# PSYCHOTHERAPY REVISED:

## New Frontiers in Research and Practice

E. Lakin Phillips

Psychology Press

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The publisher has gone to great lengths to ensure the quality of this reprint but points out that some imperfections in the original may be apparent.

# Contents

# Preface

This book is the product of many minds, many hands, and many hours at the computer. I would have to write another book of the same size to express my indebtedness to all those who have stimulated me, worked with me, criticized, and enlarged upon my ideas and practices. For many years I have tried out the ideas of others as well as some of my own, among my colleagues at the Counseling Center, members of the Psychology Department, and friends.

One's distillation is never final; that is a good thing. But one must find occasional quiet places, plateaus if you will, lagoons for resting after strenuous effort. I am at such a juncture now.

Writing this book and sharing ideas with peers and students probably has been the most exciting intellectual time of my life. I only hope it can excite similar interest and challenge among others. I am grateful for the stimulation and help afforded by those cooperating with me in writing this book and in much of the research preliminary to the writing: Peter J. Fagan, Diane M. DePalma, Michael B. Kaiser, Theodore Heavner, to my secretary, May Nakamura, to my research assistant, Renee Pettis, to Ed Trenn, graphic artist, and to all other members of my staff at the Counseling Center. Over the years I have discussed many of the issues addressed in this book with colleagues who have been of inestimable value: Arthur J. Bachrach, James N. Mosel, Charles E. Rice, John J. Sullivan, and Daniel N. Wiener. To try to name even the outstanding members of my seminars over the years would take a long time; suffice it to say I have found such time highly provocative and rewarding. My remaining hope is that others will find my particular distillation useful, productive, and rewarding; that is all one can ask from friendship or science.

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# 1 Attrition: The Number One Problem of Psychotherapy Practice and Research

Over the past several decades psychotherapy has evidenced enormous activity without demonstrating much change. The proliferation of theories of psychotherapy has grown to staggering proportions (Corsini, 1981; Garfield, Herink, 1980; 1981 Patterson). Given this much activity, there should have been a greater distillation of ideas; not a rigid prescription of what is, or how to do, psychotherapy, but a settling in on basic concepts and some unification delimiting of practice.

The reason for this proliferation of theories is more understandable than its consequences. On the one hand, the "talking cure" has stimulated a lot of thinking, guesswork, and some theorizing of value—thus accounting for many therapists/theorists throwing their hats in the ring—but has resulted in a corresponding lack of appropriate research addressing salient issues. Most research has been on very narrow problems of technique, important in some problem areas and apparently without much value in other areas (Bergin & Lambert, 1978; Glass & Miller, 1980; Garfield, 1978; Landman & Dawes, 1982; Smith, Glass, & Miller, 1980)—and much has been left dangling among studies of therapy evaluation and outcome. The matter of outcome is still an important issue in psychotherapy. How are we to judge the value of something unless we can study its consequences? The opinion that the outcome problem has been left hanging is a judgment supported throughout this book; but for the now, suffice it to say that the reason the outcome problem is so hazy, ill-studied, and lacking in generalizability leads us precisely to the main issue of the book: attrition.

Attrition has lamed or even killed off much outcome research that was testing hypotheses about psychotherapy practice, technique, diagnosis,

presenting problems, and the like. The weak offerings it has rendered have pointed to issues inherent in generalizing results to other populations. If one were to transport our culture to a different land, and were obliged to select from the vast psychotherapy literature practices and validations that would put the new society in good stead for dealing with its problems there would be a great clamor by present-day therapists/theorists to be represented, but few substantial criteria on which to base a decision. (One possible outcome of such a hypothetical challenge might be that there were no winners and everybody's notions would be up for grabs in the new land.)

The literature on psychotherapy research contains infrequent and unsystematic references to the impact of attrition. Herein lies a serious problem, open to study.

## LITERATURE REVIEW ON ATTRITION

Few of the Annual Reviews of Psychology (published since 1950) contain references to attrition. Most reviews cover studies that concern themselves primarily with internal processes among various viewpoints, and some report outcome results. However, some reviews of attrition (also called "dropping out") have been comprehensive and informative. The main review studies follow.

*Luborsky and Associates.* Luborsky, Chandler, Auerbach, Cohen, and Bachrach (1971) and Luborsky, Singer, and Luborsky (1975) reviewed 166 studies of outpatient psychotherapy among individual adult cases. They studied predictors of outcome from psychotherapy based on patient, therapist, and treatment factors. Most studies dealt with client or patient predictors, far fewer with therapist or treatment variables. The psychological status of clients—the healthier the better—their motivation for treatment, the presence of some anxiety or discomfort, and intellectual/educational/social characteristics that bode well for gainful therapeutic outcome have been more fully addressed. Few lower class or uneducated persons have been studied. That most psychotherapy has been addressed to the young, attractive, verbal, intelligent, and stable (the so-called YAVIS characteristics) began to be recognized about this time. The Luborsky et al. review defined psychotherapy as distinct from information giving, educational or occupational guidance, shock-chemotherapy treatments, laboratory analogues (more recently a common development), or behavior therapy (also more common since the 1971 Luborsky et al. review). Zax and Klein (1960) and Snyder (1947) also offered definitions of psychotherapy.

