CHAPTER SEVEN

Outcome studies in group psychotherapy

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In 1975, describing her long-term follow-up study on outpatient analytic group therapy, Barbara Dick commented:

> Evaluation studies of analytic group therapy are few. To some extent this is attributable to the considerable extent to which psychotherapists depend upon their conviction that what they do is valuable. Developments within the group sessions are frequently marked and dramatically reinforce the conviction of a powerful therapeutic agent. It is consequently tempting to avoid the challenge of scrutiny and research and to repress the awareness that potency may be for good or ill, or consist of a placebo effect. [Dick, 1975, p. 365]

There is no evidence base for using evidence-based criteria for selection of treatments. We simply do not know whether this is the most helpful, effective, or efficacious way of deciding what is the right treatment for a particular individual, because it has not yet been studied against other models of treatment selection. And yet, 40 years after Barbara Dick was writing, it is clear that the expectation that psychotherapy should be demonstrably effective has become normative: we are no longer able to succumb to the temptation to avoid the challenge of scrutiny and research. In this climate the importance of addressing the issue of outcome in psychoanalytic group therapy is evident.

In this chapter I review the current literature on outcome research for psychoanalytic group therapy. I begin with some broad issues in this field of research before outlining a number of important outcome studies and the limitations of the information they provide. I will then briefly review the research on process, before returning to this issue of the evidence-base climate and the need for research in this field.

We need to make clear distinctions between the different modalities of group treatment. When I refer to generic group therapies, I am including all modalities: from psychoanalytic to cognitive-behavioural to experiential. Group-analytic therapy is a broad church, but is generally associated with therapy based on the theories of Foulkes (1964). Psychoanalytic group therapy uses Wilfred Bion's ideas about groups as its theoretical foundation: it is the model described in this book and employed in the Tavistock Clinic.

The efficacy of generic group therapies has been evaluated by McDermut, Miller, and Brown (2001), who described 48 trials conducted between 1970 and 1998. Only eight of these were based on psychodynamic principles; the majority were behavioural or cognitive behavioural interventions. Looking at group therapy across the different theoretical orientations, some valid conclusions can be made. First, group therapy is more effective than placebo or waiting-list treatment. Second, group therapy has an equivalent therapeutic effect to individual treatment. And, finally, different theoretical persuasions yield equal results. These conclusions hold validity for the average patient in the average group. It is not possible to say more as yet due to insufficient studies. In fact, if one were to summarize the most frequent conclusion stated in the literature, particularly for psychodynamic groups, it is that there needs to be more research.

It might be helpful to pause at this moment and wonder why this is so. A brief review of the history of group therapy emphasizes the early prevalence and predominance of psychoanalytically informed work in this field across a spectrum of diagnoses and settings. Why has this not translated into a substantial body of meaningful research? Lorentzen (2006) suggests that the paucity of outcome research for psychoanalytically based group therapy may lie within the nature of the therapeutic process. Group therapists will speak of the complexity of the process that complicates any attempt to measure outcome. This may in part reflect the ambivalence among group therapists to measuring outcome, particularly to using quantitative research. Lorentzen also suggests that the "looseness" of Foulkes' group analytic theory and concepts contributes to the struggle.

Karterud (1992) goes much further in proposing that the resistance towards research is based on anxiety generated by the notion of research requiring doubt and a search for truth. Karterud argues that the group analytic community is unwilling to undergo scrutiny of its theoretical assumptions that underpin the professional and group identity.

I would add to these ideas the discrepancy between *outcome* and *process* research in group analysis as a possible further explanation of the paucity of good outcome research. The psychoanalytic tradition is, of course, firmly rooted in the notion of research in the single case study, a research methodology that began with Freud. Thus the literature abounds with writings on the *process*; the question of "how does it work?" rather than the *outcome* concern of "does it work?". Perhaps best known of these are Yalom's therapeutic factors in group psychotherapy (1975), a list that includes concepts such as universality, altruism, catharsis, vicarious learning, and the instillation of hope. While many of these factors hold a sense of rationality and intuitive appropriateness, one has to bear in mind that Bloch and Crouch (1985) suggested that an extensive review of the literature on these therapeutic factors comes to the conclusion that "there is little research linking them to outcome, so there is no clear evidence that specific factors are therapeutic". And yet, despite this, therapists remain firmly attached to the single case study and to the descriptive, as though there were something inherently heretical about numbers and diagnoses.

So what do we know about outcome in group analysis? The number of studies is small, and all have research flaws and limitations. Some of these problems link to the wider psychoanalytic research dilemmas, while others are related to specific group therapy issues. The kinds of problems encountered include the use of a retrospective design; the use of a shorter treatment time than usual for longer term therapies; the lack of standard outcome measures; the high attrition rates; and the lack of follow-up.

