determining *what* the function of criminal justice is to be. It should, however, explain why the present investigation is limited to a quantifiable, if far from perfect, measure of subsequent criminal behavior and why it does not report on "personal growth," "employability," or "response to peer pressure."

The Standard of Comparison Employed

There are a variety of technical reasons why it is rather difficult to evaluate the impact of halfway house programs on recidivism. The first of these is that researchers will have real problems defining and following up an adequate *comparison group.*[22]

Where a control group design is not available, there are a number of statistical techniques, such as matching, multiple regression, and successive dichotomization (base expectancy), which may be used to adjust comparisons between groups which, in fact, are not precisely comparable, for example, clients at Brooke House compared to all other Massachusetts parolees. These techniques rely on the data available for both groups, examine that data to discern relationships between background variables and recidivism, and then adjust the comparison for specific differences *known* to be related to recidivism.

No matter how completely the available concrete data is adjusted, there can still remain some dynamic differences of uncertain consequence between the placements at the program and the general parolee group:

> [R]esidents of a halfway house which is an independent agency . . . are by that very fact an unrepresentative group of offenders. They come to the program if they *want to* or because some one else makes the judgment that they *need to.*[23]

On the one hand, halfway house applicants might be expected to lack community and family ties—factors not directly assessed in the available background data files. On the other hand, halfway house selection processes may emphasize those who are perceived as "ready to change," another factor not likely to be available in the data base.[24] While Brooke House screeners refused to accept those who showed no interest at all in active participation, they rarely turned away applicants on any other basis, except during those few short periods when the House was oversubscribed.

The present statistical analysis is based on the successive dichotomization, or base expectancy, technique. The Massachusetts Department of Correction (D.O.C.) periodically studies the criminal history records of those paroled from state institutions during particular years, in an effort to specify the *rate* and *patterns* of recidivism.[25] Such a study was done for those paroled in 1966, using a follow-up period of two years after release. A recent D.O.C. study has been completed which does a similar analysis for 1971 parolees, using a one-year follow-up. Chart 1 is an example of the result: for parolees released from Walpole in 1966, this chart most accurately describes the patterns of recidivism over the next two years. Further, once these predictive patterns have been traced out, there are no other vari-

[22] While the evaluator's clear preference will be for a random design ("controlled experiment"), he is extremely unlikely to get it, for several reasons: (1) a random design must be clearly constructed at the start of the program period and rigorously complied with throughout; (2) halfway houses are seldom so oversubscribed that they are prepared to turn away about half of the suitable applicants; and (3) most program personnel, like other social activists, see their programs as a definite improvement on the system and not as an experiment—they are unwilling to deny their pro to allocate scarce slot

[23] SHAW REPORT, *supra*, note 20.

[24] Of course, whether either the parole boards or the houses are correct in a particular case–or even in the aggregate—is an empirical question; there is some evidence to suggest that their judgments do not improve on the base expectancy scores generated from the "hard" data. Berecochea & Sing, *The Effectiveness of a Halfway House for Civilly Committed Narcotics Addicts,* INTERNATIONAL JOURNAL OF ADDICTIONS (Spring 1972); *see also* the sources cited at note 20, *supra.*

[25] *See, e.g.,* LeClair, *An Analysis of Recidivism Among Residents Released from Massachusetts Correctional Institutions During 1971* (Massachusetts Dep't of Corrections, May 1975); Graves, *An Analysis of Recidivism Among Men Released from M.C.I. Norfolk During 1966* (Massachusetts Dep't of Correction, August 1972); Carney, *Predicting Recidivism in a Medium Security Correctional Institution: Base Expectancy Categories for M.C.I. Norfolk* (Massachusetts Dep't of Corrections, June 1966); Metzner & Weil, *Predicting Recidivism: Base Rates for Massachusetts Correctional Institution Concord,* 54 J. CRIM. L. C. & P. S. 307 (1963). The technique presently in use essentially attempts to maximize the chi square of variables tabulated against recidivism, across all possible dichotomies for each independent variable. The variable with the greatest "predictive force" is chosen, and then the process is repeated within each new cell until no further significant chi squares will emerge. *Cf.* F. SIMON, PREDICTION METHODS IN CRIMINOLOGY (1971); D. GLASER, THE EFFECTIVENESS OF A PRISON AND PAROLE SYSTEM (1969); Babst, Gottfredson & Ballard, *A Comparison of Multiple Regression and Configural Analysis Techniques for Developing Base Expectancy Tables,* 5 J. RES. CRIME & DELINQUENCY 72 (1968).

