



The Effects of Psychotherapy: An Evaluation

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The recommendation of the Committee on Training in Clinical Psychology of the American Psychological Association regarding the training of clinical psychologists in the field of psychotherapy has been criticized by the writer in a series of papers [10, 11, 12]. Of the arguments presented in favor of the policy advocated by the Committee, the most cogent one is perhaps that which refers to the social need for the skills possessed by the psychotherapist. In view of the importance of the issues involved, it seemed worth while to examine the evidence relating to the actual effects of psychotherapy, in an attempt to seek clarification on a point of fact.

Base Line and Unit of Measurement

In the only previous attempt to carry out such an evaluation, Landis has pointed out that "before any sort of measurement can be made, it is necessary to establish a base line and a common unit of measure. The only unit of measure available is the report made by the physician stating that the patient has recovered, is much improved, is improved or unimproved. This unit is probably as satisfactory as any type of human subjective judgment, partaking of both the good and bad points of such judgments" [26, p. 156.] For a unit Landis suggests "that of expressing therapeutic results in terms of the number of patients recovered or improved per 100 cases admitted to the hospital." As an alternative, he suggests "the statement of therapeutic outcome for some given group of patients during some stated interval of time."

Landis realized quite clearly that in order to evaluate the effectiveness of any form of therapy, data from a control group of nontreated patients would be required in order to compare the effects of the therapy with the spontaneous remission rate. In the absence of anything better, he used the amelioration rate in state mental hospitals for patients diagnosed under the heading of "neuroses." As he points out:

There are several objections to the use of the consolidated amelioration rate . . . of the . . . state hospitals . . . as a base rate for spontaneous recovery. The fact that psychoneurotic cases are not usually committed to state hospitals unless in a very bad condition; the relatively small number of voluntary patients in the group; the fact that such patients do get some degree of psychotherapy especially in the reception hospitals; and the probably quite different economic, educational, and social status of the State

Hospital group compared to the patients reported from each of the other hospitals—all argue against the acceptance of [this] figure . . . as a truly satisfactory base line, but in the absence of any other better figure this must serve [26, p. 168].

Actually the various figures quoted by Landis agree very well. The percentage of neurotic patients discharged annually as recovered or improved from New York state hospitals is 70 (for the years 1925–1934); for the United States as a whole it is 68 (for the years 1926 to 1933). The percentage of neurotics discharged as recovered or improved within one year of admission is 66 for the United States (1933) and 68 for New York (1914). The consolidated amelioration rate of New York state hospitals, 1917–1934, is 72 per cent. As this is the figure chosen by Landis, we may accept it in preference to the other very similar ones quoted. By and large, we may thus say that of severe neurotics receiving in the main custodial care, and very little if any psychotherapy, over two-thirds recovered or improved to a considerable extent. "Although this is not, strictly speaking, a basic figure for 'spontaneous' recovery, still any therapeutic method must show an appreciably greater size than this to be seriously considered" [26, p. 160].

Another estimate of the required "base line" is provided by Denker:

Five hundred consecutive disability claims due to psychoneurosis, treated by general practitioners throughout the country, and not by accredited specialists or sanatoria, were reviewed. All types of neurosis were included, and no attempt made to differentiate the neurasthenic, anxiety, compulsive, hysterical, or other states, but the greatest care was taken to eliminate the true psychotic or organic lesions which in the early stages of illness so often simulate neurosis. These cases were taken consecutively from the files of the Equitable Life Assurance Society of the United States, were from all parts of the country, and all had been ill of a neurosis for at least three months before claims were submitted. They, therefore, could be fairly called "severe," since they had been totally disabled for at least a three months' period, and rendered unable to carry on with any "occupation for remuneration or profit" for at least that time [9, p. 2164].

These patients were regularly seen and treated by their own physicians with sedatives, tonics, suggestion, and reassurance, but in no case was any attempt made at anything but this most superficial type of "psychotherapy" which has always been the stock-in-trade of the general practitioner. Repeated statements, every three months or so by their physicians, as well as independent investigations by the insurance company, confirmed the fact that these people actually were not engaged in productive work during the period of their illness. During their disable-

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ment, these cases received disability benefits. As Denker points out, "It is appreciated that this fact of disability income may have actually prolonged the total period of disability and acted as a barrier to incentive for recovery. One would, therefore, not expect the therapeutic results in such a group of cases to be as favorable as in other groups where the economic factor might act as an important spur in helping the sick patient adjust to his neurotic conflict and illness" [9, p. 2165].

The cases were all followed up for at least a five-year period, and often as long as ten years after the period of disability had begun. The criteria of "recovery" used by Denker were as follows: (a) return to work, and ability to carry on well in economic adjustments for at least a five-year period; (b) complaint of no further or very slight difficulties; (c) making of successful social adjustments. Using these criteria, which are very similar to those usually used by psychiatrists, Denker found that 45 per cent of the patients recovered after one year, another 27 per cent after two years, making 72 per cent in all. Another 10 per cent, 5 per cent, and 4 per cent recovered during the third, fourth, and fifth years, respectively, making a total of 90 per cent recoveries after five years.