To understand the context of these studies, it is helpful to review briefly the broader picture of psychological therapy research. Roth and Fonagy (2005) suggest that this is "an era of empirically validated treatments, that prizes brief structured interventions". Symptom-change is the flag waved as an indicator of "benefit from treatment". Complex interpersonal processes do not easily find a way into this language of change. A hierarchy of research has been broadly accepted, with large-scale consequences: from case studies, to prospective studies, to

comparison studies, to the ultimate gold standard of the Randomized Controlled Trial.

It is an article of faith that the principles of evidence-based medicine (EBM) as they apply in relation to medical conditions, apply in the same way in the specialized field of mental health generally and in the "talking treatments" specifically. There are a number of reasons to suppose that the principles of evidence-based medicine need to be qualified in order to be valid in this field. The main epistemological tool of EBM is the double bind, randomized controlled trial (RCT). Yet the conditions on which this tool depends for its validity cannot obtain in respect of treatments that require the *active and meaningful engagement of patients* as the effective vehicle of treatment.

This, and other equally important issues, may be summarized by saying that the medical model of drugs for defined disease entities may not apply to these more complex psychological entities. Suffice it to say that the narrow limitations of this way of viewing outcome in complex long-term therapies such as psychoanalytic group therapy have contributed both to the paucity of adequate studies and the struggle to address fully the very real need to investigate the question of *does this work?*

A brief overview of the highlights of *outcome research* must inevitably bring us back to the Tavistock in 1976, when Malan and a group of colleagues used a retrospective design in a study of two parts (Malan, Balfour, Hood, & Shooter, 1976). First, they attempted to comment on outcome by asking ex-group patients about their experience and their lives at follow-up, using six-month dropouts as controls. They then described a group of "star cases" chosen by therapists as those who had done well. While there are clearly significant issues one could raise in relation to the experimental design, the study remains of interest for a number of reasons. First, the conclusions reached include a clear statement that this form of group therapy did not work; that is there is no evidence for effectiveness. Slight-to-moderate improvement was gained if the patients were in treatment for a longer rather than a shorter period, although most patients expressed dissatisfaction with the treatment. Those patients who did do well had a clear marker of differentiation: they had had individual therapy in the past. Malan and colleagues' conclusions emphasize the problems when group therapy is treated as a second-class form of treatment, a form of rationing that works by giving individual treatment within the group setting. However, they hit a note of optimism in suggesting that it is only through facing the issues raised by research that ". . . Like our

patients, we therapists need our period of painful confrontation with our denials and self-deceptions before we can learn how to function effectively in our chosen role."

Barbara Dick, mentioned at the beginning of this chapter, describes a prospective study in 1975 whereby 93 neurotic and borderline patients, said to have had unsuccessful psychiatric treatment, were placed in two-year closed groups. Measures were taken before, after, and at 6-, 18-, and 30-month follow-ups using an 8-point measure of life "acceptability" covering relationships, sex, work, physical health, leisure, self-image, self-understanding, and symptoms. Of the 75% of patients left in the study at termination, 87% demonstrated positive change, with the majority independent of psychiatric services. This corresponds well with the stated primary task of the therapy to "facilitate change from patient status to that of an ongoing person". The emphasis in the selection of patients for the study was on capacity and motivation for change. The study suffers from high attrition at follow-up, making any conclusions concerning sustainability difficult.

Sigrell's study in 1992 specifically addresses this failure in the literature to achieve satisfactory measures for long-term follow-up. This is a prospective uncontrolled study of 18 patients treated in three closed outpatient groups over two years. Interviews took place before and six months after therapy. Follow-up measures were then done at 1.5 and 13 years, again by interview. The emphasis was on optimal change from a psychodynamic perspective, using a method similar to that described by Malan and colleagues. At 18 months, 17 of the 18 patients showed a successful result. At 13 years, this had fallen to 12 out of 18 patients. It is noted that 4 of the 6 patients who reverted in the intervening years had a diagnosis of borderline or narcissistic personality disorder; thus it was postulated that group therapy, although initially successful in these kinds of patients, did not show a capacity to help them achieve an improvement that could be sustained over more than a decade. However, the study does fail to discuss the impact of life events that may be a confounding variable in such a long follow-up period.

Heinzel, Breyer, and Klein (2000) used a one-page self-assessment questionnaire to study retrospectively 1,000 former outpatients who had received group or individual therapy two years previously. Both forms of treatment showed a significant improvement in their health status during and after therapy and a reduction in their use of services and in "days off" caused by ill health.

Sharpe, Selley, Low, and Hall (2001) used a prospective uncontrolled study for 27 male childhood sexual abuse survivors who entered a slow-open group over a 28-month period. There was a significant reduction in depression and anxiety during therapy, but at six months, depression had returned.

The efficacy of a treatment is measured in the controlled environment of the appropriate study method. The selection of patients can, for instance, be very tightly dictated by the trial's exclusion criteria. This leads to an often-heard criticism—namely, that efficacy studies do not reflect the real working life of the clinician. Clinical effectiveness studies attempt to redress this problem by studying clinical populations. Addressing the issue of clinical effectiveness compared to efficacy, Lorentzen, Bogwald, and Hoglend (2002) describe an "effectiveness study of real patients in a private psychiatric practice". A total of 69 patients in long-term analytic group psychotherapy were studied pre and post treatment and one year after termination. Standardized measures of social functioning, symptoms, and interpersonal problems were used. The average time in treatment was 32.5 months. The patients improved significantly during therapy, and the improvement continued at follow-up: 86% of the patients had a clinically significant change in psychosocial functioning and 61% in interpersonal and symptomatic distress.

