

Psychodynamic psychotherapies: Evidence-based practice and clinical wisdom

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This article is based on an exhaustive review of the psychotherapy outcomes literature, undertaken originally at the instigation of the UK Department of Health by Roth and Fonagy (Department of Health, 1995). We have recently updated this review (Fonagy, Target, Cottrell, Phillips, & Kurtz, 2002; Roth & Fonagy, 2004) and extended it to identify all studies of psychoanalytic psychotherapy. The usual methods for identifying studies were employed (Fonagy, Target, et al., 2002; Roth & Fonagy, in press). The key questions that should be asked of this literature given the current state of research in this area (also see Westen, Morrison, & Thompson-Brenner, 2004) are: Are there any disorders for which short-term psychodynamic psychotherapy (STPP) can be considered evidence-based, Are there any disorders for which STPP is uniquely effective as either the only evidence-based treatment or as a treatment that is more effective than alternatives, and Is there any evidence base for long-term psychodynamic psychotherapy (LTPP) either in terms of achieving effects not normally associated with short-term treatment or addressing problems that have not been addressed by STPP? In this context, short-term therapy is conceived of as a treatment of around 20 sessions delivered usually once weekly. (Bulletin of the Menninger Clinic, 69[1], 1-58)

From the standpoint of psychodynamic psychotherapy, the database of research studies has significant limitations. Westen and colleagues (2004) recently offered a powerful critique of the research methods

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used to assign the status of “empirically supported or unsupported therapies.” Research that is considered empirically supported tends to have three characteristics: (1) studies address a single disorder (usually Axis I) with diagnostic assessments to ensure homogeneity of samples, (2) treatments are manualized and are of brief and fixed duration to ensure the integrity of the “experimental manipulation,” and (3) outcome assessments focus on the symptom(s) that represent the declared priority of the study (and often the intervention). The underlying aim is the maximization of internal validity by random assignment, controlling confounding variables, and standardizing procedures. Westen et al. (2004) identify four poorly supported assumptions underpinning the application of randomized controlled trial (RCT) methodology to psychotherapy research: (1) that psychopathology is so malleable that a brief intervention is likely to change it permanently, (2) that most patients can be treated for a single disorder or problem, (3) that psychiatric disorders can be treated with psychosocial interventions without regard to personality factors that are less likely to change with brief treatments, and (4) that experimental methods provide a helpful “gold standard” for evaluating these packages. In reality, most forms of psychopathology encountered in specialist centers are treatment resistant (Kopta, Howard, Lowry, & Beutler, 1994) and comorbid with other disorders (Kessler, Stang, Wittchen, Stein, & Walters, 1999) that need to be tackled in the broader context of the patient’s personality structure (e.g., Thompson–Brenner, Glass, & Westen, 2003), and experimental methods need to be supplemented by correlational analysis to ascertain the effective components of treatment (Ablon & Jones, 2002).

Methodological considerations

Evidence for evidence-based practice

This review is based on what funders and researchers currently regard as appropriate evidence (Clarke & Oxman, 1999). The criteria that are used to determine what counts as evidence-based practice must themselves be empirically tested. Their specificity (the likelihood of falsely identifying a treatment as effective) and sensitivity (the chance of misclassifying an effective treatment as ineffective) should be established against a variety of other public health criteria. The same empirical standards should be applied to these criteria as would be expected in association with other clinical decision-making tasks. Face validity, which is what we currently have, is clearly insufficient. Treatments designated as “evidence-based” by some criteria must be distinguishable from treatments that do not meet these criteria on several concurrent in-

dependent but relevant indicators such as theoretical coherence, public health impact, and user/consumer acceptability.

The absence of evidence or evidence of ineffectiveness

Current categorization in evidence-based psychotherapies conflates two radically different groups of treatments: those that have been adequately tested and found ineffective for a client group, and those that have not been tested at all. It is important to make this distinction, because the reason that a treatment has not been subjected to empirical scrutiny may have little to do with its likely effectiveness. It may have far more to do with the intellectual culture within which researchers operate, the availability of treatment manuals, and peer or third-party payer perceptions of the value of the treatment (which can be critical for both research funding and publication). British psychodynamically oriented psychiatrist Jeremy Holmes (2002) has eloquently argued in the *British Medical Journal* that the absence of evidence for psychoanalytic treatment should not be confused with evidence of ineffectiveness. In particular, his concern was that cognitive therapy would be adopted by default because of its research and marketing strategy rather than its intrinsic superiority. He argued that: the foundations of cognitive therapy are less secure than often believed; the impact of cognitive-behavioral therapy (CBT) on long-term course of psychiatric illness is not well demonstrated; in one "real-life trial," at least the CBT arm had to be discontinued because of poor compliance from a problematic group of patients who nevertheless accepted and benefited from couples therapy (Leff et al., 2000); the effect size of CBT is exaggerated by comparisons with waiting-list controls; and a post-cognitive behavioral therapy approach (e.g., Teasdale et al., 2000; Young, 1999) has emerged that leans increasingly on psychodynamic ideas.

Although we are entirely in sympathy with Jeremy Holmes' perspective, even if our work with Tony Roth was part of the target of his criticism, it is only fair to expose the shortcomings of his communication. Nick Tarrier (2002), in a commentary on Holmes' piece, writes with passion:

Holmes relies on the specious old adage that absence of evidence is not evidence of absence [of effectiveness]. . . . I would have more enthusiasm for this argument if traditional psychotherapy were new. It has been around for 100 years or so. The argument, therefore, becomes a little less compelling when psychotherapy's late arrival at the table of science has been triggered by a threat to pull the plug on public funding because of the absence of evidence. (p. 292)

Tom Sensky and Jan Scott (2002) were similarly outraged, both by Holmes' selective review of evidence and by his allegations that some cognitive therapists are starting to question aspects of their discipline. The message from the CBT camp is this: If psychodynamic clinicians are going to address the issue of evidence-based practice, they will have to do more than gripe and join in the general endeavor to acquire data.

Of course, psychodynamic clinicians are at a disadvantage and not simply because they are late starters (after all, many new treatments find a place at the table of evidence-based practice). There are profound incompatibilities between psychoanalysis and modern natural science. Paul Whittle (2000) has drawn attention to the fundamental incompatibility of an approach that aims to fill in gaps in self-narrative with cognitive psychology's commitment to minimal elaboration of observations, a kind of Wittgensteinian cognitive asceticism. The making of meaning around a life narrative is fundamental to human nature. It is therefore inconceivable that psychoanalysis (or a process very much like it) will ever *not* be part of the range of approaches that people with mental health problems desire. However in this context, success is measured as eloquence (or meaningfulness), which is not reducible to either symptom or suffering. Moreover, psychoanalytic explanations invoke personal history, but behavior genetics has brought environmental accounts into disrepute. Although cognitive-behavioral therapy also has environmentalist social learning theory at its foundations, it has been more effective in moving away from a naïve environmentalist position. To make matters worse, within psychoanalysis there has been a tradition of regarding the uninitiated with contempt, scaring off most open-minded researchers. The engagement of psychoanalytic clinicians in research programs is a desirable goal.

Psychoanalysts are more than ever before, but not yet fully, committed to systematically collecting data with the potential to challenge and contradict as well as to confirm cherished ideas. The danger that must be avoided at all costs is that research is embraced selectively only when it confirms previously held views. This may be a worse outcome than the wholesale rejection of the entire enterprise of seeking evidence because it immunizes against being affected by findings at the same time that it creates an illusion of participation in the virtuous cycle of exploring, testing, modifying, and reexploring ideas.

The scope of the task

Most UK evidence-based treatment reviews have been uniquely based on RCTs. RCTs in psychosocial treatments are often regarded as inadequate because of their low external validity or generalizability (Anonymous, 1992). In brief, it is claimed that they are not relevant to clinical

practice—a hotly debated issue in the field of psychotherapy (Hoagwood, Hibbs, Brent, & Jensen, 1995) and psychiatric research (Olfson, 1999). There are a number of well-publicized reasons, which we have no time to go into here, why randomized trials in many areas of health care may have low external validity: (1) the unrepresentativeness of health care professionals participating, (2) the unrepresentativeness of participants screened for inclusion to maximize homogeneity, (3) the possible use of atypical treatments designed for a single disorder, and (4) limiting the measurement of outcome to the symptom that is the focus of the study and is easily measurable but practically irrelevant dimensions (Fonagy, 1999a).

RCTs cover only a limited number of treatments, and many treatments remain unevaluated in relation to many conditions. Because there are at least 200 disorders of child and adult mental health and many hundreds of different forms of intervention, most of which have many components and many characteristic patterns of delivery, it is inconceivable that a matrix of types of therapy by types of disorder could ever be populated by appropriate studies (Goldfried & Wolfe, 1996). This is no trivial issue. Studies that attempt to identify which component of a treatment program is essential to its success frequently find that apparently most of the layers of the onion can be removed and the effect is still there. Because outcome studies rarely help identify either the effective elements of treatments or the process of change that leads to improvement, many traditional influential supporters of outcome investigations are calling for fewer rather than more outcome studies. Alan Kazdin (1998), for example, recommended a “dismantling” strategy that one by one removes potential components of change until the genuinely effective component is identified. Some have suggested that meta-analyses might offer a direct solution to this problem (e.g., Borkovec & Ruscio, 2001), but this is by no means a straightforward task. A recent meta-analysis by Wilson McDermut and colleagues (2001) identified group therapy as efficacious for depression with average effect size of 1.06. However, the group treatments involved teaching a wide range of different strategies in different studies (self-control techniques, problem-solving skills, relaxation skills, disputation of negative thinking). Was teaching a skill truly addressing a depression-related deficit? Matching patients to treatments that emphasized the specific deficits they presented with did not increase effect size, and even attention-control groups resulted in a reduction of symptoms. Yalom (1995) outlined 11 therapeutic factors in group therapy, but none of the studies reviewed by McDermut et al. discussed any one of these. We simply do not know what aspect of group treatments for depression makes them so effective.

Beyond these fairly well publicized issues, the question arises of whether manualized treatments or treatment packages are the appropriate level of analysis in our search for effective interventions. For example, a study by Olfson and colleagues (1998) followed up schizophrenic patients discharged from hospital and found that patients who had contact with the outpatient clinician prior to discharge were better off in terms of symptom reduction than those who had no communication with outpatient staff. Similar, apparently minor, process parameters of care may be far more important in determining outcome than entire treatment packages. It is hard to imagine that a sufficient number of RCTs could ever be performed to assess all such potentially key parameters of care. *Evidence-based practice needs to look beyond the current database and look at practice-based evidence in order to comprehensively establish evidence-based practice.*

The ideal outcome research program

Alan Kazdin (1998) proposed the ideal outcome research program more than 4 years ago, but because it would require us to rethink our entire approach to outcome studies and evidence-based practice, it is unlikely ever to be implemented. Basically, he suggests that treatment research should begin with the *identification of key dysfunctions* associated with a disorder and the empirical demonstration of these dysfunctions in a sizable proportion of the clinical group. Further, *a conceptual link* must be established between a proposed treatment method and the dysfunctional mechanism hypothesized to underpin the disorder. Only when this has been done can *manualization* commence, followed by *the collection of the hierarchy of evidence* that forms the body of systematic reviews. *Process–outcome studies* can then be implemented to establish key treatment components and necessary treatment length. Experimental studies of hypothesized processes and mechanisms need to confirm the correlational findings from process–outcome investigations. Finally, the boundary conditions for the treatment need to be established, in terms of patient and environmental characteristics that promote or undermine the effectiveness of the therapy. You will notice that this is a radically different approach from the one normally undertaken where the starting point is the evaluation of a designated treatment. Currently, the identification of key psychological processes is at best post hoc. No wonder there are too many different treatment modalities. No wonder that we know so little about why any of them work. Reversing this process would be a remarkable achievement of scientific administration.