Although there are many variables reported on in the Luborsky et al. review (1971; Table I, p. 148), the portion of the review concerning this book

revolves around the "drop-out versus stay-in psychotherapy phenomenon" (pp. 154–157). These reviewers assert that "there is some indirect evidence . . . that *length of treatment* is positively related to gain from psychotherapy" but they did not elect to review the dropout problem as thoroughly as some other variables because they averred there was "no *explicit* evidence that this variable was consistently related to the amount of gain a patient makes" (p. 154). Despite this disclaimer—one to be challenged here—Luborsky et al. did turn up some interesting information on dropouts from psychotherapy. Although these reviewers report on 20 studies of length of sessions (Table I, p. 148) that were significantly related to outcome, two studies were unrelated to outcome, but the number-of-sessions variable did not usually include dropout figures, nor comparisons with "stayers" at the end of the therapy series. The burden of staying or dropping out fell on the descriptive or predictive power of *client* variables and included characteristics previously cited. The motivation-for-treatment variable added the most to the terminator-remainer battery that attempted to predict outcome from psychotherapy (p. 155). An interesting final remark in this section of the paper was, "Therapists have some influence, but not a large one, on the proportions of both populations (remainers and terminators) they can hold in treatment" (p. 155) Data are presented later to question this generalization.

Other generalizations from the Luborsky et al. review (1971) addressed are the following: Most research conclusions about the clients stem from studies of how they were *before* treatment; little data support generalizations about therapist characteristics or theoretical orientation; the most used criterion measure of outcome was therapists' gross improvement ratings of clients; clients who drop out are seen as not improving, or as failures; and of the 166 studies reviewed only a few meet wide-ranging criteria or predictive research adequacy in the matter of outcome: Rogers and Dymond (1954); Fiske, Cartwright and Kirtner (1964); Frank, Gliedman, Imber, Stone, and Nash (1959); Gottschalk, Mayerson, and Gottlieb (1967); Rogers, Gendlin, Kiesler, and Truax (1967); Wallerstein, Robbins, Sargent, and Luborsky (1956).

*Brandt's Review.* Brandt (1965) reviewed 25 specific studies of factors promoting client dropouts among individual adult patients. Among 29 variables investigated in 18 research reports, there was no differentiation found between stayers and dropouts in regard to sex, age, and marital status; the only differentiation pivoted on personality characteristics. Higher client educational attainment was related to staying in therapy, congruent with Luborsky et al. (1971).

Brandt's (1965) review places the cause of dropping out mainly on the patient's initiative, although allowing for some "guidance" in the matter by therapists (p. 6). Some research reported in the Brandt review allows for unsystematically explored differences in attrition among long-term and pri-

vate practice therapy cases compared to short-term cases, but no review of factors related to client or therapist behavior is identified. Sometimes therapists are said to discharge allegedly unsuccessful cases (Myers & Auld, 1955). The generality of any possibly progressive attritional pattern was unknown. The range in the number of therapy treatments in the Brandt review is extremely large – from a few sessions to years (Burnham, 1951). Sometimes the attrition issue was rendered unclear by reference to a "trial period" of 3 to 6 months before therapy evolved. In the Burnham report, even the trial period of 3 to 6 months (three to five interviews per week) would suggest a total of 35 to 120 sessions, a period several times longer than brief therapy. Attrition in short-term therapy would therefore appear to be much more definitive and observably related to other variables. Against this longer range period of therapy sessions, Brandt's Table 1 (p. 7) nonetheless identifies 25 studies that vary in the mean number of sessions from 5.6 for the shorter therapies to 12.6 sessions for the longer ones. Despite references to long-term psychotherapy, Brandt concentrates on studies in which short-term therapy (whether time-limited or not) was the prevailing model of treatment. Apropos of the importance of the *pattern* of attrition, which will be addressed more fully later, Brandt's Fig. 1 (p. 8) (possibly the first report of this kind in the literature) shows the percentage of clients dropping out after one or more sessions, study by study, for a total of 20 sessions. This figure shows that only five studies afforded a proper data base (Affleck & Medwick, 1959 Hiler, 1958; Kadushin, 1969; Kurland, 1956; and Rogers, 1951) on client loss to attrition (about 50% by the third or fourth session). Brandt's figure showed that the *first* therapy session saw a termination of about 35% of the clientele. Studies in the Brandt review that followed attrition from intake to the end of therapy, session-by-session, were rare.

Brandt reviewed factors related to pre-therapy or intake dropouts. (This topic is treated systematically below.) Suffice it to say now that the "rejection of treatment . . . by patients . . . at intake" led Brandt to state that from 3% to 35% of the clients dropped out at intake, in the seven studies surveyed that reported on this particular statistic (Table I, p. 7). However interesting this statistic appears, it seems to have been abandoned, even in research reviews as late as 1983; and Brandt observed that "an extensive literature search did not reveal a single follow-up study of pre-therapy dropout" (Brandt, 1965, p. 10). This situation about pre-therapy (or intake) attrition appears to be the same today inasmuch as few of the recent reviews of attrition or follow-up evaluation have even noted the problem, much less studied it (Garfield, 1978, 1981; Gelso & Johnson, 1983; Smith, Glass, & Miller, 1980).

