LEAA / OFFICE OF JUVENILE JUSTICE + DELINQUENCY

A MODEL FOR THE EVALUATION OF PROGRAMS
IN JUVENILE JUSTICE

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The Juvenile Justice System is now undergoing changes that are every bit as revolutionary in character as those which led to the construction of the first houses of refuge for children following the American War of Independence or the creation of the Juvenile Court a century later. These changes can be encapsulated into a now familiar list of catchwords: decriminalization, diversion, due process and deinstitutionalization.

Efforts to implement these catchwords are not without opposition and certainly not without ambiguity. But the changes they symbolize, and the ideology upon which they are constructed, are widely shared. The four D's indicate a pervasive disillusionment with the notion that the juvenile justice system can be society's super parent and portend new ways for organizing it.

This fact should give us pause. As David Rothman (1971: xiv-xv) has so cogently pointed out, there is a prevailing tendency to regard major societal innovations as "reforms," as improvements over that which existed before. The prison, for example, was regarded as a humane improvement over prior methods of punishment and the juvenile court as an improvement over older methods of dealing with the problems of juveniles. Yet, it would be difficult to maintain, in light of subsequent events, that either innovation was a pure and unmistakeable step in the progress of humanity. To do so, Rothman suggests, would not only be bad logic but bad history.

If this is the case, how should today's innovations -- the four D's -- be regarded? Are they progressive steps in the treatment of the young? Do they merit the support of wise and well-meaning citizens. Let me paraphrase Rothman's answer (1971:xv): If we are to describe any or all of the four D's as "reforms," we will be taking for granted precisely what ought to be the focus of investigation. Our innovations ought to be carefully evaluated rather than accepted outright as improvements over existing practices. Otherwise, we will fall into the same trap as all the reformers who have preceded us; namely, the tendency to equate change with effectiveness and to assume that good intentions are the same things as helping offenders or protecting society.

Why, it might be asked, do we find ourselves in danger of repeating old errors? There are a host of reasons but foremost among them is our failure to gather knowledge on the effects of our innovations -- to submit our programs to rigorous study and evaluation. Such a failure would be unthinkable in the field of medical care or even in manufacturing industries yet it has traditionally gone unquestioned in the social realm. Furthermore, much of our failure is also due to the inability of scientific and program people to collaborate successfully in the search for knowledge even though they have much to contribute to each other.

Kurt Lewin (19[illegible]:[illegible]41) has noted that new programs " . . . usually emerge from a more or less vague idea. An objective appears

in the cloudy form of a dream or wish, which can hardly be called a goal. To become real, to be able to steer action, something has to be developed which might be called a 'plan'." Ironically, the program people who must give form and substance to any "plan" are not usually trained in the kinds of theory-building that are useful in conceptualizing some new approach to juvenile problems. Social scientists who do have such training, meanwhile, are seldom involved at the construction stage of any new program. Their ideas as to what will work may be little better than anyone else's but they can be of use in helping professional colleagues to state their assumptions and plans in ways that will make them more amenable to implementation and test. In lieu of this, however, research people are usually called in after a "plan," sometimes a haphazard one, has been created and then asked to evaluate it. The result, ordinarily, has been anger and misunderstanding, if not bloodletting.

The reasons are obvious: collaboration requires common understandings and an effective division of labor. These, in turn, are best achieved <u>before</u> a new program is organized, not afterward. Unless action and research people set up a working model by which their joint efforts can be effectively integrated, they will stumble over each other constantly. Listed below, therefore, are the elements of a potential working model. These elements may not be the most desireable in every instance but they do help to indicate the kinds of things which potential collaborators must be concerned early in their relationship.[1]

1. PROJECT GOALS

The first element of any model would be a mutual set of goals -- goals that pay heed to the importance of generating knowledge as well as meaing-program needs. Unless agreement can be reached on both kinds of goals, in fact, it may be useless to involve both research and action in a common endeavor. The reason is that, if some important differences in perspective are not resolved, they will ultimately surface to the detriment of any joint effort.