Pragmatic trials

The answer to the controversy between efficacy and effectiveness studies of psychotherapy may lie in so-called pragmatic or "real-world" trials. These minimal effort trials require experimentation in addition to ongoing outcomes measurement. The experimental component of pragmatic trials includes randomization to alternative methods of care. Importantly, nonspecific aspects of care are controlled under these circumstances, yet questions of direct relevance to the clinicians may be asked and answered. Patients who participate naturally reflect the clinical population, and exclusion criteria are kept to a minimum. Comparison treatments are with routine practice, which usually involves combination treatments and treatments titrated according to the client's response. The pragmatic trial imposes minimal constraints on management. The only major sacrifice to internal validity is the loss of blindness in assessment. Blindness, which is likely to be imperfect in psychosocial treatments in any case, may offer little advantage as regards objectivity of outcome assessment. Double-blindness imposes unrealistic restrictions even on routine pharmacological care, and deviations from normal practice threaten the validity and generalizability of any cost data used in the estimation of cost-effectiveness. Concealment of allocation (the prevention of foreknowledge about the group to which the patient will be allocated if recruited), which is an important source of selection bias, is readily achievable in this context. The unique feature of such trials lies in the relevance of the questions that clinicians may ask of their routine practice. Ideally, clinical equipoise (genuine uncertainty concerning outcome) should drive the search for evidence. In evidence-based practice, clinical curiosity is sadly rarely the motivator.

Pragmatic trials could be a key additional line of information for evidence-based practice. In combination with more rigorous RCTs (particularly relevant to new treatments) and the judicious use of observational data, they will provide evidence of sufficient richness to significantly advance standards of mental health care. The establishment and support of a profession-wide methodology for pragmatic trials should be considered an important additional task of evidence-based practice psychotherapy initiatives.

Clinical guidelines

Closing the gap between practice and evidence brings us to another quantum leap in the sophistication with which evidence for psychotherapeutic clinical psychological services is considered. It is increasingly recognized that evidence does not speak for itself and to be usefully applied it needs to be reviewed and integrated by a group of un-

biased experts, including individuals whose expertise is as users and carers. Clinical guidelines that integrate evidence and front-line experience drawn up by multidisciplinary panels, which as a collection of individuals have full awareness of the limitations of everyday clinical practice, is a key step that has all too often been omitted in the past when the sole experience applied to the interpretation of evidence was one of management.

Research, with its focus on selected patient populations, cannot, of course, tell clinicians what to do with specific individuals. Clinicians have to ask the research database specific questions with an individual client in mind. How to pose this massive accumulation of data such questions and, even more challengingly, how to obtain meaningful answers? These are far more complex skills than that of generating a systematic review. Many hope that clinical guidelines can and will perform the role of translation of research into practice increasingly well. The controversy that surrounds this issue is beyond the scope of this article. It is perhaps sufficient for us to say that we cannot see guidelines, however sophisticated, ever substituting for clinical skill and experience any more than the Highway Code can substitute for skilled driving. Future research should perhaps look also at the skill with which clinicians implement particular treatments and the relationship of that to patient outcome.

In addressing the failure of translation of guidelines into clinician behavior (Chilvers, Harrison, Sipos, & Barley, 2002; Higgitt & Fonagy, 2002), it is useful to differentiate between "diffusion," "dissemination," and "implementation" (Palmer & Fenner, 1999). These are interrelated and increasingly active phases of a process. Publication in a journal article (diffusion) is a passive form of communication, haphazard, untargeted, and uncontrolled (seemingly insufficient to achieve much in the way of change in clinical practice). The development of practice guidelines, overviews, etc. is more active and targeted to an intended audience (dissemination). Implementation is yet more active, with sanctions and incentives, monitoring, and adjustment to local needs. The methods for translating guidelines into practice range from use of written materials, through educational efforts, via product champions, financial incentives, and patient-mediated interventions through to reminder systems. Notwithstanding problems of the currency of guidance, there is a very real question about the extent to which guidance is utilized. At a recent Australian meeting to review the fate of 14 guidelines, none were found to have fared well. The shorter they were, the more likely they were to have had a noticeable impact. Successful implementation was most likely if it was initiated at a local level.

The brain and the mind

The question is not, "Is psychotherapy still relevant in the days of Prozac?" but how can we make it really count against a background of increasingly fragmented social support systems, and how can we use it efficiently where current medication is not enough? Drugs and psychotherapy both work to the extent that they do because they both affect the functioning of the brain. How else could it be, because the brain is the organ of the mind? And diseases of the mind are unequivocally diseases of the brain. The outcome of psychotherapy therefore should be as easy (or easier) to measure in terms of brain function as in terms of behavior or subjective reports.

But the brain is not the final frontier of our knowledge about the mind. Emotions may be crudely changed by drugs but without giving any meaning to the experience of mental disorder, or to the drug-induced change. Psychotherapy is the crystallization of the principle of psychological causation—that mental disorder in many instances may be most usefully seen in psychological terms, as the product of specific beliefs, desires, and emotions. Without understanding mental disorder psychologically, it would be impossible to understand the self-evident social pathways to mental disorder: Poverty, unemployment, incest, homelessness, spiritual despair at the violence and heartlessness of abusive parenting, the almost limitless methods that people can find of inflicting human misery—all these ignominies influence people's expectations about others, the trust that they may be capable of feeling, their anger about their treatment, the complex ways we all find to learn to live in the social context that the fortunes or misfortunes of our birth have presented for us. Turning away from therapy could mean shutting our ears to such anguish. Psychotherapists blew the whistle on the prevalence and long-term impact of child maltreatment. It is the feelings, wishes, beliefs, thoughts, and desires in the wake of despair that psychotherapy must be retained to address.

Severe social disadvantage increases the risk of mental disorder by orders of magnitude. That this is a psychological rather than a merely physical process is borne out by the predictive power of relative rather than absolute destitution. The further down the social pile, the greater the misery, regardless of the actual material wealth of the individual. It is felt disadvantage that is psychologically toxic. Of course, in this, and every other, case, it can be argued that experience of deprivation is a brain state, but the logical conclusion from that argument was foreshadowed in Huxley's *Brave New World*. No one would seriously suggest that the most appropriate ethical way of addressing the gigantic issues raised by social disadvantage might be the suppression by medication of the misery of social exclusion.

But the native land of psychotherapy, the mental world of beliefs, desires, and emotions—central for a while in 20th-century psychiatry—is a fragile creature. Maintaining the causal significance of meaning runs the gauntlet of a powerful human need for concreteness and simplicity, which the physics and biology of brain research represent. Only a psychological Luddite would ignore the immense benefit we have drawn from brain research. But we would be equally deluded were we to deny its cost. Brain research has affected our culture. The answers it has provided, when translated into exuberant media sound bites, have undermined the ethos of seeking for psychological meaning in the way therapists work with clients. The natural human desire to create a narrative, a story, around one's experience has given way under economic as well as political pressures to a profound disrespect for the mental, born less of disillusionment with the efficacy of psychotherapy than of the reductionism of certain biological psychiatrists. The United States over the past 15 years has witnessed a devastation of concern with the psyche. Many HMOs (protectors of insurance company profit margins) will fund either no psychotherapy or at most half a dozen sessions. Not surprisingly, and coincidental with the biological revolution in psychiatry, spending on mental health care in the U.S. has decreased in real terms by a factor of 50%.

Psychotherapy is essential to whole-person mental health care. We cannot afford to abandon it if we are to offer meaningful and respectful care to those in distress. Our concern here is with the risk of irreparable, even if unintended, damage to a perspective that enshrines in psychiatry what is unique about our species—that we recognize in each other the presence of a mind, the presence of emotions, wishes, and beliefs as the motivators of behavior, adaptive or maladaptive. Turning away from psychological therapy, from the truth of the importance of mind, risks apocalyptic cultural and social changes, inevitable if we mock personal experience and start to deride the feelings, thoughts, and desires of our fellow humans.

Science and scientism

Finally, a word about science and scientism in psychotherapy research. We all have a need for certainty, experience discomfort with not knowing, and risk anxious retreat from ignorance into pseudoknowledge (so characteristic of the early years of medicine). A scientific approach has obviously been incredibly helpful and has saved many millions of lives. To argue against it is not just churlish; it is clearly unethical and destructive. But to argue for a mechanical reading of evidence, as some clinical psychologists have done (Chambless & Hollon, 1998; Chambless et al., 1996), equally skirts the risk of doing harm.

Research evidence collected as part of the present initiative will need to be carefully weighed. Multiple channels for evaluation are needed, and they need to be kept open and actively maintained. No self-respecting clinician will change his or her practice overnight. The clinician would be unwise to do so. Evidence has to be read and evaluated, placed into the context of what is possible, desirable, and fits with existing opportunities. It should be remembered that in mental health at least, but also probably in most areas of clinical treatment, method accounts for a relatively small proportion of the variance in outcome relative to the nature of the patient's problem (Weisz, Weiss, Han, Granger, & Morton, 1995; Weisz, Weiss, Morton, Granger, & Han, 1992), which may well interact with the skills of the attending clinician. This latter form of variance is to be cherished, not only because that is where the art of medicine lies, but also because it is in the study of that variability that future major advances in health care may be made, as long as we can submit these to empirical scrutiny.

Major depression

About 20 psychodynamic psychotherapy trials have been published dealing with the treatment of depressive and anxiety disorders or symptoms (Crits-Christoph, 1992; Leichsenring, 2001). Along with other therapies, it has been shown to have better effectiveness in open trials or compared to waiting-list (Shefler, Dasberg, & Ben-Shakhar, 1995) or outpatient treatment in general (Guthrie et al., 1999). In the light of relatively readily available alternative treatments, the critical demonstrations concern that of an equivalence, or perhaps even superiority, to alternative treatment approaches.

Reviews

There have been two recent relevant reviews of the literature (Churchill et al., 2001; Leichsenring, 2001). In addition, the National (England and Wales) Institute for Clinical Excellence (NICE) is conducting a systematic review in order to produce guidelines for treatment of depression within the National Health Service. The Churchill et al. review concerned treatments for depression of 20 sessions or less published up to 1998. Of the studies suitable to include in meta-analysis, six involved psychodynamic therapy. Improvement was found to be over twice as likely with CBT as it was with psychodynamic therapy. However, to conclude from this that CBT is superior to psychoanalytic psychotherapy in the treatment of depression may be premature in the light of the following considerations: There was no superiority of CBT over other therapies where follow-up was available. Differences between

CBT and other therapies were limited in severely depressed groups. A number of therapies identified in the review as "psychodynamic" were not "bona fide" therapies (Wampold, 1997). In an earlier meta-analysis by Gloaguen and colleagues (1998), which similarly concluded that CBT was superior to other therapies, the superiority of CBT could no longer be demonstrated once interventions without scientific base were removed from the comparisons (Wampold, Minami, Baskin, & Callen-Tierney, 2002). Meta-analytic reviews should not confound estimates of the effectiveness of STPP with those of "non-bona fide" therapies.

A more positive picture apparently emerges from the review by Leichsenring (2001). This review identified six RCTs that contrasted manualized STPP and CBT (Barkham, Rees, Shapiro, et al., 1996; Elkin, 1994; Gallagher-Thompson & Steffen, 1994; Hersen, Bellack, Himmelhoch, & Thase, 1984; Shapiro et al., 1994; Shapiro et al., 1995; Shea et al., 1992; Thompson, Gallagher, & Breckenridge, 1987). The review concludes that the two forms of therapy are not substantially different because only one of the studies reviewed suggests a possible superiority of CBT. We calculate the overall risk ratio (RR) for the comparison to be .91 (95% confidence interval [CI]: .77 to 1.06), indicating that CBT is only 9% more likely to generate remission than STPP. Although not reported by Leichsenring, meta-analytic comparison of follow-up data available for these studies actually reveals a slight superiority for CBT (RR = .82; 95% CI: .70, .96). This indicates that CBT increases the chance of continued remission by 20% more than STPP.

We should consider the possibility of selection bias in this review. Leichsenring includes the NIMH Collaborative Depression Trial in the meta-analysis, which is, to say the least controversial, because Interpersonal Psychotherapy (IPT) was included as an STPP merely because the therapist was psychodynamically trained (Elkin, 1994; Shea et al., 1992). Because neither the developers nor other reviewers consider IPT to be a psychodynamic therapy, it seems wiser not to include it in reviews of STPP. Even if this study is excluded, the superiority of CBT over STPP remains (RR = .82, 95% CI: .70, .96). However, the remaining four studies include a trial of social skills training relative to STPP (Hersen et al., 1984) and a study of CBT offered to carers (Gallagher-Thompson & Steffen, 1994), neither of which seem relevant to the assessment of the relative effects of CBT for depression. Of the two studies remaining, one was group rather than individual therapy carried out with an older adult population (Thompson et al., 1987). The most appropriate conclusion at this stage might be that a meta-analysis of this literature is premature.