*Miscellaneous Reviews.* In the comprehensive *Handbook* (Garfield & Bergin, 1978), there are only two index references to attrition and none to dropout. Despite this indexed lack there are, nonetheless, some informative

discussions of attrition. Bergin and Lambert (1978) discuss issues raised by Eysenck (1952, 1960, 1965, 1966, 1967) and Rachman (1971), and Rachman and Wilson (1980), who agree with Eysenck about the success rate of psychotherapy compared to people who appear to get better on their own. The voluminous reporting of pro and con opinions on this subject cannot be addressed in detail here. However, the systematic study of attrition affords the opportunity to reanalyze data presented by Bergin and Lambert concerning therapeutic outcome research, for example, from the Berlin Psychoanalytic Institute about 1930 (Bergin & Lambert, 1978, pp. 141–144; Knight 1941). During its first 10 years of operation, the BPI had 1955 "consultations" (called "ostensible therapy applicants" here) from which population 721 analyses were begun. These two figures give us an intake or "pre-therapy" figure of a 64% loss. Thus about two thirds of the potential analysands were screened out or eliminated on some therapist-determined grounds at the outset. Further, of the 721 cases beginning psychoanalysis, 361, or 50% had "concluded treatment at the time of the report" (Bergin & Lambert, 1978, p. 141). Of the 721 beginning treatment, 241 cases, or 33%, had terminated prematurely (due to patient, therapist, or mutual decision), with 117 patients still in treatment at the time of the report. Of the 363 patients completing treatment, 47 (13%) were considered "uncured," and 116, 89, and 111 cases were judged, respectively, improved in increasing amounts (total = 316, or 87%). If one takes as the basis for deciding on outcome (the 316 cases completing treatment) then 87% of the 363 "concluding treatment" would be successful or highly so; but when one considers that the figures begin with *commencement* of therapy (721 cases of psychoanalysis netting 316 successful cases), the percentage shrinks to 44%; and, when the 316 successful cases are seen as a percentage of the ostensible therapy cases ( $N = 1955$), the percentage shrinks to *16*% (the 117 patients remaining in treatment would have to be properly apportioned upon termination).

Data are presented later showing that in short-term, outpatient, individual psychotherapy, reported success ratings range from 15% to 25% of the original (ostenstible therapy) population, a figure closely similar to that of the Berlin Psychoanalytic Institute over a 10-year period. Two issues derive from the BPI data: First, *all* cases must be considered in arriving at evaluations of outcome, from pre-therapy (or intake) onward; and, second, the role of screening out, judging satisfactory or not for treatment—largely if not wholly in the clinician's hands—must be questioned more thoroughly if the delivery of mental health services to the populace is to remain an important issue.

In the Bergin and Lambert article (1978), their summary data (Table 5.1, pp. 142–143)—in disputation over the Eysenck issue—become moot. That is, the differing criteria offered by various interpreters of the BPI data fail to show an agreed-upon attritional base. Whether people get better "on their

own" significantly often, as Eysenck and Rachman and others allege, is important in its own right, but within the context of the reporting clinics, the attritional issue cuts through the Eysenck dispute and enlarges the problem of outcome evaluations. Nearly all clinics have been somewhat superficial in reporting on various types of outcome data inasmuch as they have concentrated on data at what is often some arbitrary beginning point (not from intake) and calculated successes and failures therefrom. For the same reason—the neglect of attrition—the Bergin and Lambert Table 5.2 and Table 5.3 (pp. 146–147), regarding the percentages of "remission rates" from a variety of studies, are also moot, because one seldom knows from their review what juncture—from the intake interview onward—is the basic one. The subtleties of eliminating patients/clients from psychotherapy, and the attendant reasons, are often beyond the comprehension of the reader of research reports.

*The Role of Controls.* The obfuscation of outcome research and conclusions from psychotherapy is contributed to not only by the attritional problem. Frank (1973) has shown that controls are not put "on ice," awaiting the opportunity for therapy, but up to 50% seek other therapy or informal contacts, a fact that confronts the Eysenck and Rachman contentions. Information is needed on controls in studies more widely dispersed than Frank's report elucidating their informal attempts to gain help and how these gains would compare with formal therapy efforts (see discussion of the Di Loreto study following). Bergin and Lambert end a section on the role of informal or "spontaneous" help by saying, "Perhaps selected helping persons in the 'natural' social environment provide adequate or better coping conditions for neurosis than do trained mental health experts" (p. 149). In this connection it is well known that minimally trained persons can have a salutary effect on helping emotionally disturbed people (Carkhuff & Truax, 1965; Emrick & Lassen, 1977; Gruver, 1971; Johnson & Katz, 1973; Poser, 1966; Siegel, 1973; Strupp, Hadley, & Gomes-Schwartz, 1977). Adding together nonspecific factors in mental health/psychotherapy change (Frank et al., 1959; Garfield, 1980, pp. 126–133), and the possibility that attrition from psychotherapy does not necessarily bode failure, we are forced to considerably enlarge our notions of therapeutic change and how it may be brought about.