First, consider the traditional perspective in which the social scientist has been indoctrinated. To begin with, he is a trained skeptic. However desireable some new program may appear in theory, he is inclined to be leery about it until its effects are shown to be demonstrably superior. He also knows that the ideal design for testing program effects is an experimental design -- one in which experimental and control groups are randomly selected from a common population. Members of the experimental group are subjected to the new program while the members of one or more control groups receive no "treatment" whatsoever, or are placed for "treatment" in traditional settings.

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[1] For greater detail on the application of these elements in two experiments, see LaMar T. Empey and Steven G. Lubeck, The Silverlake Experiment. Chicago: Aldine Publishing Co., 1971; and LaMar T. Empey and Maynard L. Erickson, The Provo Experiment. Lexington (Mass.): D.C. Heath and Co., 1972.

This design, in short, is about the only way that a multitude of nonprogrammatic effects can be controlled. Without its use, differences in outcome may be due to factors lying entirely outside the influence of the programs being compared.

Because of his commitment to the experimental design, however, the research purist has some blind spots. Innovations in juvenile or criminal justice programs are not like experiments conducted under highly controlled conditions in the laboratory. Instead, they are field experiments in which the beliefs and prejudices of the public, clients and staff, like those of the scientist, must be taken into account. Innovations are subject to a host of ideological, political and bureaucratic influences whose effects on program operation and data collection are profound. Yet, even though these influences are omnipresent, the purist is inclined to dismiss them as relatively unimportant. The only thing that really counts is faithful adherence to original program and experimental designs. All else is superfluous by comparison.

Contrast this view with that of the policy maker or the practitioner engaged in reform. In the first place, neither is a trained skeptic. As a result, the desireability of change is more readily accepted. For example, long experience teaches that the confinement of juvenile status offenders -- "incorrigibles," truants, or runaways -- in training schools has not helped noticeably to solve their problems. Therefore, it seems patently obvious to the reformer that almost any community alternative would be better. Given this "obvious" conclusion, why evaluate new programs? Even more to the point, why set up experimental designs which might deny to the members of some control group a new and highly desireable form of treatment? It is unethical to deny help to one group of children while providing it for another. Since we know what is best for status offenders, why not just go ahead and provide it for everyone?

The answer is that this viewpoint, like that of the experimental purist, also possesses some blindspots. Contemporary reformers soon forget that exactly this same kind of thinking characterized the nineteenth century "reforms" which we now seek to undo. The child savers of that period were convinced that houses of refuge, asylums and reform schools could become society's new superparents. If some families and communities were no longer fit places for children, then places of confinement could become effective, even superior, surrogates. Much like now, people were convinced that they knew what would work. There was no need to evaluate the efficacy of new correctional reforms. They were clearly superior.

The human tendency to engage in this kind of thinking is as illogical as it is understandable. It is illogical because it assumes that the recognition of a problem is tantamount to suggesting a solution. It is illogical because it assumes that, since old methods -- i.e., reform schools -- do not work, almost any other alternative will work. It is illogical because it equates fervency of belief with evidence.

On the other hand, the belief that new programs do not need evaluation is understandable. Those who are motivated or hired to help people, particularly children, can ill afford to be skeptical about what they are doing. Persons who do not believe in the efficacy of their work are poor workers indeed; persons disarmed by doubt are not very effective. That is why program evaluation is so threatening -- threatening both to cherished beliefs and to the jobs and reputations that hang upon them. It is no wonder, therefore, that the skepticism and methods of the scientific game seem incongrous to people engaged in reform efforts. To them, the effort is enough.

But is the effort enough? Is there no way by which the desire to reform juvenile programs might be combined with a desire to evaluate their efficacy? If the answer is "no," there is no point to further discussion. If the answer is "yes," then along with change, the pursuit of knowledge must become an acceptable social goal -- a goal that ranks much higher in priority than it has in the past. It must be acknowledged by high level administrators, by a workable organizational structure in which research people work side by side with program people, and by an appropriate budget which makes research something more than an afterthought. Should these changes not occur, then current talk about evaluation is empty.

