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CRIMINOLOGY

EVALUATION RESEARCH IN CRIME AND DELINQUENCY: A REAPPRAISAL

CHARLES H. LOGAN*

What to do for criminals and delinquents is a perpetual problem, and the volume and vigor of opposing views for prevention and treatment indicates the absence of reliable knowledge. Several writers¹ have drawn attention to this lack of knowledge and have suggested reasons for it, but, with one exception,² there has been no systematic assessment of specific research studies on the effectiveness of various correctional or preventive practices. There have, however, been many claims to knowledge or confident policy recommendations in both the professional and popular literature. Whatever policies are pursued, we would be better off to recognize the extent of our ignorance.

Accordingly, the purpose of this study is to evaluate available research on the effectiveness of specific correctional or preventive practices in terms of certain specific methodological criteria.

THE REQUIREMENTS OF A TEST OF EFFECTIVENESS

The criteria proposed below are not meant to be taken as an exhaustive list of the methodological conditions that an ideal study of effectiveness should meet. The methodological details must vary with each specific research design. There are, however, certain minimal methodological requirements that the studies reviewed here must meet in order to merit any further consideration as to their scientific adequacy.

These criteria, particularly number three, are

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¹ Cressey, *The Nature and Effectiveness of Correctional Techniques*, 23 LAW & CONTEMP. PROB. 754 (1958); Glaser, *Correctional Research: An Elusive Paradise*, 2 J. RES. CRIME & DELINQUENCY 1 (1965); Schnur, *The State of Corrections and the State of Correctional Research*, 1965 PRISON J. 23.

² Bailey, *Correctional Outcome: An Evaluation of 100 Reports*, 57 J. CRIM. L.C. & P.S. 153 (1966). One problem with Bailey's paper is that the criteria used to evaluate the reports are too general. The present study is quite explicit on this matter, and therefore more replicable.

premised on the model of an experimental or quasi-experimental research design. This does not rule out designs using statistical, rather than experimental, techniques of control. The desirability of such designs will be argued at the end of this paper. The criteria for evaluating those designs would be somewhat different from the seven proposed below, but since none of the studies reviewed were premised on a statistical model, these alternative criteria need not be considered here.

The relevant criteria should include, as a minimum, the following seven:

1. There must be an adequate definition of the program or set of techniques whose effectiveness is being tested. This definition should be sufficiently operational that the components of the program can be clearly identified. It is not enough to know that a particular program ended in success or failure if we cannot determine *what it was* that succeeded or failed. Spurious operational definitions—such as defining casework as the actions of the caseworker—should obviously be avoided. But spurious operational definitions can sometimes be very subtle. For example, defining "intensive treatment" in terms of reduced caseload is operational, but it does not constitute a definition of the "treatment" given. We cannot meaningfully compare "intensive treatment" to "regular treatment" unless we know what "treatment" means, even if we can assume that it means the same in both cases.

This means also that the treatment should not be too broad—it should not be a whole range of different activities, such as an entire prison program. Although a broad program *can* be evaluated as to its effectiveness, this will not yield the knowledge of *what* is or is not effective. That knowledge is needed if we wish to create a new program patterned after the original one.

2. The technique must be capable of routinization. This does not mean that it has to be a purely mechanical activity, but it must be something

that can be repeated in all its components at different times, with different subjects, by different administrators of the technique. Thus the technique must not be dependent on any unique personal characteristics of either the original administrator or the original subjects. The technique must be capable of application or withholding at will, or at least able to be determined as present or absent by independent observers.

3. There must be some division, preferably random, of a given population of offenders into treatment and control groups, with the two groups differing as little as possible with respect to the characteristics of the subjects and their basis of selection. The basis of selection for treatment or non-treatment should ideally be a matter of chance, but if subjects are chosen for treatment on some special criteria—such as “amenability,” I.Q., or dangerousness—the control group should also be selected on the basis of these same criteria. This can be done through random division of a special sub-population of offenders or by matching members of the treatment group against only those control offenders who are identical or highly similar on important variables.