Consideration of individual studies contrasting psychodynamic therapy with other psychotherapies

This conclusion reflects the regrettable fact that there are fewer controlled studies of psychodynamic therapy for depression than one might expect given the wide use of this treatment for this problem. In fact, most investigators employ STPP as a contrast to an alternative therapy to which they have an allegiance. Not surprisingly, dynamic therapy is usually found to be significantly less effective (Covi & Lipman, 1987; Kornblith, Rehm, O'Hara, & Lamparski, 1983; McLean & Hakstian, 1979; Steuer et al., 1984). The researchers' lack of investment in the "control" treatment means that the findings from these investigations should be treated with great caution. Better designed and implemented studies report no difference between CBT and STPP (Bellack, Hersen, & Himmelhoch, 1981; Thompson et al., 1987). Nevertheless, small sample size and design weaknesses argue against basing conclusions concerning psychodynamic therapy for depression on these investigations. Two major studies stand out as sound comparisons of a cognitive-behavioral and a psychodynamic approach to the treatment of depression because of their size, the quality of randomization, the care of implementation, baseline assessment and outcome measurement, and clarity of the therapeutic interventions assessed. These are: (1) The Sheffield Psychotherapy Project and (2) The Helsinki Psychotherapy Study.

The Sheffield Psychotherapy Project

The Sheffield Psychotherapy Project (Barkham, Rees, Stiles, et al., 1996; Shapiro et al., 1994; Shapiro et al., 1995) randomized 169 patients meeting criteria for MDD to CBT or STPP. Of these patients, 117 completed the study; 103 were followed up at 1 year. Treatment was brief (8 or 16 sessions). Most would agree that 8 sessions represents a "subclinical dose" of STPP. In line with this, those who received only 8 sessions of psychodynamic therapy were doing less well at 1 year than those with 8 sessions of CBT. At 16 sessions, the two treatments appeared to be equally effective (RR = .93, 95% CI: .68, 1.27). A further important finding was that those with more severe depression at the start of treatment were less likely to maintain gains in both arms of the trial. Patients with severe depression (Beck Depression Inventory [BDI] score >27) appeared to require longer treatment regardless of treatment type. About 30% of patients remained asymptomatic from posttreatment to 1-year follow-up. This interaction, however, was not found in a small-scale community-based attempt at replication (Barkham, Rees, Shapiro, et al., 1996). Overall, neither of these short-term treatments (CBT or STPP) appeared to be strikingly effec-

tive in the medium term, but STPP performed broadly comparably to CBT.

The Helsinki Psychotherapy Study

The fullest assessment of psychodynamic psychotherapy for mood disorder thus far comes from the initial report of the Helsinki Psychotherapy Study (Knekt & Lindfors, 2004). This in many ways exemplary randomized study compared a problem-solving solution-focused therapy (SFT) to short- and long-term psychodynamic psychotherapy. The study also had an arm for full psychoanalysis, to which subjects were self-selected and additionally screened for suitability. So far, only findings related to the two short-term arms of the trial are available.

SFT is a relatively novel approach (Lambert, 1998) that emphasizes the collaborative efforts of patient and therapist to identify a problem and find solutions to it. It is administered once every second or third week up to a maximum of 12 sessions (the actual number of sessions in the trial was 10 over about 7.5 months). STPP following Malan's (1976) approach was scheduled over 20 once-weekly sessions over 5–6 months (actual number of sessions in the trial was 15 over a period of about 6 months). Data were collected on auxiliary treatments such as psychotropic medication and the additional use of psychotherapy services. An unusually wide range of assessments are administered at various time points (five assessment points in the first year) up to 60 months after baseline measurement to pick up the long-term and “meta-symptomatic” changes believed to be associated with STPP.

A total of 638 patients were referred from clinical services (not recruited for the study by advertisement or the like), of whom 381 were eligible and willing to participate in the study. Of these, 97 patients were allocated to SFT, 101 to STPP, and 128 to LTPP. More than 82% of the sample met *DSM* criteria for depressive disorder and 43% for some type of anxiety disorder; 57% had mood disorder alone and 14% had anxiety disorder alone. The sample appears moderately severe (average GAF score was 55, and SCL-90 GSI was 1.29), slightly less severe than the Sheffield sample. The average HAM-D score was 15.7. Surprisingly, only about a fourth of the patients were on any kind of psychotropic medication.

Given the careful design and implementation of complex methodology and the exceptionally large sample size (adequate power to detect a 20% difference), the observation of an absence of significant differences between the two groups is impressive. The reduction in depression and anxiety scores was relatively large (e.g., the BDI decreased by almost 50% (from 18 to 10) and the Hamilton from 15 to 11 at 12 months. The average GAF increased to about 65. On most of the mea-

asures, the gains were apparent by 7 months, and remained stable thereafter. About 30% of the 82% of patients who were suffering from a mood disorder lost the diagnosis by 7 months and changes after that were no longer statistically significant. Both forms of therapy showed about 20% of recovery from personality disorders by 7 months. This increased to 46% in the STPP group by 12 months, but in SFT the percentage recovered from personality disorder diagnosis did not increase ($RR = 2.1$; 95% CI: 1.36, 3.25). However, we cannot make too much of this difference given the absence of adjustments for Type I error and the great number of comparisons carried out. Measuring recovery in terms of achieving below clinical cut-point scores on the BDI, and Hamilton ratings, SFT appeared to achieve somewhat more change faster, although again there were no significant differences after the 9-month time point ($RR_{BDI@9months} = .49$; 95% CI: .27, 0.88). Work ability index improved for both groups, but only by about 10%, and this was achieved by 7 months with little change thereafter. Interestingly, days of sick leave was reduced significantly for both groups. Similarly, a range of measures on social functioning improved over the first 7 months but produced no notable change later, and no differences were seen between the treatment groups.

In summary, the trial demonstrated rapid and generally similar decreases on self-reported and observer-rated depressive symptoms during the first months of therapy and less prominent reductions in symptoms later. Remission was somewhat more rapid with SFT, and continued recovery from a personality disorder diagnosis was more marked for STPP. The rate of recovery reported here is comparable to those found in other studies involving CBT. Broadly, the study demonstrates that a fairly generic form of brief psychoanalytic psychotherapy is as effective in the treatment of depression as a less generic but previously established as empirically supported form of CBT (Mynors-Wallis, Davies, Gray, Barbour, & Gath, 1997; Mynors-Wallis, Gath, Day, & Baker, 2000; Mynors-Wallis, Gath, Lloyd-Thomas, & Tomlinson, 1995). The study was unusual in the breadth of outcomes covered. It permitted the observation that recovery of work ability, social functioning, and personality functioning appears to be far smaller than reductions of acute psychiatric symptoms (e.g., Pre-Post $SMD_{BDI} = 9.65$; 95% CI: 8.66, 10.64; Pre-Post $SMD_{work} = 3.33$; 95% CI: 3.79, 12.84). Measurement in social adjustment domains does not appear to advantage the less symptom focused STPP. This is important in the interpretation of other trials. The limited gain observed in social adjustment is consistent with current models of change in psychotherapy (Howard, Lueger, Maling, & Martinovitch, 1993).

The absence of a no-treatment control group in this study makes it hard to estimate the proportion of observed reductions in symptoms that was not due to psychotherapy. However, because effective treatments for depression exist, randomization of patients to a purely placebo arm for a 12-month period would be unethical: Adequately designed studies will inevitably lack a no-treatment control arm. The treatments were well described but not fully manualized. Adherence is not reported.

Consideration of individual studies contrasting psychodynamic therapy with pharmacotherapy

When each is offered alone, psychotherapy and medication are of equivalent efficacy, and psychotherapy is rarely superior (Roth & Fonagy, in press). There is evidence of "added value" when psychotherapy is added to medication, but most of this evidence is from IPT and CBT studies. Two recent studies assessed adding STPP to an antidepressant regimen. A Dutch study (de Jonghe, Kool, van Aalst, Dekker, & Peen, 2001) assigned 84 patients to antidepressant and 83 to combined therapy. The pharmacotherapy protocol started with fluoxetine and allowed a switch to amitriptyline if participants were nonresponsive and finally to moclobemide. The psychotherapy was a psychoanalytically oriented brief supportive therapy associated with the work of the first author (de Jonghe, Rijnierse, & Janssen, 1994). An unexpectedly large number of patients refused pharmacotherapy alone, so of the 167 patients randomized, 57 started in pharmacotherapy and 72 in combined therapy. Intent-to-treat remission rate in the combined psychodynamic psychotherapy and pharmacotherapy group on the HDRS was 37%, whereas it was only 15% in the pharmacotherapy sample ($RR_{HDRS < 7} = 2.36$; 95% CI: 1.33, 4.18). The findings indicate not only that combined STPP and pharmacotherapy is accepted better, but also that symptomatic indicators of success rate, both clinician and self-rated, suggest high levels of success. The success rate of 37%, defined by HDRS as less than 7, for the combined group is not impressive. Across a range of measures, however, by 24 months the success rate of the combined therapy is nearly 60% and that of pharmacotherapy alone rises to 40% ($RR_{HDRS = 7} = 1.5$; 95% CI: 1.09, 2.06). The superiority of the combined treatment is maintained but does not appear to increase. It is not clear how combined treatment would compare with a psychotherapy-alone condition. In some analyses, this has been a difficult difference to demonstrate (Thase et al., 1997).

A second comparison study (Burnand, Andreoli, Kolatte, Venturini, & Rosset, 2002) randomly assigned 95 patients to a combination (clomipramine and STTP) or a clomipramine-alone condition. Despite

an attrition rate of 22%, the intent-to-treat analysis revealed somewhat fewer treatment failures for the combined treatment as well as better work adjustment scores. No group differences were noted on the HDRS, however. There were more frequent instances of hospitalization and more days spent in hospital for the clomipramine-alone treatment. Combined therapy was also associated with fewer days lost from work. This, together with saving on hospitalization costs, led to an estimated cost saving per episode of \$2,300. It should be noted that the benefits of STPP were detectable even though the pharmacotherapy group had some nonspecific psychological input (attention placebo) and the psychotherapy was delivered by nurses rather than certified psychotherapists. This latter finding echoes the success of other psychodynamic interventions where the therapist is supervised by but is not a fully trained psychoanalytic psychotherapist (Bateman & Fonagy, 2001).

Process factors and moderating variables

Support for a psychodynamic approach to the treatment of depression may also accrue from the demonstration that the inclusion of interventions specific to a psychoanalytic approach is associated with good outcome even in therapies for depression born of a different orientation. For example, in a study of CBT, the extent of focus on "parental issues" turned out to be positively associated with outcome (Hayes, Castonguay, & Goldfried, 1996). In another study, of CBT and STPP, the differences between the success of either intervention appeared to be correlated with the use of interventions prototypically considered "psychoanalytic" (Ablon & Jones, 1998). However, similar evidence can be readily mustered to support CBT. Using the same instrument (Enrico Jones's Psychotherapy Q-sort) on tapes of the NIMH treatment of depression trial, Ablon and Jones demonstrated the superiority of therapies where process codings of both IPT and CBT more closely resembled the CBT prototype (Ablon & Jones, 1999, 2002). Studies of psychodynamic psychotherapy process and outcome have not thus far demonstrated powerful associations between change and putative mechanism of action. For example, a negative association has been reported between the number of transference interpretations and therapy outcome, indicating that the overuse of this technique, frequently regarded by clinicians as essential to therapeutic success, may even be iatrogenic (Connolly et al., 1999).

Summary

The current evidence base of psychodynamic therapy for depression is weak relative to the number of psychoanalytic therapists and the rate at which evidence is accumulating for other approaches. The

psychodynamic approach may be marginalized, not by its relative lack of effectiveness, but by the sparseness of compelling demonstrations of its comparability to "empirically supported" alternatives. There is some evidence relating to brief psychodynamic therapy (up to 24 sessions) but no evidence for long-term therapy or psychoanalysis, despite the fact that data from trials of depression indicate the need for more intensive treatment. As Westen and Morrison (2001) observe, the evidence for the effectiveness of CBT for depression in the medium term is not sound. However, none of the therapies appears to differ from each other markedly. In the two cases where brief psychodynamic therapy was compared with CBT or problem-solving therapy, the observed size of the effects was similar in the groups contrasted and in turn similar to results reported in other studies of CBT, IPT, and couples therapy.