Beyond the need to enhance knowledge building as an acceptable goal, there is a pressing need to engage the assistance of research people in defining and clarifying program goals, and in setting up what appears to be a sensible method for realizing them. What good does it do to evaluate a program whose goals are unclear and whose methods are questionable?

One simple step would be for research and program people to consider jointly the kinds of assumptions being made about juvenile problems, the range of alternatives that might be constructed to respond to them, and how those alternatives might be affected by the organizational, bureaucratic and political context in which new programs must operate. It is common, for example, to hear program people say that their objectives are to "prevent delinquency" or to "reduce recidivism." Yet, when stated in these terms, program goals say everything and nothing. The reason is that they are not expressed in substantive terms. "Delinquency prevention" in a police-operated program, for instance, may have a far different meaning from "delinquency prevention" in a program run by street gang workers or by a neighborhood school. Unless program goals are given substance in theoretical and operational terms, therefore, it is not possible to assess their implications either for research or action.

Ordinarily, substance is best gained when broad goals like "delinquency prevention" are translated into more modest or intermediate goals. The intermediate goals of the police, for example, may be to greatly increase the personal contacts they have with juveniles through an increased use of neighborhood foot patrols. The assumption might be that this increased

contact would deter delinquent acts, improve police-juvenile relations and, thus, prevent delinquency. Or school people may seek to prevent delinquency by increasing the academic skills of young people and their attachment to the school. These would be the intermediate goals for which a great deal of specificity would be required: What academic skills are to be improved? What is meant by "attachment" to the school? How would the school have to be reorganized to realize these objectives? Why would their realization reduce delinquency?

Any tendency to ignore questions such as these is as much a detriment to program effectiveness as it is a detriment to good research. Furthermore, as soon as program and research people begin to consider such questions jointly, they soon discover that reasonable answers cannot be pursued until additional issues relative to their collaboration are examined. They soon discover that intermediate program goals -- increased educational skills, or more personal police relations with juveniles -- cannot really be defined with much precision until other elements of a collaborative working model are considered. These elements are an interlocking web in which one element cannot really be fully completed until others are considered. That is why a joint effort by program and research people might be desirable, and why a prolonged dialogue between them is necessary prior to program inception, not afterward.

II. DEFINITION OF TARGET POPULATION

The nature of the interlocking web is illustrated by the fact that one cannot finalize program goals until one is explicit about the target population for any new program. Just as program goals may change considerable depending upon the interests of the people who are to run the program -- the police, the gang workers or the educators -- so they will change depending upon the target population involved. For example, efforts to prevent delinquency among 12-year-old potential recruits to a street gang may be sharply different from those required for the 17 or 18-year-olds who now constitute the core of that gang. Because of their differences in age and sophistication, or their ties to conventional institutions, far different methods may be required. Likewise, the methods used in a school-based diversion program might vary considerably depending upon the grade-level of the participants, the location of their school or their delinquent histories.

In some cases, a single program may seek to work with a highly heterogeneous population, but that fact in no way changes the need to be concerned with the possibility that goals may change from one subpopulation to another. The definition of objectives for one subpopulation, and the methods to be used in realizing those objectives, are likely to vary.

III. THEORETICAL STRUCTURE OF THE PROJECT

One way of helping to define one or more target populations,

and to settle upon a program(s) for them, is to consider theoretical issues. Since the list of alternatives for responding to delinquency is not well-defined and supported by large bodies of confirmed evidence, the task of selecting among them is not a simple one. One way out of this maze, therefore, is to devote considerable effort to defining program assumptions so that one can be more explicit about one's program goals and the population(s) with which the new program will be working.

