



This volume is dedicated with gratitude to  
Otto Kernberg MD, President of the IPA  
Robert Tyson MD, Secretary of the IPA

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# Preface

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It is good news that this second edition of the Open Door Review has been published.

Firstly, it means that the initial edition had a wide circulation.

Secondly, we can continue this circulation process and let people know about the book, and all research work covered therein, not only in the psychoanalytical field, but also in that of research organisations, universities and mental health bodies.

In fact now, more than ever, we must make people aware of empirical research derived from the practice of psychoanalysis and the knowledge acquired through this practice.

While clinical research remains an extremely productive tool, we must widen and complement this knowledge by other methods that are close to epidemiological studies, therapeutic trials and experimental methodology.

The Open Door Review allows us to evaluate the extent and the quality of research work accomplished during past decades. We can thus review methodological progress and collect new data to assist us in our practices and give way to new research.

I hope that psychoanalysts will welcome this second edition and make students and the scientific world around them aware of it.

Daniel Widlöcher

President-Elect of the International Psychoanalytical Association

July 2001



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## About the Contributors

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# Introduction to the Second Edition

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The first edition of the open door review has been a considerable success. The IPA has sold at cost of reproduction all the copies that were printed. Nevertheless requests have continued to come in, so we had the choice of updating or reprinting. If you look at the expanded sections of the second edition, I hope you will agree with us that a new edition was the only way to go. Considerable progress has been made over the last 3 years, and we felt this should be reflected in the review. We are pleased that the review now exists in 3 languages: Portuguese and Spanish in addition to the English. The interest of Latin-American psychoanalysts in research is also reflected by the many new sections in the current edition reporting work from that region.

The new edition follows the structure of the first with both minor and major modifications. In this edition Roger Perron has the last word in the epistemological debate. We hope that by the 3<sup>rd</sup> edition the Anglo-Saxon voice will be able to generate a reply.

An entirely new section on measures has been added under the editorship of John Clarkin, with significant contributions from Andrew Gerber. Measurement methodology continues to be an urgent need for many experienced as well as novice researchers. Providing accounts of some of the most frequently used psychoanalytic instruments seems helpful and appropriate. Note that a number of measures, particularly those related to the monitoring of psychoanalytic process, are listed in the Appendix. This has enabled us to link them to studies where they have been applied at the same time as gathering them in one place for easy identification.

Two new naturalistic studies as well as two significant updates have been added to the section of the review on naturalistic, pre-post, quasi-experimental studies. Of particular interest may be Imre Szecsödy's description of the European multicenter collaboration, which represents a paradigm for effective collaboration amongst senior psychoanalysts. Amongst the 3 significant additions to the follow-up studies, the German Psychoanalytic Association's study is of particular significance for its comprehensive approach to sampling and its imaginative combination of qualitative and quantitative research methodology.

It is particularly heartening that psychoanalysts have risen to the challenge of randomization with experimental studies from Munich and Buenos Aires, and an extended follow-up of the London Partial Hospital Study.

Perhaps most additional information for the second edition is to be found in the process studies section, which features twelve new studies and five updated contributions. Amongst the changing trends are the increasing size of the studies and the increasing use of computer-assisted coding. The most interesting set of additions are to be found in the process-outcome section, with fascinating findings reported from the US and Europe. Sometimes these results have yielded uncomfortable information, but sometimes they have confirmed and strengthened our belief in the appropriateness of our techniques.

Excluding studies of psychotherapy with implications for psychoanalysis, the new edition of the ODR describes 66 investigations, many of which are ongoing research programs that continue to yield exciting new information, and this in a subject where there is supposed to be no research!

It seems to us that the psychoanalytic research project is now well and truly underway. The challenges for the coming years are publications in prestigious peer-reviewed psychiatric and psychological journals, and increasing attempts to harness advances in measurement and statistical technology in other fields. There is no doubt that psychoanalytic research is a late starter relative to other schools. It is nevertheless impossible to ignore the fact that whenever the effectiveness of the method is fairly and appropriately assessed, it yields effect sizes comparable with other therapeutic approaches. No doubt we will have to meet the challenge of costs and increasingly undertake cost-benefit and cost effectiveness analyses. However, as the recent meta-analysis by Drew Westen and Kate Morrison (Westen & Morrison, in press) has demonstrated, for serious psychological disorders



such as depression and generalized anxiety, brief treatments have fleeting effects. As information about the cost of mental illness becomes more comprehensive and as the cost of psychological distress is increasingly recognized, it is clear that the psychoanalytic approach will emerge as a valid and viable alternative for the treatment of mental disorder, notwithstanding the allure of more appealingly packaged alternatives.

The research committee of the IPA has worked hard to update the Open Door Review. We are proud of what we have been able to produce and we are grateful for the support and encouragement we have received from the Kernberg-Tyson administration. As a mark of our gratitude we are pleased to dedicate this volume to two psychoanalysts who have supported research consistently and courageously and against significant political opposition. We hope that we have justified at least some of the confidence that was placed in us.

Peter Fonagy

Editor and Chair of the Research Committee of the IPA

July 2001

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# Part 1 Introduction

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This document was produced by a collaborative effort of the Research Committee of the International Psychoanalytic Association. It covers many of the studies of the outcome of psychoanalytic treatment carried out in Europe and North America over the past decades. The document is intended as a resource to those who wish to further their knowledge of the area. It does not pretend to be much more than a collection of abstracts of work carried out by psychoanalytic researchers. It does not, for example, claim to provide a coherent integrated narrative of outcome research nor does it intend to offer conclusions concerning the efficacy of psychoanalysis as a form of treatment for mental disorder. Such reviews are available elsewhere and they tend to come to dramatically different conclusions (Bachrach, Galatzer-Levy, Skolnikoff, & Waldron, 1991; Crits-Christoph, 1992; Fisher & Greenberg, 1996; Lazar, 1997; Roth & Fonagy, 1996; Trijsburg et al., 2001).

We have decided to begin the review (Part 2) with a counter-point to the epistemological framework within which many of the studies on which we report were carried out. Roger Perron provided a brilliant summary of the reservations which psychoanalysts might appropriately have about empirical investigations of their work. In this context it serves as a kind of “health warning” concerning the rest of the report. This is followed by an epistemological statement from the empiricist position by Peter Fonagy. Following this section, some of the methodological issues of psychotherapy research of particular relevance to psychoanalysis are briefly reviewed by Fonagy.

Part 3 of the report contains abstracts of the main studies organised according to study design. Part 4 offers a more extended description of these studies. These parts represent the core of the document, which is based on the work of all the contributors, taken in some cases from drafts submitted by the researchers themselves. Part 5 contains the summaries and conclusions. These are somewhat more optimistic than some of the other similar reviews undertaken in the past, reflecting both recently reported results and ongoing promising studies.

What this report makes clear is the need for the effort to demonstrate the value of psychoanalysis to be an international one. The work has been going on internationally and many current studies are being conducted across national borders. Even more important, psychoanalysts are coming to realise that their individual and national interests are best served by pooling resources and working together towards compelling demonstrations of the value of their approach. Even where such explicit collaboration is not possible, much may be gained by building on past experience and “not reinventing the wheel”. Rather, replication studies should be planned systematically to examine methodological and conceptual problems of past work, extending knowledge where gaps still exist, and working towards an integration of the psychoanalytic research effort on outcome. We hope that this volume is a contribution to this effort.

This is an “open door” document. No claim is made about comprehensiveness, or even up-to-dateness of the review. Our intention is that the document will be made available in electronic form on the WWW and all those working in the field should send the editor material either missed in the initial review or new findings as these become available for summary and inclusion. The research committee of the IPA undertakes the regular update of this document and also will attempt to ensure its general availability to members of the organisation or to others with an interest in the field. We hope that this compilation will be of value to members of the organisation around the world. Comments on the document should be addressed to the current editor, Peter Fonagy (email [p.fonagy@ucl.ac.uk](mailto:p.fonagy@ucl.ac.uk)).



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## **Part 2 Epistemological and Methodological Background**

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### **Section A: Reflections on psychoanalytic research problems – the French-speaking view**

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#### **Foreword**

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This text has been prepared on the basis of an extensive inquiry by Roger Perron among French-speaking psychoanalysts who are IPA members (Société Psychanalytique de Paris, Association Psychanalytique de France, Belgian, Swiss and Canadian Societies); the views here expressed are widely shared by the responding analysts. Agreement seems to have been reached on two points:

Agreement is generally expressed with Otto Kernberg's wish for the IPA and the component societies to work on research programs aiming to attain more credibility, as a discipline and as a practice, among the general public, the learned public, and the scientists (including those devoted to "hard sciences", i.e. physics, etc).