4. There must be some evidence that the treatment group is in fact receiving treatment as defined, but that the control group is not. If the program is not well-defined or routinizable, it cannot be assumed, nor could it be demonstrated, that this condition is being met. On the other hand, merely having a well-defined and routinizable program does not guarantee that this fourth condition will be satisfied. While great care is almost always given to be sure that the experimental group receives treatment, only rarely is enough attention given to the control group to be sure that it does not somehow also receive important elements of that treatment.

5. There should be some “before-and-after” measurement of the behavior that is sought to be changed, and a comparison made between the two measures. This measurement must be made for both the treatment and control groups (if there is a control group). Since there is no clear referent to the terms “before” and “after” with respect to the control group, they should be measured at the same times as the treatment group, or at comparable points in their case histories, if they are released from prison at different times, for example. Actually, with a truly random control group, the “before” measure is not so vital, since only that amount of change for the treatment group above

and beyond the “after” measure for the control group can be attributed to the experimental program that is being evaluated. But for any sort of matched or other non-random design there must be some “before” measures for both groups.

6. There must be a definition of “success” and “failure” that is sufficiently operational to provide a valid, reliable measurement for determining the outcome of treatment. The use of social adjustment scales or objective personality tests whose validity and reliability have not been demonstrated is ruled out by this criterion.

6a. This definition should be compatible with ordinary notions of what would be successful or unsuccessful outcomes of treatment. To be compatible with ordinary notions, “success” should refer to the correction or prevention of criminal behavior, not to personal adjustment, happiness, mental health, employment, or family relations. This sub-criterion has to do, not with the methodological competence of the study, but with the relevance and interpretation of its findings.

7. There should be some follow-up or delayed measurement in the community for both the treatment and control groups. This is especially important with respect to criminal behavior, which cannot be measured on-the-spot since it only manifests itself over some period of time. Moreover, behavior when still under supervision is, for various reasons, not a valid test of rehabilitative success.

SELECTION OF STUDIES FOR REVIEW

The inclusion of studies for review in this paper was more a matter of acquisition than of selection. All the books, articles, reports, or studies that were available in the library of Indiana University, or that could be borrowed through inter-library loan, were included if they made some deliberate or methodical attempt to evaluate a specific program or technique designed to achieve either the correction or the prevention of juvenile or adult criminal activity.³ “Technique of corrective or

³ Most, but not all, of the research included is post-1940. The most massive and sophisticated evaluational research in corrections is that conducted under the auspices of the California Department of Corrections. Glaser has characterized this research as consisting of the “most elaborate experiments and analysis procedures in the history of penal research.” D. GLASER, *THE EFFECTIVENESS OF A PRISON AND PAROLE SYSTEM* 191 (1964). This research is all extremely recent, largely incomplete, and sometimes difficult to acquire. Nonetheless, many of the reports on this research issued by the

preventive treatment" is defined as broadly as possible, to include any and all programs that seek to achieve some behavioral change in offenders or potential offenders, whether through psychotherapy, other psychiatric treatment, punishment, simple imprisonment, probation, parole, social work, counselling services, vocational rehabilitation, community treatment, work with gangs, or any other means. The effectiveness of some of these types of treatment, notably psychotherapy, have already been the subject of some general investigations, but are here considered only as they apply to correctional or preventive treatment of criminal and delinquent behavior. The specialized areas of treatments for drug addiction, alcoholism, and the criminally insane are not considered in this paper. Different studies of the same program are included only if there are some important differences in design between them.

EVALUATION OF THE STUDIES

All of the raw findings of this review of evaluative studies can be presented in one table (see Appendix). This table has five positive features. Reading across, it allows the reader to see at a glance which studies were strongest or met the most criteria. Reading up and down, it shows how many studies met each criterion. If it can be visualized as a whole, it provides a clear picture of the areas in which this type of research in general is weakest or strongest. When studied systematically, it shows associations between type of treatment, methodological soundness, and results claimed. Finally, it allows a replication to be made on the assessment of each particular study reviewed. This is especially important since about half of the criteria are not purely mechanical, but require a certain exercise of intellectual judgement as to whether or not a given study fulfills them.

