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A RESPONSE TO ROESCH AND CORRADO

ROBERT FISHMAN*

Roesch and Corrado have raised a number of issues in a detailed critique¹ of the article,² which is a summary of part of a study³ about the effects on criminal recidivism of rehabilitation and diversion services. This is a response to those criticisms deemed most important.

REPRESENTATIVENESS OF THE SAMPLE

Roesch and Corrado state that the sample of 2,860 male clients "may be . . . completely unrepresentative of the eighteen projects, the fifty-three projects, and especially the criminal justice system as a whole."⁴ They argue that the possibility of unrepresentativeness follows from two factors. First, the sample was "nonrandom" because "[n]either random sampling nor representative sampling techniques were employed."⁵ Second, the 2,860 clients in the sample represented less than 10 percent of possible clients.

"Random sampling" of the clients (by their Standard Intake Forms), in the sense of attempting to obtain only *part* of the universe to be measured, was not used because "the *universe of all forms* from the 18 projects was used for all clients who met . . . criteria as to age, sex, and twelve month duration . . . and for . . . data needed for police record retrieval . . ."⁶ Attempting to obtain the total uni-

verse of subjects who meet criteria is, in principle, a stronger method of maximizing the likelihood of the representativeness of the group obtained than a method of "random sampling" from that universe. Methods of random sampling are intended ordinarily to include only part of a universe in a sample because of an inability to include or measure the whole universe. Since the exclusionary criteria that further reduce sample size would be the same in both methods (*e.g.*, no females or incomplete addresses), the sample which results from "random sampling" would contain fewer members and consequently less likelihood of representativeness than the sample resulting from the selection procedure used by the Evaluation.

Roesch and Corrado's use of the term "nonrandom sample" may be misleading and is not pertinent to the question of the representativeness of the sample.⁷ The fact that the sample included less than 10 percent of the total project population is not significant because it is not possible to assess solely from the proportion of the population included, be it 1 percent or 10 percent, whether a sample is representative. Such factors as the size and the composition of the sample must be taken into account. Whatever its percentage, the sample consists of 2,860 people from an atypical population. That is not a small number when compared with the less than 100 subjects contained in many important studies in criminology.

Further, important comparisons can be made

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¹ See Roesch & Corrado, *The Policy Implications of Evaluation Research: Some Issues Raised by the Fishman Study of Rehabilitation and Diversion Services*, 70 J. CRIM. L. & C. 530 (1979).

² See Fishman, *An Evaluation of Criminal Recidivism in Projects Providing Rehabilitation and Diversion Services in New York City*, 68 J. CRIM. L. & C. 283 (1977).

³ See R. FISHMAN, *CRIMINAL RECIDIVISM IN NEW YORK CITY: AN EVALUATION OF THE IMPACT OF REHABILITATION AND DIVERSION SERVICES* (1977). The summary article, Fishman, note 2 *supra*, omitted certain material contained in the book. The material must be referred to in order to respond adequately, and in some cases to respond at all, to criticisms and issues raised by Roesch and Corrado.

⁴ Roesch & Corrado, *supra* note 1, at 531.

⁵ *Id.* at 531 n.9.

⁶ Fishman, *supra* note 2, at 289 (emphasis in original). The Evaluation selected forms from one project. YCB, by a method of random selection. *Id.* at 289 n.28.

⁷ The Evaluation can be viewed as having used essentially two different types of criteria to exclude clients. The first type excluded clients who were defined as not being qualitatively part of the target population or universe, *e.g.*, females. The second type excluded clients who were qualitatively part of the target population but for whom outcome could not be determined because of incomplete or incorrect information, *e.g.*, those with an incorrect name on a form that prevented retrieval of police records. Since the second type of criterion excluded members of the target universe, the clients obtained are a sample of the universe despite the attempt to measure the total universe. Nevertheless, this sample would be larger and more likely to be representative of the universe than a sample obtained by "random sampling" given that the exclusionary criteria are the same for both methods and that there is no demonstrable sampling bias in the method used by the Evaluation.

between the sample and the population without formal statistical analysis. The arrest population in New York City, particularly for violent crimes, is primarily young, male, black, and Hispanic, with prior arrest histories, generally undereducated, and poor.⁸ The 2,860 in the sample were all male, undereducated and poor. Ninety-three percent were black and Hispanic, 87 percent were twenty-nine or younger, and 90 percent had prior arrest histories.⁹ It is possible that a *statistical* comparison of education, for example, would show them to be significantly more or less undereducated than the population of the projects or the criminal justice system, but any relationship to recidivism would be unclear.

