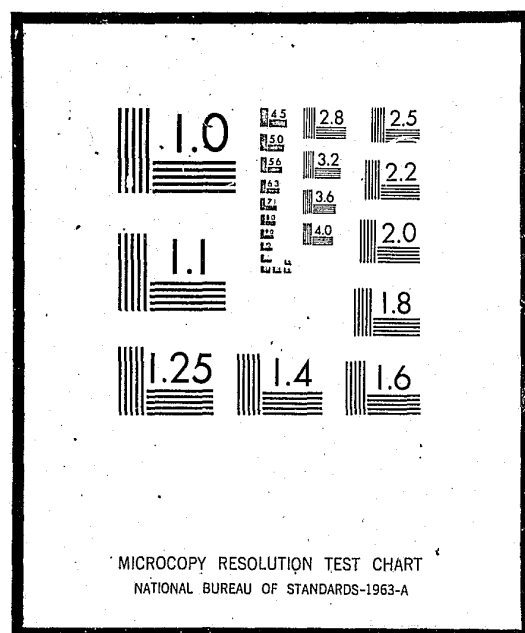


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## TYPES OF TREATMENT FOR TYPES OF OFFENDERS

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## INTRODUCTION

The relation between types of treatment and types of offender is relatively new as a subject of criminological research ; it is only eight years since the work of Grant and Grant<sup>39\*</sup> at Camp Elliott in California first demonstrated empirically that the outcome of a type of treatment could vary, depending on the type of offender to whom the treatment was given. Since this promising start, comparatively little research has been done on the problem ; and the results to date cannot be said to be very encouraging. But the subject represents a natural development of the extensive research on the effectiveness of punishments and treatments which has been carried out over the past twenty-five years, and it seems likely that much more work will be done on it in the future.

This paper will review the most important research which has been done in this field to date, and will attempt to indicate the main methodological and practical problems which surround it. An attempt will also be made to assess the potential value of this kind of research in improving the overall effectiveness of correctional systems. For this purpose, I shall assume familiarity with the report on the effectiveness of treatments and punishments presented by Hood<sup>40</sup> to the 2nd European Conference of Directors of Criminological Research Institutes, at Strasbourg in 1964. A few important studies have been published since Hood's report appeared, but its main conclusions still seem to me to be correct.

*Statement of the problem*

In its most general form, the presupposition which underlies research on this problem can be stated as follows : For any type of offender, there is one type of treatment which is (in some sense) the most appropriate.

This has, of course, long been realised to be a theoretical possibility ; as Grant<sup>40</sup> (p. 8), has pointed out, some sort of assumption

\* The notes in this report are to be found on pages 165 et seq.

about the needs of particular types of offenders already figures in practice — either explicitly or implicitly — in many treatment programmes. Moreover, in one sense a relationship between types of offender and types of treatment has long been accepted at the level of penal policy. As is well-known, the legal and penological policies originally advocated by the Italian positivist school rest entirely on this premise. Ferri's five-fold classification of criminals — criminal insane, born criminals, criminals by acquired habit, "chance" criminals and criminals by passion — was related by him to different types of sentences supposed to be the most appropriate for each type. For example, "eliminative means" were to be used for the most dangerous "born criminals" and criminal insane; whereas "temporary repressive and reparative means" were to be used for occasional delinquents or criminals by passion<sup>21 22</sup>. The whole distinction between punishments and "mesures de sûreté" is thus based on the pre-supposition stated above, which can be seen in operation most clearly today in the case of special sentences for the mentally abnormal offender, so-called "psychopaths", habitual criminals and young offenders.

But there is an important difference between their use of offender typologies in relation to treatment, and their use in more recent research. Non-punitive measures for different classes of offender have a different immediate object, depending on the type of offender on whom they are imposed: thus the object of preventive measures\* for "incorrigible" offenders is simply incapacitation, whereas for other offenders the object of the sentence is treatment or reform\*\*. In contemporary researches, however, the immediate object of the most appropriate sentence for each type of offender is usually taken to be the same — broadly speaking, it is reform or rehabilitation, i.e. "changing a delinquent into a non-delinquent" — but the means used to accomplish this end are supposed to differ, according to the types of offender involved.

This may be either because:

(i) a type of treatment which is effective in reducing the recidivism of one type of offender is ineffective when applied to another type of offender; in other words, a single type of treatment may have differential outcomes for different types of offender. In this

\* e.g. The English sentence of preventive detention (Criminal Justice Act, 1948, S.21) applicable to offenders over 30, convicted at least four times for an indictable offence and imprisoned on two or more occasions. The sentence was 5-14 years if the Court held it "expedient for the protection of the public". The Criminal Justice Act 1967 substitutes for this sentence a similar but less severe sentence for recidivists. See also e.g. Swedish Penal Code, Ch. 30.

\*\* Of course the ultimate object of all of these types of sentence is presumably the same, viz. the prevention of crime.

case, to take a hypothetical example, one might get a matrix of "success rates" (e.g. in terms of the absence of reconviction) which looked like this:

TABLE 1  
Example of Differential Outcomes  
Success rates (per cent)

Type of offender (* = expected success rate)	Type of treatment			Total
		X	Y	
A (50*)		50	50	50
B (60*)		60	80	70
C (70*)		90	70	80
Total	(60*)	67	67	67

In this case, it will be seen that for offenders of type C, treatment X is apparently effective but treatment Y is not, whereas the reverse is true for offenders of type B; the choice of treatment makes no difference to the success rate for offenders of type A, though in their case it might be possible to obtain the same results at less cost, e.g. if treatment X were imprisonment and treatment Y probation.

(ii) Alternatively, it may be that a type of treatment which is effective in reducing the recidivism of one type of offender is not merely ineffective, but downright detrimental, when applied to another type of offender; it may actually increase his chances of recidivism. In this case, one might obtain a matrix of success rates which looked like this:

TABLE 2  
Example of Interaction Effects  
Success rates (per cent)

Type of offender (* = expected success rate)	Type of treatment				Total
		X	Y	Z	
A (50*)		40	50	60	50
B (60*)		70	50	60	60
C (70*)		70	80	60	70
Total	(60*)	60	60	60	60

Here there has been interaction in the full sense between type of treatment and type of offender. Treatment X is beneficial for offenders of type B, but is ineffective for those of type C and detri-

mental for those of type A; similarly, *mutatis mutandis*, for treatments Y and Z. Of course, the "observed" success rates used in these hypothetical examples can only indicate the relative differences between treatments; it is necessary to take into account the prior probabilities of reconviction of each group of offenders, in order to determine the true nature of the observed differences, i.e. to tell whether a type of treatment is beneficial or detrimental for any group of offenders.

In each of these two cases, from the point of view of maximising success rates, there has clearly been some misallocation of offenders to types of treatment. In the second case, if all of the offenders in the sample had been allocated to the optimum treatment for their type (and if the numbers in each cell were identical), the overall success rate for the sample might in theory have been 70 per cent, instead of the 60 per cent shown\*. It can also be seen from these examples that differential outcomes, or interaction effects, may offset one another and may be masked by similar overall success rates. However, a moment of reflection will reveal that even if there are effects of interaction, their influence on the overall success rates will depend largely on the numbers of offenders of each type in the sample, and their expected reconviction rates. For example, if most of the offenders in the sample in Table 1 were of type A (for which choice of treatment is irrelevant to outcome), then even the most accurate allocation of offenders of types B and C would make little difference to the overall results.

There is one other important difference between earlier applications of offender typologies in relation to choice of treatment, and more recent studies. The choice between punishments and "mesures de sûreté" is usually made at a judicial level; the sentencing judge ascertains that the offender is of a certain type (e.g. mentally abnormal and dangerous), and deals with him accordingly. But the allocation of offenders to types of treatment may also take place at an administrative level, within the framework of what is (at a judicial level) a single form of treatment. Thus, for example, different types of offenders may, if sentenced to prison or borstal, be allocated to different institutions within the system, or subjected to different regimes within a single institution; or they may be placed under the supervision of different types of probation or parole officers. All of the recent research on this subject has, in fact, concerned the second type of situation, often in connection with experimental programmes of treatment. As we shall see there are limitations as well as advantages in approaching the problem in this way.

\* There are important reservations to be made on this point, however. See below, p. 48.

### *Are interaction effects possible?*

Research on this problem can be directed at two different, though closely related, aims. The first of these is that of establishing the existence of interaction effects; the second is the explanation of those effects.

But before considering either of these two objectives (and the research strategies appropriate to them), it is necessary to discuss briefly an argument which would, if substantiated, make it unnecessary to investigate either topic. This is the argument that there is little if any overall difference in the effectiveness of different types of treatment: so that the after-conduct of any given type of offender will be about the same.

Some evidence to this effect was presented by Hood<sup>49</sup> (pp. 108-10), who stated that the Home Office study reported in *The Sentence of the Court*<sup>47</sup> was the only research known to him which showed that different treatments do have different overall effects. Commenting on the Home Office research, Walker<sup>51</sup> has noted that even its results may conceal interaction effects (which he refers to as "the hypothesis of misclassification"). He argues, however, that it is more likely that "of the offenders who do not repeat their offences after a given form of penal treatment, all but a very few would have refrained similarly after most other kinds of penal treatment; in other words, that penal measures are for most offenders interchangeable". In favour of this view, Walker cites "the curious similarity between the reconviction rates of offenders subjected to different penal measures. On the hypothesis of misclassification this can only be attributed to coincidence. For it could only be by coincidence that the percentages of offenders allocated to the right and wrong treatments might be so adjusted as to produce similar overall reconviction rates for each treatment... on the other hand, similar rates are exactly what one would expect on the similar hypothesis of the interchangeability of penal measures".