*Length of Treatment.* Of value regarding psychotherapy outcome is the length-of-treatment variable often considered as the main therapeutic variable. In Table 5.4 (Bergin & Lambert, 1978, p. 155), studies are cited that illustrate both deteriorating and positive change from both short- and long-term therapy. Of nine studies cited, one is of short-term duration (4 months) among 96 junior high school students; one of 3–5 months duration with 72 short- and longer term psychotics; one study reports on 80 eclectically treated

college students (3 therapy sessions) involving anxiety complaints; and in the six other studies cited psychotherapy lasted from 24 sessions to six years. There is no decisiveness favoring long-term therapy in this summary. With short-term therapy, "deterioratng" cases can be noted early with possible correction following. If long-term therapy produced no clearly better follow-up results than short-term therapy (Luborsky et al., 1971), then the early detection of possibly untoward results from therapy would seem to be very important and should not wait on lengthy time periods for evidence. However, conclusions from the Bergin and Lambert summary are hard to come by, because the important issue of attrition has been neglected in all of the reported studies.

Garfield (1978) discusses the problem of premature dropout from therapy before a mutual client-therapist agreement has been reached (by definition *dropout* implies non-mutual decisions). Garfield offers a summary of some findings on length of treatment (Table 6.1, p. 195), which showed how 560 patients seen at a VA clinic (Garfield & Kurz, 1952) were distributed over a number of psychotherapy sessions. The mean number of therapy sessions was about 6, and 67% had left therapy by the tenth interview. Only 9% of the 560 patients came for more than 25 interviews in an open-ended therapy regimen.

## REPLOTTING DATA

Replotting the Garfield Table 6.1 (p. 195), the present Table 1.1 was derived from the earlier Garfield and Kurz article (1952) by adding a fourth column, "Percentage of Accumulative Attritional Loss." Table 1.1 shows what Fig. 1.1 displays graphically to be a characteristic attritional curve, a negatively

TABLE 1.1
Replotting Garfield Data (Garfield & Bergen, 1978, Table 6.1, p. 195) Regarding Length Of Treatment; Total $N = 560$

| *Number Interviews* | *No. Cases* | *Percent of Cases* | // | *% Remaining*[a] |
|---|---|---|---|---|
| Less than 5 | 239 | 42.7 | | 57.3 |
| 5–9 | 134 | 23.9 | | 33.4 |
| 10–14 | 73 | 13.0 | | 20.4 |
| 15–19 | 41 | 7.3 | | 13.1 |
| 20–25 | 24 | 4.3 | | 8.8 |
| 25 & Over | 40 | 8.8 | // | 00.0 |

[a]Calculations showing attritional loss; added by present author.

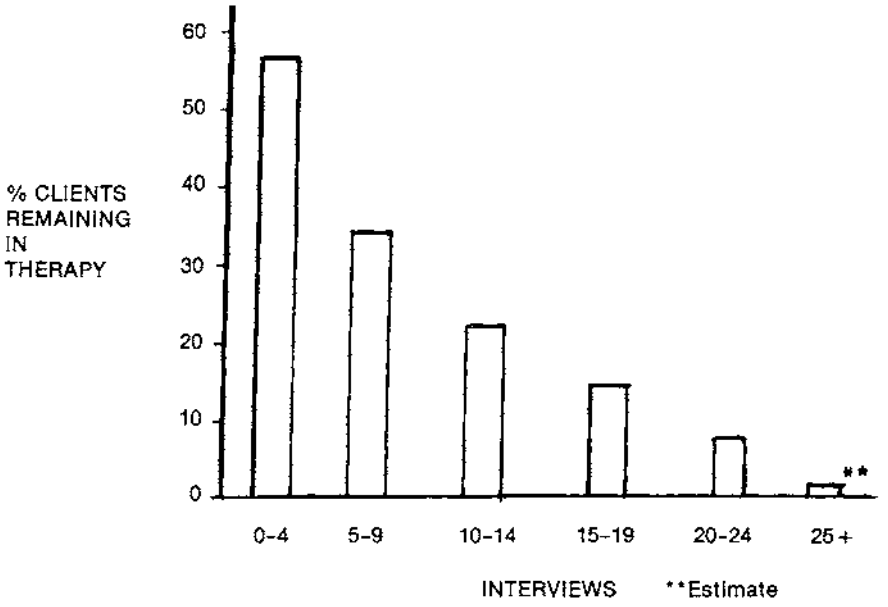


FIG. 1.1 Showing attritional curve (% remaining after each session interval); replotted from Garfield and Bergen, 1978 (See Table 1.1).

accelerating declining ("decay") curve. This curve has been found repeatedly in psychotherapy research reports, as will be shown, but was brought into focus by rearranging the data to show declining numbers of participants (vertical axis) over the series of therapy sessions (horizontal axis).

This curve suggests that most people get help (or not) in a very short period of time; a few seem to require more time. What the various portions of the attritional curve are related to in terms of client, therapist, and clinic variables is not readily available in the literature; in fact, the nature of this attritional curve has not been noted before, except in a somewhat different way in the Brandt review (1965). The central place of the attritional curve is beginning to show significance and is elaborated on further.

Garfield's Table 6.2 (p. 196) summarizes the length of treatment over a number of VA, university, and psychiatric clinics, from data reported from 1948 to 1970. In this summary the mean number of interviews was about six; and one-half the therapy clients were lost by the eighth interview (unrelated to the number lost at intake). We are not told whether these were from time-limited or time-unlimited settings. Large-scale figures are offered by the National Center for Health Statistics (1966), which show that among 979,000 patients of consulting psychiatrists, in 1963–1964, the average number of therapy sessions was 4.7. Eiduson (1968) found in a review of attrition and

length of therapy that 30% to 65% of all kinds of psychiatric patients drop out from therapy before mutual termination occurs.

