-officer cars, and the population of the city (the larger the city, the *smaller* the proportion of the force on the street). A police administrator cannot easily increase the size of his or her force (city councils and mayors will have something, often negative, to say about that) but can, in principle, decide what proportion of the patrol force will be in two-officer cars. And that decision, as suggested by Equation 1(b), may have an important effect on the ratio of arrests to robberies.

The total size of the police force (sworn officers per 100,000 persons) is largely explained in Equation 1(d) by the rate of crimes against the person, and the funds available (per capita tax base). Other things being equal, cities with a higher rate of *personal* crime (homicide, rape, assault, robbery) will devote

more resources to police services than those with lower rates; cities with larger tax bases will employ more officers per capita than cities with smaller bases. Officers' starting salary, the rate of property crime, and the region in which the city is located are less important explanatory factors as indicated by the low value of the associated t-ratios.

In sum, the results of the simultaneous equations are consistent with the following theory: Cities that experience high rates of personal crime and have higher than average tax bases will hire more police employees in proportion to their populations. That it should be the rate of personal, not property, crime that generates this response is understandable: personal crimes apparently induce greater anxieties and thus fuel political demands in a way that property crimes, such as burglary and auto theft, do not. Some departments assign a high proportion of officers to patrol in one-man patrol cars and encourage them to adopt an aggressive patrol strategy, as evidenced by their tendency to issue disproportionately high numbers of traffic tickets for moving violations. These patterns of deployment and behavior increase the arrest rates for robbery. We would expect to find that police in such "aggressive" departments also make many "street stops" or field interrogations of suspicious persons, have a quicker than usual response time to citizen calls for service, and resort to arrest in a larger proportion of police-citizen encounters.

In those cities that manage to produce higher arrest ratios by employing an aggressive patrol strategy or increasing the number of patrol units, the robbery rates are lower than one would predict knowing only the socioeconomic composition of the city and the density of criminal opportunities. Thus, citizens do not necessarily have to spend more money to get more law enforcement; they can get it by having police organizations capable of devising and maintaining a personnel, incentive, and management system that delivers more law enforcement (Leibenstein, 1976).

III. EXPLAINING PATROL AGGRESSIVENESS

In addition to deciding what proportion of his personnel to put on the street and to deploy in one-officer units, the police administrator can choose a patrol strategy that will have an effect on arrest ratios independent of resources levels, and thus (if one accepts the assumptions stated earlier in this paper) on robbery rates.

On the basis of prior research (Wilson, 1968), we suggest that cities with a certain political culture will be more likely to select and support police administrators strongly committed to conventional doctrines of "police professionalism," among which will be an aggressive patrol strategy. By "political culture" we mean those widely shared expectations about how issues will be raised, governmental objectives defined, and the administration of public affairs conducted. One such culture, frequently described in the literature of urban politics, is what is loosely called "good" or "reformed" government (Banfield and Wilson, 1963). Among the attitudes contributing to this culture are the belief that the "best person" should be hired to perform each administrative job, even if that person is politically an outsider; that police administrators should not be subject to political interference as long as they act in accordance with prevailing professional doctrine; and that public decisions ought to be made on the basis of what is best for the community "as a whole" and should not favor particular neighborhoods, constituencies, or interests (Wilson and Banfield, 1964, 1971). Historically, this political culture has been institutionalized in the council-manager form of government operating on the basis of nonpartisan, at-large elections. At any given time, the culture will most likely animate such institutions when they are placed in the hands of a "professional" city manager—someone with advanced training in urban management and experience in governing other cities—in short, a specially trained cosmopolitan.

Since we cannot measure directly the presence of attitudes that correspond to a particular political culture without costly opinion surveys, and since the mere presence of nonpartisan, council-manager institutions may tell one little about how decisions are really made, we take as our measure of the underlying political culture the presence of a "professional" city manager. In an earlier study, Wilson showed that police forces in such cities were more likely to have high arrest rates in those routine cases—larceny, drunkenness, driving while intoxicated, disorderly conduct, and simple assault—that an officer could easily ignore or handle by means other than an arrest should he wish to do so (1968:275).

We now wish to see if our "political culture" variable—the presence of a professional city manager—can help us explain why some of our large cities have an aggressive patrol strategy as measured by the number of moving traffic violations cited per sworn officer. In Equation 2 we suggest that moving cita-

tions are a function of opportunity (the number of automobiles per capita) and politics (whether or not the city has a professional city manager) (see Table 2). We also test the possibility that aggressiveness is stimulated by police reactions to the presence of a large number of nonwhites: it is possible that the police might stop, question, and ticket nonwhites out of prejudice or suspicion.