Chart 1

BASE EXPECTANCY OF RECIDIVISM CATEGORIES FOR WALPOLE

					RETURN RATE	
TOTAL WALPOLE RELEASEES DURING 1966 N= 194 45.4% Return	10 OR FEWER PRIOR ARRESTS N= 107 32.7% Return	RESIDENCE PRIOR TO COMMITMENT OTHER THAN BOSTON N= 61 21.3% Return	SERVED IN ARMED FORCES	N= 31	6.5%	CATEGORY I
			NEVER SERVED IN ARMED FORCES	N= 30	36.7%	CATEGORY II
		RESIDENCE PRIOR TO COMMITMENT BOSTON N= 46 47.1% Return	2 OR FEWER PRIOR ARRESTS FOR PROPERTY OFFENSES	N= 17	29.4%	CATEGORY III
			3 OR MORE PRIOR ARRESTS FOR PROPERTY OFFENSES	N= 29	58.6%	CATEGORY IV
	11 OR MORE PRIOR ARRESTS N= 87 60.9% Return	SERVED IN ARMED FORCES N= 37 43.2% Return	3 OR FEWER PRIOR HOUSE OF CORRECTION INCARCERATIONS	N= 21	28.6%	CATEGORY V
			4 OR MORE PRIOR HOUSE OF CORRECTION INCARCERATIONS	N= 16	62.5%	CATEGORY VI
		NEVER SERVED IN ARMED FORCES N= 50 74.0% Return	33 OR OLDER AT COMMITMENT	N= 21	52.4%	CATEGORY VII
			32 OR YOUNGER AT COMMITMENT	N= 29	89.7%	CATEGORY VIII

CHART 2

CALCULATION OF AN AGGREGATED "RISK FACTOR" FOR A HYPOTHETICAL SAMPLE FROM WALPOLE

Category*	Category Weight	Hypothetical Sample**	"Risk Weight"
I	.065	13	.845
II	.367	15	5.505
III	.294	8	2.352
IV	.586	14	8.204
V	.286	13	3.718
VI	.625	10	6.250
VII	.524	17	8.908
VIII	.897	20	17.940
			53.722

$$\frac{53.722 \text{ "Risk Weight"}}{100 \text{ Men}} = \begin{matrix} 53.7\% \text{ expected} \\ \text{rate of recidivism} \end{matrix}$$

higher recidivism rating than that for the Walpole population as a whole (45.4%).

* Refer to Chart 1 to determine the appropriate category, based on the stated background factors.

** The breakdown was artificially created; in a real calculation, of course, this would be defined by the data.

ables *in the data file* which will significantly improve the ability to specify patterns of recidivism.

In the analysis which follows, the Brooke House clients entering between 1965 and 1968 are compared to the two-year research on D.O.C. 1966 releasees, and the 1969–1972 clients are compared to the one-year follow-up research on 1971 releasees. While the use of two different comparisons adds a complication to the work (a complication compounded by the fact that each study requires a different follow-up period), it also avoids the troublesome issue of noncontemporary comparisons. As it turns out, this is a significant advantage, since Parole Board policy on revocations showed some clear changes between the 1966 and the 1971 release cohorts.

One can think of the recidivism rate attached to each pattern as a "predictor score" for individuals who fit the pattern; the scores for each member of a particular group of parolees can then be aggregated, and a "statistical risk factor" or "expected recidivism rate" achieved for that group.[26] One such hypothetical calculation, for a "sample" of 100 men distributed randomly among the various risk categories, is demonstrated in Chart 2.

A tailored baseline such as the one computed in Chart 2 can then be used as an adjusted, constructed "comparison" against which the actual behavior of the sample can be assessed. Chi square or other standard tests may be used to interpret statistically the significance of any difference between the actual pattern observed and the pattern predicted on the basis of the expectancy table.