The following seem to be the most notable observations that can be drawn from the table:

1) None of these studies of correctional or preventive effectiveness can be described as adequate. There is not one study that meets all of the criteria

Department of Corrections have been included in this paper.

In the course of my research, I discovered the article by Bailey, *supra* note 2, whose excellent bibliography served as a check and an aide to my own efforts. In spite of our similar aims, however, there is considerable difference between his bibliography and mine.

No pretense is made that this review of studies is exhaustive, but I know of no published study that would alter the major finding of this paper.

proposed in this paper as the minimal methodological requirements of a scientifically sound test of effectiveness.

2) Forty-two of the studies make some attempt at a control group, using that term in the most generous sense and including groups that should more properly be considered comparison groups, rather than controls.

2a) Restricting the term "control group" to only those studies that attempted to provide a proper control group, one selected by either random means or through matching or both, reduces this number to 31, of which 17 were just random, 6 were random and matched, and 8 were just matched.

The main criticism of correctional evaluation research in the past has centered on the lack of control groups. Chiefly because of the California research, this is becoming increasingly less of a problem. A far more serious fault is pointed out in the next observation.

3) Studies evaluating a well-defined technique or program numbered only 9. Adding borderline (\pm) cases brings this only up to 12. This criterion is certainly of equal importance to the provision of a control group, but it has been completely ignored in the literature on evaluational research in corrections.

4) When the two criteria of a proper control group and an adequately defined program or technique are combined, only 3 studies survive (numbers 19, 49 and 87). Including studies of techniques that were at least semi-well-defined, along with a proper control group, adds only one more study (number 33, the Provo Experiment).

5) If we add to the above two criteria the equally important requirement of a measurable definition of "success," the field is further reduced to one lone study (number 19, the California Forestry Camp study).

6) But adding the requirement of even a minimal follow-up in the community eliminates that final study as well, although this requirement may be met in the future.

Thus, taking only the four most crucial methodological criteria, we find that, as far as this survey and review has been able to determine, there is not yet one single study of correctional or preventive effectiveness that will satisfy the most minimal standards of scientific design. This indicates that Schnur is by no means indulging in hyperbole when he declares that:

No research has been done to date that enables us to say that one treatment program is better than another or that enables us to examine a man and specify the treatment he needs. There is no evidence that probation is better than institutions, that institutions are better than probation, or that being given a parole is better than escaping. . . . [S]o much of what is now being done about crime may be so wrong that the net effect of the actions is to increase rather than to decrease crime. Research could possibly shed some light, but none of the researches conducted to date answers these questions.⁴

Programs involving the use of psychiatric treatment, psychiatric social work, and psychotherapy received the most attention (37 studies). These were followed by "standard" programs, such as probation, parole, and institutionalization (20 studies). All the other types of treatment received about the same amounts of attention (Education, 7 studies; Counseling, 8 studies; Work or Vocational aid, 8 studies; Non-psychiatric social work, 9 studies; and Miscellaneous, 12 studies).

Claims of successful outcome are distributed as follows: "High" 16 studies; "Good to High," 4 studies; "Good," 24 studies; "Fair to Good," 11 studies; "Fair," 15 studies; "Failure," 16 studies; "Unclassifiable," 14 studies. Thus there is a strong current of optimism in these studies, with only a small minority (16%) admitting to failure. Perhaps most striking in view of the universal inadequacy of research design is the fact that so few studies insisted on suspending judgment altogether. 11 of the 14 "unclassifiable" studies made no claims, while 3 said that success varied. "Education" programs, as a group, made the highest claims of success while fulfilling the fewest methodological criteria required to support such claims.⁵

It would be interesting to correlate the strength of claims made with the rigor of design of the study. Bailey did this with his 100 studies and found that the most poorly designed studies made vague, "middle-ground" claims while the more rigorous studies made stronger claims of both a positive

and a negative nature.⁶ The present review did not uncover any such clear pattern. There is a slight negative relation ($r = -.24$) between adequacy of design (number of criteria met) and strength of claimed success for the evaluation program. The scatterplot for this relation reveals no clear nonlinear pattern.

