# THE EFFECTS OF THE POLICE ON CRIME:
# A RESPONSE TO JACOB AND RICH

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In an earlier article in this *Review*, we argued, on the basis of a cross-sectional study of 35 large American cities, that as the proportion of robberies resulting in arrest rises, the rate at which robberies are committed falls (Wilson and Boland, 1978). By estimating a set of simultaneous equations, we suggested that this negative association between arrest rates and robbery rates was evidence of the deterrent effect of arrests on robberies. Moreover, we argued that the ability of the police to produce high arrest rates was not simply a function of police resources but also a result of organizational arrangements—chiefly, decisions about what proportion of the patrol force to put on the street and how aggressively those units should patrol. Finally, we supplied some evidence that these organizational arrangements tended to be the enduring consequence of an underlying political culture (namely, having a "reformed" or "professionalized" city administration).

Recently, Herbert Jacob and Michael J. Rich have criticized these findings (Jacob and Rich, 1980). By means of a longitudinal analysis of robbery rates in nine large American cities over (approximately) a 30-year period, they conclude that our findings are incorrect. In particular, Jacob and Rich argue the following. (1) There is no correlation over time between the number of traffic arrests for moving violations and the number of robbery arrests (we had used moving violation arrests as a measure of patrol aggressiveness in our study). (2) Any negative correlation between the robbery rate and the proportion of robberies resulting in arrests is likely to be spurious because there is a common term (the number of robberies) that is found on both sides of the equation, and this common term is subject to significant measurement error. (3) Increased expenditures on the police are associated with

higher, not lower, robbery rates, probably because increased expenditures lead to increases in the number of robberies reported by citizens and recorded by police. (4)   As the police devote more effort to making robbery arrests, they also record more robbery offenses; thus, making more arrests for robbery does not reduce the robbery rate.

## I.  REJOINDER

We believe that these criticisms are seriously in error and that they reflect a misunderstanding of the theoretical issues at stake, the selection of inappropriate measures of the key variables, and the use of incorrect statistical procedures.

### Moving Violations and Robberies

Our argument was that an aggressive patrol strategy would lead to an increase in the robbery arrest rate. No direct measure of police aggressiveness exists. Based on earlier studies of eight police departments (Wilson, 1968), we selected the number of citations for moving traffic violations issued per patrol unit on the street as a proxy for patrol aggressiveness. We did not argue that small—or even relatively large— differences in the issuance of traffic tickets would, by itself, affect the robbery rate. Rather, we suggested, on the basis of first-hand observation of several police departments, that very high ticketing rates (per officer actually on patrol) would be associated with important (but, unfortunately, unmeasurable) elements of an aggressive patrol style—frequent "field stops" of suspicious persons, frequent "car checks" of suspicious vehicles, and the use of "decoy" or other anticrime procedures.

In an earlier study (Wilson and Boland, 1976) we had used a different proxy for patrol aggressiveness—namely, ratings by knowledgeable observers of the degree of "professionalism" displayed by 23 large police departments. The results of using that measure are entirely consistent with the later use of moving violations as the proxy for aggressiveness. We reproduced that equation in our 1978 article. Jacob and Rich do not refer to it.

Since we were trying to identify the effect of organizational style or strategy on police arrest rates, we were calling attention to the effect of important structural differences among cities that, as we noted, are not likely to change much in the short run (indeed, there may be no significant changes for periods as long as ten or twenty years). That the differences among our cities were very large is suggested by the fact that

the city issuing the most traffic citations issued over *fifteen times* more, per patrol unit, than the city issuing the fewest.

In light of this, the failure of Jacob and Rich to find any short-term correlation, within a given city, between moving violation citations and the robbery rate is quite understandable. Longitudinal analysis is generally an inappropriate method for isolating the effect of large structural differences across political regimes. Moreover, to judge from the figures they present for Minneapolis, the average annual change in moving violations was on the order of plus or minus 30 percent. We are well aware, as they point out, that annual fluctuations in traffic ticket rates are likely to reflect a variety of short-term considerations. A 30 percent "swing" in tickets issued is of little significance. Moreover, Jacob and Rich's measure of tickets issued is not even the same as ours—they show the total number of tickets, while we showed the number of tickets issued per patrol unit on the street.

