Experimental method and psychodynamic theory: discussion paper[1]

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Introduction

We live in a world of ambiguities, and in such a world the purpose of experiment is to allow appropriate interpretations of what is observed. Where there may be underlying regularities, the experiment makes it easier to know which hypotheses about those regularities can sensibly be entertained and which need to be changed because they are mistaken. Control conditions in experiments function, simply, to control the range of possible interpretations of phenomena.

It will be argued here that the idea of the experiment is of fundamental importance for understanding and advancing psychodynamic theory. It is important in two ways: first in evaluating the outcomes of therapy, and secondly as a description of the nature of psychodynamic therapy. Therapy is an activity in which the patient is both experimenter and subject of a kind of experiment.

Choosing among therapies

In the last ten years advances have occurred in the experimental investigation of the effectiveness of therapy. In 1975 Sloane *et al.* published a book showing how the efficacy of different kinds of therapy could be evaluated. They compared the outcomes of psychodynamic therapy, behavioural therapy, and a control condition. Each therapy patient had four months of one-to-one therapy, for an hour a week, with either a psychodynamic or a behavioural therapist. Control subjects had all the assessment interviews, but no formal therapy. Instead they were on a waiting list for therapy four months after their initial interviews. In the mean time they had the promise of help, reassurance and frequent telephone contact with a researcher.

The study of Sloane *et al.* (1975) is important because it succeeded in taking the essential steps of experimental method. From a sample of patients with disabling problems, Sloane *et al.* selected those who seemed suitable for psychotherapy. These patients had relatively well defined, and hence measurable, though troublesome symptoms which might be amenable to therapy. They were randomly assigned to one of the three treatment conditions, with 30 subjects in each. They were assessed before and after the experiment on a variety of measures, including self-report measures, interviews with a close friend or relative, the therapists' evaluations, and, most importantly, the evaluation of target symptoms and adjustment in work and social relationships by an independent assessor who was blind to which treatment each participant was receiving.

Despite some criticisms (e.g. of Rachman & Wilson 1980) the study provided evidence on questions which had become the subject of acrimonious debate. In the severity of their target symptoms, subjects in all three conditions improved. Both psychodynamic and behavioural therapy resulted in outcomes which were better than those of the control condition, and the differences were statistically significant. Moreover, rather than behavioural therapy being vastly better than psychodynamic therapy – as some behaviourists had argued – there was little difference between the two therapies. On some measures, there was a slight but non-significant difference in favour of behavioural therapy.

A second recent advance has been the method of evaluating a range of outcome studies, introduced by Smith *et al.* (1980). This method became necessary because, although the experi-

ment and its statistical analysis is an appropriate way of deciding whether some treatment has an effect, difficulties arose in evaluating large numbers of diverse evaluations of therapy. In fields like physiology important results are replicated. In the study of the effects of therapy, however, there have been few replications. Instead, there have been many kinds of test of many kinds of therapy. These tests have used different measures, in different cultures, with different kinds of patient and with different therapists. Thus, even despite the existence of some well conducted studies like that of Sloane *et al.* (1975), it was possible, and indeed usual, for authors of review papers to pick their way through the literature and come to quite different conclusions – even to conclusions consistent with their prior allegiances!

Smith *et al.* (1980) called their method 'meta-analysis'. It consisted of evaluating published studies of the outcome of psychological therapies of all kinds that had at least a therapy group and a control group, or a comparison between two forms of therapy. For each outcome measure used in each study they defined an 'effect size', the difference between the mean outcomes for therapy and control subjects, expressed in units of standard deviations of the control group. They set out to include all outcome studies published up to 1977, but estimated that they only included about three-quarters of them, i.e. 475 studies, in which there were 1766 effect size measures. They also analysed a smaller number of studies in which psychotherapy was compared with drug treatments.