This sample contrasts in many ways with that used by Landis. The cases on which Denker reports were probably not quite as severe as those summarized by Landis; they were all voluntary, nonhospitalized patients, and came from a much higher socioeconomic stratum. The majority of Denker's patients were clerical workers, executives, teachers, and professional men. In spite of these differences, the recovery figures for the two samples are almost identical. The most suitable figure to choose from those given by Denker is probably that for the two-year recovery rate, as follow-up studies seldom go beyond two years and the higher figures for three-, four-, and five-year follow-up would overestimate the efficiency of this "base line" procedure. Using, therefore, the two-year recovery figure of 72 per cent, we find that Denker's figure agrees exactly with that given by Landis. We may, therefore, conclude with some confidence that our estimate of some two-thirds of severe neurotics showing recovery or considerable improvement without the benefit of systematic psychotherapy is not likely to be very far out.

Effects of Psychotherapy

We may now turn to the effects of psychotherapeutic treatment. The results of nineteen studies reported in the literature, covering over seven thousand cases, and dealing with both psychoanalytic and eclectic types of treatment, are quoted in detail in Table 1. An attempt has been made to report results under the four headings: (a) Cured, or much improved; (b) Improved; (c) Slightly improved; (d) Not improved, died, discontinued treatment, etc. It was usually easy to reduce additional categories given by some writers to these basic four; some writers give only two or three categories, and in those cases it was, of course, impossible to subdivide further, and the figures for combined categories are given.¹ A slight degree of subjectivity inevitably enters into this procedure, but it is doubtful if it has caused much distortion. A somewhat greater degree of subjectivity is probably implied in the writer's judgment as to which disorders and diagnoses should be considered to fall under the heading of "neurosis." Schizophrenic, manic-depressive, and paranoid

states have been excluded; organ neuroses, psychopathic states, and character disturbances have been included. The number of cases where there was genuine doubt is probably too small to make much change in the final figures, regardless of how they are allocated.

A number of studies have been excluded because of such factors as excessive inadequacy of follow-up, partial duplication of cases with others included in our table, failure to indicate type of treatment used, and other reasons which made the results useless from our point of view. Papers thus rejected are those by Thorley & Craske [37], Bennett and Semrad [2], H. I. Harris [19], Hardcastle [17], A. Harris [18], Jacobson and Wright [21], Friess and Nelson [14], Comroe [5], Wenger [38], Orbison [33], Coon and Raymond [6], Denker [8], and Bond and Braceland [3]. Their inclusion would not have altered our conclusions to any considerable degree, although, as Miles *et al.* point out: "When the various studies are compared in terms of thoroughness, careful planning, strictness of criteria and objectivity, there is often an inverse correlation between these factors and the percentage of successful results reported" [31, p. 88].

Certain difficulties have arisen from the inability of some writers to make their column figures agree with their totals, or to calculate percentages accurately. Again, the writer has exercised his judgment as to which figures to accept. In certain cases, writers have given figures of cases where there was a recurrence of the disorder after apparent cure or improvement, without indicating how many patients were affected in these two groups respectively. All recurrences of this kind have been subtracted from the "cured" and "improved" totals, taking half from each. The total number of cases involved in all these adjustments is quite small. Another investigator making all decisions exactly in the opposite direction to the present writer's would hardly alter the final percentage figures by more than 1 or 2 per cent.

We may now turn to the figures as presented. Patients treated by means of psychoanalysis improve to the extent of 44 per cent; patients treated eclectically improve to the extent of 64 per cent; patients treated only custodially or by general practitioners improve to the extent of 72 per cent. There thus appears to be an inverse correlation between recovery and psychotherapy; the more psychotherapy, the smaller the recovery rate. This conclusion requires certain qualifications.

In our tabulation of psychoanalytic results, we have classed those who stopped treatment together with those not improved. This appears to be reasonable; a patient who fails to finish his treatment, and is not improved, is surely a therapeutic failure. The same rule has been followed with the data summarized under "eclectic" treatment, except when the patient who did not finish treatment was definitely classified as "improved" by the therapist. However, in view of the peculiarities of Freudian procedures it may appear to some readers to be more just to class those cases separately, and deal only with the percentage of completed treatments which are successful. Approximately one-third of the psychoanalytic patients listed broke off treat-

¹ In one or two cases where patients who improved or improved slightly were combined by the original author, the total figure has been divided equally between the two categories.