Any such effort would represent a radical departure from tradition. Attempts by program people to construct and use theory, in any formal sense, have been neglibible, first, because of a general distrust of theory and, second, because the task of theory construction is profoundly difficult. Yet, these difficulties are not adequate to discredit the overall utility of theory. Indeed, it is an error to assume that program people do not use it.

Any time a program is set up, or any time one technique is chosen over another, someone has an idea in the back of his or her mind that it will make a difference -- that it is somehow preferable to other programs and techniques. That person, in other words, does have a theory, however ill-stated, as to what leads to delinquency and how best it can be dealt with. What is needed, therefore, is to make that theory explicit rather than to leave it vague and amorphous. If this were done, both the action and research components of the innovation would be improved.

The first step in making assumptions explicit is to develop a theoretical statement of the problem to be addressed, how it comes about and what its roots are. What are the assumptions that the program will make about delinquency or conformity? How do these come about? Is it being assumed that delinquency is the result of poverty and ignorance, is a relatively normal expression of common youthful behavior, is due to the lack of effective social control by the agencies of law enforcement, is due largely to destructive labeling, or is due to some combination of these. Obviously, the adoption of different assumptions about the problem will make a great deal of difference in the kind of program that is ultimately developed. Thus, if assumptions are clearly stated, the task of deriving an intervention strategy is both clarified and sharpened.

Again, research people might make a distinct contribution if they were called upon because they could use their experience to assist in the theory-building task. They could help to clarify basic theoretical concepts and to suggest ways for organizing them into implementable and researchable statements about the causes for delinquency. In this way, program people would not only have a clearer idea about the difficulties on which an attack is to be made but the stage would be set for an examination of program assumptions to see if, in fact, they are accurate.

What is often overlooked is the fact that it is just important to find out if one's program assumptions are accurate as it is to find out if the program works. Indeed, if the assumptions are

incorrect, an intervention program based upon the, even if well-run, may make no difference at all or may be downright harmful. The program would be in the business of administering a cure for which there was no disease. In short, in this element of a collaborative working model, action and research people would be working to be as clear as possible about the assumptions they are making regarding the problems they hope to address and to evaluate.

IV. THE INTERVENTION STRATEGY

Once problem assumptions have been clarified and formally stated, the task of developing an intervention strategy is made easier. Such a strategy could be comprised of (1) a set of <u>intervention principles</u> and (2) a set of <u>operational guidelines</u>. The <u>principles</u> would be comprised of a set of theoretical statements, derived from the explanatory theory stated above, and would indicate how the problems it described might be addressed. For example, suppose an effort was being made to establish and evaluate a community program for serious delinquents -- boys found guilty of committing criminal offenses. Suppose further that a combination of control theory (Hirschi, 1969) and subcultural theory (Cohen, 1955) was used as the basis upon which the program was to be constructed. The resultant theoretical assumptions about the roots of the problem might look something like this:

1. Poor attachment to home and school leads to a decreased stake in conformity.

2. A decreased stake in conformity leads to identification with nonconventional peers.

3. Identification with nonconventional peers leads to criminal behavior.

Based upon these theoretical assumptions about the nature of the problem, a set of intervention principles might be as follows:

1. Improve attachment to home and school.-- The linkage of serious delinquents with these conventional institutions must be reestablished.

2. Increase stake in conformity.-- Find ways for making conventional institutions and activities more attractive to delinquent boys.

3. Decrease identification with nonconventional associates.-- Find ways for reducing the attractiveness of nonconventional groups and enhancing the attractiveness of conventional ones.

Such principles, once stated, would be of use both to the action and the research components of the study. Notice, first, that when invervention principles are clearly stated they become virtually synonymous with the intermediate program goals mentioned earlier, for which clarifying statements are needed -- i.e., to improve attachment, to increase stake in conformity, to decrease nonconventional identification. By working through one's basic assumptions it is possible to be explicit about the kinds of goals that must be achieved if delinquency is to be prevented or reduced. But until the principles are worked out, it is difficult to state goals precisely. Furthermore, intervention principles provide the invaluable function of clarifying the nature of the research task; that is, while action is concerned with implementing the basic principles, research will be devoted

to assessing the success and consequences of that implementation. Rather than divergent, their two roles would be complementary. The theoretical principles that give meaning to action would also give meaning to research.