However, many questions are raised about the delimitation of the possible objects of such research actions, and about the methods to be used. Many research activities that seem desirable and possible to some of our colleagues (mainly, but by no means exclusively, in the US) are strongly objected to by French-speaking analysts, particularly those which bear on the outcome and the process of the psychoanalytic treatment. It is therefore necessary to introduce distinctions among the research projects according to their objects and their methods.

The present text, which represents the views of its author, raised two questions:

a preliminary question: what is to be understood by the term "research" in psychoanalysis?

and on this basis, how can we delimit the subjects or domains of research, and, for each of these subjects (or type of subjects), what may the appropriate methods be?

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#### **What is to be understood by "research" in psychoanalysis?**

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Two types of research actions may be distinguished in this field: those where a clinical attitude prevails, and those which make use of formal and systematised procedures.

#### **Clinical research**

Clinical research follows the traditional model of case studies in medicine; it was used by Freud to create and develop psychoanalysis, and many think it remains essential.

#### *Definition*

This research approach is centred on the individual case. It focuses on understanding the specificity of the global functioning in a person. The approach is then steered by an effort to understand a functional structure, taken as a structure. We may recall that according to Freud, and to most psychoanalysts who followed him, we may understand the functioning of a structure by conceptualising the successive steps of its construction. A case is understood through simultaneous study of structure and history. The structural approach and the developmental approach cannot be separated.



Of course one cannot limit study to only one individual case: it is necessary to compare several similar cases to find out similarities and differences.<sup>1</sup> A "family" of cases is established, within which we distinguish variants. From this, we may then abstract a functional model that is structural as well as developmental. It was following this approach that Freud, on the basis of extensive clinical experience and reflection, illustrated and defined more precisely his model of obsessional neurosis with the Rat man (Freud, 1909b), his model of paranoia with Schreber (Freud, 1911), etc. Psychoanalysis subsequently has proceeded in the same way when proposing new models, and we have no reason to discard this approach.

### *The aims of clinical research*

The aims of clinical research are threefold. These are:

*to define syndromes, psychopathologies, etc.* Many instances may be cited, as in the description of infantile autism as proposed by Kanner (1943), and the development of this definition by M. Mahler (1952), D. Meltzer (1974), etc. Here, we must distinguish the work of leading to the delimiting of a syndrome and its description in terms of symptoms (which fall into the domain of psychiatry, or psychopathology), from the attempt to identify a functional model of this syndrome, using the theoretical and practical framework of psychoanalysis. For example, in the case of autism we must distinguish the descriptive picture of infantile autism from the dismantlement model as suggested by Meltzer in his understanding of these states. Of course, this raises the question whether a psychoanalytic nosology is possible to achieve without violating the core of psychoanalytic metapsychology?

*to formulate theoretical constructions.* It is the clinical research approach that has produced all the great theoretical models proposed after Freud. It was on the basis of clinical research methods that the controversies raised about rival models have developed (for instance the British controversies between the followers of M. Klein and those of A. Freud) (King & Steiner, 1991).

*to provide research foundations to the therapeutic approach.* Clinical research also provides the basis for psychoanalytic therapeutic approaches with their considerable divergences – such as those between the techniques of the followers of Klein (Klein, Heimann, Issacs, & Riviere, 1946), Lacan (1964), Kohut (1977), ego-psychologists (Greenson, 1967), etc.

### *Advantages and disadvantages of the clinical approach to psychoanalytic research*

Freud proved its value! But what is its current power to convince? There is a general agreement that the value of a model derived on the basis of clinical research is measured by its utility, as acknowledged by a wide community of psychoanalysts and other experts.

But how wide must the acceptance of a model go, for it to be judged as valuable? It is evident that no precise criterion can be defined. If we consider Freud's own theories, the rate of agreement would be seen as varying considerably depending on specific propositions. For example, not all accept the second instinct theory, nor does everyone find the concept of instinct useful. After Freud, the major models, be they Kleinian, Ego-psychological, Lacanian, Bionian, Kohutian, etc., are all only selectively accepted by psychoanalysts. How can we then specify the limit beyond which a model might be thought of as unacceptable by the community of psychoanalysts, eventually excluding its author from this community? Adler and Jung were rejected, Melanie Klein (1933) was nearly excluded, Bowlby (1960) was thought to be on the margin, etc. History is generally shaped at a political level, rather than on "scientific" criteria. Therefore, the ambiguities entailed in the evaluation of the results of clinical research raise the problem of the unity of psychoanalysis as a discipline, and its converse deep "schisms" or divisions in our discipline.

The problem is probably even more troublesome when we aim to convince non-psychoanalysts. We generally meet the objection that a clinical approach cannot but produce theories based on ad hoc facts according to an already preconceived idea. Experience shows that it is almost impossible to

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<sup>1</sup> The comparative clinical study of similarities and differences of cases as part of clinical research, as discussed here, constitutes a fundamental research paradigm, which also includes the techniques of the so-called "exact" sciences.

convince the sceptic if we appeal to experience, general theory, Freud's authority, etc. If we comfort ourselves by pointing out how our opponent's attitude may be a sign of "resistance" against unconscious ideas, the most likely consequence will be the addition of irony to scepticism. Psychoanalysis comes to be regarded as a faith and the analyst is likened to a religious believer (or a spectator). It must thus seem appealing to turn to methods which could provide a clearer epistemic base for psychoanalysis.

## **The use of objectivization and systematisation methods**

We now turn to procedures for proof whose utility is accepted in other disciplines. We could here also think of the approaches used by the historian, the pre-historian, the sociologist,<sup>2</sup> etc, but these fields are seldom evoked.

### *The scientific models*

Two disciplines are generally called upon. The first is biology. The framework of biology tends not to be used in the context of its modern approaches (using organic chemistry, molecular biology, genetics etc.), but more following the approaches pioneered by Claude Bernard: functional analysis. This was the central model for Freud, and it remains at the core of clinical research. Within this framework, some authors have tried to bring nearer the theoretical and research models of immunology and that of psychoanalysis, through their common use of the concept of "defence". In this vein we may be able to go no further than the analogy, which can prove nothing, or the evocative metaphor, which is at best illustrative, and has no probative value.

The second is the area of the "hard" sciences (essentially physics and physical chemistry). It is clear that this model appeals to many psychoanalysts – not surprising since it has many theoretical accomplishments, it uses high technology and has high prestige among the public and politicians (not least because of the great financial investments involved). Fascination with hard science at times might lead psychoanalysts to declare that "psychoanalysis is a science" by the canons of the physical sciences. This appears to me to be somewhat of a reaction formation against the doubts raised by the uncertainties of psychoanalytic knowledge discussed above. Moreover, the model adopted from physical sciences often is the one that prevailed prior to the introduction of quantum physics, which ignores the considerable modern transformations of thought in this field, as regards causality, status of time and space, definition of reality, etc.

Perhaps psychoanalysis is a science, but the question remains: what kind of science? The problem can be seen at three levels: (a) epistemology, (b) theory construction, and (c) techniques used to collect and process the data within the framework of these theories.

### *Criteria of scientificity*

With regard to the "hard sciences" model, several criteria tend to be invoked:

*Procedures for the construction of facts:* Observations must be unequivocally confirmable by qualified observers (this of course begs the question of the qualification of the observer).

*Quantification:* data must be quantifiable to provide material for subsequent logico-mathematical treatment.

*The replicability of observation:* it should be possible to repeat any observation given identical conditions and identify the same phenomenon.

*Possibility of prediction:* A good scientific theory must allow us to predict the occurrence of the events within its domains.

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<sup>2</sup> But see D. Tuckett (1996) or Grounded Theory (Ed).

*"Falsifiability"*: A criterion made popular by the work of Popper (1959) requires that a theory, if it claims to be scientific, must be amenable to procedures that can realistically disconfirm its predictions.

*Unambiguous terminology*: In building theories, terms used should have sincere and unequivocal referents and their connection to each other should be equally unambiguous.

We could list further such criteria. In the "hard sciences", these criteria are open for discussion. Thus, many disciplines are accepted as sciences even if quantification is not instrumental, and the experiments are not possible to repeat, as in palaeontology; Newton's theory is not "falsifiable", etc. Moreover, it is evident that, beyond a certain point of generality, a theory is not possible to "prove", it can only be accepted or not as organising a wide array of facts. This holds for post-Darwinian theories of evolution.