A longitudinal study is not the proper means of detecting the effect of major structural or strategic factors on police behavior. The difference between, say, San Jose and Philadelphia in police organization and style is very great, for reasons already fully developed in the literature (Wilson, 1968). The effect of these regime differences on police behavior is, to judge from our cross-sectional study, also very great—the most aggressive city in our study was *six times* as likely to make an arrest, given a robbery, than the least aggressive one. Year-to-year changes in traffic ticket rates in a given city are hardly likely to capture these structural differences.

## Measurement Error

Jacob and Rich are quite correct to point out that when one estimates an ordinary least squares (OLS) equation of the following form, there is a risk of a spurious negative correlation because of the existence of measurement error:

robberies/population = a + b (arrests/robberies).

But this observation is not a criticism of our research, for two reasons. First, the basic model in our study was a set of *simultaneous* equations estimated by two-stage least squares (2SLS). As econometricians have long noted, and as the Panel on Research on Deterrent and Incapacitative Effects of the National Academy of Sciences recently observed, the measurement error problem does not exist when one uses a simultaneous-equation model: "When simultaneous estimation methods are employed, the possibility of relations between

crime rates and sanction risk other than that due to deterrence is explicitly allowed for. The measured sanction risk is thus not assumed independent of variations in the unobservable factors affecting crime, including variations in crime reporting. This obviates the crime-measurement problem in a simultaneous formulation" (Blumstein *et al.*, 1978: 36).

Second, we had earlier published an OLS estimation of the deterrent effects of arrest rates in which, to guard against measurement error, we used as a measure of the crime rate victimization survey data (robberies reported to Census interviewers per 100,000 population). The parameter estimates derived from this equation are comparable, in sign and significance, to those derived from the 2SLS equations estimated in the 1978 article.

The measurement error problem, in short, is largely irrelevant to the research that Jacob and Rich are criticizing, and even when the problem arises (as in OLS), alternative measures of robberies produce comparable results.

### Higher Expenditures Lead to More Robberies

We are at a loss to understand what model of deterrence Jacob and Rich think they are testing by calculating correlation coefficients between expenditures on police in a city and the robbery rate in that city (either in the same year, or lagged by one year). All sophisticated studies of deterrence hypothesize that the rate at which a crime is committed is affected by, among other things, the probability of a sanction being imposed. Correlations between police expenditures and robbery rates make intellectual sense only if you assume (a) that every additional dollar spent on the police buys an increased probability of apprehension and (b) no other factors besides the apprehension risk affect the crime rate. Obviously, assumption (b) is false. In our study we estimated the supply of robberies by means of an equation that took into account differences in the age, sex, and racial composition of the city, the unemployment rate, and the population density. Jacob and Rich rejoin that the values of such variables change slowly, but we point out that their longitudinal study covers thirty years, more or less, during which time very large population changes occurred in all the cities in their sample. But even if they limit their attention to year-to-year changes in police expenditures, they still must make an argument for assumption (a).

No such argument is made. Economists have made such an argument, but in doing so point out that more crime also leads to greater expenditures on the police. To isolate the deterrent effect, if any, of police resources on crime requires a model that takes into account simultaneously these mutually interdependent relationships. Jacob and Rich have not specified or estimated such a model. Without this or some other explanation, it is impossible to interpret the meaning of their correlations. Their assertion that "increased expenditures led more to increased citizen reporting and police recording of robberies than to deterrence of them" is unsupported by any evidence.

Moreover, we argued that police arrest rates are determined in large part by factors (such as police aggressiveness and deployment practices) that are entirely independent of resource levels. As we point out, the correlation for the 35 cities 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, the correlation drops to .24 (Wilson and Boland, 1978: 377). Clearly, money spent on the police cannot affect the risk of apprehension unless it purchases additional patrol units or changes the behavior of existing units. Since this was one of the major arguments of our article, we are puzzled that Jacob and Rich proceed as if it had not been made.

*More Arrests Lead to More Robberies*

Jacob and Rich suggest that as the police focus on robberies (by increasing the proportion of all arrests that are for robbery), the number of robberies reported goes up, whether one looks at the same year or robberies lagged by one year. We are not certain what to make of this, or, indeed, what Jacob and Rich make of it. They note that, over the 30-year period covered by their data, the robbery rate and the number of robbery arrests increased. This observation cannot be given any theoretical interpretation unless one first specifies an hypothesis as to how arrests are likely to affect criminal behavior and some model purporting to explain changes in both robbery rates and the number (or rate) of arrests.