The overall effect size of psychological therapy across all 475 psychotherapy studies was 0.85 of a standard deviation, i.e. at outcome, the average person receiving some kind of therapy was better off than 80% of the control group. Smith *et al.* also examined the effects of different kinds of therapy with different kinds of client, as well as comparing psychotherapy with drugs, and identifying which features of outcome studies were spuriously associated with good outcomes. Psychodynamic therapies were found to do about as well as, though not better than, most other therapies, including behavioural therapies, with cognitive therapies doing best. Psychological therapies were also found to have effects of comparable sizes to those of psychoactive drugs.

What is perhaps surprising is their finding that different therapies had similar effects. Shapiro & Shapiro (1982), in a meta-analysis of studies published between 1975 and 1979, estimated that only about 10% of the variance of effect size of psychological therapy studies was attributable to differences in the type of therapy. This similarity of effects of different therapies totally contradicts the claims of many therapists.

The work of Smith *et al.* (1980) has ushered in a new era in the evaluation of therapy. Reviews subsequent to theirs have mostly supplemented their work. For example, Andrews & Harvey (1981) analysed those studies in the Smith *et al.* data set which used bona fide patients, and Miller & Berman (1983) have compared cognitive and cognitive-behavioural therapies in this way. Only one review to my knowledge, by Prioleau *et al.* (1983), used the technique but advocated a return to choosing just those studies that the sceptical might imagine would tend towards a particular conclusion (in their case, the conclusion of an attempted '*reductio ad absurdum*' of meta-analysis; p 306).

So unlike single experimental trials, meta-analytic studies have been replicated and have for the most part produced conclusions similar to those of Smith *et al.* (1980). Meta-analysis has made comparison of different therapies much easier. Not all the problems have been solved, however. In particular, only a small minority of published outcome studies are of the kinds of therapy with the kinds of patients which constitute the majority of clinical practice, and almost all of them are time-limited.

As an example of a result from a typical outcome study, Figure 1 shows the mean scores in an experimental design by Feinberg-Moss and myself (in preparation), which set out to test the effect of a single therapeutic component in the practice of a single therapist. In one condition 8 subjects received between ten and twelve 1.25 hour sessions of once-a-week individual psychodynamic therapy using guided visual imagery after the manner of Assagioli (1965). Imagery was the therapeutic component being evaluated. In a second condition, 6 people had the same kind and duration of therapy but entirely verbally. In a minimum-contact control condition 7 subjects had four sessions, including all the assessments.
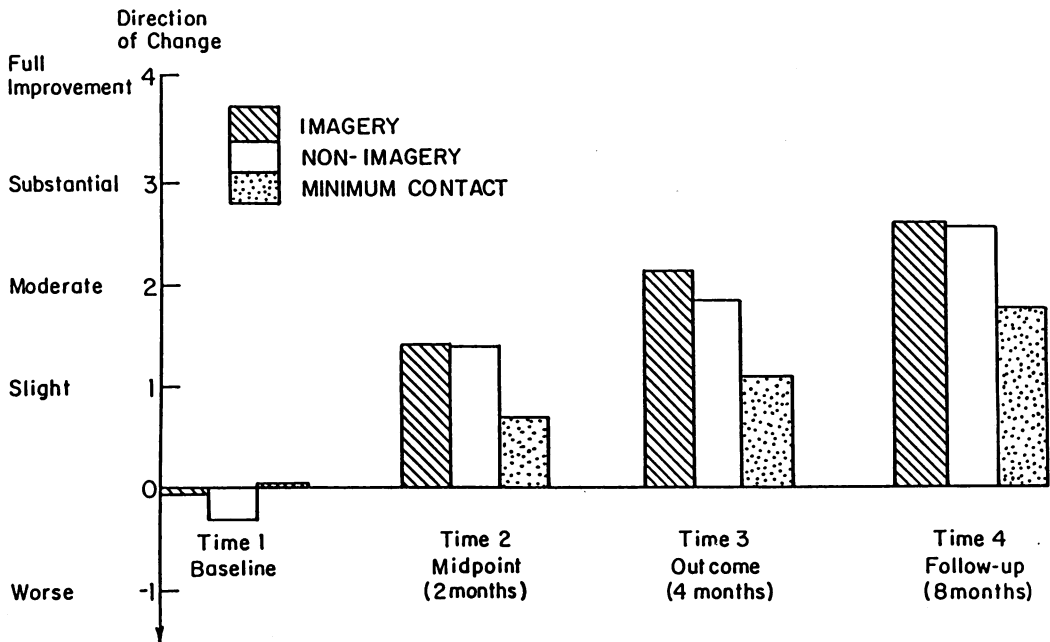
Figure 1. Comparison of mean scores on target aims at four assessment points in an outcome study with two therapy conditions (imagery and non-imagery) compared with a minimal contact control group (Feinberg-Moss & Oatley, in preparation)