The operational guidelines of the intervention strategy would be even more explicit by helping to translate priniples into action. Any new intervention strategy requires answers to a host of organizational as well as client-related questions: What is the nature of the organization that is needed to operationalize intervention principles? How should people go about the task of running the program? What shall be the nature of its social structure and its activities? What are the consequences of this kind of organization within the network of community organizations that already exist? In terms of staff, what new roles are required? What kinds of training are necessary? What kinds of difficulty, either within the program or in its relations to other organizations, might be anticipated? The operational guidelines would be designed to answer such questions.

What must not be overlooked, however, is that operational guidelines may differ depending upon the nature of the target population. That is why stress was placed earlier upon being explicit upon one's target population. For example, if the theory stated above were applied to a group of 12 or 13-year-olds, one might want to place far greater attention upon their attachment to their homes and their schools than if one were working with a group of 18-year-olds. If the theory is correct, the latter would have moved further in their ties to delinquency-prone associates than would the younger group. Greater efforts would have to be concentrated at this stage of the delinquency-producing sequence. Thus, even if one had only one set of program principles, those principles might be operationalized in far different ways depending upon the target population(s) in question.

Again, this attention to the explicit definition of intervention principles and guidelines would hope to overcome the historical inclination for action and research people to go their divergent ways. Ordinarily, action people are left to their own devices in setting up a program while research people come in after all that is done. All too often, the results are disastrous. To action people, the research that is produced often seems unrelated to their central concerns. By working from a common framework, however, it might be hoped that these sources of conflict could be reduced.

V. THE RESEARCH STRATEGY

The next major component in the model would be a research strategy. The definition of this strategy would help further to pinpoint objectives and to make them explicit for everyone concerned. Given an ideal set of conditions, research might be used to realize four major objectives.

1. Test of Basic Assumptions -- Research could be used to test the theoretical assumptions that were made in defining

the problem that the new program was set up to address. How accurate were these assumptions for the target population in question? Were their problems anything akin to the way they were defined by the theoretical statement? Were their attachments to home and school broken? Did they, in fact, exhibit a decreased stake in conformity? Were friendships concentrated among nonconventionals?

If any research tests are needed, they are tests of program assumptions like these. While various professional and practitioner groups are inclined to question the methods they use, or how well they run their programs, they are strikingly disinclined to question, or even state clearly, the assumptions upon which they operate. Yet, no matter how well any program is run, it can be no better than the assumptions upon which it is predicated. One reason that we witness so many program failures, in fact, may be due to the incorrectness of program assumptions rather than to the inadequacies of program operation. Because assumptions are incorrect, programs are irrelevant or destructive. It is for this reason that research could make a vital contribution by examining the accuracy and utility of the problem assumptions that are made.

2. Examination of Program Process -- The second way that research can be useful is to provide information on the intervention strategy -- to test the accuracy and implementation of its principles and guidelines. Did the program actually operate according to design, to the way it was described on paper? What

changes might be required to make the operation more consistent with the ideals stated in the principles? How did the clients see the program?

Again, this is an area about which amazingly little is known. In most cases, research people describe, usually in demographic terms, the kinds of people that enter any program and what happens to them after they leave it, but the program itself remains an unstudied and mysterious black box. This is analogous to a steel manufacturer who puts in a number of raw materials at the front end of an assembly line, proceeds then to ignore what happens to those ingredients and how they were combined while on the line, and then wonder why he gets lead as a final product rather than steel. It is no wonder that we have so much difficulty in trying to replicate any program or experiment. We are neither explicit about how any program is constructed nor do we study and describe what happens to it while it is in process.