Table 1
Summary of Reports of the Results of Psychotherapy

	N	Cured; much improved	Improved	Slightly improved	Not improved; died; left treatment	% Cured; much improved;
(A) Psychoanalytic						
1. Fenichel [13, pp. 28-40]	484	104	84	99	197	39
2. Kessel & Hyman [24]	34	16	5	4	9	62
3. Jones [22, pp. 12-14]	59	20	8	28	3	47
4. Alexander [1, pp. 30-43]	141	28	42	23	48	50
5. Knight [25]	42	8	20	7	7	67
All cases	760		335		425	44%
(B) Eclectic						
1. Huddleson [20]	200	19	74	80	27	46
2. Matz [30]	775	10	310	310	145	41
3. Maudsley Hospital Report (1931)	1721	288	900		533	69
4. Maudsley Hospital Report (1935)	1711	371	765		575	64
5. Neustatter [32]	46	9	14	8	15	50
6. Luff & Garrod [27]	500	140	135	26	199	55
7. Luff & Garrod [27]	210	38	84	54	34	68
8. Ross [34]	1089	547	306		236	77
9. Yashin [40]	100	29	29		42	58
10. Curran [7]	83		51		32	61
11. Masserman & Carmichael [29]	50	7	20	5	18	54
12. Carmichael & Masserman [4]	77	16	25	14	22	53
13. Schilder [35]	35	11	11	6	7	63
14. Hamilton & Wall [16]	100	32	34	17	17	66
15. Hamilton <i>et al.</i> [15]	100	48	5	17	32	51
16. Landis [26]	119	40	47		32	73
17. Institute Med. Psychol. (quoted Neustatter)	270	58	132	55	25	70
18. Wilder [39]	54	3	24	16	11	50
19. Miles <i>et al.</i> [31]	53	13	18	13	9	58
All cases	7293		4661		2632	64%

ment, so that the percentage of successful treatments of patients who finished their course must be put at approximately 66 per cent. It would appear, then, that when we discount the risk the patient runs of stopping treatment altogether, his chances of improvement under psychoanalysis are approximately equal to his chances of improvement under eclectic treatment, and slightly worse than his chances under a general practitioner or custodial treatment.

Two further points require clarification: (a) Are patients in our "control" groups (Landis and Denker) as seriously ill as those in our "experimental" groups? (b) Are standards of recovery perhaps less stringent in our "control" than in our "experimental" groups? It is difficult to answer these questions definitely, in view of the great divergence of opinion between psychiatrists. From a close scrutiny of the literature it appears that the "control" patients were probably at least as seriously ill as the "experimental" patients, and possibly more so. As regards standards of recovery, those in Denker's study are as stringent as most of those used by psychoanalysts and eclectic psychiatrists,

but those used by the State Hospitals whose figures Landis quotes are very probably more lenient. In the absence of agreed standards of severity of illness, or of extent of recovery, it is not possible to go further.

In general, certain conclusions are possible from these data. They fail to prove that psychotherapy, Freudian or otherwise, facilitates the recovery of neurotic patients. They show that roughly two-thirds of a group of neurotic patients will recover or improve to a marked extent within about two years of the onset of their illness, whether they are treated by means of psychotherapy or not. This figure appears to be remarkably stable from one investigation to another, regardless of type of patient treated, standard of recovery employed, or method of therapy used. From the point of view of the neurotic, these figures are encouraging; from the point of view of the psychotherapist, they can hardly be called very favorable to his claims.

The figures quoted do not necessarily disprove the possibility of therapeutic effectiveness. There are obvious shortcomings in any actuarial comparison and these shortcomings are

particularly serious when there is so little agreement among psychiatrists relating even to the most fundamental concepts and definitions. Definite proof would require a special investigation, carefully planned and methodologically more adequate than these *ad hoc* comparisons. But even the much more modest conclusions that the figures fail to show any favorable effects of psychotherapy should give pause to those who would wish to give an important part in the training of clinical psychologists to a skill the existence and effectiveness of which is still unsupported by any scientifically acceptable evidence.

These results and conclusions will no doubt contradict the strong feeling of usefulness and therapeutic success which many psychiatrists and clinical psychologists hold. While it is true that subjective feelings of this type have no place in science, they are likely to prevent an easy acceptance of the general argument presented here. This contradiction between objective fact and subjective certainty has been remarked on in other connections by Kelly and Fiske, who found that "One aspect of our findings is most disconcerting to us: the inverse relationship between the confidence of staff members at the time of making a prediction and the measured validity of that prediction. Why is it, for example, that our staff members tended to make their best predictions at a time when they subjectively felt relatively unacquainted with the candidate, when they had constructed no systematic picture of his personality structure? Or conversely, why is it that with increasing confidence in clinical judgment . . . we find decreasing validities of predictions?" [23, p. 406].

In the absence of agreement between fact and belief, there is urgent need for a decrease in the strength of belief, and for an increase in the number of facts available. Until such facts as may be discovered in a process of rigorous analysis support the prevalent belief in therapeutic effectiveness of psychological treatment, it seems premature to insist on the inclusion of training in such treatment in the curriculum of the clinical psychologist.

Summary

A survey was made of reports on the improvement of neurotic patients after psychotherapy, and the results compared with the best available estimates of recovery without benefit of such therapy. The figures fail to support the hypothesis that psychotherapy facilitates recovery from neurotic disorder. In view of the many difficulties attending such actuarial comparisons, no further conclusions could be derived from the data whose shortcomings highlight the necessity of properly planned and executed experimental studies into this important field.

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Carr Appointed Editor of the *Journal of Experimental Psychology: Human Perception and Performance*, 1994-1999

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