SOME OBSTACLES TO EVALUATIONAL RESEARCH IN CORRECTIONS

The factor most commonly cited as interfering with the proper design and execution of evaluational research in corrections is the politics of correctional administrators and the vested interests that they and the personnel involved in the treatment process have in the evaluation of their programs.⁷ Often this situation is condemned as though the critic could not understand why administrators must behave the way they do. In fairness to correctional personnel, however, it should be pointed out that there are situational factors that would hamper research conducted even under the most disinterested and objective administration. The most serious of these obstacles is the different purposes of research and treatment.

The demands of applied treatment and the demands of scientific research are often in automatic conflict. For example, no treatment program could deliberately attempt to *increase* criminality, regardless of the theoretical value of studying such a program. Also, it may be that no program that is routinized and adhered to rigorously can be expected to succeed in all, or even most, cases. Hence, if a member of the treatment staff believes that this particular technique would do damage to this particular person, the demands of his job would require him to depart from the program, while the demands of research would require him not to.

The main difficulty is that whereas the researcher is supposed to suspend judgement and skeptically test all beliefs, the treatment staff is forced to act upon assumptions as to the truth of many beliefs or as to the effectiveness of their programs. Thus there are many assumptions that they cannot afford to test skeptically, particularly the assumption that some kind of treatment of criminals is better than no treatment at all.

A caseworker who wants to help is unwilling to go by the theory or stick to one technique. He is eclectic; he uses his imagination; he does things

⁴ Schnur, *supra* note 1.

⁵ The types of treatment may be ranked by methodological adequacy and by strength of success claims. In descending order of methodological adequacy (number of criteria met): Miscellaneous, Work or Vocational aid, Counseling, "Standard" programs, Psychiatric treatment, Social work, and Education. In descending order of strength of positive success claims: Education, Work or Vocational aid, Miscellaneous, Psychiatric treatment, Social work, "Standard" programs, and counseling.

⁶ Bailey, *supra* note 2, at 156.

⁷ Cf. Cressey, Glaser, and Schnur, *supra* note 1.

for reasons even he can't explain, acting on empathy, insight, and intuition. It may be that such a person is the most effective worker, but until we can specify and operationalize what it is he is doing, we cannot gain knowledge from observing and evaluating him.

Perhaps too much emphasis is placed on the practical, as opposed to theoretical, usefulness of correctional research. While it is understandable that administrators might be concerned with the effectiveness of what they are doing, it is very likely, as the findings of this paper suggest, that what they are doing is not testable. Perhaps correctional research should no longer be portrayed as action-research, designed to test the effectiveness of various currently existing programs, but as basic theoretical research, designed to develop scientifically reliable knowledge that will be of chiefly theoretical value and only indirectly or eventually of any practical use. Thus portrayed, it may be less attractive to correctional administrators, but more consistent with the present state of criminology. Moreover, it may encourage public authorities to provide the setting and conditions under which research can produce knowledge by meeting such criteria as the seven set forth above.

Meanwhile, what is needed is not simply "More Funds and Freedom" for correctional research, but more research and reflection on the problems of designing and conducting the kind of research that would be methodologically capable of producing valid and meaningful knowledge.

STATISTICAL VS. EXPERIMENTAL DESIGNS

It may be that more rapid progress can be made in the evaluation of preventive or correctional programs if research designs are based on a statistical rather than experimental model. It was noted, above, that one major difficulty in evaluative research is in procuring adequate control groups. Modern statistical techniques can provide a means of resolving this problem by substituting statistical for experimental methods of control.