Not only is information desperately needed on what occurs within the confines of any program but what happens when that program, and its people, interact with other organizations and people in the community. The program guidelines outlined above suggested that the attachment of delinquent boys to home to school should be improved. To what degree was this actually accomplished? To what degree was the school itself changed so that linkage could be reestablished? To what degree were linkages established with conventional associates? What lessons were

learned when efforts were made to accomplish these program goals?

At the present time, the juvenile justice system is actively pursuing a host of new diversion and deinstitutionalization programs. The prevailing assumption is that such programs will enhance the linkage of delinquents to nonlegal, noncoercive organizations while, at the same time, it will decrease the negative effects of labeling. Given the rapid growth of such programs, one could argue just as well that, rather than decreasing the effects of coercive supervision and labeling, the locus of control over children has merely been transferred from legal to nonlegal bureaucracies. Furthermore, with increased resources, these bureaucracies will increase, not decrease, the number of juveniles actually under their supervision. Since these are distinct possibilities, they illustrate the point made earlier; namely, that "reforms" should not be taken for granted. Rather, they should be examined to determine whether the purposes for which they were created are actually being realized. If, in future years, we are to avoid lamenting the creation of diversion programs the way we now lament reform schools, care should be taken to examine that which is actually occurring, not just what we hope will occur.

3. <u>Assessment of Outcome</u> -- A third way that research can be useful is to provide information on program outcome. This is where the selection of experimental and control groups comes in. The most definitive method for determining the effects of any

program is to make a random selection of experimental and control groups from some known population of individuals. While the special program is used for experimental subjects, controls may remain in some traditional activities or may be left entirely alone. To meet a minimum standard of ethics, however, the experimental program should bear the promise of offering an alternative to the target population that is more promising than the one they could ordinarily experience -- an enriched program in a deprived school, heretofore unavailable job training, or placement in a community program for serious delinquents in lieu of total incarceration.

The measurement of outcome, to be most fruitful, should be concerned not merely with recidivism. Indeed, if the experimental strategy is to receive a definitive test, outcome should be measured in terms of intermediate goals. For example, if it is assumed that improvements in school will be useful in reducing recidivism, the "improvements" have to be defined and measured -- in terms of grades, performance on achievement tests, relations with teachers and peers and so on. Once this is done, it then becomes necessary to determine whether, if improvements did occur, they actually led to lowered recidivism rates. Since the two may or may not be connected, it is vitally important that the assumption of a connection be tested.

The fact that it is not always possible to select experimental and control groups randomly should in no way detract from the necessity to provide some kinds of outcome measures. Quasi-

experimental designs, before-and-after comparisons in which experimentals become their own controls, or a study of delinquency rates on an areal basis are other methods that might be worked out and tried. Outcome measures, along with a test of program assumptions and a study of program process, could provide a much-desired package of information.

4. <u>The Effectiveness of Collaboration</u> -- Finally, policy-makers, professionals and scientists need more information on the nature and problems of collaborative field study. If some endeavor is made to implement a model like the one being described here, how well does it work? Were the activities of action and research people effectively linked? How could they be improved? A study of the collaborative endeavor, itself, could be of inestimable value for others who may wish to improve upon it.

VI. ASSESSMENT OF IMPLICATIONS

The final element of the working model would be an assessment of research findings for basic theory, for the pragmatic concerns of the policymaker and practitioner and for the future of collaborative endeavors in which action and research people join together.

In terms of basic theory, this assessment might involve a reformulation of the basic theory and intervention principles upon which the program was based. Although it is conceivable that findings might confirm the conceptual structure of the study, that eventuality is unlikely. It would be extremely important, therefore, to indicate where basic assumptions may have been inaccurate and to suggest new lines for inquiry based upon the empirical findings of the study. Such information could be extremely important to people engaged in basic research as well as to those people who set policy and run intervention programs.