In the field of psychoanalytic research, the pertinence of these criteria varies widely according to the aims of the specific category of research (as outlined below). It should be stressed here that French-speaking psychoanalysts seem generally to agree that the criteria of "hard sciences" cannot be applied to the data and process of "classical" psychoanalytic treatments (armchair-couch). All procedures trying to use them will destroy the very object of study. This view will be justified on epistemological grounds.

### *Remarks on the epistemology of psychoanalysis.*

Every scientific approach produces and organises facts, at the boundary of theories and techniques. That this is a necessary but not an easy task is clearly indicated in the problems of contemporary physics, in specifying the relationship between mathematics and experimentation. What about the field of psychoanalysis?

### *Psychoanalytic fact versus historical event*

Psychoanalysis, by definition, involves psychical facts, and more precisely on what we could call psychoanalytical facts, that is to say on objects of observation and thought built at the boundary of psychoanalytical theories and techniques.

We must be careful to distinguish the psychoanalytical fact from an historical event. For instance, if the analyst raises the hypothesis of a psychic trauma in a patient, this psychic trauma is evidently something other than the childhood event reported by the patient, even if the latter is accepted as "real" by the analyst, and is assumed to be the root of the traumatic psychic organisation.

As pointed out above, psychoanalytic 'facts' are organised, on the level of the individual, across the two dimensions of their structure and their history. But it is generally agreed that this history is not the history of "real events" that occurred in the patient's life (as could have been observed by a neutral observer, if we can conceive of such an observer). This history is re-moulded by deferred actions, and moreover "rebuilt" in the course of the treatment by the psychoanalytic process itself. In this respect, the ideas of Serge Viderman (1971) had a great impact on French-speaking psychoanalysis.

### *The coincidence of the method and subject of observation*

The psychoanalytic approach has a unique epistemological feature: the subject and the method of study are identical, the psychic apparatus is discovered by means of the psychic apparatus. Of course, this can be an "objective" approach, as far as the distinction between the patient's psychic apparatus and the analyst's one is maintained. We know, however, that this distinction must not be too sharp; otherwise we could turn a blind eye to the dialogue of transference and counter-transference. In fact, it is to avoid such a blindness that the analyst's personal analysis is a pre-condition for analytical practice.

We may, without doubt, hope that psychoanalytical thinking (as any other framework) should use terms free of multiple meanings and relate them to each other without ambiguity. It is well known that this is hard to attain. Ambiguities may derive from the very object of its study, as this concerns

phenomena and processes characterised by sense multiplicity. If we deprive psychoanalysis of such multiple meanings, the language of psychoanalysis would deny its subject matter.

It follows from these considerations that, more than in any other discipline, theory comes first in the construction process of psychoanalytic facts: this is why these constructions are so open to the criticisms of the non-psychoanalyst sceptic.

### *Proof versus usefulness*

We may finally observe that the body of metapsychology is a general theory of psychic functioning and as a general theory, encompassing a wide array of phenomena, cannot be subjected to a process of testing. It is useful or not in linking a wide range of known phenomena, as well as integrating new facts (but as these new facts are generally produced by the theory itself, this is evidently a circular procedure). Thus, the post-Darwinian theories of evolution which cannot be proven, but are deemed indispensable by most biologists.

Metapsychology has similar qualities, not just as a general theory, but in terms of some of its specific aspects. Thus as regards psychic development: we may say, either that we describe its steps as "real", as they would be seen by "direct observation" of the baby, of the mother-child relationships, etc, or that we speak of a "virtual" child, of a useful model to account for the final structure, remembering that it is not essential that we speak of a "real child". In this second way, we may satisfy ourselves by saying that we have a useful model, one which is helpful in organising observations (this issue was developed, in particularly powerful terms, by André Green opposing, among other views, those of D. Stern (Green, 2000).

## **Identifying research aims and definition of research methods**

With regard to the problems discussed in the preceding paragraphs, the choice of aims and methods of the research should be established for several specific categories.

### *Psychoanalytic and psychotherapeutic treatment*

The first category is that of psychoanalytic treatment, in its "classical" form (here defined most simply, by its setting: the armchair-couch). French-speaking psychoanalysts seem generally to agree that in this case the clinical approach is the only one that may be used, and that any attempt to submit the data of the sessions to the "hard sciences" criteria, and treat them by derived techniques, is likely to destroy the very object of the research, and moreover could not be accepted as proof by the sceptics. Recordings (audio- or video-) are then banned, not only for ethical reasons (due to confidentiality), but also because such a situation, even with the explicit agreement of the patient, disturbs gravely the transference-counter-transference relationship. We may of course consider using notes taken during the session by the analyst himself, and apply to these data any coding scheme leading to quantitative treatment). But many objections may be raised in relation to these attempts at quantification. Most importantly it inevitably leads to the fragmentation of the material, no subsequent statistical calculation, however sophisticated, being able to restore this lost unity. We may question the use of "judges" whose objectivity may be only apparent, and many other aspects.

The same objections may be raised against psychoanalytical psychotherapies (in the armchair-armchair setting). However, in this case, the objections are less powerful, considering that the appropriateness of the use of recording and other approaches may be a function of the types of patients and types of treatments.

### *Psychoanalysts' practice in other settings*

Psychoanalysts frequently practise in other types of therapeutic situations. We may distinguish the following approaches:

Approaches where the psychoanalytic point of view prevails: such as analytic psychodrama and group dynamics, body therapies (with or without relaxation), joint mother-infant therapies, and family therapies.

Activities in professional settings where the psychoanalytic point of view is not dominant, but where the practitioner uses his or her psychoanalytical training. These include: institutional treatment, the clinical work of the psychiatrist (diagnosis and psychiatric care, including drug therapy), or of the psychologist (psychological diagnostic, projective techniques, interviews, etc). Such professional positions are held by a number of practitioners who underwent a good analysis, but did not wish to train as psychoanalysts, and people in all the "applied psychoanalytic" approaches (such as psychoanalysis applied to literature, art, history, etc).

### *Research in institutions*

We need to consider separately research activities taking place in and bearing on the functioning of institutions. In the case of institutions such as therapeutic, and educative ones, there are a number of different kinds of studies which involve:

analysis of the characteristics of the treated population (geographical and socio-economical context, family structure, etc).

analysis of the reasons for consultation, of the initial diagnosis and of the reasons to engage in treatment.

comparative studies of the means used to attain a diagnosis (for instance: comparison of the scheme proposed by DSM-IV and the "French Classification of Child and Adolescent Mental Disorders" elaborated by S. Lebovici, R. Misès and N. Quemada, which takes into account a psychoanalytic approach to these disorders).

comparative studies of therapy and education techniques actually used, taking into account the reasons why each was chosen.

studies of the process of these treatments, and of their outcome: this is the "efficacy" question, the research activity appropriately proposed by Otto Kernberg. It should be noted that to be adequate such studies need to be co-ordinated with the preceding types of investigation.

### *The specific problems of efficacy studies*

The difficulties of efficacy studies are primarily procedural or methodological. The problems are linked to:

the criteria to be used in the measurement of change. Measurement of symptom reduction will not be enough. We know that symptoms are erratic, that if one disappears it may give way to another, that some symptoms are useful because they are part of defences and their careless destruction might be dangerous, etc.

the technical translation (operationalisation) of these criteria, as used in the efficacy study: there are problems in using standard schemata like DSM-IV, but also in building schemata for particular research, or in the use of clinical evaluations.

the choice of "independent judge(s)" who should use these tools: the analyst himself, another analyst, a non-analyst, the patient? None of these possibilities should be discarded a priori. However, it is also clear that in all these cases the question is raised as to the objectivity of such attained judgements. This question cannot really be well answered by combining several judges' decisions and computing a degree of agreement, since a good agreement may reflect a common bias.

some important aspects of change are hard to evaluate by quantification (reduction of life stress, shift from "psychic misery" to "commonplace unhappiness", etc).

last, we have to take into account the rather frequent cases where the cure gets to its end without any noticeable improvement, but where we are justified to think that the situation would have been far worse without that cure (case of the patients who thus may have avoided inpatient psychiatric care).