These statistical techniques may be most useful in studying the effects of community-wide programs aimed at the prevention and control of crime, where the goal is to lower general rates of crime or delinquency, rather than to prevent or reverse criminal behavior in specific individuals.

Isidor Chein has provided an outline of how regression techniques might be used to evaluate the effects of community delinquency control

programs, using city census tracts as the units of analysis.⁸ Delinquency rates for these tracts can be regressed on a number of census tract characteristics known to be associated with delinquency. This regression produces a prediction of how much delinquency should occur in a given census tract for reasons other than the presence or absence of a given experimental program. The extent to which actual delinquency rates in tracts that have the experimental program fall consistently below the rates predicted for those tracts gives us an indication of the effect of the experimental program. This assessment should be made both before the program is introduced and after it has been in operation for some time. If the experimental tracts fall below their predicted rates even before the experimental program is introduced, they should fall still farther below the predicted rates after the program has begun to take effect if it is, in fact, an effective program.

This brief sketch does not do justice to the details of Chein's design. It is used only to outline the general logic of one way in which statistical techniques could be used to substitute for experimental control in assessment of a delinquency prevention program. If we take seriously the belief that crime and delinquency are largely social problems requiring changes at the community level for their solution, then there is a need for more evaluative research at this level using statistical, rather than experimental, designs.⁹

CONCLUSION

Past studies evaluating the effectiveness of programs for the prevention or correction of criminal or delinquent behavior, insofar as they have been consciously designed at all, have been very rough approximations of experimental designs. However, they have come nowhere near fulfilling the minimal requirements of an adequate experimental design. In view of this, and because of certain inherent limitations in applying experimental designs, future research may more fruitfully employ statistical rather than experimental methods of control.

⁸ I. CHEIN, *SOME EPIDEMIOLOGICAL VECTORS OF DELINQUENCY AND ITS CONTROL: OUTLINE OF A PROJECT* (mimeo, 1963).

⁹ Regression designs may also be useful in evaluating the effectiveness of treatment programs at the individual level, as shown by the work of Mannheim and Wilkins. For a concise discussion of their work in this connection, see R. HOOD & R. SPARKS, *KEY ISSUES IN CRIMINOLOGY* 183-86 (1970).

APPENDIX

TABLE I

Assessment of Studies of the Effectiveness of Correctional or Preventive Treatment
(See legend *infra*.)

Study (numbers refer to footnotes)	Methodological Criteria										Type of Treatment	Results Claimed
	I	II	III	IV	V	VI	VII	VIII	IX	X		
1	±	±	*				*	*	*	*	SW	H
2			*	*		*		*	*	*	P	?
3			±					*	*	*	P	X
4	*	*									W	H
5			*		*			*	*	*	S	?
6			*					*	*	*	P	G
7								*	*	*	S	H
8											E	F-G
9							*	*	*	*	M	F
10									±		P	G
11	*	*					*		?		W	G
12							*		?		S	X
13								*	*	*	S	F
14			*	*			*	*	*	*	P	?
15			*		*			*	*	*	S	X
16			*	*			*	±	?		C	?
17			*	*			*	?	?		C	?
18			*	*				*	*		S	?
19	*	*	*	*		*		*	*		W	?
20			*	*				*	*		P	X
21			*	*			*	±	*		P	H

¹ Adams, *A Cost Approach to the Assessment of Gang Rehabilitation Techniques*, 4 J. RES. CRIME & DELINQUENCY 166 (1967).

² Adams, *The PICO Project, THE SOCIOLOGY OF PUNISHMENT* (N. Johnston, L. Savitz & M. Wolfgang ed. 1962).

³ Adamson & Dunham, *Clinical Treatment of Male Delinquents: A Case Study in Effort and Result*, 21 AM. SOCIO. REV. 312 (1956).

⁴ Anderson, *Work Release Sentencing*, 28 FED. PROBATION 7 (1964).