The pattern of results was similar to that of Sloane *et al.* (1975). All three groups improved on target symptoms (see Figure 1). People in the imagery condition were significantly improved compared with the minimal-contact subjects, but the two therapy conditions were not significantly different from each other. Although using imagery suited the practice of this therapist, it made little difference to outcomes for the patients.

Cognitive changes in therapy
According to the currently influential cognitive approach in psychology, therapy will involve cognitive change. We therefore included a cognitive measure of self-acceptance using repertory grids, as devised by Kelly (1955) and applied to psychodynamic therapies by Ryle (1982). This too showed improvements with a similar pattern to that of the target aims (Figure 2).

Grids allow concepts about relationships to be followed during the course of therapy. Figure 3, for instance, shows the results of a principal components analysis using Slater's (e.g. 1976) Ingrid program, of the initial grid of a male subject in the imagery condition of our study. This produces a map in which constructs, or concepts, that a subject has about others are shown around the circumference. Elements are the subject's perceptions of people, each rated on each of these constructs. In our study there were three types of element, representations of the patient's self, of actual others (e.g. mother, father, wife) and of sub-personality figures elicited in guided imagery (shown in capitals on Figure 3). For reasons of clarity Figure 3 shows only a selection of the constructs and elements elicited.

The map is best thought of as a view of the territory of a person's conceptual space of relations with other people. In such a territory there are both regions in which elements can appear and proximities between elements. Regions are defined by the constructs. Thus, for this subject, there is a westerly region of constructs such as 'dogmatic' and 'get rid of', an easterly region which is 'warm', 'acceptable' and so on. Aspects of the patient's perception of elements