In fact, however, the picture does not seem to be as black as Walker's argument suggests. It is true that the overall reconviction rates for different treatments in the Home Office study do not differ by large amounts. But they do differ to a greater extent than could be expected by chance (or "coincidence"). Ten groups of offenders are considered in the study (first offenders and recidivists, of five different age-groups). The observed differences between reconviction rates for the different types of treatment given to each group appear to be statistically significant beyond the one per cent level of confidence for two of the ten groups, and significant between the one and five per cent levels for three others. Moreover, the differences are consistent: for most age-groups and for recidivists

as well as first offenders, fines and discharges were more effective than other treatments, when the offenders' prior probability of reconviction (base expectancy) was taken into account.

There are a number of other studies of particular forms of treatment (mostly rather specialised individual or group therapy) which show overall differences in effectiveness as measured by the reconviction rates of treatment and control groups\*. In a recent review of 100 evaluative studies of treatment of this kind published between 1940 and 1960, Bailey<sup>5</sup> found a successful outcome reported in about one-half. It is true that the great majority of these studies had serious methodological shortcomings, and that Bailey found that the more rigorous the research design, the higher the percentage reporting "no change" or detrimental effects from the treatment. Nonetheless, of the 22 studies making use of a control group, nine reported a "positive" and statistically significant change in the outcome of the group of offenders given treatment, and four more reported "marked improvement". These results are not as favourable as they might be; but they certainly do not support a general claim that different treatments are simply "interchangeable"\*\*\*.

Finally, the hypothesis of "interchangeability" cannot explain the — admittedly very limited — evidence which has been found,

\* In fact, of course, "control group" is a misnomer here, since the "controls" in each of these studies got some form of penal treatment. In a comparison of imprisonment with group counselling, for example, with imprisonment without group counselling, one cannot simply assume that the imprisonment of the two groups "cancels out".

\*\* In addition to the studies by Adams<sup>7</sup> and Warren<sup>8</sup>, to be discussed later, other recent research projects suggesting overall differences in comparative effectiveness of two or more forms of treatment include the following. Guttman<sup>9</sup> compared boys at two California institutions, who had been given short-term psychiatric treatment, with controls at the same institutions; she found that at one institution the treated group were less often reconvicted than the controls, but that at the other the treated group was more often reconvicted. Robinson<sup>10</sup> and Kevorkian have compared three group counselling programmes for young offenders, with the regular programme of treatment (without counselling) at the same institutions; while experimental subjects were less often reconvicted than the controls after one year at risk, the difference was not significant after two years at risk, and there were no significant differences between the 'after-conduct' of the three experimental groups. A pilot study by Babst and Mannerling<sup>11</sup> compared adult male offenders placed on probation in Wisconsin with those sent to prison. First offenders placed on probation were found to have significantly lower reconviction rates than those sent to prison, when other factors predictive of reconviction (type of offence and marital status) were held constant. In the Provo experiment, Empey<sup>12</sup> found no significant difference between the after-conduct of boys required to attend a non-residential centre as a condition of probation, and a randomly allocated control group given "ordinary" probation. But the control group's success rate was considerably higher than the previous success rate for probation; both treatment and control groups may thus have received a different — and more effective — treatment than "ordinary" probation in Provo.

of interaction between certain types of treatment and types of offender. There is in fact a slight suggestion of interaction effects in the Home Office study: offenders convicted of breaking and entering tended to do better than others when put on probation but worse under other types of treatment, whereas those convicted of larceny did less well on probation but rather better when fined<sup>17</sup>. (paragraphs 63-65). It must be remembered that in order to demonstrate the existence of interaction effects by means of a follow-up study, there must necessarily have been some degree of misallocation of offenders to treatments in the first place: if each type of offender received the type of treatment most effective for him, the patterns of differences in success rates of the kinds illustrated in my hypothetical examples (see above) would not emerge. Very little is now known of the relations between sentencing policies or allocation policies in institutional systems like borstal, and the effectiveness of treatment. But there is some reason to think that the courts (and institutional allocation boards) are already taking into account, consciously or unconsciously, some factors which are related to the optimum treatment for different types of offenders. To the extent that this is so, it will tend to conceal the fact that the outcome of treatment depends in part on the type of offender concerned.

But the argument just considered is not wholly mistaken and it is important to remember that treatment-offender interaction (if it occurs at all) can in practice have only a limited effect on overall success rates, and thus only a limited effect on the efficacy of the penal system as a whole. It is very easy, when one is considering comparative studies of the effectiveness of treatment, to slip into the error of thinking that "success rates" could vary between 0 per cent and 100 per cent, depending on the allocation of offenders to types of treatment. But this is, of course, completely unrealistic. All of the best and most reliable predictors of recidivism — number of previous convictions, age, sex, age at first conviction and type of current offence — involve factors which antedate the choice of sentence. Knowledge of the treatment received by the offender, or of his response to it, adds little if anything to the predictive power of the other factors just mentioned; indeed, I know of only two recent studies\* in which factors relating to treatment were of any use at all in predicting subsequent reconviction. Thus it seems reasonable to assume that the overall "success rate" will never vary, whatever

\* See Glaser<sup>13</sup> (pp. 296-301); Grygier<sup>14</sup> (pp. 34-5). It is interesting to note that both of these studies make use of the variants of the statistical method known as "predictive attribute analysis", and that treatment variables were of predictive weight for some of the offenders in each sample but not for others; this itself is *prima facie* evidence of interaction effects. For a further reference to this statistical method see below, p. 158.

choices of treatment are made, outside a range of (say) 50 per cent to 90 per cent. In other words, some offenders (in particular, first offenders) are almost certainly going to "succeed" whatever treatment is given to them; and other offenders are almost certainly bound to commit further offences no matter what is done to them.

This still leaves room for substantial gains; to raise the overall "success rate" for all offenders from 50 per cent to 60 per cent, after all, would be to reduce recidivism by one fifth, which would be no mean achievement. But it is important to bear in mind that the maximum possible "profit" which can theoretically be obtained by proper allocation of offenders to types of treatment — i.e. by taking the fullest possible advantage of interaction effects, if these occur — is strictly limited. As we shall see below, there are at present other practical limitations which further reduce the potential "profitability" of interaction effects, and thus affect the value of this type of research.

#### *After-conduct studies testing the hypothesis of interaction effects*

In all studies of this subject reported to date, the primary measure of the effectiveness of treatment has been the after-conduct of the offenders concerned — their "success" or "failure" in terms of reconviction, parole violation etc. Some researchers have used other measures, such as personality tests and ratings by treatment personnel, as well; but post-treatment criminal behaviour is the main criterion of the outcome of treatment in use, and for obvious reasons it is the most important one. Its methodological and practical limitations will be discussed below.

Within the broad research strategy based on after-conduct studies, there are three different "tactical" approaches to the demonstration of interaction effects. Either:

1. a single type of treatment may be administered to two or more types of offenders; or
2. two or more types of treatment may be administered to a single type of offenders; or
3. two or more types of treatment may be given to two or more types of offenders. Clearly, the third of these research designs is potentially the most informative; and most of the studies to be described here have been of this type. The other two approaches should not be neglected, since in practice they are likely to be much simpler and more economical to carry out. But since they necessarily involve comparisons of the results of a number of different research projects, they depend, to an even greater degree than the

third research design, on the development of clearly defined and reliable treatment and/or offender typologies. (It is just this feature which distinguishes the first design from a simple treatment-control study, and the second from a comparative study of the overall effectiveness of a number of types of treatment.) And as we shall see, there has been relatively little progress in basic typological research to date.

So far, it appears that fewer than a dozen research projects have been undertaken in English-language countries, of a kind which permit testing of hypotheses concerning interaction effects. Interim or final result are now available for only a few of these.

#### A. RESEARCH PROJECTS FOR WHICH RESULTS HAVE BEEN REPORTED

The main conclusions which can be drawn from these studies may be summarised as follows:

1. *No research has yet produced clear evidence of full interaction, i.e. of treatment which is relatively successful for one type of offender.* The original research of Grant and Grant<sup>29</sup> at Camp Elliott is often said to have demonstrated interaction effects empirically; but the implications of this study for the treatment of offenders are not, in fact, very clear. A total of 511 military "delinquents" (most of them deserters from duty) were rated for "interpersonal maturity" according to a seven-step scale originally propounded by Sullivan, Grant and Grant<sup>30</sup>, and were then classified as high-maturity (I-levels 4 and 5) or low-maturity (I-levels 2 and 3) for the purposes of the research. Groups of 20 high, low and mixed maturity-level subjects each were randomly allocated to three types of training regimes — known as "Living Groups" — each run by three supervisors, for periods of three to nine weeks. The three "Living Groups" were not described in detail in the original paper by Grant and Grant, who merely ranked them according to the predicted effectiveness of the supervisors. Other writers (including Glaser<sup>34</sup>, (p. 4), Wilkins<sup>35</sup>, (p. 53) and Gottfredson and Ballard<sup>36</sup>, (p. 2n.) have suggested that the Living Group predicted "most effective" involved intensive group counselling and group therapy, while that rated "least effective" involved merely the traditional rigid disciplinary regime. But Grant and Grant did not say this; indeed, by ranking all three sets of supervisors they imply that all three Groups were carried on in more or less the same way, but that the supervisors predicted "least effective" were simply less competent to carry out this type of treatment.