⁵ Babst & Mannering, *Probation Versus Imprisonment for Similar Types of Offenders: A Comparison of Subsequent Violations*, 2 J. RES. CRIME & DELINQUENCY 60 (1965).

⁶ Barbash, *A Study of Psychological Therapy and Post-Release Adjustment*, 25 AM. J. CORR. 26 (1963).

⁷ B. BEARD, *JUVENILE PROBATION: AN ANALYSIS OF THE CASE RECORDS OF FIVE HUNDRED CHILDREN STUDIED AT THE JUDGE BAKER GUIDANCE CLINIC ON PROBATION IN THE JUVENILE COURT OF BOSTON* (1934).

⁸ Bowman, *Effects of a Revised School Program on Potential Delinquents*, 322 ANNALS 53 (1959).

⁹ Brown & Dodson, *The Effectiveness of a Boys' Club in Reducing Delinquency*, 322 ANNALS 47 (1959).

¹⁰ Cabeen & Coleman, *Group Therapy with Sex Offenders: Description and Evaluation of Group Therapy Programs in an Institutional Setting*, 17 J. CLINICAL PSYCHOLOGY 122 (1961).

¹¹ Caditz, *Effects of a Forestry Camp Experience on the Personality of Delinquent Boys*, 17 J. CLINICAL PSYCHOLOGY 78 (1961).

¹² Caditz, *Effect of a Training School Experience on the Personality of Delinquent Boys*, 23 J. CONSULTING PSYCHOLOGY 501 (1959).

¹³ Caldwell, *Review of a New Type of Probation Study Made in Alabama*, 15 FED. PROBATION 3 (1951).

¹⁴ 1957 CALIF. DEPT. CORR. 1ST ANNUAL REP., INTENSIVE TREATMENT PROGRAM.

¹⁵ CALIF. YOUTH AUTHORITY, *Alameda County Project*, D. GLASER, *THE EFFECTIVENESS OF A PRISON AND PAROLE PROGRAM 458-59* (1964).

¹⁶ CALIF. YOUTH AUTHORITY, *Assessment of Group Counseling in a Youth Training School: The Paso Robles Project*, THE STATUS OF CURRENT RESEARCH IN THE CALIFORNIA YOUTH AUTHORITY (1963) and (1966).

¹⁷ CALIF. YOUTH AUTHORITY, *Assessment of Group Counseling in a Youth Training School: The YTS Project*, THE STATUS OF CURRENT RESEARCH IN THE CALIFORNIA YOUTH AUTHORITY (1963) and (1966).

¹⁸ CALIF. YOUTH AUTHORITY, *A Comparative Study of the Community Delinquency Control Project*, THE STATUS OF CURRENT RESEARCH IN THE CALIFORNIA YOUTH AUTHORITY (1967).

¹⁹ CALIF. YOUTH AUTHORITY, *Forestry Camp Study: Comparison of Parole Violation Rates of Camp-Eligible Boys Randomly Assigned to Camp Programs and to Institutional Programs*, THE STATUS OF CURRENT RESEARCH IN THE CALIFORNIA YOUTH AUTHORITY (1967).

²⁰ CALIF. YOUTH AUTHORITY, *The Fremont Project: Assessment of a Therapeutic Living Unit*, THE STATUS OF CURRENT RESEARCH IN THE CALIFORNIA YOUTH AUTHORITY (1963) and (1967).

²¹ CALIF. YOUTH AUTHORITY, *THE FRICOT RANCH*

TABLE I—Continued

Study	Methodological Criteria										Type of Treatment	Results Claimed
	I	II	III	IV	V	VI	VII	VIII	IX	X		
22			*					*	*		M	F
23								*	*		S	H
24									?		P	F
25							*	*	*	*	M	H
26									?		C	G
27									?		P	G
28											P	G
29			*	*	*						P	?
30									±	±	E	H
31			*	*	*		*	±	*	*	P	H
32							*	*	*	*	S	X
33	±		*	*				?	?		M	?
34								*	*	*	S	F-G
35			*		*			*	*	*	C	F-G
36			*		*			*	*	*	S	H
37											E	H
38											W&SW	F
39							*	*	*	*	P	F-G
40							*	*	*	*	SW	F
41			*	?	*		*				P	F-G
42								*	*	*	S	G
43								*	*	*	P	?
44							*	*	*	*	S	X
45								*	*	*	P	X
46			*	*				*	*	*	S	?