If prior criminal history is considered, then the sixteen (or thirteen)¹⁰ subgroups of the sample representing levels of severity by age ranged from groups of first offenders to groups with extensive and severe charges, which is also similar to the target groups of the projects. Such factors also support the representativeness of the sample even without formal statistical comparison. It should be stressed that the Evaluation is primarily generalizing from each of the sixteen types of clients in the sample to only similar types in the projects and the criminal justice system.¹¹

Roesch and Corrado also contend that possible unrepresentativeness may have resulted from some unknown systematic bias, such as selective record-keeping by projects that would have produced more extensive or detailed records on more "difficult" clients. The only record items needed by the Evaluation for retrieving police arrest records were such identification items as name, address, and age. There seems to be little if any reason and no evidence to suggest that these would be more accurate for "difficult" than for "easy" clients.¹²

Roesch and Corrado's reservations about the representativeness of the sample appear to reduce to how much confidence there can be, in the absence of statistical demonstration, that the sample is *not unrepresentative*. To be sure, this cannot be ruled out in this case, or even where there is a statistical demonstration. But given the characteristics of the population and of the clients in the

sample, it takes quite a leap of faith, on their part, to assume that it probably is unrepresentative.

OUTCOME MEASURE

Roesch and Corrado accept the use of arrest as a measure of outcome, but criticize the failure to use outcome measures of *noncriminal* behavior as well, stating that they were just as important and also may have been affected by project services. They illustrate such a measure: "Other projects attempted to provide job counseling and referral; increases in employment would be a measure of success for these programs."¹³ But one cannot use outcome measures of noncriminal behavior to assess the effectiveness of these projects because to do so assumes or implies that these measures are dependent variables. In the Evaluation, such measures cannot be validly classified as dependent variables.

As documented in the original article, the basic goal of the enabling legislation and the LEAA was the reduction of the high incidence of crime.

Therefore . . . the basic goal of CJCC's LEAA-funded "people projects" would be to reduce the incidence of crime Accordingly, a particular program's services would be measured *criminologically*. These services were treated as the methods (the independent variables) by which the goal of reducing crime was to be reached. The common [shared] measure of achievement of that goal (the dependent variable) was criminal *behavior*¹⁴

as measured by the amount and type of arrest recidivism.

In other words, for LEAA funded projects providing some combination of remedial education and job training, such results as an increased ability to read or placement in a job become methods by which the projects proposed to reduce the criminal behavior of their clients. But these results could not be justified logically as ends in themselves or dependent variables unless, for example, the projects were funded by the Department of Labor or the Department of Health, Education, and Welfare through their enabling legislation.

Furthermore, confusing independent variables and dependent variables is the same as confusing treatment and outcome or cause and effect. With the "success" measure of increased employment recommended by Roesch and Corrado for the Evaluation, a "deductive chain" that would link the hypothetical construct to the dependent vari-

⁸ Fishman, *supra* note 2, at 283.

⁹ *Id.* at 289, Table 3 at 296.

¹⁰ *Id.* at 293, Table 3 at 296.

¹¹ *Id.* at 289 n.30.

¹² The methods used by the Evaluation in collecting, analyzing, and reporting its findings are described in an appendix to the book. FISHMAN, *supra* note 3, at 95-179.

¹³ Roesch & Corrado, *supra* note 1, at 532.

¹⁴ Fishman, *supra* note 2, at 285.

able would have to be long, intricate, and of doubtful validity.

THE EVALUATION DESIGN, CONTROL GROUPS, AND RECOMMENDATIONS

Roesch and Corrado argue that the design of the Evaluation is fundamentally flawed because of limitations of the control group used. Consequently, unsupported conclusions are reached by the Evaluation and the results are of limited utility and "generalizability." They state that: "A valid control group for all eight projects cannot be selected by only one project."¹⁵ Roesch and Corrado are wrong. One project may obtain a valid control group for eight other projects. The validity of a control group is ascertained by determining whether significant characteristics of its members are the same as those of the members of the experimental groups. By chance alone, the control group may be valid despite a questionable method of client assignment, or invalid despite a textbook application of random assignment; this holds for eight or eighty treatment or experimental groups.