A second obstacle in the structuring of an "impact" evaluation comes in the specification of an appropriate indicator for recidivism. The problem is two-fold: one wants an indicator which one is confident *is* related to the underlying realities of criminal behavior; one also wants an indicator which will have the same meaning for both the comparison group and the sample being studied. The choice here was predetermined by the reliance placed on the Massachusetts Department of Correction's existing base expectancy research.

In its analyses, the Department of Correction defines recidivism as reincarceration for a period in excess of thirty days, commencing during the follow-up period. This reincarceration can come from a new commitment, from a revocation of parole based on a new arrest, or from a revocation based on a "technical" violation of parole.[27] One form of such technical violation is the violation of a condition of parole; for example, leaving a residential placement without staff approval.

This is both a difficult aspect of analyzing program recidivism data and a significant issue, since 22 per cent of Brooke House reimprisonments were based on a technical violation. (This type of revocation was highest in the early years of the program, and had dropped substantially by the 1971–1972 period.) While a technical violation is *not* a new crime, it can hardly be assumed that those whose parole was revoked on technical grounds would otherwise never have been reimprisoned. Further, absconding can be viewed as a form of "program failure"—at least, once the client has become a program participant.[28] The problems which the technical revocation creates for an analysis of the crime-reducing effect of a halfway house are handled in the discussion section by a series of alternate assumptions. As it turns out, none of those assumptions would alter the overall conclusion.

The third barrier faced in attempting to do research on program impact on recidivism is the absence of data and the difficulty of obtaining existing data. The full cooperation of the program being studied is absolutely essential, and the author gratefully acknowledges the cooperation of Brooke House and of Bryan Riley, the Director of Massachusetts Halfway Houses, Inc. But in order to get criminal histories data—and the critical comparison data—much more is necessary. The author is especially grateful, therefore, to the Department of Correction and its research staff both for allowing access to data,[29] and for sharing the results of their own research on recidivism.[30]

[26] It must be remembered that these are really aggregate pred[illegible]rticular prisoner's chan[illegible]remely high error factors. *See generally,* Ball, *The Moment of Truth: Probability Theory and Standards of Proof,* 14 VAND. L. R. 807, 810 (1961).

[27] The imperfection of any measure of recidivism is clearly recognized throughout the literature; the author was constrained here to adopt the measure used in the recidivism tables, a measure which does have the virtues of accuracy and a focus on more serious violative conduct.

[28] Five per cent of those paroled to Brooke House, however, remained there less than forty-eight hours.

[29] Pursuant to a plan for preserving the confidentiality of information, approved by the state's Criminal History Systems Board, Department of Correction, and Parole Board.

[30] Even cooperation is not enough: it took more than four months after the research design was completed to obtain the necessary clearances for access to the data files. Collecting the data then required over two hundred hours of work by Helene Whittaker, assisted by Gerry Bryant and Betty Farrell. Even so the analysis then had to be postponed another six months until the necessary comparison data on the 1971 parolees was available.

TABLE 2

REIMPRISONMENT RATES FOR THOSE PAROLED TO BROOKE HOUSE

N = number returning to prison out of total number paroled to Brooke House

	All Clients	Clients Who "Split"	Clients Who "Stayed"	Clients Who "Completed"	Clients Who Stayed But Failed To "Complete"	
1965–1968 Clients N = ( )	52.3% (45 of 86)	80.0% (8 of 10)	48.7% (37 of 76)	23.0% (8 of 35)	70.7% (29 of 41)	Two year follow-up
1969–1972 Clients N = ( )	30.9% (46 of 149)	37.5% (3 of 8)	30.1% (43 of 141)	14.0% (11 of 80)	52.5% (32 of 61)	One year follow-up

Note: "Splitting" was defined as remaining at the program for less than one week. Whether or not the client had "completed" the program at the time he left was determined from notations in the program journal.

Patterns of Recidivism

Roughly half of the 1965–1968 clients were reincarcerated during the two years they were followed; roughly a third of those in the 1969–1972 group were returned to prison during the year after their conditional parole to Brooke House (Figure 1). For those rated as completing the program the recidivism rates are much lower: 23 per cent of the two-year group and 14 per cent for the one-year group.