It was found that the high maturity subjects did significantly better overall than low-maturity ones; and the Grants concluded from their findings that "... military recidivism (sic) could be reduc-



ed by installing a closed Living Group programme with effective supervision for high maturity inmates". Since the sample of offenders was somewhat selected and the criterion of "success" was restoration to military duty (after an unspecified follow-up period), and since Base Expectancy data were not available, it is a little difficult to see how this claim can be supported; the overall "success rate" for high and low maturity subjects in the study was 62 per cent, whereas the overall rate of restoration to duty at Camp Elliott was said to be "between 55 and 60 per cent".

It was also found that among the inmates in the Living Group predicted to be most effective, there was a significant difference between the success rates of high maturity and low maturity subjects. There was a similar, though less marked, difference among those in the Group predicted "next best"; but there was no difference in the success rates of the two types of offenders among those given the predicted "least effective" supervision. Commenting on these results, in a later paper, M. Q. Grant has written that:

"Not only were the treatment methods of some supervisory teams (psychodynamic oriented) effective in increasing the success rates of some kinds of delinquents (high maturity), but also they were markedly detrimental to the success chances of other kinds of delinquents (low maturity). Furthermore, the custody-oriented supervisory team had the reserve effect on high- and low-maturity subjects"<sup>40</sup> (p. 11).

Many other writers have echoed this conclusion; but it is, in fact a non sequitur, and is not supported by the Grants' data. What the results actually showed was that for the predicted "best" treatment, offenders, restoration to duty depended on their maturity levels, whereas for that predicted "least effective", this was not so. A parallel conclusion from medical research might be that in a group of patients given penicillin, an individual's chances of improvement were greater if he suffered from illness A than from illness B; whereas in a group of patients given aspirin this was not so. It would be unwise to conclude from these premises that penicillin was good for illness A but bad for illness B; and still more rash to conclude that aspirin was good for illness B, but bad for illness A. In the same way, we cannot conclude, from the Grants' findings just quoted, that any of the three Living Groups was "better" or "worse" for either type of offender. In fact high-maturity subjects did slightly worse after the predicted "least effective" supervision, and low-maturity subjects did slightly less well after the predicted "best" supervision. But in neither case were the results statistically significant; as the Grants themselves concluded, "...no significant differences were found which could be attributed to predicted supervisory effectiveness"<sup>40</sup> (p. 134).

Two other studies have produced results suggestive of full interaction between type of treatment and type of offender, though in neither case is the evidence very strong. Stuart Adams, in a study of the Pilot Intensive Counselling Project reported in 1961<sup>41</sup>, compared the parole performance of 200 juvenile offenders given intensive individual counselling in a Californian Youth Authority institution, with that of 200 controls in the same institution. Each group was rated for "amenability" to this type of treatment, and consisted of 100 "amenable" and 100 "non-amenable" subjects; the former group was described as typically "bright, verbal and anxious", as well as being "aware of problems", insightful and desirous of treatment. It was found that there was no relationship between amenability to treatment (as rated) and post-institutional adjustment, since the after-conduct of the "amenable" and "non-amenable" controls was virtually the same. But the treated "amenable" group did markedly better than the "amenable" controls, by several criteria, and were significantly less often returned to custody after discharge. The treated "non-amenable" group by contrast, did slightly worse than the "non-amenable" controls. Though several factors suggest that the latter difference may have been a real one, it was not statistically significant; and in a later study Guttman<sup>42</sup> found "amenability" unrelated to outcome of psychiatric treatment.

Finally, Reiss *et al.* have published an interim report<sup>26</sup> on the effectiveness of Pre-Release Guidance Centres (PRGC) for young offenders in the US Federal prison system, in which the after-conduct of 122 offenders released from the Centres is compared with that of two groups of offenders discharged from Federal Youth Correction Act institutions direct to the community. The PRGC group and the two "control" groups, were each classified by Base Expectancy scores into four risk groups. While the results of this study to date are somewhat difficult to interpret, they suggest that the Centres may have been effective for the relatively high-risk offenders, but detrimental for the better prospects.

2. *Though two studies to date have found types of treatment to have differential outcomes with different types of offenders, an equal number have had negative results.* The first of these projects was carried out by Jesness<sup>43</sup> at Fricot Ranch, a Californian Youth Authority institution for 8 to 14-year old delinquents. The project was intended to test the hypothesis that residence in a 20-boy lodge in the institution would increase the impact of staff-inmate interaction and peer-group influence, and thus provide more effective socialisation than residence in the normal 50-boy units. The outlook and training of the staffs of the two types of lodge were the same, and the size of the living unit was the only variable in the research

design. Boys were randomly assigned to the experimental 20-boy lodge, and their responses to treatment compared with that of boys in the larger units, by means of staff ratings, interviews and a variety of personality tests, as well as a follow-up study of parole performance. After fifteen months at risk, parole revocations for boys from the smaller experimental lodge were significantly lower, overall, than those of the controls. However, this difference tended to decrease the longer the follow-up was continued, and at three years it was no longer significant; moreover, almost 80 per cent of each group had been returned to the reformatory by the end of three years.

Jessness developed an offender typology based on 103 items drawn from test scores, behavioural ratings, and interview and social background data concerning the boys in the study. Canonical factor analysis of this data produced fifteen factors; an inverse analysis then classified the boys into eight groups, each representing a more or less distinct delinquent type. For example, the first of the eight types — described as “socialised, conforming thieves” — were rated high on conformity, school interest, and responsibility; they were of relatively high social status, were generally better-than-average on psychological tests, were older than average, less often came from “problem families”, always had delinquent companions and had long police records; they behaved well in the institution and generally stayed there only a short time. Type 4 — described as “immature aggressive” were generally the opposite of Type 1; and similar sets of criteria distinguished the other six types (“immature-passive”, “neurotic-anxious”, “cultural delinquent”, “manipulator”, “neurotic acting-out” and “neurotic-depressed”). Though the numbers involved were small, the fifteen-month follow-up showed that boys of the three neurotic types did markedly better in the smaller experimental unit; only 14 per cent had had their parole revoked, compared with 51 per cent of the neurotic boys housed in the normal 50-boy lodge. There were no significant differences between the revocation rates of treatment and control groups of the other five types. Unfortunately, however, three-year follow-up data are not given for any of the eight delinquent types; and it may be that the difference in parole outcome was not maintained, even for boys of the three neurotic types.

Undoubtedly the most elaborate attempt to date to relate types of treatment to types of offenders is the Community Treatment Project, Phase One of which has been in progress, under the direction of Marguerite Q. Warren, since 1961. The subjects in this research project are delinquents committed for the first time to California Youth Authority institutions by the juvenile courts of two cities (Sacramento and Stockton). These are randomly allocated to an

Experimental group, which receives immediate parole with intensive supervision in the community, and a control group which undergoes the normal institutional treatment. In addition to comparing treatment in the community with treatment in an institution, the programme is designed to discover and develop the most appropriate treatment methods for each of the different types of offender in the experimental group. (For general descriptions of the project, see 82, 83, 84, and for a discussion of one of the specific treatment strategies see 63.)

The typology used in this project is based on the “interpersonal maturity” levels proposed by Sullivan, Grant and Grant<sup>77</sup>, with delinquents of each level of maturity then being further classified into sub-types said to represent their “typical mode of interaction with the world”<sup>82</sup> (p. 40). This low-maturity subjects (I-level 2) are further divided into “unsocialised aggressive” and “unsocialised passive” sub-types; middle maturity subjects (I-level 3) into “conformist, immature”, “conformist cultural” and “manipulator” sub-types; and high-maturity subjects (I-level 4) into “neurotic acting-out”, “neurotic-anxious”, “cultural identifier” and “situational emotional response” groups. Performance of the Experimental and Control subjects in each of these nine sub-types is being compared by means of extensive pre- and post-testing, as well as by parole follow-up data.

The latest published results of this research<sup>83</sup> show that the experimental group as a whole has had a much lower parole failure rate than the control group; of those at risk for 15 months prior to 31 March 1966, 28 per cent of the Experimentals had had their parole revoked, against 52 per cent of the Controls. A similar difference is present for the smaller numbers at risk for 24 months: 38 per cent of the Experimentals had failed by this time, against 61 per cent of the Controls. The results by sub-type, for the cohort at risk for fifteen months, are shown in the following table:

I-level and sub-type	Experimentals		Controls	
	No.	% failed	No.	% failed
I-2 Unsocalised aggressive	0	—	3	66.7
Unsocalised passive	11	18.2	11	54.5
I-3 Conformist, immature	22	18.2	26	57.7
Conformist, cultural	13	15.4	24	45.8
Manipulator	25	32.0	29	48.3
I-4 Neurotic, acting-out	22	22.7	31	71.0
I-4 Neurotic-anxious	29	41.4	29	48.3
Situational emotional res.	5	20.0	6	16.7
Cultural identifier	7	57.1	9	22.2
TOTALS	134	28.4	168	51.8



It will be seen that there are several differences in the relative performance of the Experimental and Control subjects, in the eight sub-types which can be compared (there being no "unsocialised aggressive" delinquents in the Experimental group). In two of the sub-types — "conformist, immature" and "neurotic-anxious" — the differences between Experimental and Control groups are statistically significant; and they approach significance for two others ("conformist, cultural" and "unsocialised passive"). Three of these four differences also appear among the group at risk for 24 months. Thus the programme to date has clearly had differential outcomes, being markedly successful for some sub-types but not for others; though there is no sub-type in which the Experimental group has done significantly worse than its Controls.