Virtually every recent study of which we are aware, including our own, assumes that sanctions discourage criminal behavior only to the extent that they represent an increase in either the risk or severity of punishment. It is not an increase in arrests, but an increase in the probability or risk of an arrest,

that deters crime. This risk is customarily measured by the ratio of arrests made to crimes committed. Jacob and Rich do not employ this measure, and the ones they do use are not proxies for risk.

In addition, all sophisticated studies of deterrence assume that crime and sanctions are to some degree jointly determined and that each must be explained in a way that recognizes the possibility of a simultaneous influence. We offered a cross-sectional four-equation model to do this for police activities; Wolpin (1978) offered a longitudinal three-equation model to account for changes over time in the crime rate in England and Wales. The results of these two approaches are broadly consistent—namely, that increases in the risk of sanctions, other things being equal, lead to reduced levels of certain kinds of offenses. Nothing in the correlations between arrests and robberies shown by Jacob and Rich casts doubt on these findings. They do not employ the correct measure of deterrence; nor do they make an effort to discover what factors cause the increase in robberies and the increase in arrests and whether, at the margin, changes in the probability of arrest affect the robbery rate.

## II.  CONCLUSIONS

In sum, the Jacob and Rich analysis of longitudinal changes in arrests and robberies in nine American cities does not advance our knowledge of the deterrent effect of police patrol because it provides no model of the deterrence hypothesis that attempts to explain the supply of offenses, the risk of sanctions, or the interaction between the two. Moreover, even if they had provided a model, their measures of the key variables are inappropriate: they use aggregate values for police arrests and the issuance of traffic citations, instead of stating these variables as arrests or citations per officer actually on patrol. They apparently misunderstand the significance of the traffic citation variable—it is a proxy, perhaps better represented, as we once gave it, as a dummy variable distinguishing aggressive, "legalistic" departments from passive "watchman" departments. Since this variable is intended to detect large differences among police strategies, it is not likely to have any significance when employed in a longitudinal study that, of necessity, examines short-term, relatively minor fluctuations in behavior rather than fundamental structural changes. Finally, Jacob and Rich offer no evidence (and within their theoretical framework, could not

offer evidence) to support their speculations that higher police expenditures lead citizens to report or police to record more crimes.

This rejoinder is not meant to re-establish the conclusion that we have "proved" that more aggressive police patrol will lead to an increase in the apprehension risk for criminals which in turn will reduce the robbery rate. As we stated in our 1978 article, the most we can say is that our cross-sectional study is consistent with that inference. We cannot say whether the hypothesized relationship between changed patrol methods and lowered robbery rates can actually be achieved by plan. To answer that question —which, for policy purposes, is the key question—requires mounting carefully controlled experiments in particular cities. The San Diego field interrogation experiment is virtually the only one of which we are aware that observed the effect of changing routine patrol *behavior*. It produced results that are consistent with our statistical inferences. But there has not been, to our knowledge, an experimental evaluation of the effect of an increase in patrol aggressiveness on robbery rates. Indeed, given the fact that aggressiveness tends to be the result as much of a particular departmental style and local political culture as it is of police expenditures, we doubt that it would be easy to conduct an experiment in which there were sufficiently large and lasting changes in patrol behavior to test adequately the relationships we find in our data. *A fortiori*, we are confident that bivariate longitudinal correlations of aggregate data within a given department, without any effort to measure patrol strategy or control for demographic changes, will tell us nothing at all.

## REFERENCES

BLUMSTEIN, Alfred, Jacqueline COHEN, and Daniel NAGIN (eds). (1978) *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*. Washington, D.C. National Academy of Sciences.

JACOB, Herbert, and Michael J. RICH (1980) "The Effects of the Police on Crime: A Second Look," 15 *Law and Society Review* 109.

WILSON, James Q. (1968) *Varieties of Police Behavior*. Cambridge, Mass.: Harvard University Press.

WILSON, James Q. and Barbara BOLAND (1976) "Crime," in William Gorham and Nathan Glazer (eds.), *The Urban Predicament*. Washington, D.C.: The Urban Institute.

———(1978) "The Effect of the Police on Crime," 12 *Law and Society Review* 367.

WOLPIN, Kenneth I. (1978) "An Economic Analysis of Crime and Punishment in England and Wales, 1894-1967," 86 *Journal of Political Economy* 815.