The first cohort also showed substantial differences in recidivism between those who stayed in the program for at least a week and those who "split" at once (Table 2). This pattern is not apparent for 1969–1972 clients, apparently because the Parole Board was no longer almost automatically revoking the parole of "early splits." (Remaining at Brooke House until rated a "completion" was, after all, the condition set for the parole.)

Our analysis found relatively few variables which discriminated between recidivists and nonrecidivists at Brooke House. The associations can be summarized as follows:

> The individual admitted to Brooke House who is most likely to make a success of his parole is somewhat older, did a longer stretch (and was more likely to serve it at the State Prison); while he had more convictions as an adult (but not more overall time incarcerated at the state/federal level), he had less of a juvenile and county-level record. He is more likely to have been sentenced after a trial. He is perhaps more likely to come from outside Boston, to have been married at the time he was incarcerated, and to have [illegible] He has a lower risk [illegible] House, and is quite [illegible] completion."

On all other background and institutional variables gathered by this study, the individual who was reincarcerated did not differ substantially from the individual who successfully finished out the follow-up period.

Many of these factors are relevant to recidivism because they are relevant to whether or not the client completes the program, and completing the program, in turn, is highly relevant to recidivism. If the focus is narrowed to consider only those parolees who completed the Brooke House program, a somewhat different pattern of association can be discerned:

> The individual who completes the Brooke House program and then is more likely to make a success of his remaining parole [first, the stronger trends] did not have a record of drug use, and may have done some time in departmental segregation; moreover [weaker trends] he was more likely to have come from outside Boston, and to have held a job for a longer period of time.

Whether these particular variables define categories of offenders upon which the program has a "differential impact," or whether they merely reflect relationships which exist among the general releasee population can be analyzed using the base expectancy technique outlined above to control the comparison between client recidivism and the recidivism patterns among the full releasee groups of 1966 and 1971.

The Comparative Results

Table 3 sets out the comparison between the actual reimprisonment rates for the 1965–1968 and

TABLE 3

RECIDIVISM RATES FOR BROOKE HOUSE CLIENTS COMPARED TO RATES PREDICTED FOR THEM FROM DEP'T OF CORRECTION BASE EXPECTANCY TABLES

	Rate Predicted from B.E. Table	Group's Recidivism Rate	
1965–1968 Clients	53.4%	52.3%	N = 86; two year follow-up
1969–1972 Clients	23.1%	30.1%	N = 144*; one year follow-up

* Five cases were lost in the calculation of 1969–1972 base expectancy rates due to inadequate information on essential predictor variables. Two of these clients had been reimprisoned during the follow-up period, and three had not.

TABLE 4

RECIDIVISM AND PREDICTED RECIDIVISM FOR BROOKE HOUSE CLIENTS, BY YEAR

	Rate Predicted from B.E. Tables	Group's Recidivism Rate	Number of Clients Received That Year
1965–1968 Clients*	50% (est.)	49%	86
1969 Clients	22%	31%	29
1970 Clients	21%	34%	35
1971 Clients	25%	37%	35
1972 Clients	25%	22%	45

* For the first year only; the predicted rate is based on the proportion of Brooke House total two-year recidivism which had occurred by the end of one-year of follow-up.

1969–1972 Brooke House clients and the rates which had been predicted for them. In a sample of this size, the sorts of differences displayed in Table 3 should be expected as a result of sampling fluctuations, and should not be attributed to any characteristic of the program.