Garfield, Eiduson, and others fail to report the number of dropouts at the *intake* point. In the Baekeland and Lundwall (1975) review, about 50% drop out at intake (data presented later concerning present research confirm this figure). Thus, the attrition rate *during* therapy—shown in the Garfield and Kurz (1952) report and in the Eiduson (1968) report—represents only a part of the problem; the equally large issue, or possibly larger for a number of reasons, is the dropping out at intake (Baekeland & Lundwall, 1975; Phillips & Fagan, 1982a, 1982b).

Although it is not the main purpose here to discuss research on patient characteristics that favor staying in therapy—this may no longer be a fruitful topic for research—it should be noted most such research fails to find characteristics in patient populations that detect the potential for early termination (Garfield, 1978, pp. 202–206). It appears to some that staying in therapy is not so much one of client characteristics (although, to be sure, some of the variance may be so related), but is more likely one of how the therapeutic situation adapts to the client's needs and perspective, and how the delivery system, qua system, operates overall in the clinic. If more study focuses on the attrition curve, therapists could *then* look at the curve and find therapist, client, policy, and system variables that might relate to greater therapeutic flexibility, and thereby improve service delivery.

The delineation of therapist variables related to process and outcome (Parloff, Waskow, & Wolfe, 1978) nets about the same results as does the delineation of client variables related to attrition from therapy. These authors state, in concluding their review of therapist variables, "The therapist variables most frequently selected by the researcher for study are, unfortunately, such simplistic, global concepts as to cause this field to suffer from possible terminal vagueness" (p. 272). The finding from the Baekeland and Lundwall (1975) and Luborsky et al. reviews (1971, 1975) end on similar notes (also, see Garfield, 1982, pp. 239–266).

*Psychoanalytic Patients.* Some research on therapeutic outcome has been done on psychoanalytic therapy patients (Luborsky & Spence, 1978). These reviewers found five quantitative studies of psychoanalytic patients wherein initial and final status of the patients was interrelated. The number of patients in these studies was small: 30 patients (Klein, 1960); 27 patients (Knapp et al. 1960); 21 patients, each, in two groups, one psychoanalytic psychotherapy and one group psychoanalytically oriented psychotherapy (Kernberg et al., 1972; Wallerstein et al., 1956); 183 patients in a retrospective study (Sashin, Eldred & Van Amerongen, 1975); and a survey of about 3,000 patients on whom therapists filled out initial and final questionnaires (Hamburg et al., 1967). Although patients with the best initial personality

functioning tend to show the best outcomes (Luborsky et al., 1971), their review of 26 studies showed that 13 studies revealed no relationship between initial and end-of-treatment status, and 13 studies showed, as stated, that the better initial status predicted better final status. The five quantitative research reports cited by Luborsky and Spence (1978) fit this generalizaion. The Menninger Foundation study (Kernberg et al., 1972) found a correlation of .50 between patient anxiety at the outset and therapist-rated global improvement at the end of therapy. However, none of these studies dealt systematically with the problem of attrition, and none began with the number of patients originally applying for psychoanalytic therapy (ostensible therapy cases) from which the final patient population was selected. Hence, research on predictor variables relating the beginning of therapy to the termination (completion) is greatly vitiated in effectiveness due to considerable patient selection.

Long-term therapy, which appears in some quarters to be on the increase rather than decreasing as a result of knowledge distillation (Garfield, 1980, p. 278), also complicates the study of attrition and evaluation of outcome. One study reports psychoanalysis/psychodynamic therapy to take an average of 835 hours (Bergin & Lambert, 1978), and longer periods—up to 20 years—are reported (Zilbergeld, 1983). What the attrition curve would look like in these settings would be interesting to know.

*Drugs.* Sometimes the use of drugs in psychotherapy, as an explicit decision at the start of a project, in combination or not with psychotherapy, encourages early assessment and keeping track of patients. However, the lengthy report by Hollon and Beck (1978) in which 33 studies are summarized (Table 12.1, pp. 446–459) does not account for attrition. They do, however, discuss the attrition problem generally and suggest some remedies (p. 443), including replacing dropouts from experimental and control populations (not very satisfactory), doing assessments earlier and more often, examining reasons for dropping out, and multiple data analysis. This discussion is one offering the most discernment of the problem of attrition in the Garfield and Bergin book (1978), albeit very brief and not directly applied to the research studies. Hollon and Beck have more to say about attrition: In the Johns Hopkins Group (see Covi, Lipman, Drogatis, 1974), where 218 depressed females between ages 20 and 50 were assigned to one of six cells in a 3 × 2 factorial design, various combinations of therapy and medication were employed. Hollon and Beck (1978) say, "Overall attrition was high. Twenty percent of the initial sample failed to survive the placebo 'washout' period, while another 32 percent of those surviving did not complete the 16 weeks of active treatment. Overall, 47 percent of the sample screened into the study failed to complete the treatment protocol" (p. 461). The dropouts were not studied further for whatever enlightenment they might provide. The authors say it re-

mained in doubt whether the members of this population constituted treatment failure or remission, or suffered possible side effects.