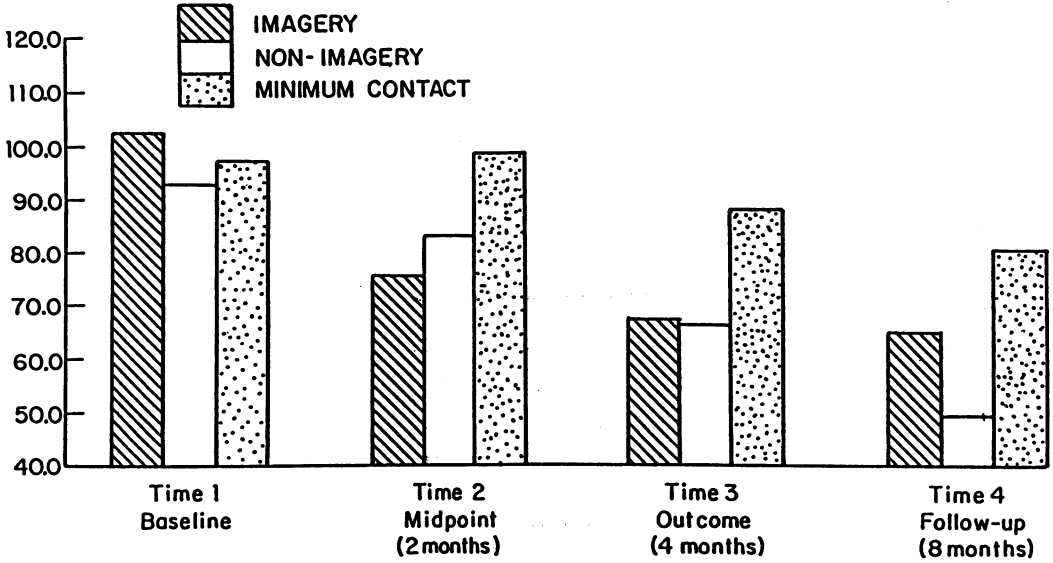
Figure 2. Comparison of mean self-acceptance scores (from the table of relations between constructs and elements in degrees from Slater's (1976) Ingrid program). Other details as for Figure 1
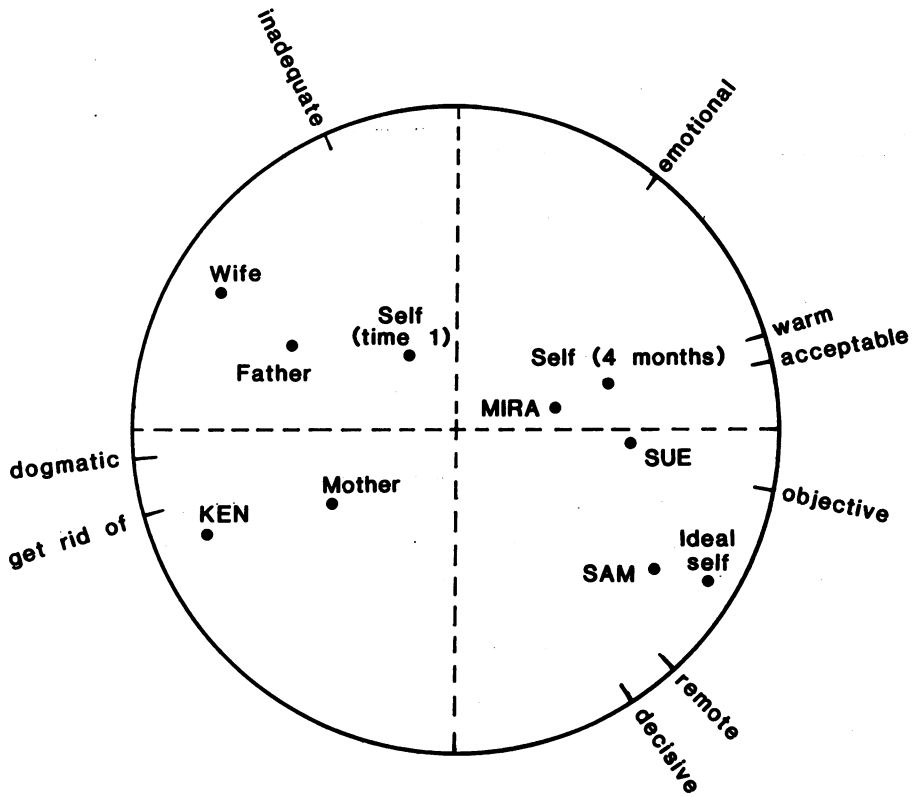

Figure 3. Map generated by principal components analysis, using Slater's Ingrid program, of a repertory grid for a single subject in the imagery condition of Feinberg-Moss & Oatley's study (in preparation). The x-axis shows the first principal component and the y-axis the second principal component; elements and constructs are plotted with respect to them

in his grid can be seen by the region of the territory they occupy. Before therapy (time 1) this subject placed himself in the 'inadequate' region, while the fantasy element 'Sue', was seen as 'acceptable'. Secondly, geographical proximity of elements in the grid implies a similarity in the way a subject sees them, while polarizations indicate dissimilarity. Thus this subject saw his father and his wife as rather similar, and himself at time 1 as very different from himself as he would like to be (ideal self).

During therapy, elements typically move within the territory of the map. Using repertory grids, a therapy case is like a journey through different regions, with meetings of various kinds: a kind of 'Patient's Progress'. Where therapy has a successful outcome, the self, after perhaps a series of meetings (identifications) and changes of distance between various elements, has typically moved into a region which is more 'acceptable' to the patient. By the outcome of therapy, the subject of Figure 3 had moved closer to two positively valued fantasy elements Sue and Mira, and into the 'acceptable' region. It is the relation to this construct that provides the score on the self-acceptance scale, the means of which are shown in Figure 2. (Note that 'self at 4 months' indicates here the outcome position of that element plotted on the time 1 map. In the complete map at 4 months some other elements had also moved as had some constructs – for instance father had become less to be 'got rid of'.)