One important result (Figure 1 and Table 4) is the sharp drop in the level of recidivism between the 1965–1968 and the 1969–1972 sample groups, and the further dramatic drop for 1972 clients. The 1972 rate is significantly lower than that of clients in the three preceding years despite the fact that the predicted rate ("risk rating") for the group is actually slightly higher. This drop is consistent with the informal perception of those within the Massachu[TTNA EXHIBIT 29]ard's revocation pra[Page 12 of 17]g where only a "technical" violation, and not a new arrest, is involved. A particularly sharp shift came in late 1972 or early 1973, apparently as a response to overcrowding within the prison system. It is precisely these sorts of changes which limit the validity of any base expectancy model as a research comparison tool; had the 1966 model been used on the entire client group, a very strong "effect" would have been produced. Such a spurious "effect" would in fact be only a reflection of a change over time which occurred throughout the system and which affected both participants and nonparticipants equally.[31]

The fact that a client has been adjudged a "program completion" is, as previously noted, strongly related to his ability to survive the follow-up period without reimprisonment. This remains true even if the analysis eliminates all cases in which the parole of a noncompleting client was immediately revoked as a result of his having left the program. The effect is not dissipated when base expectancy ratings are factored in, as Table 5 demonstrates for the 1969–1972 group. The texture of this result is rather interesting: some of those who leave the program without permission have parole revoked at once; noncompleters who survive this point perform about as the expectancy tables might have predicted. Beyond the very real threat that parole will be revoked for a violation of the residential condition, there is no "failure effect" at Brooke House. There is, however, a distinct "success effect" for those who do complete the program. *When the success effects for both the 1965–1968 and 1969–1972 clients who*

[31] Thus, use of these tables to evaluate patterns of recidivism for releasees in 1972, 1973 and succeeding years will be increasingly suspect, and likely to generate an illusory "program effect." *Cf.* LeClair, *An Analysis of Recidivism Among Residents Released from Boston State and Shirley Pre-Release Centers During 1972–73* (Massachusetts Dep't of Correction, August 1975) [hereinafter cited as LeClair].

TABLE 5

RELATIONSHIP BETWEEN PROGRAM COMPLETION AND RECIDIVISM, 1969–1972 CLIENTS

	Completion	Number	Rate Predicted from B.E. Tables	Group's Recidivism Rate
All Clients, 1969–1972	No	66	26%	52%
	Yes	78	21%	13%
Those 1969–1972 Clients Who Left the Program Without Being Revoked For That Reason	No	38	27%	29%
	Yes	78	21%	13%

Note: Of the five 1969–1972 clients who were dropped from the base expectancy analysis because of insufficient background data, two had completed the program and three had stayed but had not completed.

completed the program are pooled, the result is statistically significant ($p \leq .05$).

We must emphasize, however, that it is difficult to justify this kind of analysis in terms of the available methodology. The parolee samples which are used to generate the base expectancy score do not include a screen similar to the test of "in-program failure." Refocusing the analysis on completions only is a selection process of substantial importance, and one not really capable of statistical control within the base expectancy methodology. While the patterns of recidivism revealed by this analysis do suggest that a halfway program may provide a useful "screening stage" within the correctional system's release process, the program must nevertheless be judged in terms of its impact upon all those sent to it for assistance. Working with the best available data, we conclude that there is no evidence to suggest that recidivism among Brooke House clients is reduced by virtue of their being conditionally paroled to the program. This seems to be true even when the analysis is restricted to those clients who give the program at least "a try."

DISCUSSION

Dealing with Alternative Interpretations

There are two remaining plausible alternative explanations—potential reasons why this set of results might be considered "equivocal" rather than "definitive." Both these sources of reconsideration relate to the dynamic of the conditional placement.

The first of these, the problem of technical v[illegible]missed after trying o[illegible]. However, those alternative hypotheses which would attach some unadjusted-for meanings to the Parole Board's insistence on a conditional placement cannot be totally rejected, although their plausibility seems very weak indeed.

1) *Technical revocations.* A significant portion of reimprisoned Brooke House clients were specifically returned to prison because they left the program without permission. Between a fifth and a quarter of Brooke House recidivism was of this sort. By definition, of course, the general releasee population does not face this particular risk.

This could become an operational dilemma for programs like Brooke House, which feel that the threat of revocation for leaving the program is important in keeping the individual at the House while he is dealing with the personal stresses of a changing lifestyle. The program's effect on recidivism is supposed to be brought about by a changing of lifestyle, but revocation of parole for leaving the program "inflates" the official recidivism rate.