Further evidence of attrition in drug-and-psychotherapy studies, reviewed by Hollon and Beck in regard to attrition, netted the following results: 278 females were the patients for a 4- to 6-week trial period; of these, 150 (54%) showed a reduction in symptomatology and were screened into the study. Of these 150, 106 (70%) survived the full treatment without relapse (106 survivors equals 38% of the original 278 patient population). We do not know from the report whether the 278 patients given the 4 to 6-week trial period were, as a group, screened in from a large group of applicants. The attrition may have been greater than reported figures state.

## CRISIS THERAPY

Crisis-oriented brief psychotherapy may yield lower attritional rates, owing in part to the brevity of the therapeutic contacts and to the foci of intervention being manifestly salient to the patient (Avnet, 1965a, 1965b; Malan, 1973; McNair, Lorr, & Callahan, 1963; Sifneos, 1972, 1979). Success figures ("improvement," a global rating) as a generalization appear to hover around the 70% mark (Butcher & Koss, 1978) for crisis-oriented or very brief therapy. The Butcher and Koss review (1978, Table 19.3, pp. 751–753) covers 41 research reports, between 1956 and 1976, and shows a follow-up improvement rate (various measures, self-reports, etc.) in the vicinity of 70%. This figure, however, cannot be taken at face value because the percentage of returns at follow-up is not related, in many cases, to the original number treated, or to the total number of applications or ostensibly therapy cases. As to the differences between brief therapy and long-term (or psychoanalytic) therapy candidates, the Wallerstein et al. (1956) finding may be of some value. Wallerstein et al. used a 100-point Health-Sickness Rating Scale; it was found at the Menninger Clinic that among the initial state of patients receiving various forms of psychotherapy, those in psychoanalysis rated higher on this scale (51.4) than patients receiving expressive psychotherapy (38.2 rating), or those receiving supportive psychotherapy (36.7 rating). Thus, in this population—and we must take into consideration the population of patients likely to be at the Menninger Clinic in the 1950s—the more strictly psychoanalytic patients were the "healthier," which seems on the surface to be a contradiction. Why, then, would less healthy patients be relegated to less intensive treatment? There may be a good deal of therapist bias in choosing patients with whom one prefers to work, especially if the therapeutic work runs into the hundreds of hours. Health-Sickness or other ratings at a pre-therapy or beginning therapy juncture are not available for direct comparison among different lengths or types of therapy, or in relation to attrition.

*Baekeland and Lundwall Review.* The summary in the Baekeland and Lundwall review (1975) points up the attrition problem in outpatient psychotherapy to an extent not matched in other literature reviews. Some summary statements from their review are salient: "In general psychiatric clinics, 20–57% of the patients fail to return after the first visit" and "31–56% attend no more than four times" (p. 738). Many of these dropouts will, however, return later or go elsewhere to treatment within a short period of time (Baekeland & Lundwall, 1975, p. 743). It is not known from present reviews of the literature whether these "returnees" will again follow a similar dropout pattern (data are presented later on this issue), the issue of dropping out and returning not having been systematically studied. These authors point up that attritional loss does not mean no gain for these patients, differing with Eysenck and Rachman. However, these authors suggest ways to curb attrition: Eliminate waiting lists; offer a wider range of ancillary services and better explanations of therapy as a process; and determine if a patient has dropped out of therapy before—among other suggestions.

Gelso (1979) offers a general discussion of counseling and psychotherapy issues of a methodological and professional nature. Gelso covers many issues relating to process and outcome but leaves out attrition altogether. The seemingly temporary interest in attrition during the decade between 1965 (Brandt, 1965) and 1975 (Baekland & Lundwall, 1975) appears to have pointed to a problem that has had too little study since (except for very recent analogue/meta-analysis research, see Chapter 3).

## TIME-LIMITED CONDITIONS

Gelso and Johnson (1983) have reported on systematic studies of time-limited and time-unlimited therapy, which has raised interesting points concerning attrition. They do not bring forth the attrition problem but they do offer data on dropping out of very brief, time-limited therapy. They report that 79 students from the University of Maryland Counseling Center sought personal counseling or psychotherapy during a 6-week period in 1973. Twenty-two of them (28%) were screened out via MMPI selection procedures, and 15 (19%) were lost for other reasons. Beginning with 79 cases, they ended with a study population of 42, a loss of 37/79 of 47% at the outset. The researchers' contribution to this loss, via selection, however, was only 28%; the remaining loss came from clients not showing for the first session, not taking the tests, or cancelling. This order of attrition is typical (Baekeland & Lundwall, 1975; Brandt, 1965; Garfield, 1978; Luborsky et al., 1971). Add to this attrition the fact that 18 of 42 clients were available for follow-up 2.5 years later (i.e., 43% of the study population or 18/79 of the original ostensible therapy population [23%], showing attrition of 77%).