These data are subject to an important reservation, however, concerning the sub-type classification. A number of pencil-and-paper tests (to be discussed in a later section of this paper) have been developed for classifying offenders according to interpersonal maturity. These tests can apparently distinguish high-maturity subjects (I-levels 4 and 5) from low-maturity ones (I-levels 2 and 3). But it appears that no such test is now available for classification according to the sub-types within each I-level. In the Community Treatment Project this has so far been done by ratings based on interviews; and while Warren and her associates report high inter-rater reliability no other researchers have yet replicated this. Moreover, the validity of the sub-type classification is possibly suspect: in some cases an offender's maturity level has changed during treatment (from I-level 3 to 4, or 2 to 3), and he has not then fitted into any of the sub-types of the higher maturity level<sup>24</sup>. It may thus be that the only division of offenders which can reliably be made in practice (as opposed to the present experimental conditions) will be a broad, dichotomous classification into high-maturity and low-maturity subjects. If so, the differential outcomes found for the sub-types will disappear completely: for there are no statistically significant differences when the failure rates of high-maturity and low-maturity subjects are compared, in either Experimental or Control groups, at either 15 or 54 months at risk\*. In other words, unless the sub-type classification can be reliably applied in practice, the only useful difference found by the Community Treatment Project will have been an overall difference in outcome between the Experimental and Control groups, and nothing more. Even this overall difference may be a little doubtful; since there were apparently marked differences in the strictness of the parole supervision of the Experimental and Control groups. (See, e.g. 82, 83.)

Against the somewhat tentative results of these two studies must be set those of two other researchers, who were unable to

demonstrate any differences in treatment outcome for different types of offender. Havel<sup>25</sup> classified parolees in the Special Intensive Parole Unit study (SIPIU), into high-maturity and low-maturity groups (again following Sullivan, Grant and Grant<sup>27</sup>). Parole officers were also classified, according to whether they adopted a primarily "external" (situational) approach in dealing with the parolee, or an "internal" (individual) one. It was hypothesised that low-maturity offenders would do better under "external" supervision, and that the high-maturity parolees would do better under "internal" supervision. But neither hypothesis was confirmed; there were no significant differences in parole violation rates for either group, in large, medium or small case-loads. ("Nor", according to Havel, "can much comfort be taken by looking for trends".)

Finally, Tagaki<sup>28</sup> classified a group of offenders according to the typology developed by Schrag<sup>22</sup>, based on social roles in the prison. There were no statistically significant differences in parole violation rates among the four groups, after eight months and 20 months at risk; and the Schrag classification did not improve on the predictions of parole violation made by using the California Base Expectancy tables. An attitude test was given to the parolees before release from the institution, and after eight months on parole; while some degree of attitude change was found, it was not related to parole violation, or to any prisoner type.

Mention should also be made here of two projects, one in England and one in California, in which offenders have been classified according to risk group by means of prediction tables.

\* The figures (computed from 83, pp. 51, 54) are as follows:

15-month Cohort				
	Experimentals		Controls	
	No.	% failure	No.	% failure
High-maturity (I-level 4)	63	35.0	93	51.5
Low-maturity (I-levels 2 & 3)	71	22.5	75	52.0
TOTAL	134	28.4	168	51.8
$X^2 = 2.52, 1 \text{ df. } 20 > p > 10 \quad X^2 = .003, 1 \text{ df. } p = .95$				
24-month Cohort				
	Experimentals		Controls	
	No.	% failure	No.	% failure
High-maturity (I-level 4)	36	38.9	64	56.3
Low-maturity (I-levels 2 & 3)	50	38.0	68	64.7
TOTAL	86	38.4	132	60.5
$X^2 = .007, 1 \text{ df. } 95 > p > 90 \quad X^2 = 0.99, 1 \text{ df. } 50 > p > 30$				

Beverley<sup>11</sup> has carried out two comparative studies of the parole performance of young offenders released from California institutions. Both of these studies showed several significant differences between the expected and actual parole violation rates of those released from different institutions. These differences might be taken to show that some of the institutions were more effective than others. On the other hand, they may simply show that the offenders discharged from the different institutions were not really comparable risks, despite having similar Base Expectancy scores (or expected violation rates). Beverley, in fact, is of this opinion: he gives several reasons for thinking that the differences he found were due to uncontrolled selection factors related to outcome, rather than to differences in treatment effectiveness of the institutions.

A similar study carried out by Cockett<sup>17</sup> at seven English borstal institutions produced slightly stronger evidence of differential treatment effectiveness. Expected reconviction rates were calculated for 110 boys discharged from each institution, using the Mannheim-Wilkins prediction tables<sup>18</sup>; these were then compared with the actual reconviction rates of the 770 boys. It was found that there were no statistically significant differences between the overall reconviction rates and expected reconviction rates, for any of the seven institutions. But when each risk group on the Mannheim-Wilkins table was considered separately, significant differences did emerge; the data suggested that some institutions were more successful than others in training boys in the higher risk-groups (in particular, those in grade C on the Mannheim-Wilkins table). But as Cockett points out (and as earlier studies by Gibbens<sup>20</sup> and Hood<sup>18</sup> have also shown), the risk groups identified by the Mannheim-Wilkins table are not homogeneous; and since factors relevant to outcome may well be taken into account in borstal allocation, the apparent differences in the institutions' effectiveness with grade C cases may be misleading.

Both of these studies, then, are compatible with the view that there are differences in the effectiveness of training programmes in different institutions, for some offenders but not for others. But further research needs to be done before these apparent differences can be accepted as real. The relation between offender classification by means of prediction tables (especially those making use of multiple regression, like the Mannheim-Wilkins tables or the California Base Expectancy tables) and other offender typologies, is further considered below.

## B. OTHER RESEARCH PROJECTS, FOR WHICH NO RESULTS ARE YET AVAILABLE

Phase One of the Community Treatment Project is still in progress at the time of this writing, and further results will presumably be forthcoming in the future. In one important respect, however, the research design of this study has been changed. Originally, the project involved a comparison between an Experimental group of offenders given treatment in the community according to a programme specifically designed for their maturity levels and subtypes, and a Control group which received the standard California Youth Authority institutional programmes. But since 1966, treatment methods based on the theory of interpersonal maturity have been introduced in at least three Youth Authority institutions. The result, as Warren points out<sup>23</sup> (p. 25), is that the Community Treatment Project "no longer has Control subjects in the sense originally described". Thus, the "Experimental-Control comparison no longer reflects differences in theoretical orientation of treatment programmes. The comparison will continue to reflect differences between intensive treatment in the community and intensive treatment in an institutional setting".

Further evidence of treatment-offender interaction effects may appear from Phase Two of the Community Treatment Project, which was begun in San Francisco in the autumn of 1965. As described by Warren<sup>23</sup>, this study involves a comparison between the differential treatment model used in Phase One (described above), and a treatment unit modelled on that used by L.T. Empey in the Provo (Utah) experiment<sup>20</sup>. (In the Provo research, offenders placed on probation were required to attend a non-residential centre at which they were subject to a form of counselling known as "guided group interaction". An important part of this research design — which was based on a sociological theory of delinquency — was the use of delinquent peer group pressures on the individual offender). It was originally intended that subjects should be randomly assigned to the two types of community treatment, and to the traditional Youth Authority institutional programme; though in fact entirely random allocation has not been possible in practice. This project should make possible a comparison of two very different treatment programmes; it will also permit further testing of the "interpersonal-maturity plus delinquent subtype" classification used in Phase One of the Community Treatment Project. (No offender typology was used in the original Provo project.) Unfortunately, certain practical difficulties have arisen in the early stages of this research; both treatment units have (at latest report<sup>23</sup>) a very high proportion of high-maturity subjects, and a shortage of low-maturity ones.

A number of research projects making use of offender and/or treatment typologies are now being carried out in England. The largest of these is the study of probation being made by the Home Office Research unit. This project, part of a larger study of probation and related topics begun in 1961, will investigate differences in outcome among different types of male probationers aged 17-21, under different types of supervision. The hypothesis to be tested is that there is no particular form of supervision which is most suitable for all probationers\*. The treatment and offender typologies used are derived from social psychological theory, and attempt to take into account both personal and social factors in the probation treatment relationship. Eight types of treatment are distinguished, according to the emphasis placed on individual or situational approaches to the offender, and the degree of "support" and "control" used. Thus, for example, "Individual Control" supervision involves low home visiting, low support and high control; "Situational Support" involves high home visiting, high support and low control<sup>24</sup>. The main offender typology used attempts to distinguish probationers by reference to the main "areas of difficulty" in their lives, taking into account both personal and environmental factors; eleven types in all have been identified, on the basis of terms frequently used by probation officers in assessing their clients<sup>7</sup>. In addition, a prediction table will be constructed and expected reconviction rates derived for each type of offender identified. The usual geographical allocation of offenders to probation officers is being followed in most cases; but in one area a matching experiment is being carried out, in which an attempt is being made to allocate probationers to those officers who are considered most capable of providing the type of treatment which the offender seems to need.