### *Research involving the psychoanalytic institution*

*Historical research studies.* These studies concern the history of psychoanalysis and of its agents, of the development of concepts and theories, etc. The analysis of the origins and development of the conflicts marking this history is particularly important. To what extent are these conflicts specific, due to the training of analysts, ways of transmission, analytic practice itself?

*Studies on the functioning of our institutions.* Such studies concern the development of psychoanalytic communities from the creation of new workgroups to becoming component societies as well as the evolution of the IPA itself. An objective analysis of the group dynamics involved in these historical developments would be particularly useful. One can imagine interdisciplinary studies with sociologists or ethnologists and so on.

## **Conclusions**

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The identification of research aims and the choice of methods should be done on the basis of the considerations presented in the last sections, but in very different ways according to the topics distinguished above.

It ought to be a major purpose for the IPA Research Committee to work towards this dual delimitation; it seems highly desirable for the Committee to urge, at the level of Regions and Societies, the creation of Research Committees to collect the necessary material.

## **Section B: Reflections on psychoanalytic research problems – an Anglo-Saxon view**

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### **Foreword**

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By contrast to the French-speaking contribution, this alternative perspective, drafted by Peter Fonagy, is not presented on the basis of a significant sampling of views of psychoanalytic colleagues in English-speaking countries. This is not because of lack of opportunity and certainly does not reflect an absence of concern. Rather, the reason why the views presented here are solely those of the author is because currently the radical message to psychoanalysis proposed is clearly only held by a small minority of psychoanalysts, or so the current writer believes (Schachter & Luborsky, 1998). It is not impossible that change is in the air. The new generations of psychoanalysts who received their professional education since the revolution in the biological and cognitive sciences in the 1970s and 1980s are probably more reluctant to shed the general principles and specific understandings which these rapidly advancing disciplines have equipped them with. Sadly, as for Freud, for many of the psychoanalysts originally trained in the 50s and 60s, there was no true compelling framework of knowledge genuinely addressing problems of mental functioning – other than psychoanalysis.

The situation within which psychoanalysis has to exist today has radically changed from the conditions which prevailed 30 or 40 years ago. There are two major aspects to this change: (a) there have been major advances in the basic sciences underpinning clinical work in the mental health field; (b) there has been a rapid development of relatively “effective” approaches to the treatment of many of the mental disorders which had previously been the unique purview of psychoanalytic clinicians. Under the first category, one could single out the biological revolution, particularly our increased understanding of brain function and under the second the cognitive revolution in psychology.

This summary is divided into three parts. The first will review the current epistemic problems of psychoanalysis including some worrying indications of a fragmentation within the discipline. The second will consider an alternative epistemological approach, which, if adopted, might ultimately radically change the status of psychoanalysis as a discipline. The third section will consider some of the philosophical problems and difficulties which efficacy studies of psychoanalysis entail. We shall conclude that efficacy studies are necessary – but they are the right answer to the wrong question and as such are unlikely to yield entirely satisfactory results.

### **The current epistemic problems of psychoanalysis**

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#### **Crisis! What crisis?**

We have become quite accustomed to worrying about the future of psychoanalysis. Mostly, when concerned about the future of our discipline, we tend to focus on the lack of psychoanalytic patients, lack of appropriate candidates, persistent and increasingly well-received critiques of psychoanalytic theory and practice, the strengthening of alternative therapeutic approaches (particularly biological psychiatry and cognitive-behaviour therapy). Perhaps even more worrying is the spawning of more or less psychoanalytically oriented psychotherapeutic approaches, often masquerading as psychoanalysis, which insidiously invade our practice. What I would like to focus on is far worse than any of these, and may even be responsible for some of our other problems - the knowledge base of psychoanalysis.

## The fragmentation of the psychoanalytic knowledge base

### *The Citation Index study*

My colleagues and I have reviewed the *Social Science Citation Index* (Fonagy, 1996). We were curious to explore how often the average article in the *International Journal of Psychoanalysis* and *The Journal of the American Psychoanalytic Association* is referred to in other major journals (medical and non-medical). Overall, the numbers are on the decline, even when adjusted for the tendency for more recent papers to be somewhat less frequently cited across the entire Citation Index. This means that the scientific impact of psychoanalysis upon other disciplines may be on the wane. This trend is even clearer when we look at the expected number of citations of all the articles selected from the first issue of the *International Journal* over the past decade. What is this apparent loss of interest due to? Is it that non-analysts (those publishing in psychiatric or literary studies journals) are less interested in what we write? When we looked at these journals, the trend indicating a decreasing interest disappeared. Admittedly the base rates are not very high but they have been the same for quite some while. The surprising results emerged when we looked at the number of times that an article in the *International Journal* was likely to be referred to in psychoanalytic journals. It seems that this is where the declining interest in psychoanalysis originates. With other psychoanalysts!

What does this imply? If these observations are to be believed, the clear implication is that we no longer take sufficient notice of each others' publications to want to refer to them in our papers. We are no longer accumulating knowledge – but rather (to exaggerate the point somewhat) we are all developing the discipline in our own individual directions, no doubt building on the classics, but by and large and increasingly, ignoring contemporary contributions.

These are statistical trends and I am sure that they could be interpreted in a number of ways. It is likely that psychoanalysis is not the only discipline manifesting this trend and while we adjusted the figures for the overall trend for recent articles to be less frequently cited, there may be certain disciplines including psychoanalysis which are characterised by this same trend.<sup>3</sup> It is possible that the decline is specific to the *IJPA* and *JAPA* and is in fact an artefact of the emergence and increasing prominence of new journals over the historical period which the study covered. In this case the declining trend would merely index the declining market share of the 'classical journals'. However, the absolute reduction in citations remains an important observation, even if the suggestion is that one cause of the fragmentation may be the great multiplication of channels of publication. By contrast it may be that this phenomenon is specific to English language journals and a similar effect could not be demonstrated in the Spanish, French or German literature. More worryingly, it could be that more recent articles are genuinely of poorer quality; it could be that people simply do not read the journals. Surveys conducted by the American Psychological Association have shown that most psychologists in clinical practice read less than one new article per year. I fear that the most likely explanation is that this phenomenon signals a major epistemological problem of conceptual fragmentation and the loss of an organising paradigm.

### *Implications and possible causes*

It seems fairly evident that fewer and fewer English publications achieve sufficient acclaim to merit citation. The consequence is obvious. We might have experienced difficulties in professional communications up till now (e.g. Wallerstein, 1992), but such difficulties are negligible compared to the problems we shall be facing in a few years time. It could be argued that the so-called major psychoanalytic schools which have emerged to organise our discipline over the last half of the 20<sup>th</sup> century are breaking down. Ego psychologists are no longer ego-psychologists, Winnicottians are no longer just Winnicottian, self-psychologists have fragmented, Kleinian-Bionians have less and less in common beyond these two giants of the field, Anna Freudians were probably an improbable grouping even during her lifetime, and inter-personalists never had a coherent theme beyond the citation of Harry Stack-Sullivan. From this point of view Victoria Hamilton's book *The Analyst's Pre-*

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<sup>3</sup> Dr Stephen Ellman (personal communication) mentioned a similar study undertaken by him and his colleagues in the field of neuroscience where very similar declining trends were observed.



*conscious*, exploring in depth the conceptual frameworks of over 80 eminent psychoanalytic practitioners, makes sobering reading (Hamilton, 1996).

This fragmentation and confusing absence of shared assumptions is what spells, to me, the inevitable demise of psychoanalysis – more than any of the external challenges that we face. In the absence of a common language, we are forced to occupy increasingly smaller intellectual territory. Increasing fragmentation of the psychoanalytic knowledge base has, after all, been a feature of psychoanalysis from its very inception. Ultimately, we shall all be on our own, fiercely protecting our personal psychoanalytic patch. So, what is responsible for the tendency towards theoretical entropy in psychoanalysis? Roger Perron (this volume), in his incisive and erudite analysis of epistemology, draws attention to this in his discussion of the advantages and disadvantages of the clinical approach to psychoanalytic research. He identifies the lack of power of the functionalist criteria (whether a model is sufficiently useful to a significant number of clinicians) as a significant disadvantage of the clinical research approach. I concur with Perron's analysis and would suggest that a somewhat more in-depth examination of this problem may be in order.