Kelly (1955) argued that personality consists of having a set of constructs to anticipate aspects of the world, and particularly those most important aspects of the world, other people and one's relations with them. To have a personality, then, is to inhabit an implicit theory, and it is an aspect of this theory which is externalized in grids, and which (in cognitive terms) is the locus of change in therapy. Moreover, maps derived from grids can themselves be discussed with the patient in the course of therapy.

The personal relationship of therapy
One conclusion that could be drawn from the similarity of effects of different kinds of therapy, including that demonstrated in this study of our own, is that although therapists like developing new methods, specific methods may have more to do with structuring the therapist's mind than with enabling the patient's mind to change. On the basis of the experimental evidence, the potent factor in therapy, at least in the short term, would seem to lie elsewhere. The most likely explanation is that there is a 'common factor' (see e.g. Shapiro 1980) in the therapies that have been studied empirically. The obvious candidate is the personal relationship between therapist and patient (see e.g. Oatley 1984). The one factor that all psychotherapies have in common is such a relationship.

Even if psychodynamic therapy does not have a privileged place as far as efficacy is concerned, it does have an articulate theory about the nature of the therapeutic relationship, i.e. the theory of transference (e.g. Freud 1912a, 1915, 1917). Since almost all outcome studies are of short-term therapy and many of them also on volunteers rather than patients with seriously disabling symptoms, the psychoanalytic account would be that such outcome studies derive their positive results from 'transference cures': that is to say from suggestion, direct or indirect. The therapist takes on the mantle of a benign parental surrogate while the help-seeking patient becomes compliantly childlike, and hence suggestible.

We need not doubt such an effect. What may be important for many patients is just such a relationship of trust. Just this might allow them to turn a difficult corner in their lives, and hence be able to experience themselves in a different way; to be more responsible for their actions and interactions and less victims of the circumstances about which they complain.

Behavioural and cognitive therapy is more directive than psychodynamic therapy. According to the theory of transference, over the short term at least, it is not surprising that directive therapies should be as successful in promoting the remission of symptoms, and perhaps in some cases even more successful, than non-directive ones.

Some conclusions for psychodynamic theory
What then are the implications of these experimental trials for psychodynamic theory? First, a great many generalizations are made about therapy on the basis of clinical intuition, of strong

commitment, and even on the basis of mere supposition. What the outcome trial supplies is the possibility of generalization on the basis of evidence. Thus, for instance, on the basis of meta-analyses the generalization can be made that symptoms remit, on average, more quickly in most kinds of therapy than 'spontaneously'. Secondly, it is essential for the coherence of psychodynamic theory that effects of psychoanalytic therapy are evaluated.

The most substantial argument against the coherence of psychoanalysis is that it is difficult to rule out the possibility that both the analytic remission of symptoms, and the research results of analysis, stem from the patient's compliance to the powerful influence of the therapist. Very many critics have pointed this out, starting at least as early as Fliess in 1901 (see Freud 1954, pp 334 and 337). If the objection is upheld, there is no reason to suppose that symptomatic improvement in analytic therapy would differ from that in directive therapies. The 'discoveries' of psychoanalysis moreover, e.g. conclusions about repression, infantile sexuality and so on, would be ambiguous. We would have no more reason to suppose that they were universals of human mentality than that they were projections of Freud and his followers.

Grünbaum (1980) has discussed in detail how the central defence offered by Freud against the charge of epistemological contamination of analytic findings by suggestion is the 'tally argument' (Grünbaum 1980, p 322). It derives from Freud's 1917 lectures: 'The doctor has no difficulty, of course, in making [the patient] a supporter of some particular theory and in thus making him share some possible error of his own. In this respect the patient is behaving like anyone else – like a pupil – but this only affects his intelligence, not his illness. After all, his conflicts will only successfully be solved and his resistances overcome if the anticipatory ideas [i.e. interpretations] he is given tally with what is real in him' (Freud 1917, p 452). Freud thus rests his epistemological claim on the issue of whether interpretations 'tally' with what is real in the patient, and this in turn can be tested by whether conflicts are resolved and whether new pieces of memory open up (see also Freud 1937).