A pilot study, making use of the treatment typology (but not the offender typology) has already been carried out on a sample of 602 offenders in the Middlesex probation area<sup>24</sup>. The results of this pilot study, though of course only provisional, are nonetheless suggestive. The Middlesex probation officers generally agreed about the high importance of providing psychological support for their clients. But they tended to disagree about many other aspects of probation treatment, including the importance of discipline, work and peer groups as treatment settings, use of other social services and treatment within the family. (Moreover, over 40 per cent of those surveyed attached relatively low importance to "the protection of society" as an objective of probation). It was also found

\* It should be noted that this is a different, and especially weaker, hypothesis than the one set out at the beginning of this paper; it is entailed by, but does not entail, the hypothesis that there is one type of treatment which is most suitable for every type of offender.

that relatively high success rates were associated with those types of treatment involving a low degree of "control" (as opposed to "support"). The highest success rates were found among probationers given "nominal" supervision, who accounted for exactly half of the total sample. It may of course be that these offenders were mostly good risks who did not need much supervision.

### *The value and limitations of research to date*

It can be seen that the results of research to date are both meagre and vague; and they obviously furnish no basis whatever for administrative or policy decisions of any kind. So far, not even the nature of our ignorance about interaction effects has been revealed, by the handful of studies just reviewed.

In particular, no typology of offenders or treatments has yet been shown to be either valid or reliable; a fortiori no definite relationships have yet been established between any type of treatment and any type of offender. The offender typology most fully explored so far is that based on the theory of interpersonal maturity<sup>7</sup>, which has figured in three projects apart from the Camp Elliott research of Grant and Grant. It is interesting to note that the essentially descriptive typology derived by Jesness in the Fricot Ranch study<sup>50</sup> is in many respects similar to the I-level and sub-type classification though of course Jesness's typology has not yet been independently validated. But the relations between interpersonal maturity and delinquency are not yet clear. According to Grant and Grant, low-maturity subjects, though not invariably delinquent, are much more likely to become involved in delinquency than high-maturity ones; and, as we have seen, the high-maturity offenders in the Camp Elliott study had the highest overall "success rate". In the Community Treatment Project, however, the high-maturity delinquents in both the Experimental and Control Groups have been somewhat less successful in terms of subsequent parole revocation, though as already noted the differences between I-levels are not statistically significant for either group. Commenting on an earlier part of Havel's SIPU research<sup>40</sup>, Glaser has suggested that special treatment services succeed in reducing failure rates appreciably only for "middle risk" cases, having little impact on the best and worst risks<sup>52</sup> (p. 6). This does seem intuitively likely; but it is difficult to identify any such trend in the studies just discussed, since — apart from Cockett's study of Borstal, on which only limited weight can be put — the probability of reconviction for the different types of offenders in question are unknown. Most projects have concerned institutionalised delinquents, who may be

presumed to have typically more serious criminal records (and thus a higher probability of recidivism) than delinquents in general.

The research in this field to date has certain other limitations. About half of the projects reviewed above relate to experiments in treatment, and attempt to develop and/or to test ways of dealing with offenders which are not already in use. Such experiments are of course of great potential value, in that they lead to the development of more effective training methods. But their immediate utility is limited; since even if they do show a new form of treatment to be especially effective with a certain type of offender, it may not be feasible to provide that treatment in practice, on more than a limited scale. It may also be that the type of offender shown to respond to a certain form of treatment is not very common, or cannot usually be given that type of treatment (e.g. because typically his criminal record and type of offence are not serious enough to warrant a custodial sentence). Seen from this perspective, the Home Office probation study seems especially promising, since it is attempting to investigate an existing (and well-established) form of treatment now given to about 18 per cent of all persons convicted of indictable offences in England, and to about 16 per cent of the 17-21 age group with which the research is concerned. (On the other hand, as is well-known, probation is often used for first offenders and "situational" or "accidental" delinquents, many of whom would probably not commit further offences even if they were simply discharged without supervision of any kind. For these offenders the potential "payoff" from research on offender-treatment interaction is probably relatively low.)

In this connection, it may be noted that no research has yet been carried out on the relations between types of offenders and alternative types of sentences (for example imprisonment versus probation, or short-term versus long-term institutional sentences). Of course, it may well be that even if interaction effects were found at this level, full use could not be made of them in practice by the courts. The prevention of recidivism is not — and never can be — the only object of sentencing policy; even if we leave aside the bogey of "retribution", the choice of sentence for any offender is necessarily limited by many considerations, of which the most obvious is the relative cost of the penal measures available. Nonetheless, the courts do have some freedom of choice in practice. If, for example, it could be shown that offenders of type X did well on probation but worse than expected when fined, and that the reverse was true for offenders of type Y, it might be possible for the courts to apply this knowledge, since the two types of non-custodial measures must often be feasible alternatives. Since the total number of offenders dealt with by the courts is much greater than

the numbers placed on probation or sent to prison, the potential "payoff" of research on interaction at the sentencing stage is probably higher than that of research on different types of treatment within a single form of sentence. Of course, there are many methodological problems encountered in typological research at the sentencing level, the most serious of these being the inadequacy of records and the consequent difficulty of obtaining sufficient information about the offenders concerned. But, on balance, these difficulties may be no greater than those encountered in typological research at later stages of the penal process.

Despite all of these limitations, and the generally negative results obtained, the research done to date on interaction effects has certainly not been completely useless. These few studies are but a starting-point; and they have brought to light many of the difficulties inherent in typological research, which later researchers may be able to avoid. It is important not to try to evaluate any type of penal treatment too soon after it has begun, and before it has worked out all of the "bugs" to which any innovation is prone; negative results, which may discourage further endeavour, may simply be due to the fact that the treatment in question has not yet really been put into practice. The same point may be made with regard to attempts to evaluate research which attempts to evaluate treatment. After all, even negative results from typological research can be of some practical use — however disappointing they may be to idealists (if there are any left) in the fields of correctional administration and research.

### *The explanation of interaction effects*

For the purpose of allocating types of offenders to types of treatment, it is enough to be able to identify interaction effects between the two. If it is known, for example, that probation officer A tends to be most effective with offenders of type X, or that institutional regime B tends to be detrimental to offenders of type Y, then those offenders can be dealt with accordingly, without any need to consider why the differences in response to treatment occur. Many instances of this kind of "empiricism" can be seen in connection with other types of treatment. In psychiatry, for example, the distinctions between "process" and "reactive" schizophrenia, and between endogenous and reactive depression, were both made initially on a purely descriptive (symptomatological) basis; differences in response to certain types of therapy have been found in each case, though in neither case, so far as I know, is the etiological difference between the two types of illness satisfactorily understood. (See, respectively, 14 and 54; and 13 and 44.)

But from a scientific point of view, to stop at identifying relationships between types of penal treatment and types of offender is to leave the job only half-finished. Nor is this just a matter of scientific curiosity: especially if it is hoped to develop new types of treatment which will be useful for particular types of offender, some attempt must be made to explain any interaction effects which are found. Why should intensive individual counselling, for example, be effective in reducing the recidivism of one type of offender but detrimental to the after-conduct of another type?\*. Why should one type of inmate become "prisonised" when another does not? Why, of two offenders who apparently become equally "prisonised", should one return to crime after discharge from prison, but the other not? To answer these and similar questions might well lead, in time, to the development of new, type-specific treatment strategies; eventually, it might even bring us closer to that will-o'-the-wisp, a coherent and empirically verified theory of treatment.

In our present state of ignorance, it is clearly premature to think about explanations and treatment theories of this kind; a fish must be caught before it can be cooked, and there will be no scope for explaining treatment-offender interactions until some have been shown to exist. But the research methods which might be used to test and develop such explanations seem worth considering briefly, if only because they offer a possible alternative to follow-up studies as a strategy for establishing the existence of interaction effects. A relatively small-scale study of group counselling, for example, might produce reliable evidence of differences in response to treatment (e.g. in attitude change) among different types of offenders; these might then be related to treatment variables such as the size of the group, or the personality of the group leader or other group members, the content of the discussions and the degrees of participation of each type of offender. A certain amount of information of this kind is already available, from small-group research in other contexts (see, e.g. 6, 8, 25; and cf. 91); a comparison of this data with that obtained for offenders could help to illuminate the specific contribution (if any) which group counselling can make to the modification of anti-social attitudes, improved staff-inmate relations etc.

Eventually, of course, it would be necessary to show that any observed differences in immediate response to treatment were related to long-term differences behaviour. It may well be, for example, that the high participation of a certain type of offender in group sessions in prison has no effect whatever on the recidivism of that type of offender. Moreover, the methodological and practical dif-

\* Assuming of course that this is so. Adams (1, pp. 41-43) does briefly discuss this problem.

ficulties of small-scale clinical and experimental research of this kind may be formidable, especially in penal institutions; and it must be said that the majority of "clinical" studies of the treatment of offenders to date are not very encouraging from a methodological point of view. Nonetheless, there may be some advantages in beginning to look for treatment-offender interactions in this way, rather than (or in addition to) carrying out comparative follow-up studies. In particular, it is quicker — and cheaper — to discard false hypotheses and invalid typologies through research of this kind, than to wait for them to be falsified (or, worse, spuriously verified) by a two-year follow-up study.