## **The logical status of theory in practice**

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### **Inductive versus deductive arguments in clinical theory building**

The problem of clinical theory as it relates to the clinical practice of psychoanalysis is at core a philosophical one, usually considered in philosophy of science under the heading of *methodology*. The subject matter of methodology is defined in opposition to that of *logic* (Papineau, 1995). Whilst logic is the formal description of deductively valid reasoning, methodology covers all the reasoning that we undertake that tends to fall short of deductive reasoning. In making clinical judgements and decisions we use arguments that may give us good reasons for believing in certain conclusions but they do not *compel* acceptance in the manner that deductive arguments might.

All psychoanalytic clinicians work with inductive inferences and therefore, by definition, so does clinical research. In psychoanalytic work we are confronted with a finite set of observations, based on formal or informal assessments, as well as the evolving treatment process. From such a sample, the psychoanalyst then moves to conclusions about how the patient generally behaves and formulations about why he or she does so. In practice, induction is made not simply on the accumulation of past observations about a particular individual, but formalisations of past cases by other psychoanalysts in so-called 'clinical theories' (Klein, 1976). We consider theories to lend support to inductive observations because we assume that theories imply that the number of observations on which an inductive inference is based is very considerable and this somehow lends weight to the conclusions. In so doing, however, we are merely generating inductive arguments for induction. We simply maintain that inductive arguments are acceptable clinically because they *work*. Even if our premises do not logically guarantee our conclusions, they normally turn out to be true anyway. Arguing that inductions are generally acceptable because our experience has shown them to work so far, is, of course, itself an inductive argument. Even if observed patterns have tended to hold good so far, what guarantees that they will continue to do so? As Bertrand Russell (Russell, 1967) argued, it can hardly help to observe that *past* futures have conformed to past pasts. What we want to know is if *future* futures will conform to *future* pasts. The argument of past co-occurrence has little probative value (it is merely rhetorical, it does not prove anything).

Thus, psychoanalysts have implicitly raised the status of 'clinical theories' to laws and have claimed to explain the client's behaviour using Carl Hempel's (1965) *Covering-Role Model*: given that certain initial conditions are satisfied and covered by a specific law that also specifies consequent events, a specific event that is accompanied by the initial conditions is considered as explained by the law. Because they involve deduction via a law, such explanations are termed deductive-nomological explanations. This process has all the appearance of a piece of deductive reasoning. But such explanations do not rescue us from the problems of induction, since the 'laws' were actually established by induction from past observations of results. In fact, most clinical laws are, in any case, only probabilistic (Ruben, 1993), therefore they could allow only inductive *statistical* explanations rather than deductive-nomological ones. While we know that child maltreatment can give rise to

behavioural disturbance, this is by no means inevitably the case (e.g. Anthony & Cohler, 1987). The *Covering-Role Model* thus has crucial philosophical limitations and the impact of these is well illustrated by the history of theory in psychoanalytic clinical practice.

The central point here is that the key function of theory for practitioners is to explain clinical phenomena – in other words it is a mere heuristic device rather than a tool for genuine deduction. This approach, however critical from the standpoint of every day clinical practice, is of limited value in terms of theory construction and elaboration. The value of theory based on clinical research is in supporting clinical work. Its weakness is its extensive reliance on induction and therefore its dramatic failure to aid the construction of a coherent, integrated and sound knowledge base which can systematically evolve and define the psychoanalytic approach.

There are three conditions that should be met for clinical research to be an adequate sole methodology of psychoanalytic theory building. These are: (a) a close logical tie between theory and practice, (b) appropriate deductive reasoning in relation to clinical material and (c) the unambiguous use of terms. The first of these is an essential precondition for us to be able to assume that theory is not generated by technique. In order to be confident that there is no irreparable confound between technique and theory, we must be able to show that technique is entailed in theory; that is, that technique has a known and specifiable relationship with theory and thus the contamination of observations by technique, even if not possible to discount, can be specified. The second criterion, the one of deductive reasoning, must be satisfied if we are to show that observations serve both to prove and to disprove theoretical premises. The third criterion pertains to the possibility of the reliable labelling of observations. In the following sections I intend to show that none of these three criteria are met by current clinical research strategies.

## **Practice is not entailed in theory**

One of the major causes of the failure of the clinical research method is that, while we might wish this to be otherwise, in reality psychoanalytic clinical practice is not logically deducible from psychoanalytic clinical theory. While this is quite a radical premise, and one which even I only believe to be partially true, it is neither new (e.g. Berger, 1985; Fonagy, 1999), nor without considerable corroboration from the psychoanalytic literature. There are powerful arguments that support the general suggestion that psychoanalytic practice bears no logical relationship to theory. We shall only touch briefly on six of these:

Psychoanalytic technique has arisen largely on the basis of trial and error, rather than as driven by theory. Freud (1912) willingly acknowledged this when he wrote: “the technical rules which I am putting forward have been arrived at from my own experience in the course of many years, after unfortunate results had led me to abandon other methods” (p.111).

It is impossible to achieve any kind of one-to-one mapping between psychoanalytic therapeutic technique and any major theoretical framework. It is as easy to illustrate how the same theory can generate different techniques as how the same technique may be justified by different theories. For example, Gedo (1979) states that: “principles of psychoanalytic practice...[are]...based on rational deductions from our most current conception of psychic functioning” (p.16). His book makes the claim that the unfavourable outcomes of developmental problems can be reversed “only by dealing with those results of all antecedent developmental vicissitudes that later gave rise to maladaptation” (p.21). However, what sounds like a deduction, on closer examination turns out to be a hypothesis. It is one thing to presume and quite another to demonstrate that in therapy developmental vicissitudes require to be sequentially addressed. Many have powerfully challenged the overuse of the developmental metaphor (Mayes & Spence, 1994) and, even from within the self-psychology orientation to which Gedo belongs, the support for his strong assertion is limited (Kohut, 1984, pp. 42-46). By contrast, it is equally striking how clinicians using very different theoretical frameworks can arrive at quite similar treatment approaches (Wallerstein, 1989).

The fact that we are not in agreement about how psychoanalysis works also suggests that practice is not logically entailed in theory. The nature of the therapeutic action of psychoanalysis is an inveterate theme for psychoanalytic conferences – started perhaps at the IPA conference at Marienbad (Panel,

1937). Since that time, at roughly 10 year intervals there has been a major symposium on the topic at either the meeting of the American or at the International Psychoanalytic Association and probably at least one in each of the intervening years in one of the major component organisations. If practice was logically entailed in theory, we would undoubtedly have a clear theoretical explanation for therapeutic action.

Theory and practice have been progressing at very different rates, with practice changing only in minor ways, relative to the major strides made by theories. It is quite realistic to contemplate a single volume account that would encompass most major technical advances (e.g. Clarkin, Kernberg, & Yeomans, 1999; Greenson, 1967; Kernberg, Selzer, Koenigsberg, Carr, & Appelbaum, 1989; Luborsky, 1984). Yet, no single person could hope to provide a scholarly and integrated account that would be faithful to all the enormous theoretical developments that have taken place over the past 100 years. The discrepancy in rates of progress between theory and practice is staggering and would be hard to understand were it not for the relative independence of these two activities.

Psychoanalytic theory is largely not about clinical practice. Scarcely a single volume of Freud's 23 volume corpus is devoted to papers on technique. So what is psychoanalytic theory about, if it is not about practice? It was intended as and remains an elaboration of a psychological model and the way that this may be applied to the understanding of mental disorder, and to a lesser extent, to other aspects of human behaviour – literature, the arts, history etc.

The role of theory in practice underscores the inductive nature of clinical research. The value of theory to the psychoanalyst is in elaborating the meaning of behaviour in mental state terms. Thus there can be no question that theory is valuable – it is, however, intrinsically contaminated by practice. It is driven by what is practically helpful rather than the other way around, that is, practice being dictated by what is true about the mind. Thus the major criterion for assessing validity of clinical research findings is contaminated by a set of considerations unrelated to their accuracy. Certainly, in principle, a theory may be true but of little practical value (e.g., a mathematical theorem) or untrue but great practical relevance (e.g., religion, politics etc.). The loose relationship between technique and theory is a significant burden which clinical research carries. Theory serves to justify practice largely through analogy and metaphor and we must at all times be aware that what we are practising is based on cumulative clinical experience and what we are theorising may be a useful adjunct to clinical practice – but it cannot be its epistemic justification.

## **The problems of inductive reasoning explain the overabundance of theorisation**

Clinical work and clinical observations provide the chief source of theory building in psychoanalysis. There is no question but that the psychoanalytic treatments provide a unique window on human behaviour and thus psychoanalytic theories are rich and imaginative in developmental, clinical and applied accounts. The limitation imposed on it is in part logical and in part psychological.