Freud (1917) made it clear that the principal difference between analysis and the suggestive therapies, in which success depends on a positive transference, is that analysis involves 'constantly resolving the transference ... In every other kind of suggestive treatment the transference is carefully preserved and left untouched: in analysis it is itself subjected to treatment and ... must itself be cleared away' (1917, p 453). Only in this is there any lasting psychodynamic change. The resolution of transference has continued to be seen as the fundamental therapeutic endeavour of psychoanalysis (e.g. Strachey 1934, Laplanche & Pontalis 1973).

Unfortunately, published outcome studies do not bear as directly on this issue as one would wish. Almost all of them are of short durations during which it is unlikely that significant transferences could be resolved. Most psychodynamic theorists would reserve the term 'analysis' for transformations of personality, i.e. of a person's implicit theory, and not just remission of symptoms. But such transformations might plausibly take four years or more, rather than the four months that is typical of therapy investigated in controlled trials.

Though Freud saw no reason to appeal to empirical study of either his therapy or his theoretical concepts, it seems nowadays clear that for psychoanalysts to persist in eschewing trials of psychoanalytic therapy smacks at least of timidity, if not of defensive rationalization. For psychoanalysis to be taken seriously by the scientific or medical communities, nothing would be so convincing as controlled trials of longer analyses. Nor do the studies so far published absolve analytic theorists from paying attention to them. For instance, Nicholson & Berman (1983), in a further meta-analytic study, found that changes due to many types of psychotherapy tend to be durable over time, i.e. the purely suggestive therapies are not necessarily liable to relapse as Freud (e.g. 1917) assumed.

What is at issue is the empirically testable assertion, central to psychodynamic theory, that long-term personality change is possible with therapy based on the resolution of transference, and that psychoanalytic therapy is the therapy of choice for such change. Only such a change, only the 'successful' solution of conflict, has been proffered to warrant the probity of psychoanalytic findings.

Therapist's or patient's theories?

It seems unlikely that the clinical intuitions and metaphors of a single person, however creative, should stand as incorrigible truths. By Freud's own account he was ambitious for cultural conquest and, as Kardiner (1977, pp 68–69) relates, he acknowledged that as a therapist he was perhaps too interested in pursuing his own theories, and was himself too much the patriarch to be in a position to resolve certain kinds of admiring and awesome transference onto himself.

I would like to argue that the case for psychoanalysis does not just rest with the question of whether there is anything demonstrably superior in psychoanalytic therapy for longer term changes of personality, important though this is. The case I would like to put is that in discussing the scientific status of psychoanalytic theory, we often mistake which theories are at issue.

For the most part the argument about psychodynamic theory is conducted as if the theories at issue are the therapist's theories about universals of mental life. The emphasis is on the therapist's suggestions, the therapist's interpretations, the findings of psychoanalysts, etc. This is of course consistent with the usual approach to illness in Western medicine. Thus medicine equips its practitioners with scientific knowledge qualifying them to give advice. Hence the debate about the verisimilitude of psychoanalytic theory is of interest to medical practitioners: if psychodynamic theory were to be found purely to be a fanciful, though interesting, construction then medically qualified practitioners might well discard it along with the Hippocratic theory of humours, and look elsewhere for more soundly based theories.

I have previously put forward an argument, however, that to think about what analysis is about in terms of the analyst's theories seriously misrepresents its value (Oatley 1982). Focusing on the therapist as the conductor of scientific researches (as Freud himself thought) and the purveyor of truths neglects a much more radical understanding of psychoanalysis. It is an understanding to which Freud refers only rather indirectly (e.g. in his technical papers (1912b, 1915) where he counsels evenly hovering attention, abstinence, that the therapist be not eager for a result, etc.).