At a "tactical" level, the same three research designs are possible in this field as in after-conduct studies: either one type of treatment can be studied in relation to two or more types of offenders, or several types of treatment can be studied with one type of offender or, a number of both treatment- and offender-types can be compared. Some measure of treatment is needed; this can be difficult in the case of complex treatments like imprisonment, through the objective ratings for "programme participation" in prisons, devised by Suziedelis<sup>18</sup>, indicates one way in which this problem may be overcome. In addition, some way to measure the impact of treatment on the offender is needed. The most usual device for this purpose — apart from subjective clinical impressions — is of course some variety of attitude or personality test, administered before and after treatment. Most of the research projects described in the preceding section of this paper have, in fact, made use of such tests; in particular, both Experimental and Control subjects in the Community Treatment Programme and the Fricot Ranch study were extensively pre- and post-tested. The limitations of such instruments as measures of the effectiveness of treatment, i.e. as supplementary criteria of "success", are considered in a later section. For the moment, we are concerned with their use in explanatory research, and with the study of the process of treatment as this affects different types of offenders.

So far, it appears that even less research making use of offenders and/or treatment typologies has been done on this subject than on the effectiveness of treatment. Perhaps the closest approach to date is to be found in recent American studies of the impact of the social system of the prison on the individual inmate. Clemmer, in his book *The Prison Community*<sup>19</sup> first described the process by which prisoners assimilate the norms and values of the inmate code requiring loyalty to other inmates, opposition to prison staff and (in consequence) an increased commitment to anti-social or criminal values. Clemmer recognised that this process — which he called "prisonisation" — was not undergone to the same degree

by all inmates; it depended on such things as length of sentence, primary group affiliations within the prison, and the continuation of "outside" contacts. But he seems to have thought that all prisoners became "prisonised" to some degree, and that the process was a simple linear one, increasing (*ceteris paribus*) directly in proportion to the time the inmate spent in prison. This picture has since been shown to be too simple. Wheeler<sup>87</sup> found that prisoners' conformity to staff role-expectations tended to vary according to a U-shaped curve over the period of their sentences; they began by adhering (relatively) to conventional values, conformed least to conventional standards during the middle phase of their institutional careers, but then seemed to shed the prison culture and become re-orientated to conventional values as their release from prison became nearer. Similar results were reported by Glaser and Stratton<sup>88 89</sup>; and Garrity<sup>27</sup> found that there was no consistent relation between length of sentence, prior penal commitments and other possible indices of "prisonisation", and subsequent parole violation rates.

All of these researchers stressed that "prisonisation", however exactly it may take place, is unlikely to occur in a uniform fashion for all offenders, in all penal institutions; and more recently both offender and institutional typologies have been used in the study of this process. Street, Vinter and Perrow<sup>70</sup> distinguished three types of treatment organisations, which they classified by reference to the official goals of "obedience conformity", "re-education/development", and "treatment"; they produced evidence (chiefly from attitude tests) to illustrate the differences which each type of organisation apparently had on the value-systems of inmates. A similar study by Grusky<sup>41</sup> found significant differences in inmate attitudes toward the institution, staff and treatment programmes, in treatment-oriented as opposed to control-oriented prisons. Still more recently, a study by Berk<sup>9</sup> has corroborated these findings, and also partly confirmed Wheeler's finding that inmates' orientation toward conventional and criminal values varied during their sentences in a U-shaped curve, despite a general shift away from the conventional values throughout the sentence. Berk also found evidence of differences in the degree of informal organisation and the character of leadership among the inmates, which reflected the differences in emphasis on treatment versus control at three different prisons.

An offender typology derived from social roles in prison, as described by Schrag<sup>71 72 73</sup> has also been used in research on "prisonisation". Schrag's classification distinguishes between pre-social, anti-social, pseudo-social and anti-social offenders (in prison argot, "square Johns", "right guys", "con politicians", and "outlaws"; a similar typology has been propounded by Sykes<sup>70</sup>).

Garabedian<sup>28</sup> has reported consistent differences between inmates of these four types, in institutional adjustment, reported social contacts with other prisoners, and conformity to staff norms; and Garrity<sup>27</sup> found variations in parole violation rates for the four types, consistent with their hypothesised differential responses to "prisonisation". Still more recently, Welford<sup>86</sup> has replicated Wheeler's attempt to measure "prisonisation" by means of a questionnaire indicating adherence to staff and/or inmate values, and has related the results to the "pro-social" and "anti-social" type categories developed by Schrag. Welford's results tend to confirm Wheeler's finding of a U-shaped pattern of attitude change during sentence; but he also found significant differences between the "prisonisation" of inmates of the pro-social and anti-social types.

These studies are, of course, only a beginning; each is open to certain methodological criticisms, and their results are suggestive rather than definitive\*. The concept of "prisonisation" is still not fully understood; and as we have seen, a follow-up study by Tagaki<sup>80</sup> found no differences in parole violation rates among offenders classified by Schrag's social types, once Base Expectancy scores were taken into account. Nonetheless, these studies illustrate one way in which further typological studies of the process of treatment might be carried out, as a supplement to typological studies of offenders' after-conduct.

### *Methodological problems concerning typologies*

#### A. OFFENDER TYPOLOGIES

An enormous number of offender typologies have been described in the criminological literature of the past twenty-five years. No attempt will be made to give an exhaustive bibliography of these here; useful lists of references have been provided by a number of writers (see, in particular, Grant<sup>40</sup>, Gibbens<sup>30</sup> and Kinch<sup>33</sup> and there seems little point in providing another one. Since virtually no empirical research of any kind has been done on most of these suggested typologies, there is little point in trying to assess their relative utility. Instead, I shall briefly suggest some of the requirements of a good typology of offenders, and indicate the main

\* The brief summary given here does not pretend to do justice either to these authors' work, or to possible criticisms of it; my purpose is illustrative only. It is important to note, however, that most of these researchers have been at least as interested in the sociology of institutions as in the process of trying to reform offenders in prisons. No doubt their work would have followed slightly different lines if they had been primarily interested in explaining the mechanics of treatment or in its evaluation.



approaches to typological formulation and the problems surrounding each.

What is meant by a "type" of offender in this context, and why should a set, or a system of these "types" be thought to be useful? *Prima facie*, if we divide any set of things of the same kind into sub-sets, by reference to one or more of the attributes of those things, then each one of the sub-classes exemplifies a "type" of the thing in question; and almost any attribute will serve for this purpose, though of course not all will be equally useful. But to sort things into "types" is generally thought of, it seems, as something more than arranging them with respect to a single variable (i.e. along a single continuum, such as a scale of length or intelligence). Indeed, if only a single variable is used to classify offenders into types, there will probably be some loss of information, since the cutting-points used are almost bound to be somewhat arbitrary, and may not be the most useful. (It may be, for example, that the notion of "interpersonal maturity" developed by Sullivan, Grant and Grant<sup>77</sup> would be better treated as a continuous variable rather than in seven supposedly discrete stages.) A typology is generally only useful, then, if a number of attributes or qualities or traits exemplified by the population is to be taken into account by a single classification; and a typology is strictly speaking only necessary if there is, or may be, some interaction between these attributes, so that they are not independent or do not "add together" in a straightforward linear way.

At the other extreme, to classify offenders into types is different from — though perfectly compatible with — a "diagnostic" approach to the special problems peculiar to an individual case. And a typology is only useful in dealing with offenders if (1) it is based on relatively stable or persistent features of the population in question, and (2) the number of possible courses of action contemplated (e.g. allocation to types of treatment) is fairly small. Thus, for example, it might be useful to classify probationers into a dozen types, and allocate those of Type X to a certain sort of probation officer; the probation officer may then find it necessary to vary his approach to the offender slightly, in the light of the individual's special circumstances. But if he cannot generally follow the same basic treatment policy with offenders of a certain type, or if offenders constantly change types under treatment, then the typology will merely be a waste of time.

Broadly speaking, there seem to be two approaches to the creation of offender typologies; let us call these the "empirical" and the "theoretical" approaches. The first proceeds simply by grouping together individuals according to their most obvious apparently relevant features, so that each group contains members

which are as similar as possible to each other and as different as possible from all other groups. Usually one has some vague idea of which features are, or might be relevant, e.g. when classifying offenders one will consider features with some conceivable relationship to crime. But the choice of these is not dictated by an particular theory, and the primary basis of classification is usually some readily ascertainable first-order facts about the individual, rather than an abstract theoretical variable. Examples of this kind of typology are the "criminal types" identified by Roebuck<sup>68 67 68 69</sup>, Spencer's typology of violent offenders<sup>75</sup>, and the offender typology used in the Home Office Probation study<sup>7</sup>, based on the main "areas of difficulty" in probationers' lives (as seen by probation officers). Often the features used in intuitively-derived classifications of this kind are heterogeneous, and not obviously related to one another; for instance, Roebuck uses race as well as criminal career to define several of his criminal types. It is basically this method of grouping which is reproduced by statistical methods of taxonomy such as "association analysis", applied to offenders by Wilkins and MacNaughton-Smith<sup>85 87</sup>, and the more complicated calculations used by Jesness<sup>80</sup>.