This figure (23%) is slightly better than figures previously presented for the Berlin Psychoanalytic Institute, but equal to the figures presented in Chapter 2. Follow-up data from the Gelso and Johnson study have to be considered in the light of the attritional figures presented. For example, the statistics at the time of the 2 1/2-year follow-up revealed that of the 18 clients in both time-limited (8 and 16 session limits) and time-unlimited therapy "all three groups improved over time" (p. 9). The three groups were time-limited, time-unlimited, and controls; where the controls ($N$ = 13 at start of study) numbered 5 at follow-up. The two time-limited conditions netted a mean of 6.9 sessions (range = 1 to 12 sessions) and 8.61, respectively, for the 8 sessions and 16 sessions time-limited conditions (p. 51). These mean figures are strikingly close to those presented by Brandt (1965), Garfield (1978), the National Center for Health Statistics (1966) without the possible benefit of contrasting time-limited conditions, and in data reported in Chapter 2. Time-unlimited therapy in the Gelso and Johnson study was said to be in the vicinity of 20 sessions (p. 51).

Gelso, Spiegel, and Mills (1983) studied 87 clients at the same Counseling Center (over a 28-month period, beginning November 1973), by giving them a battery of tests. Follow-up times were 1 and 18 months later. At the initial follow-up, 41 of 87 clients (47%) responded (loss of 53%); later, through intensive phoning, the attrition rate was dropped to about 10% (78 of 87 responding to the call for a completed Counseling Center Follow-Up Questionnaire), a remarkably good return. Following this information, the authors say, "It should also be noted that 21 client applicants were assigned to therapists but failed to attend any sessions, and are not included in any of the analyses" (p. 24). These 21 clients either did not show or openly declined counseling. Is the 21-client loss, then, 24% of 87 clients pool, or a 27% loss from 78 clients?

The 18-month follow-up reveals that the researchers were able to obtain usable results from 67 of the original clients (87%), or 67 of the 71 clients who could be located (p. 24). The original plan, however, was for 90 clients, 30 each for three conditions, 8 session (time-limited), the 16 session (time-limited) and the time-unlimited session. Table 2.1 (p. 25) reveals how close the groups were to the originally planned figures, with $N$s of 27, 28, and 23, respectively, at the 1-month follow-up, and $N$s of 21, 24, and 22, respectively, for the 18-month follow-up; a good record. Significant for the problem of attrition is the reported attitudes of therapists about short-term psychotherapy wherein therapists are said to prefer working with the better adjusted clients and to feel that time-limited therapy (especially 8-session) would probably be inimical to good therapeutic progress. This therapist bias was challenged in the Gelso and Johnson research yet obtains in much published research. The issue of therapist preferences and/or satisfaction in therapy can often influence the choice of clients for therapy (Beuter, Johnson,

Newell, Warburn, & Elkins, 1973; Burton, 1975) and thereby influence attrition.

*Miscellaneous Psychoanalytic-Oriented Studies.* Strupp, Fox, and Lessler (1969) were able to study a sample of private patients seen by 11 psychiatrists and psychologists (p. 12). In a comprehensive questionnaire, they posed questions about therapist-patient relationships to 76 former patients; forty-four (58%) responded with usable data, a 42% loss. The nature of the 42 nonresponding cases was not reported on. Quite unlike short-term psychotherapy in duration, the Strupp et al. study reported an average of 166 interviews for this young (median age 31–32 years), male, upper middle-class, educated clientele. Strupp et al. (1969) noted a substantial therapist-patient concordance in attitudes toward the therapy offered, an intensive, psychoanalytically oriented one; patients reported improvement in general well-being as well as disappearance in specific complaints (p. 14). However, the more favorable ratings of their therapy experience by patients came from those receiving less intensive therapy (p. 15), suggesting evidence for the value of brief therapy, even in the face of study attrition. We do not know what hidden attrition may have resulted in the selection of the 76 patients identified at the beginning of this study. The authors say, in corroboration of this point, "In addition to the differences attributable to the form of psychotherapy, we discerned the existence of an important selection factor . . . [where] . . . patients were selected for intensive psychotherapy mainly on the basis of age (young patients were preferred), sex (male patients were preferred), and education (more highly educated patients were preferred), although other factors appeared to be involved as well: motivation to enter a prolonged therapeutic relationship, degree of disturbance, level of anxiety and discomfort, defensiveness, and other clinical considerations" (Strupp et al., 1969, p. 16). With this much subtle selection going on at the hands of the 11 psychotherapists involved, it is difficult to ascertain how representative these patients were of even intensive psychotherapy, not to mention the attritional impact.

In the second study reported on by Strupp et al. (1969)—in contrast to the first study where ratings were made *ex post facto*—ratings were obtained from clinical records before and after the therapy (p. 46), as well as ratings via questionnaires completed by the patients. The second study was on patients seen in a hospital outpatient clinic. The basis of the study was 696 completed cases, from which pool 91 cases had been seen for 20 interviews or more. Of the pool of 696 patients, 257 had been referred for therapy, the others primarily for diagnostic purposes, allowing us to take now as the "ostensible therapy cases" the 257 cases specifically referred to this clinic for therapy. The basis for deciding on elements of attrition is further complicated by the authors asserting that of 450 patients seen in one year, a large number terminated within a year, indicated in a quotation from Pfouts, Wallach, and Jenkins (1963) that "Too often policies and procedures are set up as