According to this understanding of analytic therapy, it is not the therapist's theories that are really at issue. More correctly, and as implied by Kelly's (1955) view of personality, therapy is an activity undertaken by the patient (who then presumably should be called an agent). It is an activity in which the analysand can test his or her implicit theories in a quasiexperimental way. Thus to do his or her job properly the analyst provides not suggestions, nor even expert psychodynamic understandings. Rather the analyst takes part in a dialogue with the analysand in which the analysand can, as it were, test out in the relatively safe, controlled, laboratory-like atmosphere of the therapeutic relationship actions deriving from his or her implicit theories.

In some ways our implicit theories are like explicit theories of natural science. Thus as historians of science have shown, public and explicit theories of science sometimes change because of inner incoherences, sometimes because of new applications of an idea, sometimes because of refutations, even because of propaganda, class interests or cultural fashion. The vicissitudes of scientific theory are an instructive study for therapists: there is no reason to suppose that our private and implicit theories are not susceptible to like influences for change. Philosophers and historians such as Butterfield (1949), Koestler (1959), Popper (e.g. 1963), Kuhn (1962), Feyerabend (1975), Lakatos (1978) and others, in offering accounts of the change of scientific theories, therefore offer possible analogies for the transformation of people's more personal implicit theories, as well as for the ways in which theories can remain static.

The value of Popper's work in all this is his proposal that theories, in order to progress rationally, need to be refutable – need to be open to questioning. If theories are to improve rather than just change in some undefined direction, they must be capable of making mistakes. (Freud did not default in this respect, as Popper charged: his theories can be tested in the way I have discussed; see also Grünbaum 1980).

If we learn best from our mistakes, we must create environments where it is possible to make mistakes and to recognize them. The laboratory is an environment where natural scientists

who want to improve their scientific theories might learn from mistakes, even though resistance can be strong in these environments. Psychoanalytic therapy, similarly, is a place where analysands might change their implicit theories of personal interaction from the mistakes they make.

Freud's image of the analyst as a kind of archaeologist of the person, who observes and occasionally prompts the patient into the correct view of his or her pre-history, is misleading. It is not the therapist whose theories are at issue, or are communicated to the patient. As well as providing the laboratory-like space, the therapist is more in the role of a philosopher of science, listening to and reflecting on the variety of personal theories that emerge in therapy. His or her job is to create an interpersonal space where the patient can make and experience interpersonal mistakes and inconsistencies and hence be able to discover how his or her own implicit theories might be improved.

Natural science and human science

Although there are informative commonalities in the processes of change of both public theories of science and the more private, implicit theories of interpersonal action, there are also differences. Implicit personal theories are what Aristotle called 'practical': they have to do with interpersonal action, rather than with transcendental or objective knowledge to which natural scientific theorists aspire.

What Freud discovered is not so much a set of human universals, but the equivalent of a laboratory for practising a certain kind of human science. This science is not empirical but cultural, and rooted in language. It is an activity that Dilthey, in seeking to describe the distinctive attributes of human science (see e.g. Hughes 1959, Dilthey 1961) would have seen as appropriate to its human subject matter.

In this activity there is not a domain of observation relatively fixed, and with the observer outside it. Rather, there is interaction between two people, each with their own implicit theories. In psychoanalytic therapy one might ask whether, via a series of evasions, symptoms and other displacements, the patient can move forward via an intersubjective relationship in which, as Lacan (1966) says, 'the non-action of the analyst guides the subject's discourse towards the realization of his own truth' (p 93).

My argument is in agreement with that of Rycroft (1985) that psychoanalysis is more closely related to linguistics than to natural science. More specifically, the personal theories of analysands, and of all of us, are concerned in part with negotiating relationships with other people. The aspect of language involved is not the semantics of a natural world, but the pragmatics of our interactions with each other (Austin 1962, Searle 1969, Levinson 1983). The theory of 'speech acts' in linguistics involves the idea that in all utterances the speaker wishes to act on the hearer in some way. A speaker has, as Austin (1962) puts it, an illocutionary intention. He or she seeks to make requests, give advice, promise, complain, etc. to the hearer. Linguists who are interested in pragmatics then concentrate on explicating the necessary conditions for such acts to be, in Austin's sense, 'felicitous'.