Broadly speaking, in 4–6 months of therapy, about half of those treated are likely to remit. About half of these will experience a remission in the succeeding 12 months. As Roth and Fonagy (in press) observe, the data are consistent with the assumption that a proportion of patients in any research sample will respond to therapeutic intervention of any kind. An appropriate future strategy for psychodynamic psychotherapy research on depression might be to compare the effectiveness of relatively long-term psychodynamic psychotherapy with alternative forms of intervention in patients who are nonresponders in trials of CBT, IPT, or pharmacotherapy. A further weakness of the evidence base for psychodynamic treatment studies of depression is that no STPP was tested twice by independent research groups (Chambless & Hollon, 1998). Most manuals subjected to systematic inquiry are idiosyncratic, and their testing is restricted to the location in which they were developed.

Anxiety disorders

Research on anxiety disorders is normally subdivided into research on phobia, generalized anxiety disorder (GAD), panic disorder (with and without agoraphobia), posttraumatic stress disorder (PTSD), and obsessive-compulsive disorder (OCD). These, often comorbid with depression (Brown, Campbell, Lehman, Grisham, & Mancill, 2001), are the most commonly encountered disorders both in community surveys and in primary mental health services. Anxiety disorders are central to psychoanalytic theory (Milrod, Cooper, & Shear, 2001) and are probably the most common presenting complaints in psychodynamic therapeutic practice. Disappointingly, for at least two of the most common anxiety problems (social phobias and specific phobias) there are no diagnosis-specific controlled trials of psychodynamic therapy. The field

is dominated by CBT packages that combine a range of approaches with almost no studies of nonbehavioral approaches except for a small trial of interpersonal therapy (Lipsitz, Markowitz, Cherry, & Fyer, 1999).

Generalized anxiety disorder

Treatments developed for generalized anxiety disorder are dominated by anxiety management (relaxation, positive self-talk) and cognitive therapy focusing on identifying and modifying worrying thoughts. Because of the sparsity of the literature, meta-analytic and systematic reviews are not very informative. Westen and Morrison's (2001) meta-analysis identifies a study of psychodynamic therapy (supportive expressive therapy) (Crits-Christoph, Connolly, Azarian, Crits-Christoph, & Shappell, 1996) but does not address differences in efficacy across the four types of therapy examined. In another meta-analysis, Fisher and Durham (1999) established a clinical cutoff on the Spielberger Trait Anxiety Scale to identify the proportion of participants who recover at posttherapy and remain in remission at six months as well as those who recover in the 6-months, posttherapy period. By these criteria, overall, at 6-months, 36% are unchanged, 24% are improved, and 38% are recovered. Only 4% of patients are considered as recovered following psychodynamic therapy. But this is based on just one study (Durham et al., 1994). This contrasts with 60% for individual applied relaxation therapy and 51% for CBT. Notably, nondirective therapy was shown to have recovery rates of 38%, suggesting that neither relaxation nor restructuring of worrying thoughts is necessary for substantial improvement. However, the review includes too few trials to be a definitive statement about the relative effectiveness of these therapies in anxiety.

A number of individual studies of psychodynamic psychotherapy have provided some relevant data. Two open trials have been reported. Crits-Christoph and colleagues (1996) followed 26 patients for a year. The patients had 16 once-weekly sessions followed by booster sessions once every 3 months. At posttherapy, 79% no longer met diagnostic criteria for GAD.

A second study by Durham and colleagues (Durham et al., 1999) contrasted analytically based psychotherapy with CBT and anxiety management training in an RCT. Ninety-nine patients with a GAD diagnosis for at least 6 months (mean duration 30 months) were randomly assigned to either high-frequency or low-frequency (weekly or fortnightly) CBT or analytic psychotherapy. Unfortunately, the groups ended up poorly matched, with the CBT patients in the low-frequency condition significantly less severely affected. Importantly, the gains

from psychoanalytic psychotherapy were less than those from cognitive therapy but were also less likely to be maintained at one-year follow-up. Thus, although 60% of patients treated by CBT met clinically significant change criteria at 1-year follow-up, only 14% of those in analytic psychotherapy did so ($RR_{HARS} = .11$; 95% CI: .03, .37). At this stage, higher frequency was associated with better maintenance of gains for both CBT and analytic psychotherapy. However, when the sample was followed-up almost a decade after the end of treatment (Durham, Chambers, MacDonald, Power, & Major, 2003), only about half of those who had achieved recovery at 6 months maintained their gains in the very long term. By this stage, there was no difference in outcome between CBT-and non-CBT treated participants.

A major limitation of this trial, but also a helpful pointer, was the nature of the psychoanalytic therapy offered. The two therapists, although both experienced psychiatrists, were training in psychoanalysis and are not reported to have had special training in brief therapeutic methods, and had no manual to follow. Under these circumstances, it is highly likely that the therapy delivered was not an effective version of STPP. If the effectiveness of cognitive therapy were assessed on the basis of two individuals trained in generic CBT without further training specific to the trial, this would generally be considered unacceptable. It is possible, because the techniques used are not specified in the report, that these therapists used inappropriate techniques borrowed from the long-term therapy in which they were currently training and unhelpfully applied these in a time-limited context.

Panic disorder

As with GAD, nondirective "psychodynamic" therapies have been used as control treatments in a number of CBT trials (e.g., Beck, Sokol, Clark, Berchick, & Wright, 1992; Craske, Maidenberg, & Bystritsky, 1995). The relative absence of therapeutic equipoise makes these trials suspect, and evidence from them can have few implications for the effectiveness of short-term psychodynamic treatments. When more effort is made to create a credible placebo, differences between treatment and control groups are reduced or disappear completely. For example, one study randomized 45 patients with panic disorder to 15 sessions of CBT or a therapy described as "nonprescriptive" in which the therapist was encouraged to offer reflective listening (Shear, Pilkonis, Cloitre, & Leon, 1994). No significant differences were found either at posttherapy or at 6-month follow-up. In a partial replication, the same group (Shear, Houck, Greeno, & Masters, 2001) did find a difference between CBT and this nonprescriptive form of therapy, but the difference is small relative to less credible control interventions. In the

meta-analysis by Nordhus and Pallesen (2003) of psychological treatments for later life anxiety, a number of studies are noted where the effectiveness of CBT is reduced or even reversed when the control condition is a highly credible psychotherapeutic placebo (e.g., Wetherell, Gatz, & Craske, 2003).

There are few trials of brief psychodynamic therapy for panic, and none unequivocally addresses the effectiveness of this approach. Milrod and her colleagues have worked over a number of years to establish the evidence base for a manualized panic-focused psychodynamic psychotherapy (Busch, Milrod, Cooper, & Shapiro, 1996; Busch, Milrod, & Singer, 2000; Milrod, Busch, Cooper, & Shapiro, 1997). This team reports an open trial of psychoanalytic psychotherapy with 21 patients seen twice weekly over 12 weeks and with a 6-month follow-up (Milrod, Busch, et al., 2001; Milrod et al., 2000). Sixteen of the 21 participants showed remission (defined as a reduction from baseline of more than 50% on the panic disorder severity scale). The results proved stable at an unusually long follow-up period of 40 weeks. Of those completing treatment, 93% were considered remitted at end of treatment and 90% at follow-up.

The study is in many ways exemplary. In particular, there was careful attention to measurement issues, careful training of therapists, and attention to treatment integrity. It is also remarkable for the involvement of senior psychoanalysts in the design and delivery of this structured and symptom-focused but unequivocally psychoanalytic treatment. Although the effect sizes are comparable to those observed in the best trials of cognitive-behavioral therapy (e.g., Barlow, Gorman, Shear, & Woods, 2000), the absence of a control group and relatively small sample size limits the generalizability of the conclusions.

Wiborg and Dahl (1996) reported a controlled study that examined the effect of adding psychodynamic therapy to treatment with clomipramine. Thirty patients were randomized to clomipramine or clomipramine with 15 weeks of manualized dynamic psychotherapy based on Davanloo, Malan, and Strupp and Binder's work. There was a follow-up at 6, 12, and 18 months of treatment. At the end of treatment, all patients in the combined treatment group were free of panic attacks ($RR_{\text{HRSD}} = 1.33$; 95% CI: 1.04, 1.72) but at 6-month follow-up all clomipramine-treated subjects were panic free. By 18 months, 75% of the drug-only group had relapsed, but only 20% of the group who had received psychotherapy ($RR_{\text{HRSD}} = 3.2$; 95% CI: 1.45, 7.05). The differences were apparent even when adjustments were made for initial severity of symptoms and social adjustment. Further advantages were observed in terms of the psychotherapy group reporting fewer side effects from the medication. This study provides evidence that STPP is an

effective adjunct to pharmacotherapy in panic disorder, but of course it cannot speak to the effectiveness of this treatment in the absence of clomipramine. It is also important to note that clomipramine was administered by the treating psychotherapist in the experimental arm of the trial but by a nonpsychiatrically trained general physician in the control group. The findings confirm the results of an early trial (Klein, Zitrin, Woerner, & Ross, 1983) where the effect of imipramine for phobic problems was (to the authors' surprise) as strongly enhanced by STPP as by behavior therapy (RR = .97; 95% CI: .66, 1.43).

As described above, the Helsinki Psychotherapy Study (Knekt & Lindfors, 2004) included a significant number of individuals diagnosed with panic disorder ($n = 34$) and generalized anxiety disorder ($n = 37$). In addition, the authors observed that practically all the patients (more than 95%) had Hamilton Anxiety scores higher than 7 at baseline. There was a slight, but not statistically significant, difference between the groups in terms of the impact of the treatment on anxiety problems: 56% of those with anxiety disorders lost their diagnosis after 7 months of STTP compared with 42% of those in SFT (RR_{HARS} = 1.34; 95% CI: 1.0, 1.78). By 12 months, the difference had narrowed to 62% and 52%, respectively (RR_{HARS} = 1.19; 95% CI: .93, 1.51). Although this difference is not significant, it underscores the potential for STPP to rapidly assist in the problems of anxiety as well as depression. There was a 34% decrease in the Hamilton Anxiety Rating Scale for STPP and a 28% decrease for SFT at 12 months, but almost all of this had been achieved by 7 months.

Stress-related conditions: Post-traumatic stress disorder (PTSD) and complex grief reaction

Psychodynamic approaches to PTSD focus on the meaning of the traumatic event for the person's sense of self and his or her place in the outside world (Horowitz, Marmar, Weiss, DeWitt, & Rosenbaum, 1984). Studies supporting this approach are either case reports or open trials. Open trials of the psychodynamic treatment of female victims of sexual assault have tended to be positive but methodologically problematic (Cryer & Beutler, 1980; Perl, Westlin, & Peterson, 1985; Roth, Dye, & Lebowitz, 1988).

Scarvalone and colleagues (1995) contrasted a psychodynamic group therapy with a wait-list control group. All participants ($N = 40$) had histories of sexual abuse, although not all had current symptoms of PTSD. After treatment, 39% of those in the psychodynamic group, as opposed to 83% of those in the control group, met diagnostic criteria. (RR_{PTSD} = .47; 95% CI: .27, .83).

A relatively large RCT from Holland (Brom, Kleber, & Defares,

1989) contrasted psychodynamic therapy, hypnotherapy, and trauma desensitization in the treatment of 114 individuals with a PTSD diagnosis. Many were bereaved, and only about 20% reported experiencing a traumatic event. There was also a wait-list control group. Mean length of treatment varied somewhat between conditions (trauma desensitization = 15 weeks, hypnotherapy = 14.4 weeks, STPP = 18.8 weeks). The study reported all treatments to be superior to the wait-list control group, with clinically significant improvements in about 60% of the treated patients and 26% of the untreated patients ($RR_{\text{PTSD}} = 2.28$; 95% CI: 1.12, 4.64). At posttreatment, psychodynamic therapy seemed to have the weakest effects, but therapeutic changes continued in this group and by follow-up they matched or exceeded those of other therapies. Interestingly, trauma desensitization had a stronger influence on intrusions and psychodynamic therapy had more influence on avoidance.

Complex grief reaction was the focus of a further RCT of brief individual psychotherapy contrasted with a group therapy run by nonclinician volunteers who were experts by experience (Marmar, Horowitz, Weiss, Wilner, & Kaltreider, 1988). The therapy was based on Horowitz's model of pathological bereavement and was delivered by experienced therapists. Sixty-one patients were randomized, and outcomes were assessed at the end of treatment, at 4 months, and at 1-year follow-up. Patients received 12 sessions of therapy in each group. The superiority of the psychotherapy group was principally in terms of reducing attrition in the early and late phases of the treatment. Nearly a third of those in group treatment terminated in the first third of the treatment ($RR_{\text{DROPOUT}} = .42$; 95% CI: .24, .73). Self-report results provided evidence of the superiority of psychotherapy at 4 months and 1 year on the general severity index (GSI) of the SCL-90 ($SMD_{\text{SCL-90}} = .65$; 95% CI: -1.1, -0.8). Other than the GSI, observer ratings and self-report differences favored the psychotherapy group but were not statistically significant. This was most likely because the self-help group was surprisingly effective, at least for those who attended it. This trial was designed before the wider recognition of the value of self-help groups; otherwise perhaps the investigators might have chosen a less challenging comparison.