Since we hardly can prophesy the "actual" rate of recidivism that such revokees would have had, had they remained on the street after leaving the program, we cannot accurately "discount" for this complication. We can, however, try out the relatively optimistic assumption that their "actual" rate would have been no higher than the predicted rate. The implications of this assumption for the basic recidivism comparison are not strong enough to alter the conclusion of this study. For the 1969–1972 group we ignored all clients whose revocation was premised on the fact that they had left the program without permission. Even this assumption does not alter the basic conclusion.

Finally, a client's going A.W.O.L., or never appearing at the House, or being expelled may be

thought of as forms of "early warning" to the Parole Board that he is an extremely bad risk for the immediate future. In that case, reincarceration for the remainder of the period would "save" crime that would otherwise be committed. Since reducing *crime,* rather than reducing *reimprisonment,* is the goal of the correctional program, the data might be reanalyzed, a bit ingenuously, to give Brooke House "credit" for preventing new-crime recidivism in each case where an improper exit from the program was followed by parole revocation for that reason.

Even this version of the analysis does not produce a net positive effect for the program, although it does point to a further important characteristic of the client group's pattern of recidivism: many of those who are returned to prison are arrested while still in residence, for crimes they have committed while residents of Brooke House. That is what "in program failure" is all about, both at Brooke House and at pre-release programs. The difference is that the Brooke House program, because it operates at the parole stage, must take responsibility for this sort of failure by having it recorded as an instance of "recidivism." The pre-release program, by contrast, dismisses such returns to prison as "in program failure," and insists that the program must be judged only in terms of those who are paroled from it; that is, who are program-completers. This study's analysis of Brooke House completions demonstrates that on such terms Brooke House is *also* a successful screening program. Why a crime which occurs immediately *before* a parole date should be denominated a "screening success," while a crime which occurs the day *after* parole is called a "negative outcome," remains unexplained in current research on pre-release programs.[32]

2) *Unmeasured selection factors.* The first section of this article sets out a number of differences between Brooke House clients and others who were given either parole or a straight release. The base expectancy methodology has been developed precisely for the purpose of adjusting comparisons to account for such differences insofar as they are relevant to the dependent variable (here, recidivism). Once the base expectancy factors specified by the Department of Correction are taken into account, there are no other variables *in the data set available to us* which would further affect the prediction of recidivism. The fact that Brooke House clients differ from the general population in a number of known w[illegible]e of the general population's release experience in formulating a baseline against which the program's effect might be measured.[33]

To the degree that the Parole Board was responding to variables reflected in the data set in requiring a conditional placement at Brooke House, this research design fully controls for the differences between the Brooke House and general parole populations. To the degree that the Board was responding to variables not adequately correlated with our data, the design is still methodologically sufficient except insofar as those additional factors are *highly* correlated with recidivism.

The methodology applied, however, cannot preclude the logical possibility that the Parole Board is responding to still other "soft" factors which (1) are not accounted for by the risk rating and (2) are not substantially correlated with any variable in our data set, insofar as they are substantially correlated with increasing recidivism. To the degree that this seems possible, it may still be argued that Brooke House clients are "specially handicapped" in some way not adjusted for by the base expectancy computation and that a recidivism rate which matches the predicted score is actually a very successful outcome. While that alternative interpretation has no substantive basis anywhere in this research, it is unlikely ever to be totally ruled out unless a random-control experiment is conducted. It is only fair to point out, however, that for the program to demonstrate a statistically significant effect on the 1969–1972 sample, this unknown set of factors must be sufficiently powerful to almost double the predicted recidivism rate.

The Question of Differential Impact

A number of recent commentators have stressed the fact that programs are not "black boxes," to be tested solely in terms of output, but rather are intervention strategies based, to one degree or another, on theoretical assumptions.[34] Thus, the more

[32] Cf. LeClair, *supra* note 31. A. Reiss, R. Sarri & R. Vinter, Treating Youth Offenders in the Community (J. Galvin ed. 1966).

[33] To double check, we considered the effect of these variables within our Brooke House sample on "recidivism effectiveness," *i.e.,* after factoring out the base expectancy ratings. Very small relationships without consistent direction were found on military experience (negative relationship), job stability (positive), and absence of drug use (positive). Taken together, their effect on the recidivism rate is negligible.