TABLE 2
ESTIMATING THE NUMBER OF MOVING TRAFFIC
CITATIONS PER SWORN OFFICER

Independent Variables	Coefficient	t-statistic	Elasticity
Professional City Manager	0.49	2.2	
Automobiles per capita	0.028	2.4	0.25
Percent nonwhite	-0.0067	-1.09	-0.04

Constant = 3.15; corrected $R^2 = .32$; $F = 6.2$
 Method: Ordinary least squares
 Sources and definitions: see Appendix
 Sample: 35 largest U.S. cities in 1975 (see Appendix).

The results of ordinary least-squares regression are about as predicted. Both opportunity and political culture contribute significantly and independently to high rates of traffic ticketing. The correlation with proportion nonwhite is not significant.

In sum, a city with a "reformed" or professionalized municipal management system will be more likely to have a police department with an aggressive patrol strategy, though not necessarily one with many patrol units on the street. Aggressiveness and a larger number of patrol units, separately and in combination, will lead to a higher arrest ratio for robbery, and this higher ration, in turn, leads to a lower robbery crime rate.

We now relax somewhat the assumption we made at the outset that aggressiveness lowers the crime rate by changing the probability of arrest for that crime. Suppose that aggressiveness also affects crime directly, by making would-be offenders more apprehensive of the chances of being arrested, as well as indirectly, by making them worried. There is no way we can test the extent to which police activity affects crime through direct rather than indirect deterrence. But we do not believe that such a test is necessary given our purpose, which is to show the effect of police practices generally on robbery

rates. The model estimated in Equation 1 is consistent with both theories. Police practices do affect crime rates, directly or indirectly, and these practices are, so far as we can tell, the product of decisions made over the long term and not in response to short-term changes in crime rates or resource levels.

In our earlier study we used a different and admittedly more subjective measure of police style: three expert judges rated 23 big-city police departments as "professional" or "non-professional" in terms of their adherence to norms of efficiency and legalism. We entered this rating as a dummy variable in an ordinary least squares equation that estimated the rate of serious robberies. We repeat the results in Table 3.

TABLE 3

ESTIMATING THE EFFECTS OF POLICE STYLE AND THREE DEMOGRAPHIC VARIABLES ON THE RATE OF SERIOUS ROBBERIES IN 23 CITIES IN 1973

Independent Variables	Coefficient	t-statistic	Elasticity
Police professionalism (0,1)	-2.37306	-2.5967	
Percent nonwhite	0.06234	2.2598	0.28657
Labor force participation rate	0.06152	0.1431	0.16067
Density	0.00040	3.5974	0.48416

Constant = 1.887; corrected $R^2 = .68$; $F = 13.529$

Method: Ordinary least squares

Data sources and definitions:

Rate of Serious Robberies: number of robberies in which more than \$10 was stolen per 1000 population. Unpublished data from the LEAA victimization surveys provided by the National Criminal Justice Information and Statistics Service.

Police Professionalism: cities judged professional were: Cincinnati, Dallas, Los Angeles, New York, Oakland, Portland, St. Louis, San Diego, and Washington, D.C. Other cities in the sample were: Atlanta, Baltimore, Boston, Chicago, Cleveland, Denver, Detroit, Houston, Miami, Milwaukee, Newark, Philadelphia, Pittsburgh, and San Francisco. Unpublished data collected by the authors.

Percent Nonwhite: U.S. Department of Justice (1974a, 1974b).

Labor Force Participation Rate: labor force participation rate of men ages 16-34 living in low income areas (U.S. Department of Commerce 1972c).

Density: central city population per square mile (U.S. Department of Commerce, 1972d).

The consistency between this model and the one presented earlier in this paper, despite the use of different measures of the crime rate and of police style, strengthens our confidence that the police do make a difference and that this is not entirely dependent on resources.

IV. DOES IT REALLY WORK THAT WAY?

Two things are missing from this study that would be important to anyone wishing to change police practices in order to change crime rates. First, this study looks at many cities during one year whereas more might be learned by looking at a few cities over time. Differences among cities may not be the result of underlying factors we have not measured or factors that, if measured, would prove quite resistant to change. If it could be shown, however, that in several cities the arrest rate changed over a five- or ten-year period and that this was associated with changes in the crime rate (controlling for population change), then we could be more confident both that the correlation between crime and arrests is real and that we can change the former by changing the latter.

Second, this study does not investigate in detail how arrests are made or what happens on patrol when different strategies are employed. This sort of fine-grained analysis would be particularly valuable but it is especially hard to carry out. The day-to-day work of the patrol officer is no more visible to the academic observer than it is to the police administrator. Ideally, we would like systematic knowledge about the circumstances confronting an officer whenever he encounters an opportunity to make a street stop or an arrest, together with detailed information about what the officer did and the result. Obviously, such information would be very difficult to collect.