An impressive program of work by William Piper and colleagues (Piper et al., 1991; Piper, Joyce, McCallum, & Azim, 1998) examined the effects of time-limited interpretive therapy, in both an individual and a partial hospitalization context (Piper, 1996). However, these studies were conducted with heterogeneous samples of psychiatric outpatients and therefore are not relevant to a diagnosis-based review. An important trial explored the value of interpretive versus supportive

group therapy for individuals with complicated grief reactions (Ogrodniczuk, Piper, McCallum, Joyce, & Rosie, 2002; Piper, McCallum, Joyce, Rosie, & Ogrodniczuk, 2001). Both therapies were modified to be appropriate for group treatment of grief, but while interpretive therapy was aimed to enhance insight about repetitive conflicts associated with the losses, supportive therapy included praise and gratification. The treatments were manualized, sessions were videotaped, and the groups met 12 times for 90 minutes once a week. Both treatments were effective in terms of an exceptionally broad range of outcome measures, covering general symptoms, grief symptoms, and target problems. The most informative finding to emerge from this study was the interaction between type of therapy and a measure of object-relatedness. Psychological-mindedness was associated with improvement in both interpretive and supportive therapy. An intriguing and complex interview-based measure of the quality of object relations, the QOR (Piper et al., 1991), was found to interact with these modes of therapy in a fashion meaningfully related to psychodynamic formulations. High-quality or mature object relations on the QOR appear to predict greater benefit from interpretive therapy, whereas those whose relationships are judged to be more primitive, searching, or controlling were more likely to benefit from supportive therapy. Subsequent investigation (Piper, Ogrodniczuk, McCallum, Joyce, & Rosie, 2003) suggested that the balance of positive and negative affect expressions during therapy was the important mediator of this association. This well-conducted program suggests that STPP may be a relatively effective treatment for complex grief reactions and that individuals with more mature representations of interpersonal relationships are most likely to benefit.

Summary

Anxiety treatment research represents the “home base” of cognitive behavioral approaches. GAD represents its most significant challenge. One uncontrolled trial suggested that STPP might have something to offer these patients. A controlled trial comparing an unspecified form of STPP to cognitive therapy showed the latter to be substantially more effective in the short and medium term. The disappointing results from the psychodynamic arm of the trial may relate to the nonspecificity and unstructured nature of this therapy.

Panic attacks appear to be relatively well treated by 15 to 20 sessions of CBT. There is a promising psychodynamic therapeutic approach to panic that might match CBT in efficacy which requires extension and replication in multicenter controlled trials. The superiority of CBT over other approaches is probably limited, as shown by reduced effect sizes

in controlled trials that have active placebo treatments. The challenge for a psychodynamic approach is to identify a way to address limitations in CBT, either in terms of long-term efficacy (Milrod & Busch, 1996) or in terms of a more pervasive impact on social functioning. Interestingly, evidence from the Helsinki trial, where this was a focus of investigation, did not support the view that short-term psychodynamic therapy had a wider impact on social functioning than problem-focused treatments. Evidence on the treatment of PTSD is also sparse, notwithstanding the central involvement of psychoanalytic clinicians in mapping the consequences of childhood and later trauma. Available controlled studies concern complicated grief and bereavement reactions, and not exposure to trauma. Nonetheless, findings from such trials are generally positive, although by no means uniquely effective.

It is striking that little research has been done to establish the pertinence of psychodynamic approaches to anxiety, which is so central to both psychoanalytic theory and practice. Possibly psychodynamic therapists do not consider anxiety symptoms important enough, as Freud's term *signal anxiety* might suggest. Bypassing the surface problem of the anxiety symptoms, they attempt to achieve change in the underlying structures even in brief therapies. Losing focus, they find change relatively difficult to bring about. It requires an approach such as Milrod's, which retains focus on the symptom at the same time that it explores unconscious determinants, to achieve rapid change. The importance of anxiety-related problems demands that further studies be initiated.

Eating disorders

Anorexia nervosa

Most current treatment approaches to anorexia nervosa (behavior therapy [BT], CBT, family therapy, psychodynamic psychotherapy) recognize the importance of establishing an appropriate dietary regime that is common to all treatments. An early study (Hall & Crisp, 1987) assigned 30 anorexic patients to either dietary advice or psychotherapy. The psychotherapy was psychodynamic, delivered either individually or involving the whole family for 12 sessions at two weekly intervals. The dietary advice group entailed 15 one-hour sessions at weekly or fortnightly intervals with a dietician. There was no difference between the groups on body weight at posttreatment or at 1-year follow-up. Those receiving psychotherapy showed better social and sexual adjustment scores. The sample sizes made it difficult to interpret these results, because the majority of the participants in the anorexic arm showed substantial weight gain and the inferior mean relative to the control groups was due to three individuals who showed a substantial weight

loss. Needless to say, in this early study the psychotherapy (STPP) was not manualized.

An influential study by Russell and colleagues (1987) contrasted family and individual therapy for 80 inpatients, the majority of whom (57) were anorexic. The therapy was not strictly psychodynamic as it included cognitive and strategic techniques. Broadly, the majority of patients in both groups had poor outcome at one year (61%), with just under a fourth having good outcomes. Although indicating the general difficulty of treating this group of patients, this study also suggested that late-onset patients (onset after age 19) did better with individual therapy. This finding was partially confirmed in a 5-year follow-up (Eisler et al., 1997). The superiority of family therapy is restricted to anorexic patients with early onset and relatively short history.

The largest trial involving STPP is that of Dare and colleagues (2001). Eighty-four patients were recruited to STPP based on Malan's approach, family therapy (Dare & Eisler, 1997), and cognitive analytic therapy (Ryle, 1990). The control group received standard psychiatric care (from trainee psychiatrists). This trial was carried out in a tertiary care setting; most patients had had one or more treatment failures. Not surprisingly, outcomes were poor (mean weight gains were small and left patients with undernutrition) but provided evidence for all three treatment approaches. After 1 year of treatment about a third of the patients in the three specialist psychotherapies no longer met DSM criteria for anorexia nervosa compared with only 5% of those in routine treatment ($RR_{DAN} = 7.0$; 95% CI: 1.0, 48.9). It was not possible to differentiate clearly between the three specialized psychotherapies in terms of an improvement based on weight gain, menstruation, and bulimic symptoms, although the odds ratio for improvement in STPP and family therapy was better and significantly different from routine treatment ($RR_{FOCAL} = 1.99$; 95% CI: .85, 4.68) ($RR_{FAMILY} = 1.55$; 95% CI: .63, 3.84). The study was, however, underpowered, and follow-up was partial.

Crisp and colleagues (1991) report an RCT with 20 patients allocated to each of four treatment arms: inpatient treatment, outpatient individual and family psychotherapy (12 sessions, over 10 months), 10 group therapy sessions and initial assessment along with treatment as usual in the community. The individual therapy group was significantly better than both the inpatient and the minimal treatment group at the end of treatment. The individual psychotherapy was structured, somewhat eclectic in orientation, and delivered by experienced psychotherapists. The inpatient group received a range of treatments based on behavioral techniques coupled with milieu treatments and individual and family therapy. Gowers and colleagues (1994) reported on the out-

come of the outpatient group compared with the minimal treatment group 2 years after the end of treatment. The outpatient group members maintained and further improved their weight gain, which was more than twice that of the control group. Although 60% of the psychotherapy group were judged to be well (within 15% of mean matched population weight [MMPW], normal menstruation, and eating habits) or nearly well (within 15% of MMPW, nearly normal menstruation, and/or abnormal eating habits), only 20% of the control group were judged to be so ($RR_{DAN} = 3.0$; 95% CI: 1.16, 7.73). Despite the small sample size, the study suggests that substantial benefits accrue to anorexic patients from psychodynamic therapy supported by family therapy. However, the form of therapy offered was eclectic rather than a pure form of STPP; therefore, effective elements may actually be associated with the strategic or directive elements.

Bulimia nervosa

The majority of trials in the literature (around 30) employed either behavioral or cognitive-behavioral techniques (Hay & Bacaltchuk, 2001; Thompson-Brenner et al., 2003). Intent-to-treat recovery rates are only around 33%, with an advantage to individual therapies and no clear superiority to CBT over alternative active psychological interventions. No non-CBT studies report long-term follow-up and meta-analytic studies show a tendency toward deterioration in effect size of follow-up samples relative to posttherapy observations. The meta-analyses do not speak to the specific efficacy of psychodynamic therapies.

Garner and colleagues (1993) reported a study contrasting STPP based on Luborsky's (1984) supportive-expressive model with CBT. Sixty patients were randomized to 19 individual treatment sessions over an 18-week period. Five patients withdrew from each treatment arm. The two treatments were equally effective in their impact for binge frequency. CBT was somewhat (but not statistically significantly) more effective in reducing vomiting frequency and significantly more effective in reducing depression. Both treatments are considered by the authors to be effective, but where differences emerged these favored CBT. No follow-up is reported for the study, which would be critical in this context.

A different psychodynamic approach was taken in a study of 33 bulimic patients (Bachar, Latzer, Kreitler, & Berry, 1999) assigned to either nutritional counseling alone or in conjunction with a form of cognitive therapy or a form of self-psychological dynamic therapy. A range of measures were used, including measures of symptomatology, attitudes to food, self-structure, and general psychiatric symptoms. The groups were inappropriately small and preclude comments on relative

efficacy. However, pre-post effects appear to be substantial for psychodynamic treatment, less notable for cognitive therapy, and almost negligible for nutritional guidance. The study is further weakened by the mixture of anorexic and bulimic patients identified and the nonstandard cognitive approach applied. Nevertheless, it suggests that psychodynamic treatment may have significantly better outcomes compared to an almost untreated group.

Walsh et al. (1997) reported an impressive investigation that contrasted psychodynamically oriented supportive psychotherapy with CBT and antidepressant medication (desipramine followed by fluoxetine). One hundred and twenty women with bulimia were randomized to five treatment arms. In this trial, CBT was superior to psychodynamic psychotherapy in reducing bulimic symptoms. Supportive psychotherapy appeared to offer little additional benefit to medication alone, but CBT and medication combined produced greater improvement than medication alone. The results suggest that supportive psychotherapy is not particularly helpful for bulimia.

Obesity

Beutel and colleagues (2001) reported an unusual trial involving inpatient treatment of obesity ($BMI > 35 \text{ kg/m}^2$). Ninety eight consecutive patients were randomly allocated to 6-week inpatient behavioral ($n = 46$) or psychodynamic ($n = 52$) treatments entailing individual and group work in both approaches. They measured weight loss, eating behavior, body image, and life satisfaction. Although both interventions were associated with substantial gains, there were no differences between the two programs in terms of weight loss or eating behavior changes.

Summary

There have been four trials of psychodynamic psychotherapy for anorexia nervosa, all of which found it to be as effective as other treatments, including intensive behavioral and strategic family therapy. None of the trials was powered adequately to distinguish conclusively between alternative treatments. Taking the results together, it seems that relative to treatment as usual, psychodynamic therapy for anorexia nervosa holds its own. The trials were performed in two London specialist units, but the particular brands of psychoanalytic psychotherapy practiced were not comparable, so they cannot be considered replications.

STPP fares less well in the treatment of bulimia. One trial indicated that STPP was somewhat less effective than CBT, while in another study the superiority of STPP is based on a tiny sample size and an un-

usual implementation of cognitive therapy. In a trial exploring combined pharmacological and psychosocial treatments, nonspecific supportive STPP turned out to be less effective than CBT in enhancing the effect of medication.

Overall, as in other contexts, when STPP is modified for a specific clinical problem, it is far more likely to be effective. It is comparable to a similarly refined cognitive-behavioral approach. As a generic supportive treatment, it is unlikely to be an appropriate recommendation for any of the eating disorders considered, but as a specific approach, it is perhaps more likely to be of benefit.