if the clinic were almost exclusively a long-term intensive treatment center, when in reality it is for the majority of patients a diagnostic and brief therapy center" (p. 48). Strupp et al. continue "For these reasons, patients selected for our sample cannot be regarded as a cross-section of all patients seen at the clinic" (p. 48). The clientele, then, consisted of 244 patients seen for more than 25 interviews by psychiatric residents, advanced graduate students in clinical psychology, or staff members of the clinic. They add: "The requirement of 25 interviews was imposed because we wished to concentrate on individuals who had remained in therapy for a reasonable period and for whom therapy might be presumed to have been a significant experience. *By eliminating early dropouts we also hoped to obtain a more homogeneous sample of stayers*" (italics added p. 28). Thus, 244 questionnaires were mailed to former patients, yielding at first 92 returns, followed a month later with a mailing that yielded an additional 39 questionnaires, providing a total of 131 returns, showing an attritional figure of 113/244 (46%). These 244 cases were apparently selected from the 257 cases (ostensible therapy?) referred for treatment (p. 47), the basis for which we are not told. The rate of return—54%—represents a commendable effort. In selecting only clients with 25 or more interviews, they limited their coverage considerably. What of the clients who had less than 25 interviews and the benefits they received? These are important lost data. Although researchers have the right to choose a study population—if this is done objectively via replicable criteria—the interposition of other subtle, often subjective factors muddies the research waters. The 131 questionnaire returns were reduced to 122, due to 9 incomplete replies, yielding an attritional loss of 122/244 (50%): or 122/257 (47% retained, or 53% attritional loss) based on the total population from which the study population of 244 cases was derived. The mean number of interviews per week for this population of 122 cases was 1.4 and the average number of interviews was 70.4. Most were seen for 26 to 49 hours of therapy, much longer than short-term therapy, but far less than intensive therapy cases. Recomputing Table 3, p. 54 (Strupp et al., 1969) allows for a rough table of progressive attrition by subtracting the number of cases left in the therapy population after each stated interview limit, resulting in a negatively accelerating declining curve similar to that found elsewhere (see Fig. 1.2). There is more descriptive value in viewing in a whole cloth manner the attritional decline curve compared to many other statistics. The declining (or "decay") nature of the attritional curve raises questions about the cases dropping out early and those taking more time or whatever benefits they obtain. The literature on outcome from psychotherapy has allowed to lie fallow the nature of the attritional curve phenomenon and has thereby lost much valuable information and prematurely forfeited heuristic problems.

Looking further at the Strupp et al. data (1969), we note on p. 64 that of the 122 cases reporting benefits—the average number of visits was

TABLE 1.2
Repeating and Extending Strupp et al., Table 3, p. 54 (1969); $N = 122$

| *Total Treatment Hrs.* | *N* | / / | *Attritional Decline (N)* | *Attrition (%)* |
|---|---|---|---|---|
| 25–49 | 54 | | (122–54 = 68) | 55.7 |
| 50–74 | 26 | | (122–80 = 42) | 34.4 |
| 75–99 | 14 | | (122–94 = 28) | 23.0 |
| 100–124 | 10 | | (122–104 = 18) | 15.0 |
| 125–149 | 05 | | (122–109 = 13) | 10.0 |
| 150–174 | 02 | | (122–111 = 11) | 09.0 |
| 175–199 | 02 | | (122–113 = 09) | 07.4 |
| 200+ | 04 | | (122–117 = 05) | 04.1 |
| Unknown | 05 | / / | (122–122 = 00) | 00.0 |

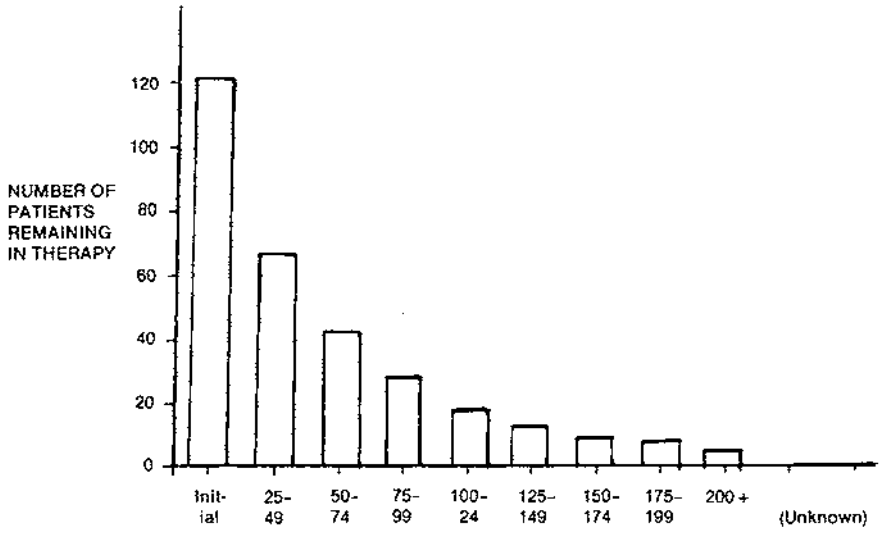


FIG. 1.2 Replotting, added attritional data (from Strupp et al. 1969, 54, Table 3) $N = 122$ (See Table 1.2).

70.4—27% reported "marked change" after 1–3 months (mean = 1.4 visits per week), or 7 to 21 visits, a figure closely similar to short-term therapy results in different settings. Moreover, another 18% report the next category of time—4–6 months of therapy—as the time interval within which they noted "marked change." Is it possible that the entire study of intensive psychotherapy in this report is a lost opportunity to set up a firmer structure of short-