[34] Glaser, *Remedies for the Key Deficiency in Criminal Justice Evaluation Research,* 11 J. Res. Crime & Delinquency 144 (1974); Glaser, *Achieving Better Questions: A Half Century's Progress in Correctional Research,* Fed. Probation at 3 (Sept. 1975); Palmer, *Martinson Revisited,* 12 J. Res. Crime & Delinquency 133 (1975).

important question to be answered is held to be: can we learn from this program whether this modality is particularly appropriate for any particular kind of client? (What we have really been asking so far has been, in effect, a variation of that question: is there any evidence that this program is effective with the kinds of clients with whom *the parole board* wants it to be effective?)

As noted earlier, there are a number of background factors which are related to lower recidivism rates among Brooke House clients, at least in part because they are related to the likelihood of program completion. The question remains whether this relationship was a special characteristic of the program modality or a general one for the releasee population. After factoring in the base expectancy scores the "net program impacts" for sets of background characteristics can be estimated, and such a comparison was made for both the 1965–1968 and 1969–1972 groups. Several variables did show statistically significant effects, although in no case was the correlational relationship particularly powerful. When the 1965–1968 group was sorted through, for example, positive net impacts were shown for clients who did not come from Boston, who were not presently incarcerated as the result of a parole violation, who had low educational achievement, and who had no co-defendants at their current trial.

While these characteristics would make sense in terms of the program focus, it must be recognized that "data dredging" of this sort *ought* to turn up *some* "statistically significant" relationships as an artifact of the statistical method. Moreover, *none* of these variables was significantly associated with positive program impact within the 1969–1972 client group. This latter fact strongly suggests that some of the selective impact that we might have felt called upon to "discover" for the program, had this study been limited to the first client group, would have been little more than a minor statistical illusion.

This raises, of course, an unfortunate point which must be weighed against the emerging *post hoc* reinterpretation of correctional program research. The scientific method calls upon the researcher to make his predictions *before* assessing his data, and to design his research to test the hypotheses he has already set. If this research had stopped with the 1965–1968 clients, it would have been easy enough to look at the results, to "discover" groups within [illegible] an effect, and to th[illegible]*oc* each observed effect.

While this is a perfectly acceptable technique for generating *new* hypotheses to be *tested,* it is not a particularly appropriate model for "validating" theory. The reviewer who feels that this argument is unfair might ask himself how difficult it would be to "reconcile" *other* relationships which were not found, or even the *obverse* of the relationships which were found. The recent review of the correctional program literature by Professor Glaser does a masterful job of reconciling the partial effects found in a variety of research attempts.[35] But the bulk of the studies covered were not planned to test those effects directly, and few ever went on to repeat the research process with such new hypotheses in focus.

The two-phase structure of the present research allowed us to test those "emerging generalizations" hinted at in the 1965–1968 research, and the negative results of those tests forced us to recognize that such generalizations can prove illusory, transitory, or both. Thus chastened, we report here only one of the "impact factors" found for the 1969–1972 sample and only because it has critical implications for the Brooke House program. It was clear that those who had records of involvement with narcotics did substantially worse than their base expectancy scores would predict. It is also clear that an increasing proportion of Brooke House clients have such records. The program's administrators have been aware of these trends during recent years, and have taken steps to adapt the program to those with drug problems without surrendering the basic approach of reality therapy. They have not responded, as some programs might have, by avoiding those with drug records.

It is not clear whether these adaptations will be effective in reducing recidivism. But it is clear that the program's aggressive response to the challenge of dealing with drug offenders in a program that is *not* drug-abuse centered has met with favorable reaction among those evaluating Brooke House and its place in the Massachusetts correctional process.[36]

Conclusion

Even after all the appropriate methodological fallbacks are explored, the evidence on Brooke House discloses no net impact for the program in terms of reducing recidivism. This is consistent with other research on residential programs, whether pre-

[35] *See* authorities cited in note 34 *supra.*

[36] Ohlin & Janvier, *Report of the Massachusetts Adult Correction and Parole Project* (Massachusetts Committee on Criminal Justice, October 1975).

release, parole, or referral, whatever the program modality employed.