The epistemic strategy of practising clinicians is, as we have seen, by necessity inductive. They are predisposed to find patterns in the therapeutic interaction which they can explain using existing theoretical constructs. In observing clinical material psychoanalysts opt for inductive reasoning in favour of pointing to instances where the antecedent is *not* followed by a consequent. The predominant psychodynamic epistemic strategy, encapsulated in the clinical case report, became one of enumerative inductivism (the sometimes exhaustive enumeration of instances consistent with the premise).

From a clinical point of view this is an appropriate strategy. To enumerate examples of the influence of an unconscious pattern is not only a useful adjunct to interpretations (“every time you are feeling such and such you do so and so”) but also helps the psychoanalyst to feel on firmer ground in working creatively to elaborate a picture of the patient's internal world.

But, remembering Bertrand Russell's quip once more, it is not persuasive to show that *past* pasts conform to *past* futures; that an association we have already observed is one more instance of a known family of associations. What the clinician's mind finds much harder to tackle is the

identification of negative instances – when A was not followed by B – which may lead him to question the premise that A is always followed by B.

Psychoanalysts are not alone with this problem. All human reasoning is substantially flawed in this regard (Johnson-Laird & Byrne, 1993; Wason & Johnson-Laird, 1972). Even when specifically asked to do so, we are reluctant to recognise the relevance of not observing B following A when evaluating the premise A always follows B. This is referred to as the failure *to negate the consequent*. We neither observe, nor use in psychoanalytic theory building, the many instances where the patient's reaction is not as we should anticipate it to be on the basis of a specific premise.

To take a deliberately simplistic example, signs of unconscious anger with an ambivalently cathected object are readily identified in cases of depression (Freud, 1915). But what of cases where the inward direction of anger does not appear to lead to depression? If such cases were treated with equal attention as cases where the premise clearly holds, the development of the theory of depression might, just might, have been more orderly. To ask clinicians to pay attention to such negative instances, however, seems to me to be asking them to do something profoundly counter-therapeutic and to be specifying a clinical situation where the therapeutic and research aims can no longer be simultaneously pursued in equal measure. The limitation of human reasoning identified by Wason, Johnson-Laird and their colleagues may be a core limitation on clinical research methodology.

## **The deliberate polymorphy of psychoanalytic concepts**

As clinical material is used in a limited way by theoreticians who are themselves clinicians, new theories tend to be developed and readily obtain confirmation. Unfortunately this process tends to occur without systematic reference to the old as 'supplemental' to the original theory. Thus new ideas have been observed to overlap, rather than replace, the original formulation (Sandler, 1983). This very quickly gives rise to partially incompatible formulations which, nevertheless, need to be employed concurrently. To give just one example, Freud's move from the topographical to the structural model completely reconfigured the nature and role of an object. As psychoanalysts still needed to talk to their patients about issues conveniently taken up in the context of the topographical model (e.g. dreams, drive fixations) at the same time as wanting to address issues of adaptation and relationships (using ideas derived from structural theory), they were forced to extend the definition of the notion of the object.

This strategy was extensively used to deal with the many instances where several partially incompatible or partially applicable frames of reference needed to be used side-by-side (Sandler, 1983). Again, this is neither unusual nor reprehensible. It is the way that human language and, in fact all human conceptual systems, deal with the complexity of the phenomena we require them to signify. Rosch (1978), building on the work of Wittgenstein (1969), termed such fuzzy-edged concepts polymorphous concepts. They cannot be defined by distinctive features (a set of necessary and sufficient features). Rather, exemplars of a category are identified in terms of a required level of similarity with a prototype. Thus "chairs" represent such a heterogeneous category that they cannot be defined in terms of either their function, their structure, their constituent properties, their shape etc. For example what do a barstool and an aircraft seat have in common which differs from a seat at a bus stop? Yet most people would identify the first two as chairs, but rarely the third. The problem of psychoanalytic language is in essence no worse than the problem of every day language.

What is disappointing is that psychoanalysts have tended to accept the argument that complexity precludes unequivocal definition as an adequate reason for rarely attempting operationalisation and frequently embracing ambiguity. Here I would disagree with Roger Perron who also denies the possibility of unequivocal definitions for our concepts. Yet there can be little doubt that while the same term may be used with very distinct scientific meanings, the tendency for fragmentation will be reinforced since the use of the same term in quite different contexts undermines the possibility of explicating important differences between theoretical approaches. We need to reach beyond clinical research if we are to overcome the problem of multiple meanings.

# A new epistemic framework for psychoanalysis

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## The historical perspective

Psychoanalysis has developed in somewhat different ways in most of the countries where it has been practised. Depending on the particular cultural context, it integrated to a greater or lesser degree with local institutional mental health services such as psychiatry, psychology, social work etc. In some countries, as in England, the integration between psychoanalysis and statutory mental health care was minimal. In others, such as Scandinavia, Germany or Canada, the integration with psychiatry has been extensive, with state funding for medical psychoanalytic treatment and in some cases even financial support for training. In the United States, insurance companies have been responsible for funding until relatively recently.

A relatively fair generalisation of international historical trends might be that, in countries where high levels of integration between the standard (statutory) provision of mental health care were established, psychoanalysis grew faster, remained under medical domination, developed politically powerful professional bodies but defined itself in distinction to other branches of medicine. By contrast, in countries where psychoanalysis was rejected by the leaders of the mental health professions (particularly psychiatry), psychoanalysis remained a smaller profession, more inwardly turned, arguably more creative, with a greater influence of non-mental health professionals. In essence, although psychoanalytic identity and epistemology exists for both groups, it is more powerfully established as independent of and unrelated to mental health issues in the latter group, whilst it is subtly and intricately tied to the philosophy surrounding mental health care in the former.

These differences were almost imperceptible until the changes in mental health care which have had very different, yet profound, effects on both types of psychoanalytic groups. The focus here will be on those societies which are highly integrated with the delivery of mental health services, as these are the groups most affected by the pressure to provide outcome information.

First we will review the major developments challenging psychoanalysis in the mental health field over the last half century and then propose a realignment of the relationship between psychoanalytic knowledge and other fields of mental health inquiry.

## The isolationism of psychoanalysis

Psychoanalysts over the last 50 years have attempted to define their field independently of two major branches of scientific activity which pertain to their field: (a) neurobiology and (b) psychology. We shall take these two fields in turn.

### *Psychoanalysis and neurobiology*

#### *The original objections*

With notable exceptions, psychoanalysts since Freud have repudiated the relevance of neurobiology to psychoanalytic ideas. The pressures of caring for patients and the inadequacy of neuroscience combined to make psychoanalytic science primarily a form of psychology, ultimately only concerned with ensuring that psychological treatment was provided in the most systematic and disciplined manner possible. The rejection of biology was not arbitrary but reasoned – not political but conceptual. These may have been some of the reasons:

Psychoanalysts were powerfully influenced by Freud's failure to create a psychoanalytic neurobiology (Freud, 1895) and opted for a purely mentalistic model based around verbal reports of internal experience.

In the 40s and 50s neurobiology was dominated by mass action theory (Lashley, 1923; 1929) which held that the cortex was largely indivisible from a functional point of view and behaviour could not be usefully studied from the point of view of the brain.

Neuroscientists were, by and large, unconcerned with mental health problems, their focus being on deficits of cognitive functioning rather than affect regulation.

Psychoanalysis evolved in radical opposition to a prevailing view that mental disorders represented a constitutional vulnerability of the individual, which could not be remedied by environmental manipulations.

An unhelpful distinction between so-called functional and so-called organic disorder was accepted within psychiatry and other mental health professions, which although rarely scrutinised from this point of view, ultimately implied the acceptance of a mind-body dualism.

### *Progress in neurobiology*

While in general, in terms of the quality of patient care and the development of the discipline of psychoanalysis, particularly the unwavering focus on unconscious determinants, it may have been helpful to isolate psychoanalysis from the brain sciences, a number of by-products of this isolationist stance have created problems as the original objections to a closer link between the two disciplines began to shift. The last 30 years have seen a revolutionary advance in all the neurosciences which negated all the historical reasons for the isolated development of psychoanalysis (Westen, 1998). If Freud were alive today he would have an enormously complex set of findings and theories to draw upon in reconceptualising *The Project* and would be hardly likely to abandon the enterprise of developing a neural model of behaviour. Much is now known about the way the brain functions, including the development of neural nets, the location of specific capacities with functional positron emission tomography and neuroscientists can hardly be said to be exclusively concerned with cognitive disabilities or so-called organic disorders (Kandel, 1998; LeDoux, 1995, 1997).