Unpublished data gathered by researchers from the Police Foundation are suggestive but permit few definite conclusions. For two large cities, all the arrests for major property crimes (robbery, burglary, larceny from the person) and stranger-to-stranger crimes of violence (aggravated assault) were tabulated for a period of time, together with available information on what the officer was doing just before the arrest and what happened to the arrestee after he was brought to the station. In one city, nearly 60 percent of the arrests occurred when an officer answered a call for help. About 60 percent of the arrests were dismissed before being presented to the prosecutor, usually for lack of sufficient evidence. What is most intriguing is that officers differed greatly in the extent to which their arrests survived this review process: of those officers who made 3 or more arrests, all the arrests made by 31 officers went to prosecution, whereas none of those made by 89 other officers was prosecuted. In Washington, D.C., another study found that fewer than 10 percent of the officers made over half the arrests; indeed, nine accounted for more arrests than 450 of their

fellows (Forst *et al.*, 1977). What explains these differences and whether we can change the behavior of officers who make few arrests, or whose arrests do not lead to prosecution, is beyond our present knowledge.

What are required to settle these issues are not more statistical analyses of current police practices and crime rates, but carefully designed experiments that measure the effect of innovations in police strategies on crime rates. Though there have been experiments on policing, most have attempted to measure only the effect of a greater or lesser police *presence* on crime, as in the Kansas City patrol experiment (Kelling *et al.*, 1974) and various quasi-experiments in New York City involving the 20th Precinct (Press, 1971) and Transit Authority police in the subway (Chaiken *et al.*, 1974). None of these made a significant effort to monitor what the police actually did. The San Diego field interrogation experiment, which analyzed the effect of street stops on crime and community attitudes, is the closest approximation in the existing literature (Boydston, 1975; Boydston *et al.*, 1977). The next step would be to introduce a generally aggressive patrol strategy (street stops, high traffic citation rates, quick response time) in an experimental area of a city and compare the crime rate with that of a control area where the same number of officers follow a passive patrol strategy.

Two studies of police response time contain some evidence that aggressiveness can make a difference. Isaacs (1967), studying the Los Angeles police, and Clawson and Chang (1977), studying the Seattle police, came to similar conclusions: a greater proportion of patrol unit responses to citizen calls resulted in arrests as response time decreased. There are limits, of course, to how much the arrest productivity of police units can be raised by accelerating their response: once a certain minimum has been achieved (roughly three minutes) differences in response time cease to affect arrests.² We believe that our studies, together with other research findings, are consistent with the view that police activity can reduce the rates of some serious property crimes, and at least offer a compelling case for experiments designed to test this conclusion and identify the processes by which arrest rates can be increased.

Suppose that statistical studies and experiments do confirm our beliefs. A police chief may still be unable to obtain the additional resources or change his deployment strategy so

2. Technically, the proportion of patrol car responses resulting in an arrest is a constant plus an exponentially decreasing function of response time (Chaiken, 1977:22).

as to achieve more aggressive patrols and higher arrest ratios. It may turn out that the principal constraints on arrest ratios are community attitudes that the police cannot alter: citizens may be tolerant of minor crimes, reluctant to call the police or offer evidence, unwilling to report suspicious activities or assist officers in pursuit of suspects, or irritated by frequent street stops and high rates of traffic tickets. Police unions may successfully resist efforts to redeploy the patrol force so as to put more units on the street in high-risk periods (such as the evening) or more officers in one-officer patrol cars. For though a recent carefully executed experiment suggests that officers who patrol alone produce as many arrests, and are as safe, as those who patrol with a partner (Boydston *et al.*, 1977), we are not optimistic that police who believe differently will be persuaded by its evidence.

We cannot offer any advice on how to deal with these problems. Social scientists can, ideally, tell practitioners what will happen *if* some changes are made, but they can rarely give scientific guidance about the political and human processes that might bring about these changes. Indeed, social scientists are likely to ignore or underestimate the unanticipated consequences—both good and bad—of any change, especially since the studies they conduct (like this one) typically consider only two kinds of results—crime rates and police practices.

APPENDIX

I. The Sample

Equations 1(a)-1(d) were estimated with data from 35 of the 46 largest central cities for which complete data were available. Unless otherwise indicated, all data refer to central city jurisdictions in calendar 1975.