Substance misuse

Psychoanalytic therapies do not have a strong tradition in the treatment of substance misuse. There have been theoretical and clinical investigations of the problems but few substantive case series studies (Hopper, 1995; Johnson, 1999; Radford, Wiseberg, & Yorke, 1972). However, in the light of emerging data concerning the prevalence rates of alcohol and drug dependence, perhaps in excess of 7% (Hickman et al., 1999; Kessler et al., 1994; Kraus et al., 2003), it seems important to establish whether psychoanalytic psychotherapy has anything to contribute to this major public health problem.

Alcohol misuse

A research group led by William Miller has produced an exhaustive and periodically updated report concerning the relative effectiveness of a range of psychosocial treatments for alcohol dependence (Miller & Wilbourne, 2002; Miller, Wilbourne, & Hettema, 2003). Using a simple but relatively robust (Finney & Monahan, 1996) method of identifying empirical support in terms of the number of studies in the literature yielding positive outcomes, they identified brief interventions and motivational enhancement as best supported, followed by community reinforcement, bibliotherapy, then various behavioral interventions (contracting, self-control etc), followed by social skills. Psychotherapy is placed last in a long line.

Indeed, evidence for the efficacy of psychotherapy and counseling is very limited. An early study (Levinson & Sereny, 1969) described assigning inpatients to a generic insight-oriented therapy, with additional educational sessions or treatment as usual, which at the time included recreational therapy and occupational therapy. At 1-year follow-up, no differences were observed in terms of drinking behavior, with somewhat greater improvements being reported from the control group.

Miller's database contains other similar examples (e.g., Pattison, Brissenden, & Wohl, 1967; Tomsovic, 1970).

Looking at more specific dynamic psychotherapies, we find that there are some studies of dynamic therapy that show superiority to a no-treatment control group (Brandsma, Maultsby, & Welsh, 1980; Kissin & Gross, 1968). However, when the contrast is with minimal intervention approaches, there appears to be little demonstrable benefit from dynamic psychotherapeutic treatment (Crumbach & Carr, 1979; Zimberg, 1974). Studies that do suggest differences (e.g., Pomerleau, Pertshuk, Adkins, & d'Aquili, 1978) are characterized by poor methodology (Miller & Hester, 1986). Only one trial (among the 381 listed in the Miller database) supports a psychodynamic approach (Sandahl, Herlitz, Ahlin, & Rönnerberg, 1998). Investigators randomized 49 patients meeting criteria for alcohol dependence to group psychodynamic or group cognitive-behavioral therapy. All patients had already completed inpatient treatment. There were no statistically significant differences between the groups but at posttherapy and, more importantly, at 15-month follow-up, both groups improved. Although not statistically significant, the tendency was for the psychodynamic psychotherapy group to show better maintenance of gains.

Even with the most effective treatment, the prognosis for alcohol dependence is not good, particularly for patients with greater chronicity. There is insufficient evidence in the literature to support psychodynamic psychotherapy as an adequate first-line therapy for patients with alcohol dependency problems. Given the comorbidity of alcohol dependence with other psychiatric problems, STPP may possibly be helpful in dealing with residual psychiatric disorder after alcohol dependence has been addressed. This suggestion will need to be subjected to empirical tests.

Cocaine dependency

According to naturalistic studies, the outcome of psychodynamic treatments for cocaine abuse is similar to that for alcohol. A study of the effectiveness of community treatment (Simpson, Joe, Fletcher, Hubbard, & Anglin, 1999) revealed that 90-day relapse rates for long-term residential treatment were 15%, for short-term inpatient treatment, 38%; and outpatient programs, 29%. Given the underreporting of cocaine use, the likely figures of relapse are 10–20% higher (Simpson, Joe, & Broome, 2002).

Kang and colleagues (1991) reported a major trial to study the efficacy of once-weekly psychotherapy or family therapy contrasted with group therapy led by a paraprofessional among patients with cocaine use disorders. Participants were recruited from those seeking outpatient

treatment, and 168 consented to be randomized to one of three arms in the trial. Of those in the trial, 122 were interviewed 6 and 12 months later, and addiction severity indexes were compared. Attrition was extremely high, with only about 50% attending more than one session and 22% more than six sessions. Nineteen percent of those who attended more than three sessions reported abstinence at 6 months. There was a significant impact for the cohort as a whole. This was reflected by the 19% of the 122 subjects who were no longer using cocaine at follow-up. There was a strong relationship between achieving abstinence and improvements in psychiatric symptoms and family problems. There was no relationship between attendance at therapy sessions and outcome. The results of the trial were considered by the authors to indicate that outpatient once-weekly psychotherapy is an insufficient treatment for cocaine use disorder. The results indicate that inpatient treatment or more intensive outpatient treatment may be required.

A second trial, the National Institute of Drug Abuse Collaborative Cocaine Treatment Study (Crits-Christoph et al., 1997; Crits-Christoph et al., 1999; Crits-Christoph et al., 2001), randomized 487 severely cocaine-dependent patients who met criteria for cocaine misuse, with 75% smoking crack cocaine and 33% also meeting criteria for alcohol dependence, to one of four treatments: group drug counseling (GDC) following the 12 steps model; GDC combined with individual drug counseling (IDC); GDC combined with CBT, or GDC combined with STPP. Treatments were offered over 6 months, during which participants had 24 group sessions and a maximum of 36 individual sessions. The treatment lacked acceptability. By month 3, 50% of the participants had left treatment, and overall only 28% completed. All treatments showed significant improvements from baseline to postbaseline in cocaine use (past 30 days). The greatest improvement was with IDC. By month 6, an estimated 40% of available patients in the IDC group reported cocaine use compared with 58% in CBT, 50% in STPP, and 52% in GDC alone ($RR_{IDC\ vs\ STPP\ @\ 6\ months} = 1.24$; 95% CI: .94, 1.64) ($RR_{IDC\ vs\ CBT\ @\ 6\ months} = 1.35$; 95% CI: 1.03, 1.77). At 12-month follow-up, these percentages were 40%, 46%, 48%, and 47%. The difference between IDC and other treatments was statistically significant. CBT and STPP retained patients better, but IDC produced more improvement in terms of abstinence. Interestingly, higher levels of alliance were associated with better retention in IDC and STPP but with worse retention in CBT (Crits-Christoph et al., 2001). There was no difference between the treatments in terms of associated psychological, social, and interpersonal measures (Crits-Christoph et al., 2001). It should be noted that, although this is one of the most sophisticated tests of the value of STPP, there are some crucial flaws in the design. In the

extended (2-week) assessment, 46% of the initial participants (those with the highest level of dependency needs) were lost to the trial, and patients on psychotropic medication who might have benefited most from STPP or CBT were excluded.

Opiate dependency

Meta-analyses of the treatment of opiate dependency (Brewer, Catalano, Haggerty, Gainey, & Fleming, 1998; Prendergast, Podus, Chang, & Urada, 2002) identified short treatment length, low treatment integrity, and low levels of staff training to be associated with relatively poor outcome. Meta-analyses reveal effect sizes to be relatively low (.3) and, unusually, fail to identify a particular treatment type as especially beneficial.

Woody and colleagues (Woody, McLellan, Luborsky, & O'Brien, 1987; Woody et al., 1983; Woody, McLellan, Luborsky, & O'Brien, 1990; 1995) performed a crucial randomized study of supportive-expressive therapy contrasted with CBT in opiate-dependent individuals on methadone. These two forms of therapy were compared with 24 weeks of drug counseling alone. The numbers of sessions for the three conditions were not equal: 17 for drug counseling alone, 12 for STPP, and 10 for CBT. Of 305 patients who met criteria, 185 of these agreed to take part but only 110 engaged in the trial. Patients in all three groups showed improvements in lessened drug use, reduced criminal behavior, and better psychological function at 7-month follow-up. There were advantages to the STPP group in psychological problems, days worked, money legally earned, and drug use, and advantages to CBT in dealing with legal problems. Both therapy groups did better than the drug-counseling-only group. At 12-month follow-up, data were available from 93 participants, with those in the two psychotherapy groups showing more improvement. CBT showed the same kind of advantages on most measures as STPP. However, 44% of STPP versus 26% in CBT and 18% in drug counseling alone were off methadone at 12 months (Woody et al., 1987) ($RR_{STPP\>CBT\>METHADONE} = 1.71$; 95% CI: .88, 3.31) ($RR_{STPP\>CONTROL\>METHADONE} = 2.44$; 95% CI: 1.12, 5.3).

A partial replication of this study (Woody et al., 1995) in a community setting addressed a limitation of the previous study: that psychotherapy participants had both a drug counselor and a psychotherapist. In the replication study the clients randomized to drug counseling alone ($n = 41$) had access to two drug counselors, matching the psychotherapy group ($n = 82$). The number of sessions between the arms were also equated in the community replication. There were no differences between the groups in positive opiate urine samples, but there was a difference as far as cocaine urine analysis was concerned (22% in STPP

and 36% in drug counseling, $RR_{\text{COCAINE/STPP/DC}} = .6$; 95% CI: .34, 1.06). The pattern of results across time consistently indicated that although the groups were equivalent at end of treatment, the drug counseling group worsened during follow-up whereas the STPP group improved over the same period. This study, however, encountered the problems that face effectiveness as opposed to efficacy studies (rapid staff changes, suboptimal clinical protocols, lack of cooperation from clinical sites). Thus the degree of change observed is even more impressive.

Summary

Many studies considered the efficacy of brief interventions for alcohol dependence. For alcohol problems of low severity, brief interventions seem to be the interventions of choice. Psychodynamic psychotherapy along with other formal psychological therapies appears not to be particularly helpful when offered as a stand-alone treatment. On the whole, successful interventions appear to be targeted at drinking behaviour. Testable psychodynamic protocols of this kind have not yet been developed.

Again, for low levels of cocaine dependency, briefer treatments appear to be appropriate. But for individuals with more severe problems, both engaging with treatment and maintaining commitment to formal psychotherapy appear problematic. Supportive-expressive psychotherapy appears of almost no value in the context of cocaine misuse. In fact, treatments that do not engage with clients in the community context appear to be of limited relevance. It is an obvious question whether STPP could be modified to incorporate community involvement.

A different picture emerges in the context of opiate abuse, where psychodynamic treatment was shown to be efficacious in two trials, unfortunately (from the standpoint of EST criteria) carried out by the same team. However, in this context, there is a *prima facie* case for the unique effectiveness of supportive-expressive therapy because neither IPT (Rounsaville, Glazer, Wilber, Weissman, & Kleber, 1983) nor certain cognitive therapies (Dawe et al., 1993; Kasvikis, Bradley, Powell, Marks, & Gray, 1991) appear to have quite the same impact. Nevertheless, generic counseling or certain types of family-based interventions may enhance the effectiveness of methadone treatment just as STPP appears to. In this area, there is urgent need for replication by an independent group of workers willing to implement the supportive-expressive therapeutic strategy.

If a place is to be found for psychodynamic psychotherapy in substance abuse protocols, it is unlikely to be in offering formal therapy as a primary treatment. Rather, taking the lead from the opiate work, a

niche needs to be found where psychodynamic intervention provides appropriate support for what is ultimately a physical dependency requiring physical treatment rather than hoping that a psychological intervention by itself is capable of resolving a physical dependency. There is urgent need to identify protocols that sequence traditional forms of psychosocial treatments with interventions for physical dependence within a single integrated package.

Personality disorders

Personality disorders represent a special challenge for outcomes research because of the high level of comorbidity between Axis I and Axis II diagnoses and within Axis II diagnoses (Swartz, Blazer, & Winfield, 1990; Zimmerman & Coryell, 1990). Treatment research is somewhat limited, but has been powerfully enhanced by recent activity in new approaches to cognitive-behavioral therapy (Blum, Pfohl, John, Monahan, & Black, 2002; Koons et al., 2001; Linehan et al., 1999) as well as psychodynamic treatments (Bateman & Fonagy, 2004; Clarkin et al., 2001; Clarkin, Levy, Lenzenweger, & Kernberg, 2004b; Ryle & Golyukina, 2000).

There have been two meta-analyses of psychological therapies. Perry and colleagues (1999) identified 15 studies, including 6 randomized trials. Substantial effect sizes were identified pre- to posttreatment ($ES = 1.1-1.3$), which reduced to around .7 in studies where active control treatments were used. A more focused meta-analysis (Leichsenring & Leibing, 2003) considered only trials that used either CBT or psychodynamic therapy and identified 22 studies, 11 of which were RCTs. Pre-post effect size for psychodynamic therapy was 1.31 based on 8 studies and for CBT was .95 based on 4 studies. There was an insignificant correlation between treatment length and outcome.