In therapy, perhaps the most frequent speech act uttered by the patient is the complaint; indeed, one can view psychiatric symptoms precisely as 'complaints', though as Freud pointed out they often speak in languages other than those of words! What we do as patients is to complain and utter other speech acts to the therapist. The analyst tries to understand such utterances as interpersonal acts – as attempts whose illocutionary force is to induce the therapist to say 'I'll take care of that for you', etc. They exhibit just the kind of child-parent transference relationship which Freud argued is unrecognized by us, and needs to be analysed. Insofar as our relating with one another re-enacts aspects of our earliest relationships, such complaints and many other of our interpersonal acts are inappropriate. A therapist cannot take care of our life.

Psychodynamic therapy, then, is the attempt to offer a quasi-experimental setting in which the ways in which we compulsively and transferentially try to act on others (by complaining, pleading, sulking, punishing, seducing or whatever) can conceivably be experienced by the patient as such.

The therapist's role is to recognize, not by empirical observation, but by what Dilthey (e.g. 1961) called *Verstehen* (understanding), what interpersonal act is at issue, and to speak of it in a way that is recognizable to the analysand. This is a difficult discipline. It requires the therapist, as Freud counselled, to be non-interventive: to avoid responding with the speech acts of advice or retaliation which would be the counterpart of just the kind of interpersonal theory that the patient was lodged in, and which would simply confirm the patient's implicit theory. Instead, a transference interpretation is the speech act of recognition of what is being uttered by the patient. It involves the therapist in both taking part in the relationship and being able to understand what kind of relationship it is, what kind of illocutionary intention is being expressed. An interpretation may slightly anticipate the patient's being able to put this act more directly into words. But in this atmosphere, the psychoanalytic claim is that something much more important than the usual effects of suggestion can occur.

In treating the analyst as a parent surrogate, or other transference figure, the patient makes a mistake. But in interpreting what kind of act this is, the therapist also recognizes and accepts something important about the longings of the patient. Both for patient and analyst there can emerge an important moment of understanding which, it seems, can be transformative. As such a wish is recognized it need perhaps no longer be evaded, displaced and compulsively enacted. It thereby approaches more closely an interpersonal truth.

It is on some such idea as this that the distinctive claims of psychoanalysis rest. But just as the analytic session is not a fixed universe in which the analyst pursues empirical researches into the nature of the human psyche, neither is it an environment in which the patient makes 'observations'. Psychoanalytic therapy provides a place in which what Winnicott (1971) called a cultural space opens up. Each influences and is involved with the other. There is no external Archimedian point on which to stand in order to move this interpersonal world.

Nonetheless, the radical idea of therapy as a place in which it is the analysand who acts, and may learn from the mistakes of treating the analyst as a transference figure, is important. It requires the analyst to be in a relationship but not just to be coerced into the expectations of the patient's implicit theory. The space of therapy, like an experimental laboratory, is one in which because of the control procedure of the therapist's non-action, the analysand has a chance to experience the results of his or her interactions. This experience becomes one of ambiguity rather than compulsion, and then can resolve in a different way than usual.

The evidence from clinical practice, from the experience of therapy, or from empirical studies does not make it clear whether this image of psychodynamic therapy is itself a fantasy. There is not yet enough evidence to know whether psychodynamic therapy really does touch those parts that other therapies cannot reach. But the idea it points to is the possibility that a person might improve his or her implicit theories in a way which would involve something other than 'a particularly well-disguised . . . form of suggestive treatment' (Freud 1917, p 452). Whether or not psychoanalysis achieves this other ideal, it has asked whether such an activity may not be possible. So the idea remains that there might be forms of interaction within which a person might conceivably learn from interpersonal mistakes, before such mistakes cause damage. And if psychodynamic therapy does not provide this kind of opportunity, then it is an important question to ask what kind of practice might.

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