The second ("theoretical") approach, by contrast, starts with a theory from which a relatively specific basis for classification is deduced; descriptive criteria are then found for grouping individuals in accordance with this theoretical scheme. The best (and by far the commonest) examples of this kind of typology, of course, are those derived from psychiatric or psychological theories (e.g. Jenkins<sup>80</sup>, Argyle<sup>3</sup>, Andry<sup>2</sup>); but sociological typologies such as those based on reference groups (e.g. Schrag<sup>72</sup> and Sykes<sup>79</sup>) are of this type too. These approaches are not mutually exclusive; on the contrary, they represent differences of emphasis rather than absolute opposites, and they can to some extent be combined, as the work of Gibbons<sup>80</sup> shows.

What, then, are the main requirements of a good typology of offenders, for this purpose? The ultimate object, of course, is that it should be valid, i.e. that it should separate offenders whose treatment needs are different; it is this validity which we seek to establish by research. Ideally, perhaps, a typology should at least suggest the optimum forms of treatment for each of the offender types it contains; for this reason the second ("theoretical") approach is, on paper, the most useful. But in the present state of criminological knowledge, this requirement can hardly be thought of as an essential; in practice, there is probably little to choose between the two approaches to typology construction, on this point. Gibbons has suggested that a single offender typology might be used in etiological theory as well as for treatment; he remarks that

"it seems but a small jump from the view that the causes of illegal behaviour vary among types of delinquent or criminal careers, to the conclusion that efficacious therapy procedures *similarly* vary with the kind of behaviour to be treated or changed" (30, p. 40, my italics). This seems to me to be a jump of positively gargantuan proportions; unfortunately it is by no means clear that knowledge of the causes of an offender's delinquency is of any use at all in getting him to stop breaking the law.

A second requirement concerns the scope of the typology; all other things being equal, the most useful typology is that which includes the greatest number of offenders. Often, however, typologies seem to achieve this spuriously, by tacking on a "miscellaneous" category unrelated to the other categories or the basis of classification; Schrag's prisoner typology<sup>73</sup>, which included a category of non-conformists ("dings") is perhaps an example. Typologies like those of Roebuck, which group together offenders who generally commit offences of the same type (in legal terms, e.g. robbery or fraud), may be limited from the point of view of scope. Peterson et al<sup>62</sup>, have reported a high proportion of homogeneous offence-career patterns among a group of offenders convicted of violence; but other researchers, including McClintock<sup>55 56</sup> and Robin<sup>64</sup> have found that "specialisation" of this kind is rare among offenders generally, and even Roebuck<sup>60</sup> (p. 175) admits that many offenders' careers are "mixed", and show no pattern of concentration on a single type of offence.

But it is easy to make too much of the requirement that a typology should have a wide scope. It is sometimes suggested that to be useful, a typology should include the majority of all offenders; but this is surely unrealistic. The most that is needed is that the typology should apply to the majority of those offenders for whom the treatment choices in question may be applied; e.g. a typology for use in probation should include the majority of possible probationers. At the outset, however, a good typology should be as rich in types as possible, and should contain at least as many types as there are treatment choices. The initial use of only two or three broad classes will probably mean a loss of both power and information; and any categories which are shown by research to be unrelated to treatment outcome can always be discarded later.

A third, and very important, requirement is that the types should be easily and reliably identified. Here, typologies which are based on "external" data (such as criminal career or work record) obviously score over psychiatric and psychological typologies for both treatment and research purposes, unless some "external" criteria can be found for the psychological categories. Most of the typologies on which research has been done to date make use

of ratings, based either on interviews or on records. This may be feasible in practice, if the numbers of offenders involved are not too large; even so, the use of ratings may severely limit the utility of a typology. Considerable research has been done to establish the reliability of ratings for the I-level and sub-type classification used in the Community Treatment Project; Warren and her associates report inter-rater agreement, on average, in about 85 per cent of the cases<sup>83</sup>. But this result has been achieved by a small and highly experienced team of researchers who have worked together for several years; and according to Conrad it is "prohibitively costly"<sup>18</sup> (p. 199). Moreover, other researchers have not achieved the same level of reliability. Havel<sup>45</sup> found agreement between two raters classifying offenders by level of interpersonal maturity (I-levels 2, 3, or 4) in 93 per cent of 138 cases when the classification was based on interviews; but in only 56 per cent of 566 cases when it was based on case summaries.

Three pencil-and-paper tests have been used in an effort to classify delinquents according to I-level. Gottfredson and Ballard<sup>80</sup>, using scales from the California Personality Inventory<sup>87</sup>, were able to classify correctly about 75 per cent of a sample of 302 offenders, with a "middle group" of 48 cases left unclassified; if these latter cases had been randomly allocated, the total correctly classified would have been about 83 per cent, or five cases out of every six. Beverley<sup>12</sup> has developed two eighteen-item scales, one from the Jesness Inventory and one from the Beverley-Grant Opinion Scale, each of which correctly classified about three quarters of a group of 280 boys (with no "middle group" left unclassified). These are promising results — rather better, in fact, than those usually obtained with psychiatric diagnosis<sup>10 61</sup>. But each of these tests classifies offenders into only two broad groups — high maturity or low maturity; Beverley<sup>12</sup> was unable to distinguish I-level 2 cases from I-level 3, and difficulty on this point was also reported by Havel<sup>45</sup>. The consequences of this for the Community Treatment Project have already been noted (p. 17 above). So far as I am aware no similar tests have been devised for other typologies; and there is little evidence on the reliability of other typologies, though difficulties in rating have been reported by Adams<sup>4</sup> for his classification of "amenability", and by Glaser<sup>36</sup>, (p. 385) in connection with the Schrag role-typology.

\* For some of the sub-types, agreement in as many as 96 per cent of the cases is reported. However, the statistic used to measure this is somewhat misleading; called a "reliability estimate" it is in fact the square root of the percentage of agreement between two raters. Thus, a "reliability estimate" of .85 really indicates agreement in about 72 per cent of the cases rated. In fact, if cases over which the raters disagreed were randomly allocated to the disputed categories, the total percentage of "correct" classifications would, in the long run, be about 85 per cent.

Classification always reflects some purpose, and the "best" classification will depend on the purpose for which the classifying is done. There is thus no reason to suppose that some one typology of offenders will be found to serve all legal, administrative and therapeutic ends. In practice, what is needed in order to accommodate these different objectives is successive classification; thus, for instance, offenders might first be classified as "incorrigible", "corrigible", or "accidental"; the "corrigible" group might then be classified according to optimum training sentence, and further classified before allocation to different institutions. Some writers (e.g. M. Q. Grant<sup>40</sup>) have noted the wide range of psychological, sociological and situational variables represented in existing typologies, and have suggested that a systematic approach might succeed in linking all of these in a single taxonomy. Technically, this is no problem; but it is difficult to see why it should be thought desirable, apart from an *a priori* belief — or a desperate hope — that this "integration" will turn out to be useful for treatment purposes. What is needed instead, surely, is what might be called "basic typological research", which would show the relations between different sorts of typologies, and make it possible to compare them in scope and reliability. Samples of offenders — preferably drawn at random, from those given different types of treatment — could be classified in eight or ten different ways, and the resulting groups compared in order to show empirically the relations between, say, interpersonal maturity and role-career, or self-image, and rated "amenability" to a certain type of treatment. But until clearer operational criteria are provided for many of the typologies in the literature, this kind of research will be impossible.

One important respect in which typologies need to be compared is in relation to overall prediction of recidivism. Classification of offenders into risk groups, by Base Expectancy or other similar tables, is not strictly speaking "typological" classification, since the offenders in each risk group need have nothing in common except membership of that risk group. There is one statistical method which combines prediction of recidivism and descriptive classification; this is the method variously called "predictive attribute analysis" (MacNaughton-Smith<sup>57</sup>) or "configuration tables" (Glaser<sup>58</sup>). This method allows for interaction between factors, and thus overcomes one of the disadvantages of multiple regression (see Hood<sup>49</sup> for a discussion of this point). But little research has been done with this method so far and not much is known about its utility; it may be difficult to use in connection with theoretically-derived typologies.

## B. TREATMENT TYPOLOGIES

In contrast to the plethora of offender typologies reported to date, there has been very little done to develop treatment typologies. Though there have been many suggestions as to the best type of treatment for particular types of offenders, there have been few systematic attempts to classify treatments so as to reveal relevant similarities and differences in their constituent elements. (The discussion by Gibbons<sup>59</sup> is a conspicuous exception.) Moreover, there has been very little research on the varieties of existing forms of treatment. The Home Office probation study<sup>24</sup> and the SIPU project<sup>45</sup> are the only studies to date to investigate this matter in any detail. While Street et al.<sup>60</sup>, Berk<sup>9</sup> and others have made broad distinctions between types of prison according to institutional goals, a satisfactory typology of institutional treatment must obviously take into account many other factors as well, such as size and composition of the inmate population, training of staff and the nature of work, treatment and other elements in the institutional regime. Plainly it is not enough to consider only deliberate variations in treatment; descriptive research is needed which will bring out any differences in the experiences which prisoners may actually undergo, whether these are as a result of treatment policy or other factors. This is likely to be very difficult to do; indeed, according to Folkard the problems of studying treatment in an institutional setting were one reason for choosing probation as the setting for the Home Office research<sup>24</sup>. It may turn out that there is no really useful typology of institutional treatment — in other words, that every institution in a given system (like the borstal system) is so different as to be virtually unique.