Secondly, study findings could be assessed in terms of their implications for public policy -- the relative utility of the experimental program versus other alternatives, the comparative costs of the two approaches, the implications of the experiment for the sponsoring organization or the unanticipated and perhaps negative consequences of the reform effort.

Finally, the assessment of implications would be concerned with the philosophy and the methodology of field experimentation -- the problems of relating action, theory and research together in a common endeavor, the ethical and philosophical issues that are encountered and the potential of the field experiment for future research and youth programs. If the people involved in this endeavor could adhere, even roughly, to the working model sketched above, they might contribute substantially to the understanding of crucial problems that have plagued social scientists, practitioners and policymakers for a long time.

SUMMARY

This paper has suggested that contemporary reformers in juvenile justice run the risk of repeating an age-old error;

namely, that of assuming that change can be equated with effectiveness and that modern programs will succeed where others have failed. In order to avoid such an error, it was suggested that more might be done to gather knowledge on our innovations. They ought to be carefully evaluated rather than accepted outright as improvements over existing practices.

If this is to be accomplished, improved models for collaboration between action and research people are required. Such models would make possible a more effective division of labor -- a division that should be worked out and settled upon <u>before</u> a new program is actually put into the field. Program evaluation should not be a distasteful afterthought.

The kinds of elements that might be included in any working model were listed and described. It was pointed out, however, that these elements constitute an interlocking web, each giving substance and meaning to the other. They were as follows:

I. <u>Agreement on Project Goals</u> -- Project goals should be of two types: (A) goals having to do with the program itself and what it hopes to accomplish; and (B) goals having to do with the generation of new knowledge. The legitimacy of both types of goals must be acknowledged at the outset and provision made by which both can be realized.

II. <u>Definition of a Target Population</u> -- In order to be as precise as possible about program goals it is necessary to define a target population(s) and to recognize that goals may vary depending upon the population(s) involved. A target population(s) must be specified because, in defining goals, it is necessary to be more precise than to say that they are to prevent or reduce delinquency. The most sensible goals are intermediate goals which specify how delinquency prevention or reduction is to be realized for the specific population(s) in question.

III. Theoretical Statement of Problem -- In order to be precise about goals and to indicate how a program is to be organized and operated, it is also necessary to develop a careful statement of the problems(s) to be addressed -- to make clear the assumptions that are being made about the causes of delinquency or the roots of conformity.

IV. The Intervention Strategy -- Once problem assumptions have been clarified and formally stated, the task of developing an intervention strategy is made earlier. Such a strategy would be comprised of (A) a set of intervention principles that are derived logically from the problem assumptions and which indicate theoretically how the problem is to be addressed; and (B) a set of operational guidelines that can be used to translate the intervention principles into action -- that is, to indicate what the program elements might be and what it is that staff members are expected to accomplish.

V. The Research Strategy -- The research strategy is a logical counterpart of the intervention strategy. It would provide for four things: (A) a test of the basic assumptions made about the nature of the delinquency problem; (B) an examination of program operation in terms of the adequacy and actual

implementation of the intervention strategy; (C) an assessment of program outcome and whether it was successful in realizing both its intermediate and general objectives; and (D) the gathering of information on the nature and problems of a collaborative field study.

VI. Assessment of Implications -- Once the program and research have been completed, the final component of the model would include an assessment of findings for basic theory, for the conduct of new or similar programs -- i.e., for public policy -- and for other scientists and practitioners who may wish to collaborate in future study and field experimentation.

The working model just described might be used for two purposes: (1) as a guide for those who are hoping to organize new action-research programs; and (2) as a kind of checklist for those who must decide whether a potentially new program shows promise. If steps have been taken to insure that both knowledge-building and programs needs have been met, then the potential program may well merit support. If, however, important gaps exist, then perhaps they should be filled before the new program is put into the field, especially if one of its avowed purposes is evaluation as well as intervention. Otherwise, the chances may be slim that much will be learned. Effective collaboration requires advance, not *post hoc*, planning.

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