SCHOOL STUDY: AN ANALYSIS OF A TRAINING SCHOOL EXPERIENCE, PROGRESS REPORT No. 4 (1963).

²² CALIF. YOUTH AUTHORITY, *The Marshall Project: Assessment of a Short-Term Residential Treatment Program at a Youth Authority Reception Center, THE STATUS OF CURRENT RESEARCH IN THE CALIFORNIA YOUTH AUTHORITY* (1967).

²³ Cass, *Parole Can Be Successful*, 31 J. CRIM. L. & C. 7 (1940).

²⁴ Chiles, *The Reception Center: An Evaluation*, 22 AM. J. CORR. 16 (1960).

²⁵ Clark, *The Texas Prerelease Program*, 30 FED. PROBATION 53 (1966).

²⁶ Clarke, *Group Therapy in Rehabilitation*, 16 FED. PROBATION 28 (1952).

²⁷ Conn, *The Psychiatric Treatment of Certain Chronic Offenders*, 32 J. CRIM. L. & C. 631 (1942).

²⁸ Corsini, *Group Psychotherapy with a Hostile Group*, 6 GROUP PSYCHOTHERAPY 168 (1954).

²⁹ D. GLASER, *The Collage Life Intervention Program, THE EFFECTIVENESS OF A PRISON AND PAROLE SYSTEM 208-10* (1964).

³⁰ Crossley, *Group Training for Predelinquents, Delinquents, and their Parents*, 22 FED. PROBATION 25 (1958).

³¹ Davitto & Scullion, *An Experiment in Community Treatment of Delinquents*, 48 SOC. CASEWORK 10 (1967).

³² Dunham & Knauer, *The Juvenile Court in its Relationship to Adult Criminality*, 32 SOC. FORCES 290 (1954).

³³ Empey & Rabow, *The Provo Experiment in Delinquency Rehabilitation*, 26 AM. SOCIO. REV. 679 (1961).

³⁴ England, *A Study of Post-Probation Recidivism Among Five Hundred Federal Offenders*, 19 FED. PROBATION 10 (1955).

³⁵ Fox, *The Effect of Counseling on Adjustment in Prison*, 32 SOC. FORCES 285 (1954).

³⁶ Fox, *Michigan's Experiment in Minimum Security Penology*, 41 J. CRIM. L. & C. 150 (1950).

³⁷ Franklin, *The All-Day Neighborhood Schools*, 322 ANNALS 63 (1959).

³⁸ Fried, *A Work Camp Program for Potential Delinquents*, 322 ANNALS 38 (1959).

³⁹ Frisbie, *The Treated Sex Offender*, 22 FED. PROBATION 18 (1958), followed up in Frisbie, *Treated Sex Offenders Who Reverted to Sexually Deviant Behavior*, 29 FED. PROBATION 52 (1965).

⁴⁰ Gandy, *Preventive Work With Street-Corner Groups: Hyde Park Youth Project, Chicago*, 322 ANNALS 107 (1959).

⁴¹ Gersten, *Group Therapy With Institutionalized Juvenile Delinquents*, 80 J. GENETIC PSYCHOLOGY 35 (1952).

⁴² D. GLASER, *THE EFFECTIVENESS OF A PRISON AND PAROLE SYSTEM* (1964).

⁴³ Glover, *Control of the Sex Deviate*, 24 FED. PROBATION 38 (1960).

⁴⁴ S. & E. GLUECK, 500 CRIMINAL CAREERS (1930).