Genetics has progressed, if anything, even more rapidly and mechanisms which underpin and sustain a complex gene-environment interaction belie original naïve assumptions about constitutional disabilities (Plomin, DeFries, McLearn, & Rutter, 1997). To take just a small sample of significant leaps forward which such scientific progress generates in the delivery of mental health care: the effectiveness of selective serotonin re-uptake inhibitors (SSRIs) in both depression and obsessive-compulsive disorder (Joffe, Sokolov, & Streiner, 1996; Piccinelli, Pini, Bellatuno, & Wilkinson, 1995), the undoubted benefits for children suffering from attention deficit hyperactivity disorder to be treated with methylphenidate (Fonagy, 1997b), the relative efficacy of neuroleptics in psychosis (Barbui & Saraceno, 1996; Barbui, Saraceno, Liberati, & Garattini, 1996), the growing recognition concerning the lack of efficacy of prolonged periods of hospital care and – its counterpart – the benefits of assertive community treatment (Holloway, Oliver, Collins, & Carson, 1995; Johnstone & Zolse, 1998), the potential for early diagnosis via brain imaging of neurosurgically treatable lesions (Videbech, 1997) etc. In fact, for the past 15-20 years the field of neuroscience has been wide open for input from those with an adequate understanding of environmental determinants of development and adaptation.

### *Obstacles to integration*

Paradoxically, the response of psychoanalysts has been defensive rather than welcoming of these remarkable advances in knowledge. Notwithstanding the commitment of most individual analysts to embracing all understanding, however painful and anxiety provoking, by and large the response of the psychoanalytic community has been unnecessarily dismissing and critical. The response has been as to an encroachment, withdrawing further and further into increasingly specialist areas rather than seeking to join and develop together with the evolution of brain science. The irrational prevailing belief appears to be that hard-won psychoanalytic insights could somehow ‘be destroyed’ rather than elaborated and enriched by the new methods of inquiry.

A further obstacle generated by the dichotomization of biology and patient care has been the anti-intellectual tendency of many psychoanalytic groups (Kandel, 1998). There is an assumed incompatibility between an astute and acute attention to the mental state of the patient. It is as if our observation of intellectualisation in our patients could somehow be automatically generalised to our own activities: from observing that a patient who reads and talks about science rather than feelings is not doing analysis, we appear to assume that an analyst who reads science also cannot be feeling and therefore cannot be practising analysis. There is an obvious element of truth in this attitude insofar

that reading and keeping up with science is time consuming and must take away from time devoted to clinical work. However, to claim that the two activities are hostile to one another is clearly an expression of prejudice rather than fact and somewhat self-serving on the part of those who do not wish to engage in such activities. Fortunately, the generation of psychoanalytic clinicians whose original professional training has already encompassed the rapid advances we are discussing neither understands, nor can have much sympathy with, this approach.

None of the major advances in psychiatric care are without their problems. SSRIs may turn out to have a significant placebo component (Verkes et al., 1998); ADHD is overdiagnosed, at least in the US (Goldman, Genel, Bezman, & Slanetz, 1998); there are common problems of compliance with neuroleptic medication (Kasper, 1998); there are well-publicised individual cases which document the failures of assertive community treatment; neuroimaging and genetic investigations have currently only a limited practical value. Arguments such as these should not be used to oppose developments in psychiatry but rather should be seen as opportunities for applying psychoanalytic insights in areas where there are significant shortcomings in the biological revolution. This requires taking a different approach: one of collaboration rather than confrontation. Before spelling out the specifics of such a collaborative approach, we should examine parallel developments in psychology.

### *The isolation from psychology*

#### *The original objections*

The psychoanalytic attitude to psychology mirrors the attitude of psychoanalytic psychiatrists to experimental medicine and the rest of biology. Progress in psychology has been largely ignored by psychoanalysts, despite the fact that an increasing number of psychoanalytic practitioners received their basic training in clinical psychology. Again, historically there are a number of valid reasons for this:

Psychology until the 1960s had an almost exclusive concern with behaviour and its models were largely based on studies of learning in animals (Skinner, 1953).

Psychology traditionally had an antagonistic attitude to psychoanalysis, seeing it as a major, medically dominated rival in offering psychological care in mental health settings (Eysenck, 1952).

Psychology retained a positivist influence upon its epistemology longer than most other social science disciplines. In fact its liberation from positivism is as much to be credited to progress in disciplines such as linguistics and sociology as to progress within its own domains (Chomsky, 1968).

Principally as a consequence of the previous factors, clinical psychology was frequently purposely naïve in its approach to the evaluation and treatment of mental disorder (Ullmann & Krasner, 1969; Wolpe, 1969) – a naivety that was abhorrent to psychoanalysts who had fought hard to acquire a sophistication concerning the nature of mental processes and mental phenomena.

#### *Progress in psychology*

About the same time as the revolution began in the brain sciences, psychology underwent a radical transformation, moving it from the periphery of the study of the mind to its current position as the recognised leader in the scientific study of mental processes (Westen, 1999). The chief driving forces behind these changes were:

The elaboration of the computer metaphor for psychological processes and the use of computer modelling for testing the appropriateness of psychological theories (e.g. Schmajuk, Lamoureux, & Holland, 1998).

The harnessing of technology for improved quality of observation, including the ready availability of video recordings, improved physiological measurements, endocrine and genetic analysis (e.g. Plomin et al., 1997).

More sophisticated methods of data analysis including techniques for causal analysis and special methods for analysing large data sets (McClelland, 1997).

Recognising the limitations of their early attempts at psychological intervention, clinical psychologists have worked hard to provide adequate psychological treatments, rarely seeing themselves in opposition to other treatment approaches, but rather as adjuncts bridging the gaps which cheaper pharmacological treatments left behind (Salzman, 1998; Thase, 1997).

By contrast to the attitude of psychoanalysts, psychologists embraced and built upon developments in related fields and have undertaken many significant large-scale collaborative investigations (e.g. Offord et al., 1992; Rutter, Tizard, & Whitmore, 1981).

### *Obstacles to integration*

The problems created by the combination of psychoanalytic prejudice against non-medical disciplines in general and psychology in particular have grown over the years. One aspect of the problem is the voluntary abandonment by psychoanalysis of opportunities for major contributions to the behavioural sciences. A good instance of this is the controversy concerning developmental studies referred to by Roger Perron. The attempt to reduce psychoanalytic developmental work to a mere metaphor flies in the face of Freud's intentions as indicated by his own observational studies (see Freud, 1909a; 1919; 1920) as well as the work of some of the most distinguished psychoanalytic clinicians including Anna Freud, Renee Spitz, Margaret Mahler, Esther Bick, Donald Winnicott – all of whom saw value in observing the young child, particularly in interaction with a caregiver. These efforts have been meaningful sources of inspiration to theory building and to draw a sharp line between observational studies and psychoanalytic theory as a matter of principle at this particular time seems arbitrary, unscientific and counter-productive. There is no discernible rationale except apparent incompatibilities between the psychoanalytic theories arising out of psychoanalytic observation and those cherished by certain theoreticians. To suddenly rule out observations because these no longer fit in with preconception is certainly not what Freud taught us about science. The scientific developmental model has never been metaphorical – nor has it ever been closer to empirical validation (see, for example, Westen, 1998). For example, while Anna Freud and Glover criticised Klein for the extravagant developmental claims implied by her theory, more recent observational evidence is by and large consistent with her claims – certainly those in terms of the cognitive capacities of the human infant (Gergely, 1991).