Moreover, there is a special problem of "reliability" arising even with deliberate variations in treatment, such as the eight types of probation treatment identified in the Home Office study<sup>27</sup>. Though probation officers may *intend* to rely on "support" or "control" in dealing with certain types of offender, and may have offenders allocated to them on this assumption, they may for various reasons *actually* follow a different type of approach; only continued research can show whether or not this is so. Still further complications may arise when two or more types of treatment are combined — for example, imprisonment and parole. Different variations of the first type of treatment may be combined with different variations of the second, and have different effects on different types of offenders. But since research on this subject is still in its infancy, it will clearly be some time before refinements of this type can be considered.

*Other methodological problems*

Hood<sup>49</sup> has discussed in detail the methodological and practical problems of investigating the effectiveness of treatment; obviously, these same problems surround the investigation of offender-treatment interaction. Apart from difficulties relating to the derivation of typologies, there seem to be few special problems of methodology raised by this type of research. But two issues, not mentioned by Hood, have been revealed by some of the studies reviewed in the earlier sections of this paper.

The first concerns the measurement of outcome of treatment by criteria of "success" other than reconviction or parole violation. In research in California, extensive use has been made of two tests — the California Personality Inventory<sup>57</sup> and the Jesness Inventory<sup>51</sup>, both of which have been administered to delinquents at the beginning and end of the treatment, in order to measure changes in attitudes supposedly relating to delinquency. Both of these tests have been shown reliably to discriminate between delinquents and non-delinquents<sup>58 51</sup>. But so far as the outcome of treatment is concerned, the results to date have not been at all clear. Warren et al have found various differences in pre- and post-test results in the Community Treatment Project<sup>53</sup>; but it is difficult to trace any consistent pattern in these, or to relate them to reconviction. For example, in the "conformist, immature" sub-type (I-level 3), both Experimental and Control subjects showed "improved" attitudes on post-testing with both the CPI and the Jesness Inventory; the Controls' improvement was in fact slightly greater. Yet the reconviction rate of the Controls was significantly higher than that of the Experimentals, and that of the sub-type as a whole was higher than the average failure rate for the project as a whole. Similarly inconclusive results were obtained by Jesness at Fricot Ranch<sup>50</sup>. Further research is now being done in England with the Jesness Inventory, by Mott and Davies<sup>59</sup>; and by Mesness<sup>52</sup> in California. An earlier study by Fisher<sup>23</sup> suggested that this test might be invalid for an English population; but more recent results indicate that it does distinguish between English delinquents and non-delinquents. For the moment, however, it seems that neither the Jesness Inventory nor any other test furnishes an adequate criterion of the outcome of treatment. Still less can they now be used to explain that outcome.

If the object of a follow-up study is to measure the effectiveness of treatment, it is necessary to eliminate from the "failure" group those offenders reconvicted solely because of abnormal social stress

after treatment, or because of other "accidental" factors; and to relate these environmental or situational factors to offender and personality types. Unless the offender is under supervision during the follow-up period (e.g. on parole or probation) there may be insufficient information available about him to make this possible. But an interesting attempt to study the effect of social stresses on probationers is now being made by Davies, as part of the Home Office probation research\*. A simple scoring method has been devised in order to provide an overall measure of those difficulties which might be thought to hamper an individual on probation (for example, having incompatible parents, being of no fixed abode, suffering from ill-health or being unhappily married). A preliminary study has found a highly significant association between this stress score and reconviction within a one-year period.

A final methodological problem revealed by some of the projects discussed earlier, and by other recent studies, is the extent to which research on the effectiveness of treatment may alter the outcome of that treatment. For example, Secker<sup>74</sup> has noted that in three research projects in California institutions those offenders completing the programmes in their early stages had higher success rates than those finishing later. This, he suggests, may be due to a kind of "Hawthorne effect" among staff and inmates: "mere knowledge of membership in a unique experimental programme, particularly during its early stages, engenders more enthusiasm and active participation than would be revealed later on, when its special nature is taken for granted". Similarly, in the Provo experiment, Empey<sup>19</sup> found a sharp increase in the success rate for the Control group, as well as the Experimental group. At a somewhat different level, Warren<sup>53</sup> has noted that while teachers were at first sympathetic to the Community Treatment Programme, they have since tended to become hostile to it (and thus perhaps to offenders involved in it). In addition, she writes that "it has proved impossible to operate the programme without the Experimental or Control status of subjects being known, not only to decision-making personnel in the Youth Authority but also to school systems and to some extent law enforcement agencies". The equivalent of a "double-blind technique" is extremely difficult to use in research on the treatment of offenders; but unless it can be used, the results may be seriously distorted. But perhaps these problems have arisen because the studies just mentioned were studies of experiments in new forms of treatment, rather than studies of existing types.

\* I am indebted to Dr. Martin Davies for this information about his research, and for permission to refer to it in this paper.

### Summary and Conclusions

In conclusion, it seems reasonable to try to assess the potential value of research on the relations between types of treatment and types of offenders. Should it be encouraged? Or does it deserve — in Christie's words<sup>18</sup> — “death by intellectual and economic strangulation”? It may seem Philistine to think of criminological research in terms of “pay-off”. In recent years, as criminology has approached the status of a science, many of those engaged in research have naturally tended to justify their work mainly in terms of its contribution to scientific knowledge, without regard for its utility as an instrument of social policy. This attitude may be appropriate for research on the causes of crime, and for research which can make a wider contribution to sociology, psychology or other disciplines. But it must surely be resisted in the case of research on any aspect of the effectiveness of treatment; if this cannot pay its own way, it should not be done at all.

The temptation to ignore “pay-off” is especially strong in the case of research on interaction effects; for there are strong non-utilitarian attractions to this kind of research. It holds the tantalising promise of explaining away the apparent ineffectiveness of many kinds of treatment; it requires sophisticated techniques and considerable research skill, and thus has a quasi-aesthetic attraction for any research worker. And — dare one say it? — the topic is extremely fashionable just now. For these reasons, I think, its prospects need an especially careful weighing-up.

We have noted that there are inherent limitations to the reduction of recidivism which can be accomplished through taking advantage of interaction effects. There are further practical limitations, since in many cases it would not be possible to take advantage of interaction effects even if these were shown to exist. As was said earlier, the prevention of recidivism is not, and never can be, the only objective of sentencing policy; justice, general prevention, and comparative cost all limit the extent to which any type of offender can be sentenced to the type of treatment most likely to be effective for him. There are similar limits to the optimum allocation of offenders within any single type of treatment; in the case of penal institutions, for example, considerations of security must always play an important part. Moreover, the scope for deliberate variations in treatment is in many cases limited, at present, by the fact that so little “treatment”, in any real sense of that term, exists. In many prisons in England and the United States (and no doubt elsewhere) this is certainly so.

It is also important to note that in the case of treatment which involves groups of offenders, the best overall result may be obtained by allocating some offenders to a type of treatment which is not the most effective for them. The research of Grant and Grant<sup>19</sup> suggested that low-maturity subjects did best, under the predicted “worst” supervision, in groups consisting solely of low-maturity subjects; but that high-maturity offenders did rather better, under the supervision rated “best”, in groups of mixed high- and low-maturity offenders. Thus it may be that even if what I have called “full interaction” is present, the maximum overall success rate will be something less than the theoretical maximum, i.e. the weighted sum of the highest success rates for each type of offender. Put another — and crueller — way, it may be necessary to “write off” some offenders of one type, in order to make greater gains in reforming offenders of another type.

Given these limitations, it would seem reasonable to begin looking for interaction between types of treatment and types of offender (i) where there absolute numbers of offenders involved are greatest, and (ii) where there is the greatest potential scope for allocating offenders to the type of treatment found to be most effective for them. Unfortunately, as so often happens, these two criteria tend to point in opposite directions; the first to the sentencing stage of the penal process, the second to allocation of offenders within specific types of sentence. But a balance between them can surely be struck; and at each level of the penal process there is some scope for research on interaction effects. In the case of research on the comparative effectiveness of different sentences, a typological approach should surely become a standard part of research procedure; and descriptive typologies based on social or sociological factors would seem especially promising. (In each case, of course, the offenders to concentrate on are the “non-incorrigible recidivists”; these can at least be defined by exclusion of the “accidental” offender on the one hand and the hard-core persistent offender on the other.) There is some scope, then, for research of this kind in connection with existing forms of treatment; and if a typology of offenders is shown to be invalid, in the sense of being unrelated to differences in outcome, it can cheerfully be abandoned and other ones tried. Even a negative result of this kind need not be wholly useless to sentencers or penal administrators. There is also scope for small-scale clinical and/or explanatory research related to types of offender, both in penal institutions and with treatment “in the open”. What I have called “basic typological research” is a prerequisite for any real gains in this field, if only because no real progress is likely to be made until the work

of one researcher can be reliably replicated by another. The development of new forms of treatment for specific types of offender may seem the most promising field of all; but this will become a reasonable research subject only if the introduction of new forms of treatment becomes a practical reality. In the nature of things penological, this is unfortunately not likely to happen very often in the near future. But there is surely enough to be getting on with, meantime.

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