A soon to be published review from a group in Sheffield is entitled "A Systematic Review of the Efficacy and Clinical Effectiveness of Group Analysis and Analytic/Dynamic Group Psychotherapy" (Blackmore et al., in press). The authors assess the efficacy and effectiveness of group analysis and group psychotherapy and evaluate the evidence on the numbers and types of patients using groups. This includes the size of groups, the numbers of patients, and the duration of therapy.

While other outcome studies for group therapy are certainly reported in the literature, the majority of these are either for specific diagnoses and settings (e.g., Bateman & Fonagy, 1999, 2001, for borderline personality disorder in day hospital settings; or Valbak, 2001, for bulimia) or for much briefer group therapy (e.g., Conway, Auden, Barkham, Mellor-Clark, & Russell, 2003, for a 12-week period in an intensive day-unit setting). An interesting preliminary report on a multi-centre study of outpatients was reported by Tschuschke and Anbeh in 2000, comparing the early effects of analytic, psychodrama, and eclectic group therapy. After 3 to 4 months, patients in different therapies seemed to have equivalent effect sizes. This study appeared to lead to some conflicts in the research net-

work, fuelling debate in the journal *Group Analysis*. Further results have not yet been published.

Turning briefly to the *process research*, there is a body of work that examines therapist and patient factors that may contribute to outcome in psychotherapy. However, the number of good-quality process research trials for analytic group therapy is still small. Looking at the more generic psychotherapy process research, the kinds of therapist factors that have been studied include experience, training, competence, and adherence to the theoretical model. (For a more complete view of this complex area, Roth and Fonagy's 2005 edition of *What Works for Whom* is recommended.) Their summary of recent research suggests that adherence and outcome are probably only weakly related. The evidence suggests that greater competency improves outcome, although this suggestion still needs to be refined. More training and greater competency are perhaps particularly important with more difficult patients, when the therapist may be required to deviate appropriately from the technical recommendations. This links with the small amount of existing evidence that more disturbed patients do better with more experienced therapists. The use of a manual reduces the outcome variance attributable to differences between therapists. The variance is also reduced when more experienced therapists conduct the therapy.

The majority of trials indicate a small but robust link between therapeutic alliance and outcome across different interventions. For group analytic therapy, Lorentzen, Sexton, and Hoglend (2004) showed that "early therapist ratings of the alliance and an early development of concordance between patient and therapist alliance ratings, were both related to symptomatic outcome".

Patient factors that predict outcome would potentially allow one to match patients to therapies, and this would greatly advance efficacy and efficiency. There is, however, little evidence yet to aid this endeavour. However, variations in the quality of *object relations* appear to be more powerful as predictors of treatment response than other patient features such as gender, class, or ethnicity.

Lambert (2004) addressed the question of what it is that contributes to improvement from group analytic therapy. From a list including formal change theory, patient factors, structural factors, leader factors, and small group processes, he suggests that only 15% of improvement is caused by theoretical orientation/techniques; 15% derives from placebo effects, 30% from human factors, and 40% from "extra-therapeutic factors".

Lorentzen, Sexton, and Hoglend (2004) found that a treatment duration of up to 2.5 years of analytic group therapy was a strong positive predictor of outcome. Other process research has attempted to use a repertory grid to determine at the onset of therapy patients likely to be successful (Catina & Tschuschke, 1993) or alternatively a Bionian coding manual (Kapur, 1993) for the same purpose. Colijn, Hoencamp, Snijders, Van Der Spek, and Duivenvoorden (1991) used a questionnaire based on Yalom's curative factors and determined that only the factor of identification was highly predictive. Marziali, Munroe-Blum, and McCleary (1998) showed "alliance" and "cohesion" to be significant.

This brief overview returns us to the initial quotation from Barbara Dick and the reality that even after so many years, good empirical research studies in group-analytic therapy or psychoanalytic group therapy still remain small in number. However, there is a growing recognition that these are needed in the political and financial climate of the NHS in the twenty-first century. In 2002 David Carter reviewed the current state of group-analytic treatment in the world of evidence-based practice in psychological therapies. His conclusions point to the need for a more thoroughly grounded theoretical model that would allow a basis for research if group analysis is to survive. Without this in place, he argues, qualitative research is the initial prerequisite to "build a more clearly defined group-analytic theory and proving its worth".

This places this current volume firmly in place as a grounding in the theory underpinning the practice of *psychoanalytic group therapy*; moreover, it contains a Manual that will help to reduce the outcome variance between less experienced therapists and, one hopes, will provide the basis for much needed future outcome research.

PART III

GROUP RELATIONS AND THE WIDER WORLD