The limited number of studies, compounded by heterogeneity of clinical populations and methods applied, suggests that meta-analysis at this stage may be premature. Furthermore, many of the studies included in these meta-analyses did not have the aim of treating Axis II disorders. Notwithstanding these limitations, the broad conclusion from these aggregated figures would be that CBT and psychodynamic therapy are equally effective.

Borderline personality disorder

There are more studies of borderline personality disorder (BPD) than of other personality disorders. There have been a number of uncontrolled open trials of the psychodynamic treatment of BPD. The Menninger study of 42 patients carried out in the 1950s is historically important as

the first serious, methodologically relatively sound attempt to evaluate the outcome of any type of psychological therapy (Wallerstein, 1986, 1989). It was a study of psychoanalysis and expressive or supportive psychodynamic psychotherapy. One may well ask what happened to this pioneering spirit? The study's findings are complex but broadly imply that more mature personalities with better interpersonal relationships responded well to expressive-interpretive therapy, whereas those with low ego strength responded better to supportive interventions. There have been a number of other naturalistic studies (Antikainen, Hintikka, Lehtonen, Koponen, & Arstila, 1995; Karterud et al., 1992; Monsen, Odland, Faugli, Daae, & Eilertsen, 1995; Tucker, Bauer, Wagner, Harlam, & Sher, 1987; Waldinger & Gunderson, 1984; Wilberg et al., 1998). These studies with varied sample sizes speak to the relative efficacy of various forms of psychodynamic therapy but had too little in common in terms of treatment protocols to permit conclusions concerning the effectiveness of this approach.

An Australian uncontrolled trial stands out in terms of methodological rigor (Meares, Stevenson, & Comerford, 1999; Stevenson & Meares, 1992, 1999). In this open trial, 48 patients received twice-weekly interpersonal self-psychological psychodynamic outpatient therapy over 12 months. The contrast was with patients on a waiting-list for 12 months. Unfortunately, allocation was not random and severity in the waiting-list group was slightly less. Thirty percent of the treatment group no longer met criteria for BPD at the end of 1 year. There was little indication of change in the control group. However intent-to-treat calculations would only estimate a 19% remission rate, which is comparable with the spontaneous change in follow-along studies. A waiting-list control group is problematic and sometimes referred to as a "nocebo" group because the implicit contingencies of being on a waiting-list imply no change.

A further large-scale uncontrolled trial of psychodynamic psychotherapy is worth singling out. Dolan and colleagues (1997) reported on the outcome of a therapeutic community run on strictly democratic principles; for example, patients had a veto on the appropriateness of an admission. Of 598 patients referred, 239 were admitted and 137 (23%) returned assessment questionnaires at 1-year follow-up. About equal numbers of admitted and nonadmitted patients returned the questionnaires, about 80% of whom met diagnostic criteria for BPD. Clinically significant change on self-reported borderline symptomatology was seen in 43% of the treated and 18% of the untreated patients (30 vs. 12; $RR_{\text{SIPPUS CONTROL}} = 2.39$; 95% CI: 1.34, 4.27). Length of stay was associated with improvement. The comparison group places profound limitations on the study, not just because of the

absence of randomization and the varied reasons for being in the no-treatment group, but also because the pre-post time period covered in the treatment group was significantly longer (19 vs. 12 months). Nevertheless, the study provides data concerning the likely change to be observed in a specialist but routine service context.

The Cornell group (Clarkin et al., 2001) reported the outcomes of 23 female patients treated in transference-focused psychotherapy. The trial, which was a pilot study for the Personality Disorders Institute Borderline Personality Disorder Research Foundation RCT (Clarkin et al., 2004b), was a carefully conducted follow-along study of 23 female patients. After 1 year of treatment, suicidal behavior substantially decreased and the pre-post comparison of inpatient days suggested significant cost savings.

Gabbard and colleagues (2000) reported a prospective, naturalistic, and uncontrolled study of consecutive patients admitted to the Menninger Hospital. Only 35% of the 216 completed in the sample were diagnosed with BPD. About half the patients had mixed personality disorder or personality disorder not otherwise specified. An important feature of the study was the telephone follow-up at 1 year. GAF scores increased: only 3.7% had GAF scores above 50 on admission, which increased to 55% at discharge and 66% at follow-up. Other measures reflected a similar pattern. The study suggests that inpatient treatment can initiate improvement even in relatively severely dysfunctional patients. But the absence of a comparison group and the unknown selection bias introduced by limited participation reduces the generalizability of the data. Furthermore the treatment package offered, although relatively consistent across patients, was not monitored in relation to each discipline. Given the wide diversity of length of stay, it is hard to link progress to psychotherapeutic experience.

Chiesa and colleagues (Chiesa & Fonagy, 2000, 2003; Chiesa, Fonagy, Holmes, & Drahorad, 2004; Chiesa, Fonagy, Holmes, Drahorad, & Harrison-Hall, 2002) reported a controlled but not randomized trial of inpatient psychodynamic treatment. Two forms of hospital-based treatment were contrasted with a general community-based psychiatric treatment model. In the first protocol, patients were admitted for approximately 12 months with no aftercare. In the second, patients were admitted for only 6 months, but this was followed by 12 months of outpatient therapy with community support. The third arm received community psychiatric care (medication and brief hospital admissions as necessary). Two hundred and ten patients with at least one diagnosis of personality disorder were allocated according to geographical criteria into the three groups. Outcome was evaluated at 6, 12, and 24 months on self-harm and suicide attempts

and on self-reports of symptom severity and social adaptation. At 24 months, only the phased or step-down condition showed improvements, whereas patients in the long-term residential model showed no improvements in self-harm, attempted suicide and number of readmissions. There were significant reductions in symptom severity, and improvements in social adaptation and global functioning. Patients in the general psychiatric group showed no improvement in these variables, except for self-harm. Forty seven percent of the inpatient group and 73% and 71% of the step-down and general psychiatric groups, respectively, reported no self-harm in the previous 12 months ($RR_{INPAT\<small>vs\</small>TAU@24MONTHS} = .66$, 95% CI: .47, .93; $RR_{SDPAT\<small>vs\</small>TAU@24MONTHS} = 1.03$; 95% CI: .81, 1.32). At 24 months, more of the inpatient group had hospital admissions in the previous 12 months (49% for inpatient group compared to 11% for step-down and 33% for the general psychiatric care group; $RR_{INPAT\<small>vs\</small>TAU@24MONTHS} = 1.5$; 95% CI: .92, 2.45; $RR_{SDPAT\<small>vs\</small>TAU@24MONTHS} = .34$; 95% CI: .14, .85). Thus, in terms of clinical outcome, the general psychiatric treatment group was somewhat inferior to the step-down group and superior to the inpatient group. The findings indicate that long-term inpatient therapy may be iatrogenic and may undermine some of the effective components of a treatment mode that results in substantial positive outcomes in more moderate doses. Only about 10–12% of the general psychiatric group showed clinically significant change in symptomatology and social adjustment, compared to over half of the step-down group and only about a fourth of the inpatient group. Mean GSI scores were more or less unchanged in the general psychiatry group. They were reduced by half a standard deviation in the inpatient group and by a whole standard deviation in the step-down group.

Only one randomized controlled study is available. Bateman and Fonagy (1999, 2001, 2003) reported on a study of 38 patients assigned to specialist partial hospitalization or to routine care. Over 18 months, partial hospitalization showed significant gains over controls on measures of suicidality, self-harm, and inpatient stay. These gains became apparent at 6–12 months of treatment and increased over time. Follow-up at 18 months, which included an intent-to-treat analysis, demonstrated that not only did patients in the program maintain their gains but they also made further improvements. At the end of treatment 84% of the treatment-as-usual patients and 36% of the partial hospital patients had showed self-harming behaviors in the previous 6 months ($RR_{SH@18MONTHS} = .43$; 95% CI: .24, .75). At 36 months, 58% of the controls and 8% of the partial hospital patients had self-harmed in the previous 6 months ($RR_{SH@36MONTHS} = .14$; 95% CI: .03, .55). A cost-benefit analysis suggested that in the course of the treatment, additional costs

of the program were offset by reductions in inpatient and emergency room care costs as well as in medication. The difference in costs per patient became apparent in the follow-up period. The mean annual cost of service utilization was \$15,500 for the treatment-as-usual group and \$3,200 for the partial hospitalization group.

The second controlled trial carried out by Clarkin and colleagues (2004a) is the most ambitious and comprehensive trial of psychodynamic psychotherapy in any context. It contrasts transference-focused psychotherapy (TFP; Clarkin, Kernberg, & Yeomans, 1999) with dialectical behavior therapy (DBT; Linehan, 1993) and psychoanalytic supportive psychotherapy (SPT; Rockland, 1987). TFP is based on Kernberg's object relations model and is a twice-weekly outpatient therapy using clarification, confrontation, and transference interpretations. Psychodynamic supportive psychotherapy eschews transference interpretation and instead focuses on the strengthening of adaptive defences, forming an alliance, and providing reassurance. All therapists were experienced in relation to their modality. Of 207 patients interviewed for the trial, 109 met criteria. Nineteen refused randomization, but the remaining 90 were randomized to TFP, DBT, or SPT. The baseline GAF score was about 50, quite severe for an outpatient sample. Results are available to 12 months. In all therapies, GAF scores increased by about 10 points. BDI scores decreased significantly, and social adjustment scores increased. There was no significant change in anxiety scores. The majority of patients showed a reduction in their suicidality. Only a minority appeared to be getting worse. Hierarchical linear modelling showed that TFP and DBT significantly reduced suicidality but patients in SPT treatment did not significantly improve. However, all three treatment groups improved significantly in terms of global functioning and depression. On the Adult Attachment Interview (AAI; Main & Goldwyn, 1998), ratings of coherence (closely related to attachment security) improved for all three groups. The improvement was most marked for the TFP group, but this difference was not statistically significant. Reflective function scores (Fonagy, Target, Steele, & Steele, 1998), based on the AAI and related to mentalization, showed slight improvements in the other two treatments, but was only significant for the TFP group (Levy & Clarkin, in press). Other than this significant interaction, there were no differences between the treatment groups, except for a significantly higher early termination rate from DBT that could reflect more rapid improvement or lower acceptability of this treatment with this group.

Antisocial personality disorder

There are no trials of psychodynamic treatment of antisocial personality disorder, and a small number of observational studies of individuals

detained in high security settings (e.g., Reiss, Grubin, & Meux, 1996). It is likely that at least some of these individuals would meet criteria for antisocial personality disorder but this cannot be assumed. The studies are reviewed by Warren and colleagues (2003). In general, improvements are noted, but the methodology is too weak to permit generalization.

More recently, Saunders (1996) contrasted CBT and STPP. The treatment was offered to men who were violent with their partners; of the 136 participants, 40% met criteria for antisocial personality disorder. No differences are reported between the groups in terms of recidivism. In the absence of a no-treatment control group, it is difficult to judge if either treatment was effective. A prison service in the UK (Grendon) is currently run along relatively coherent psychodynamic principles. Taylor (2000) describes outcomes from a 7-year follow-up of 700 individuals who participated in this therapeutic community. The comparison groups in the report consist of demographically matched individuals who were never admitted to Grendon from the wait-list group and 1,400 individuals treated from a general prison population. Attendance at the psychodynamic therapeutic community at Grendon was associated with a reduced rate of reoffending. Furthermore, there was a link between length of stay at Grendon and outcome. However, when prior criminal histories are controlled for, the apparent impact of Grendon is reduced. Thornton and colleagues (1996) looked at a subgroup who were sex offenders. When matched with a group with similar forensic histories, those at the chronic end of the severity spectrum (at least two previous convictions for sex offenses) had better outcomes.

Cluster C (anxious-fearful) personality disorder

Cluster-C personality disorders include avoidant personality disorder (social discomfort, timidity), dependent personality disorder (dependent on reassurance), and obsessive-compulsive personality disorder. These are the most prevalent (10%) personality disorders in the general population (Torgersen, Kringlen, & Cramer, 2001).

We know of only one open trial of psychodynamic therapy that explicitly focuses on avoidant personality disorder (Barber, Morse, Krakauer, Chittams, & Crits-Christoph, 1997). The researchers used supportive-expressive psychotherapy in the treatment of 38 individuals, two thirds with avoidant and one third with obsessive-compulsive personality disorder. Attrition was high, with 50% of those avoidant personality disorders leaving therapy prematurely. Forty percent of those with avoidant personality disorder who stayed with therapy retained their diagnosis. Those with obsessive-compulsive personality disorder had better retention rates and better outcomes.