The cities included were:

New York City, New York	San Antonio, Texas
Los Angeles, California	Boston, Massachusetts
Philadelphia, Pennsylvania	St. Louis, Missouri
Detroit, Michigan	New Orleans, Louisiana
Baltimore, Maryland	Phoenix, Arizona
Dallas, Texas	Columbus, Ohio
Washington, D.C.	Seattle, Washington
San Francisco, California	Jacksonville, Florida
San Diego, California	Denver, Colorado
Kansas City, Missouri	Portland, Oregon
Atlanta, Georgia	Oklahoma City, Oklahoma
Buffalo, New York	Oakland, California
Cincinnati, Ohio	Omaha, Nebraska
San Jose, California	Miami, Florida
Minneapolis, Minnesota	Tulsa, Oklahoma
Fort Worth, Texas	Honolulu, Hawaii
Toledo, Ohio	El Paso, Texas
Newark, New Jersey	

II. Variable Definitions and Sources

Age/sex

Definition: Number of males age 15-29 per 100 residents

Mean Value: 12.24

Source: U.S. Department of Commerce (1972b)

Automobiles per capita

Definition: Number of automobiles per single resident

Mean Value: 0.37

Sources: U.S. Department of Commerce (1972a, 1977)

Crimes per patrol unit

Definition: The ratio of total index offenses to the total number of patrol units

Mean Value: 197.02

Sources: Federal Bureau of Investigation (1976) and unpublished police data, obtained directly from respective police departments

Housing density

Definition: The number of single family detached units per 100 available housing units, vacant and occupied

Mean Value: 47.384

Source: U.S. Department of Commerce (1972a)

Moving citation per sworn officer (log)

Definition: The ratio of the number of moving citations issued to the number of sworn officers in log form

Mean Value: 4.19

Sources: Federal Bureau of Investigation (1976) and unpublished police data

Moving citations per patrol unit (log)

Definition: The ratio of the number of moving citations issued to the number of patrol units in log form

Mean Value: 5.8

Sources: Federal Bureau of Investigation (1976) and unpublished police data

Northeast

Definition: A dummy variable for those cities located in the Northeast, 1 = located in the Northeast and 0 = located elsewhere

Mean Value: 0.14

Officers' salary

Definition: The midpoint of the starting salary range for police officers

Mean Value: 1220.2

- Source:* International City Management Association (1971)
- Patrol units per capita**
Definition: The number of patrol units in an entire day, derived as $(1.5 \times \text{the number of patrol units at 10 A.M.}) + (1.5 \times \text{the number of patrol units at 10 P.M.})$, per 100,000 residents
Mean Value: 55.29
Source: Unpublished police data
- Per capita tax base**
Definition: The equalized per capita tax base; the ratio of gross assessed property value (1971) to population divided by the assessment-sales ratio (1971)
Mean Value: 8954.7
Source: U.S. Department of Commerce (1973a)
- Percent nonwhite**
Definition: Number of nonwhite residents per 100 total residents
Mean Value: 25.36
Source: U.S. Department of Commerce (1973b)
- Personal crime rate**
Definition: The number of personal crimes (assault, robbery, rape and murder) per 100,000 residents
Mean Value: 1113.1
Sources: Federal Bureau of Investigation (1976), U.S. Department of Commerce (1973b)
- Population**
Definition: Number of central city residents in thousands
Mean Value: 827.35
Source: U.S. Department of Commerce (1977)
- Population density**
Definition: Population per square mile
Mean Value: 6596.4
Sources: U.S. Department of Commerce (1973b, 1977)
- Professional city manager**
Definition: A dummy variable indicating if city is run by a city manager, 1 = city manager
Mean Value: 0.34
Source: International City of Management Association (unpublished data)
- Property crime rate**
Definition: The number of property offenses (auto theft, burglary and larceny) per 100,000 residents
Mean Value: 8185.8

Sources: Federal Bureau of Investigation (1976) and U.S. Department of Commerce (1977)

Robbery arrest ratio

Definition: Robbery arrests per 100 reported robbery offenses

Mean Value: 32.07

Sources: Federal Bureau of Investigation (1976) and unpublished arrest reports obtained from the Federal Bureau of Investigation

Robbery crime rate

Definition: The number of robbery offenses per 100,000 residents

Mean Value: 621.71

Sources: Federal Bureau of Investigation (1976) and U.S. Department of Commerce (1977)

Sworn officers per capita

Definition: The number of sworn officers per 100,000 residents

Mean Value: 275.31

Sources: Federal Bureau of Investigation (1976) and U.S. Department of Commerce (1977)

Two-officer patrol cars percent

Definition: Percent of all patrol units in two-officer cars

Mean Value: 40.33

Source: Unpublished police data

Unemployment rate

Definition: The unemployment rate (number of unemployed persons per 100 labor force participants)

Mean Value: 9.32

Source: U.S. Department of Labor (1976)

West

Definition: A dummy variable for those cities located in the West, 1 = located in the West and 0 = located in all other parts of the country

Mean Value: 0.25

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