This does not mean that *some* programs may not help *some* people. But it does seem to imply that the *general* utility of such programs as a tool in crime reduction is minimal, given the limits of present knowledge about instigating behavior change. The case of Brooke House demonstrates that this is probably true even where the program is stable, the house well-run, and the staff well-trained.

Of course, the promise of an effect upon the recidivism rate is by no means the only ground on which halfway houses can be justified. Proponents of St. Joseph's House, for example, may properly respond that from their perspective:

> The success or failure of the Pittsburgh halfway house, or, for that matter, any halfway house, cannot be measured by counting the number of men who have returned to prison as compared to the number who have gone "straight." In evaluating any such program, due regard should be taken of the number of men who have had an opportunity which would otherwise have been denied, of leaving prison under the aegis of such a program.[37]

Those who make such an argument can certainly use the Brooke House experience to support their case. Whatever the basis for their decision, it remains clear that the Massachusetts Parole Board was unwilling to release these 245 men without a conditional placement.

The comparison of Brooke House clients to the general sample of those released from Massachusetts correctional institutions in 1971 indicated that the Parole Board requiring the placement seemed to be responding primarily to inferences about the inmate's social background and to the character of his present offense. Thus the Brooke House client was more likely to have been committed for a sex-related offense (eleven per cent versus six per cent for the total 1971 releasee group) or for a robbery involving a weapon (twenty-eight per cent versus twenty-one per cent for the 1971 releasee group).

Neither of these offender categories is linked to high recidivism by Department of Corrections researchers. Recidivism rates of twenty-one per cent (armed robbery) and eight per cent (sex-related offenses) are clearly below the twenty-five per cent recidivism rate for all 1971 releasees.[38] But clearly these *are* categories of offenses which particularly offend and unnerve the public, including Parole Board members. Placement of such offenders at a halfway house allows the Board, the agency responsible for the *safe* release of offenders, to offer parole without feeling that it has relinquished control.

It is in such an organizational context that a crucial "effect" for the House can be pinpointed: *Brooke House provided an avenue by which men who would otherwise not then gain release from prison could do so, and men released through Brooke House did not return to prison with any greater frequency than those who obtained direct release.*

Moreover, the rationale for community facilities as a pre-release tool is somewhat different than that for parole. Indeed, some correctional administrators would argue that so long as the results on recidivism are no worse, community corrections are to be preferred to incarceration. Since these administrators hesitate to approach their legislatures or the press with what is at bottom a humanitarian argument, the rationale becomes a cost-efficiency claim.[39]

Other correctional administrators approach the cost benefit possibilities from a rather different perspective, recognizing that the use of community facilities may allow both short and long term extension of the incarceration network. Such extensions are critical organizational resources at a time when prisons are overcrowded, but public pressure for more extensive use of imprisonment is accelerating.

As Norman Carlson noted in a recent speech on prison capacities:

> We may be able to lighten the burden on jail and prison facilities to some extent by an increased use of community-based corrections, such as probation, parole, halfway houses and other programs designed to keep some offenders under supervision without incarcerating them in traditional correctional institutions.[40]

The research here reported upon contributes to arguments against correctional strategies which sur-

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[38] These figures are derived from LeClair, *An Analysis of Recidivism Among Releasees From Massachusetts Correctional Institutions During 1971* (Massachusetts Dep't of Correction, May 1975). That study defines recidivism as reimprisonment for a period to be at least six months, and used a follow-up period of one year.

[39] Such a public rationale may eventually work against community corrections, however, since the evidence on cost is ambiguous at best.

[40] Quoted in Orr Kelly, *Prison Overflow Predicted.* Washington Star, October 29, 1975. This also appears to be the hidden premise of arguments for the incarceration of greater numbers of offenders. *See* J. Q. WILSON, THINKING ABOUT CRIME (1975).

round the decision to release an inmate with a series of special "conditions" like residence at a halfway facility. The "new view" provides a strategy for obtaining partial control over defendants who, because of system logistics and sentencing practices, are not now incarcerated. It does not seem unfair to insist that this new version of community corrections should receive similarly strict scrutiny of its political, as well as its bureaucratic, bases.

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