Roesch and Corrado suggest that the most important shortcoming of the design was that there was no control group composed of clients who, instead of diversion, were continued through the usual criminal justice system and were exposed to court processing, probation, or incarceration. The theoretical desirability of such a control group is evident. On the other hand, success in obtaining a valid control group of this type by use of methods such as "random" assignment is very difficult, and all too often impossible, for many field evaluations. Despite this, I do not share Roesch and Corrado's conclusion that without such a control group,¹⁶ useful or valid conclusions cannot be drawn from either this Evaluation's design and results or, implicitly, from those of any other evaluation.

Suppose that in this Evaluation it had been found that all clients recidivated, or that none did, and there was no control group of any kind. It is clear that the relevant policy makers could and would have been able to translate such a finding into an appropriate policy. Their decision would have been based on an implicit comparison with existing criminal justice system recidivism rates, which would, in effect, constitute the equivalent of a control group.

This is not meant to deny that in the great

majority of outcomes formal control or comparison groups are necessary if outcomes are to be unambiguous. The point is that the decision has to be made not by the rote application of an inflexible rule, but by a more sophisticated assessment of what can be accomplished, under the circumstances, with the data that can be obtained.

The central fallacy of Roesch and Corrado's argument is their forced assumption that the most necessary comparison is between the projects and the present criminal justice system, which Roesch and Corrado consider as the "nontreated" control group. The Evaluation suggested in the article and explicated in the book that the appropriate comparison group would have to stem from basic revisions of the criminal justice system. The key revisions recommended were the implementation of adequate, immediate, consistent, and certain incarceration by mandatory minimum sentencing of those convicted of violent crimes against strangers and the institution of preventive detention for those charged with such a crime.¹⁷ Theoretically, the most pertinent and useful comparisons would be of arrest recidivism and violent crime measured between the projects and the recommended criminal justice system approaches and, most important, between the approaches presently used by the system and the recommended approaches.

In principle, it should not be necessary to have to go through the procedures of establishing the "control group" just to make this point, since in terms of the key factor of comparison—recidivism—the group is obviously self-defining. It would, by virtue of detention or incarceration, be incapable of recidivism.

This point also answers another of Roesch and Corrado's main criticisms. Roesch and Corrado note that the high costs imposed on society by the recidivists was one of the primary reasons that the Evaluation recommended discontinuing the programs.¹⁸ Roesch and Corrado dispute this point by arguing that the program did not cause any increased incidence of crime. They point out that if the Evaluation's assertion that relatively few offenders are incarcerated in New York City is true then those offenders ... eligible for diversion would probably be even less likely to be sent to prison ... since the selection criteria usually excludes serious offenders. The fact that these individuals were diverted did not cause the subsequent crimes as it is

¹⁵ Roesch & Corrado, *supra* note 1, at 534.

¹⁶ *Id.* at 536.

¹⁷ FISHMAN, *supra* note 3, at 72-77, 83-85.

¹⁸ Roesch & Corrado, *supra* note 1, at 536.

likely that they would not otherwise have been imprisoned. Thus abolishing these programs may not decrease the number of crimes . . .¹⁹

But let it be assumed that none would have been imprisoned by the present criminal justice system, that the recidivism would have been significantly higher with the present system's diversion policy and no diversion projects, and that the recidivism rates of the projects would be significantly lower than the present system. Even then, the *actual* 41 percent rate of recidivism, which represented 2,072 arrests for 1,182 of the 2,860 clients, and especially the fact that of those arrests, 605 or 29 percent were for the violent crimes of murder, rape, robbery, and assault,²⁰ would have and did lead the Evaluation to conclude that the services provided by the projects failed as effective means of controlling and preventing crime, particularly violent

crime. As has been stated before, the appropriate comparison group would be one derived from the proposed revisions to the criminal justice system. Since the proposed revisions would result in a much higher rate of incarceration, the actual 41 percent rate of recidivism does provide a reason to conclude that the projects failed.

Finally, it should be pointed out that "abolishing" or abandoning these programs was not the actual recommendation of the Evaluation. The Evaluation actually recommended continuing and perhaps expanding the programs in the sense of educational and employment services but from *noncrime control* sources, *e.g.*, the Department of Labor,²¹ and that diversion services be continued for clients with nonserious present or past criminal charges, but not for those with present or past charges of violent crimes against strangers.²²

¹⁹ *Id.* at 535-36 (footnote omitted).

²⁰ Fishman, *supra* note 2, at 299.

²¹ *Id.* at 305.

²² FISHMAN, *supra* note 3, at 85, Errata.