⁴⁵ S. & E. GLUECK, *ONE THOUSAND JUVENILE DELINQUENTS: THEIR TREATMENT BY COURT AND CLINIC* (1934).

⁴⁶ Gordon, *An Experiment in Intensive Supervision*, 1953 NATIONAL PROBATION AND PAROLE ASSOCIATION YEARBOOK 24-33.

TABLE I—Continued

Study	Methodological Criteria										Type of Treatment	Results Claimed
	I	II	III	IV	V	VI	VII	VIII	IX	X		
47			*				*		?		P	F-G
48			*	*			*	±	*		P	X
49	*	*	*	*		*	*	±	*		W	X
50			*					*	*	*	P	X
51								±	*	*	P	G-H
52											E	H
53											P	F
54								*	*		S	H
55	±	±					*				W	H
56									*		SW	F
57								*	*	*	P	F-G
58			*	*				*	*	*	P	G-H
59			*	?					?		P	F
60								*	*		E	H
61			*	*	*			*	*	*	C	X
62	*	*						*			W	H
63			*	*			*				P	X
64	*	*	*			*		*	*		M	G-H
65	*	*						*		*	M	G
66								±	±		SW	F
67							*	*	*	*	SW	F-G
68									?		P	G
69									?		S	G
70								*	*	*	M	G
71									?		SW	G
72											P	G

⁴⁷ Grant & Grant, *A Group Dynamics Approach to the Treatment of Nonconformists in the Navy*, 322 ANNALS 126 (1959).

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TABLE I—Continued

Study	Methodological Criteria										Type of Treatment	Results Claimed
	I	II	III	IV	V	VI	VII	VIII	IX	X		
73			*	*			*		?		P	G
74											C	F-G
75											S	X
76									?		E	G
77									?		S	G
78											P	G
79			*	*	*		*	*	*	*	C	X
80			*					*	*	*	SW	F
81								*			M	?
82			*		*		*		?		P	G
83			*				*				S	F-G
84			*					*	*		W	F
85			*		*			*	*	*	E	F
86											P	G
87	*	*	*	*	*		*		?		M	F
88											P	H
89			*	*				*	*		S	X
90			*	*				±	±		SW	?
91							*				P	G-H
92											P	G
93											C	F
94									?		M	G
95	*	*					*	*	*	*	M	X
96								*			P	G

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TABLE I—Continued

Study	Methodological Criteria										Type of Treatment	Results Claimed
	I	II	III	IV	V	VI	VII	VIII	IX	X		
97			*	*	*		*	*	*		P	G
98			*		*	*	*	*	*	*	M	G
99							*	*	*		P	F-G
100								*	*	*	S	?
N	9-12	9-11	41-42	23	14	5	31	50-59	51-55	30	—	—

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LEGEND

Columns:

- I. Adequate definition of program or technique
- II. Capable of routinization
- III. Provision of control group
- IV. Control group selected on random basis
- V. Control group selected by matching
- VI. Evidence that only Treatment group received treatment
- VII. Before and After comparison
- VIII. Measurable definition of "success"
- IX. Compatible with normal notions of "success"
- X. Follow-up in the community

Column Symbols:

- * = criterion fulfilled
- ± = criterion partially fulfilled
- ? = couldn't determine
- [blank] = criterion not fulfilled, or not applicable

Type of Treatment:

- C = Counseling, group and individual (non-psychiatric)

E = Education

P = Psychiatric treatment, individual and group psychotherapy, psychiatric social work

S = "Standard" techniques: probation, parole, institutionalization

SW = Social work (non-psychiatric), community services

W = Work therapy, vocational rehabilitation, employment services

M = Miscellaneous (Boys' Clubs, recreation, religion, etc.)

Results Claimed:

- H = Quite high to very high success
- G = Good success
- F = Fair success, or limited success
- X = Failure (no effect, same effect as control group, or harmful effect)
- ? = Unclassifiable (no judgment made, effectiveness varied, etc.)