A small Norwegian trial (Svartberg, Stiles, & Seltzer, 2004) compared STPP with cognitive therapy for outpatients with cluster C personality disorder. Fifty one patients were randomly allocated to receive 40 weekly sessions of dynamic therapy (Malan's approach) or cognitive therapy (Beck's approach). Sessions were videotaped, and adherence and integrity checks were performed on both therapies. Only two patients did not attend follow-up assessments at 6, 12, and 24 months. Both groups improved and continued to improve after treatment, both symptomatologically and in terms of personality profile (Millon's Clinical Multiaxial Inventory). On the SCL-90, 38% and 17%, respectively, for STPP and Cognitive therapy were recovered to an asymptomatic status (below the clinical cutpoint) at the end of treatment ($RR_{SCL-90@12MONTHS} = 2.6$; 95% CI: .94, 7.22), and at 2-year follow-up these had increased to 54% and 42%, respectively ($RR_{SCL-90@24MONTHS} = 1.46$; 95% CI: .8, 2.65). These figures were somewhat lower for personality change on the Millon inventory (63% of the STPP group and 48% of the CBT group changed on this measure; $RR_{MILLON@24MONTHS} = 1.39$; 95% CI: .83, 2.31). The results clinical equivalence test (within interval of 20% of a zero difference) suggests that the group differences on the Millon are probably trivial but that the differences on the SCL-90 may be of clinical significance. Sadly, the study is underpowered to detect more than a large effect size, which is unlikely to be observed in this kind of context.

A randomized trial compared STPP for predominantly cluster C personality disordered individuals along the lines developed by Malan and Davanloo ($n = 31$) with brief adaptive psychotherapy (BAP) developed by the authors (Winston et al., 1994) ($n = 32$). There was also a waiting-list control group ($n = 26$). The former form of STPP is believed by the authors to be more confrontational, but both appear to address defensive behavior and elicit affect in interpersonal contexts. Thirty two patients were randomly allocated to BAP and 31 to more traditional STPP. Twenty five completed STPP and 30 completed BAP. Mean treatment length was 40 sessions, but the waiting-list control group lasted only 15 weeks. A large number of therapists participated. Treatment manuals and videotaping were employed for adherence checks. A variable length follow-up reached about two thirds of the treated group. The two treated groups both showed significant change on the GSI of the SCL-90 of approximately one standard deviation and some change on the Social Adjustment Scale (SAS). There were no significant differences between the two treated groups—not surprising given the similarity of the approaches. There were some differences between the groups in terms of gender and Axis I diagnoses and heterogeneity as to Axis II diagnoses. The study was underpowered to look at specific benefits of

each therapy in relation to particular personality disorder types. An earlier study by the same group (Winston et al., 1991) contrasting the same therapies reported essentially the same results with similar effect sizes on the GSI and the SAS. A more robust follow-up of this study at 18 months (Winston et al., 1994) indicated that gains were maintained.

Long-term psychotherapy

In the previous sections we considered evidence available to support therapeutic interventions that are derivatives of psychoanalysis. However, there is a certain degree of disingenuity in psychoanalysis embracing these investigations. Most analysts would consider that the aims and methods of short-term, once-a-week psychotherapy are not comparable to "full analysis." What do we know about the value of intensive and long-term psychodynamic treatment? Here the evidence base becomes somewhat patchy, and we cannot restrict the review to randomized controlled trials.

The Boston Psychotherapy Study (Stanton et al., 1984) compared long-term psychoanalytic therapy (two or more times a week) with supportive therapy for clients with schizophrenia in a randomized controlled design. On the whole, clients who received psychoanalytic therapy fared no better than those who received supportive treatment. The partial hospital RCT (Bateman & Fonagy, 1999) included in the psychoanalytic arm of the treatment included therapy groups three times a week as well as individual therapy once or twice a week over an 18-month period. A further controlled trial of intensive psychoanalytic treatment of children with chronically poorly controlled diabetes reported significant gains in diabetic control in the treated group, which was maintained at 1-year follow-up (Moran, Fonagy, Kurtz, Bolton, & Brook, 1991). Experimental single case studies carried out with the same population supported the causal relationship between interpretive work and improvement in diabetic control and physical growth (Fonagy & Moran, 1991). The work of Chris Heinicke also suggests that sessions four or five times a week may generate more marked improvements in children with specific learning difficulties than a less intensive psychoanalytic intervention (Heinicke & Ramsey-Klee, 1986).

One of the most interesting studies to emerge recently was the Stockholm Outcome of Psychotherapy and Psychoanalysis Project (Blomberg, Lazar, & Sandell, 2001; Grant & Sandell, 2004; Sandell et al., 2000). The study followed 756 persons who received national insurance-funded treatment for up to 3 years in psychoanalysis or psychoanalytic psychotherapy. The groups were matched on many clinical

variables. Analysis four or five times a week had similar outcomes at termination when compared with one to two sessions of psychotherapy per week. During the follow-up period, psychotherapy patients did not change, but those who had had psychoanalysis continued to improve, almost to a point where their scores were indistinguishable from those obtained from a nonclinical Swedish sample. Although the results of the study are positive for psychoanalysis, certain findings are quite challenging. For example, therapists whose attitude to clinical process most closely resembled that of a "classical analyst" (neutrality, exclusive orientation to insight) had psychotherapy clients with the worst, commonly negative, outcomes.

The German Psychoanalytic Association undertook a major follow-up study of psychoanalytic treatments conducted in that country between 1990 and 1993 (Leuzinger-Bohleber, Stuhr, Ruger, & Beutel, 2003; Leuzinger-Bohleber & Target, 2002). A representative sample ($n = 401$) of all the patients who had terminated their psychoanalytic treatments with members of the German Psychoanalytical Association (DPV) were followed up. Between seventy percent and 80% of the patients achieved (average 6.5 years after the end of treatment) good and stable psychic changes according to the evaluations of the patients themselves, their analysts, independent psychoanalytic and nonpsychoanalytic experts, and questionnaires commonly applied in psychotherapy research. The evaluation of mental health costs showed a cost reduction through fewer days of sick leave during the 7 years following the end of long-term psychoanalytic treatments. Qualitative analysis of the data also pointed to the value that patients continued to attach to their analytic experience. In the absence of pretreatment measures, it is impossible to estimate the size of the treatment effect.

Another large pre-post study of psychoanalytic treatments has examined the clinical records of 763 children who were evaluated and treated at the Anna Freud Centre, under the close supervision of Freud's daughter (Fonagy & Target, 1994, 1996; Target & Fonagy, 1994a, 1994b). Children with certain disorders (e.g., depression, autism, conduct disorder) appeared to benefit only marginally from psychoanalysis or psychoanalytic psychotherapy. Interestingly, children with severe emotional disorders (three or more Axis I diagnoses) did surprisingly well in psychoanalysis, although they did poorly in once- or twice-a-week psychoanalytic psychotherapy. Younger children derived greatest benefit from intensive treatment. Adolescents appeared not to benefit from the increased frequency of sessions. The importance of the study is perhaps less in demonstrating that psychoanalysis is effective, although some of the effects on very severely disturbed children were quite remarkable, but more in identifying groups for whom the

additional effort involved in intensive treatment appeared not to be warranted.

The Research Committee of the International Psychoanalytic Association has recently prepared a comprehensive review of North American and European outcome studies of psychoanalytic treatment (Fonagy, Kachele, et al., 2002). The committee concluded that existing studies failed to unequivocally demonstrate that psychoanalysis is efficacious relative to either an alternative treatment or an active placebo. A range of methodological and design problems was identified, including absence of intent-to-treat controls, heterogeneous patient groups, lack of random assignments, and failure to use independently administered standardized measures of outcome. Nevertheless, the report, which ran to several hundred pages and briefly describes more than 50 studies, is encouraging to psychoanalysts.

Another overview (Gabbard, Gunderson, & Fonagy, 2002) suggested that psychoanalytic treatments may be necessary when other treatments prove to be ineffective. The authors concluded that psychoanalysis appears to be consistently helpful to patients with milder disorders and somewhat helpful to those with more severe disturbances. More controlled studies are necessary to confirm these impressions. A number of studies testing psychoanalysis with state-of-the-art methodology are ongoing and are likely to produce more compelling evidence over the coming years. Despite the limitations of the completed studies, evidence across a significant number of pre-post investigations suggests that psychoanalysis appears to be consistently helpful to patients with milder (neurotic) disorders and somewhat less consistently so for other, more severe groups. Across a range of uncontrolled or poorly controlled cohort studies, mostly carried out in Europe, longer intensive treatments tended to have better outcomes than shorter, nonintensive treatments (demonstration of a dose-effect relationship). The impact of psychoanalysis was apparent beyond symptomatology in measures of work functioning and reductions in health care costs.

Conclusions

Considerable evidence has accumulated for the evidence of psychoanalytic approaches for a range of diagnostic conditions. The strength of the evidence varies across clinical groups and in the case of some groups there is little current evidence supporting the approach. In no area is the evidence compelling, but in most areas where systematic investigation has been carried out, outcomes are comparable to those obtained by other therapeutic methods. There are disorders where the outcome of psychoanalytic therapy is in certain respects better than that of alterna-

tive treatments (e.g., borderline personality disorder). In other areas (e.g., depression), opportunities for the psychodynamic approach are created by the known limitations of rival orientations. In relation to treatment-resistant conditions, technical innovation is called for, and this has been a significant barrier to psychoanalytically oriented investigations. Even where original and effective approaches have been developed, replication of findings by groups other than those responsible for the development of the program are hard to find.

There can be few valid excuses for the currently thin evidence base of psychoanalytic treatment. In the same breath that we, as psychoanalysts, often claim to be at the intellectual origin of other talking cures (e.g., systemic therapy, cognitive-behavioral therapy), we also seek to shelter behind the relative immaturity of the discipline to account for the absence of research evidence for its efficacy. Yet the evidence base of these “derivatives” of psychoanalytic therapy has been far more firmly established than evidence for the approach at the root of the psychotherapy movement (Holmes, 2002).

Of course there are reasons for this—reasons such as the long-term nature of the therapy, the subtlety and complexity of its procedures, the elusiveness of its self-declared outcome goals, and the incompatibility of direct observation and the need for absolute confidentiality. None of these reasons can stand up to careful scrutiny. In recent years, the scientific method has been extended to a wide range of highly complex phenomena, and our understanding of the psychological processes that are at work in mental disorders and their psychosocial therapy is a focus of intense inquiry from a range of perspectives. Neither the subject matter nor the difficulties inherent in its systematic investigation account for the relative lack of support for empirical and systematic qualitative studies of the process and outcome of psychoanalytic therapies. Currently, political considerations make funded research in this field difficult, but historically this has not always been the case. A more likely reason for the absence of psychoanalytic outcome research lies in the fundamental incompatibilities in the world views espoused by psychoanalysis and by most of current social science (Whittle, 2000). This will require a shift in epistemology on the part of psychoanalytic psychotherapists.

There are several components to this attitude change: (1) incorporating data-gathering methods beyond the anecdotal, methods that are now widely available in social and biological science; (2) moving psychoanalytic constructs from the global to the specific, which will facilitate cumulative data gathering and identifying the psychological mechanisms that are entailed in change in psychodynamic therapy; (3) considering alternative accounts for behavioral observations of change;

(4) increasing psychoanalytic sophistication concerning social and contextual influences on pathological behavior and its response to treatment; (5) ending the splendid isolation of psychoanalysis by undertaking active collaboration with other scientific and clinical disciplines; (6) using the knowledge base of psychoanalysis to generate innovative treatment approaches to currently treatment-resistant conditions; (7) integrating successful psychotherapeutic manipulations from other disciplines into a psychodynamic approach; (8) identifying clinical groups for whom the psychodynamic method is particularly effective; and (9) adopting a scientific attitude that celebrates the value of the replication of observations rather than their uniqueness. Rather than fearing that fields adjacent to psychoanalysis might destroy the unique insights offered by long-term intensive individual therapy, psychoanalysts must embrace the rapidly evolving "knowledge chain," focused at different levels of the study of brain-behavior relationships. As Kandel (1998; 1999) pointed out, this may be the only route to the preservation of the hard-won insights of psychoanalysis.

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