There is an even more problematic area concerning psychological therapies where the isolationist attitude of psychoanalysts has undoubtedly created a long-term problem. The pressure for cheaper, more cost-effective therapies has prompted some psychoanalytic clinicians to experiment with alternative methods of treatment – briefer, more focussed therapies, special therapies for particular groups (e.g. Malan & Osimo, 1992; Sifneos, 1992). These experiments were, on the whole, poorly supported by the psychoanalytic establishment who may have been over-concerned about the apparent superficiality of brief therapy. The gap was rapidly filled by alternative therapies, with often very limited observational or theoretical basis, borrowing increasingly heavily, and relatively openly, from psychoanalytic discoveries (e.g. Ryle, 1994). This has reached a point where certain schema focused therapies which represent an extension of the cognitive behavioural tradition are hard to differentiate from psychoanalytic therapies (Meichenbaum, 1997; Young, 1990). We have tried to show above, that psychoanalytic technique is only illusorily based on psychoanalytic theory. Both the discoveries and the effects of cognitive behavioural therapy and even behaviour therapy, are as easy to explain in terms of psychoanalytic ideas as in terms of behavioural ones (Fonagy, 1989; Wachtel, 1977). It seems, therefore, regrettable that psychoanalysts were not more vigorous over the last 25 years in experimenting with and evolving new psychotherapeutic techniques, but rather rigidly sticking to the 'one size fits all' principle. They abandoned the field of technical innovation to psychologists who, in part at least because of the opposition of psychoanalysts, have come to define themselves as "new and innovative" in contrast to psychoanalytic ideas.

This situation has altered somewhat, but only over very recent years. Many American institutes of psychoanalysis have started training psychotherapy candidates, only some of whom are expected to go on to full psychoanalytic training. Others have accepted directly the challenge of alternative therapies and are either working towards integrating effective components of these into psychoanalytically oriented treatments (Goldfried, 1995) or are working towards differentiating the effective elements of each (e.g. Jones, 1997). There is still a major gap in the integration of psychoanalysis and psychology, particularly in taking on board the major advances that the controlled,



experimental study of human mental processes has brought to the psychology of language, perception, memory, motivation, emotion, development, social relationship and so on.

The geneticist, Eric R. Kandel (1998) argued in a convincing way that “the future of psychoanalysis, if it is to have a future, is in the context of an empirical psychology, abetted by imaging techniques, neuro-anatomical methods, and human genetics. Embedded in the sciences of human cognition, the ideas of psychoanalysis can be tested, and it is here that these ideas can have their greatest impact” (p. 468).

### *Further obstacles*

The self-imposed isolation of psychoanalysis from the medical as well as the psychological sciences form but two of the major obstacles in the way of establishing a place for psychoanalysis at the table of the academy of the 21<sup>st</sup> century. There are several practical and epistemological challenges that need to be overcome if the suggested integration of psychoanalysis with contemporary science is to become a reality.

### *The case report*

The first of these is the unique focus of psychoanalytic writers on single case methodology that, as has been argued, shares a major burden of responsibility for the fragmentation of psychoanalysis as a discipline. There is no question but that single case studies are highly informative and much may be learned from the in-depth study of the single case. Our approach to the study of the single case may be improved, as indeed it undoubtedly has if we compare the quality of case reports from the 40s and 50s to current ones.

The case study by itself, however, is insufficient as a method of investigation. It needs to be supplemented by other confirmatory procedures such as replication, detailed experimental studies, anatomical, genetic and neurophysiological investigations. Roger Perron appropriately underscores the benefits that medicine has derived from intensive single case investigations. This undoubtedly was, and, to a limited extent, remains the case. It, however, should be remembered that the usefulness of some of these single case investigations was not simply in the clinical insights they generated but in the support that they received from independent and objective methods. Neuropsychology, which makes extensive use of the single case (Shallice, 1979), strengthens its conclusions through neuropsychological testing, brain imagery and extensive replication.

### *Background training*

Second, many psychoanalysts, particularly those trained by Institutes where psychoanalysis had limited involvement with the delivery of mental health care, may appear to be at a disadvantage in this new framework for psychoanalytic epistemology. Importantly, many extremely talented clinicians in these societies come to psychoanalysis from disciplines other than psychiatry or psychology – the arts, philosophy, or education. They have contributed enormously to the richness of the discipline with giants such as Erik Erikson, Anna Freud, Melanie Klein and current key figures such as Kit Bollas, Charles Hanly, and many others. They joined a mental health profession appropriately opened by Freud to all-comers (Freud, 1926). The fact that no science background was necessary to practise psychoanalysis in the early decades of the century, does not, however imply, that this remains the case. Societies that train individuals without mental health backgrounds normally ensure that these individuals acquire mental health experience. A similar case could be made for ensuring that those practising psychoanalysis and therefore in a position to develop the subject have adequate grounding in pertinent biological and social sciences. This is perhaps less important than a concerted initiative to identify and cherish a special group of psychoanalytic practitioners who will pursue the development of psychoanalytic science within the framework of the new sciences (Kernberg, 1993).

### *The dialectic between preserving the purity and enhancing the quality of observation*

Roger Perron implicitly invokes the important dialectic between the imperative of making reliable observations and, in so doing, distorting the phenomena to a point where meaningful observation is no longer possible. His commentary is carefully restricted to the study of psychoanalytic process – the

patient in intensive psychotherapy. Basically, I agree with Dr Perron in his analysis, even if not in his conclusions.

Audio recordings entail the risk that what is observed is no longer psychoanalysis in much the same way that comparative psychology has found laboratory conditions to constrain the range of animal behaviours which could be subjected to scientific scrutiny (Hinde & Stevenson-Hinde, 1973). I, however, struggle with the prescriptive tone of Perron's analysis and the certainty which it implies. I do not believe that we know to what extent audio-taping might or might not interfere with the psychoanalytic process. We anticipate that it might, but this does not mean that it will. Even if it does, it is not certain that it will do so in ways which would prevent the study of certain key aspects of the process.

What we can be reasonably categorical about is that narrative reports, however carefully crafted, are necessarily selective in ways which clearly undermine their scientific usefulness (Brown, Schefflin, & Hammond, 1998). A core element of our theory concerns non-conscious aspects of psychic functions. Our theory tells us that we cannot and should not expect any participant of an interpersonal interchange to be unbiased, to be random in the errors and omissions they make in their report. I do not think that any psychoanalyst could seriously defend the claim that the mere fact of having participated in an analytic process themselves guarantees lack of bias in their observations.

Far more important than bias, however, is the degree to which any of us can claim to acquire insight into the detail of interpersonal interaction between patient and analyst, purely from participant observation. We know that for the most part such interactions are governed by non-conscious mechanisms, quite opaque to introspection. There are quite dramatic illustrations of this – but some of the most striking are Rainer Krause's (1997) studies of facial expressions of affect in face-to-face psychotherapy and Beatrice Beebe's (1997) and Ed Tronik's (1989) work on mother-infant interaction.

Imaginative studies making use of the advances in recording and coding techniques and particularly phonetic and linguistic speech analysis could undoubtedly advance our understanding of the psychoanalytic process (Fónagy & Fonagy, 1995). To ban such procedures outright is to tie our hands behind our backs in competing with other psychotherapeutic procedures. To me the issue of recording depends strongly on the research questions being asked. As long as it is kept in perspective as but one of many windows for the study of psychological processes and their change in the context of psychoanalytic treatment, and given the patient's and the analyst's willingness to accept the recording, it is hard to see in what way it may harm. However, if we end up confusing recorded analysis with psychoanalysis per se – i.e. conflate the observation of the phenomenon with the phenomenon itself – we are in trouble on a number of counts, not just those pertaining to the validity of our observations.

## **Is psychoanalysis a science?**

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There can be no question but that at the moment psychoanalysis is not a science. It simply does not meet any of the major canons for such activity. Many of these were listed by Roger Perron. The question is more usefully phrased in terms of our vision for psychoanalysis. Should we aim to modify it so it might be more acceptable to the community of scholars who call themselves scientists? Or should we be content to continue to occupy a middle ground between art and science, that we currently inhabit? As usual, there are many strong arguments on both sides of the debate. Most of these, however, are couched in terms of the greater respect which would be accorded to our discipline were it to meet the canons of science versus the sacrifices we would have to make in order to do so. There have always been those who entered the murky waters of the philosophy of science in order to show that by this or that definitional framework psychoanalysis might or might not qualify (Shevrin, 1995).

Important as these debates might be, I think they miss the essence of the issue for three reasons. First, even if we meet criteria for scientificity, there is no guarantee that our theories will be taken seriously. There are plenty of examples of scientific theories which are of little concern to anyone. The question is perhaps as much of perceived relevance as of possession of the label of science. Second, as Roger Perron's review demonstrated, there is obviously a limit to how far the discipline of psychoanalysis

can go in meeting these criteria before it ceases to be psychoanalysis. Third, the criteria are abstracted from the properties of disciplines generally agreed to be sciences but there are plenty of exceptions. Which are the criteria that psychoanalysis must take seriously? And which are the ones we can neglect? And who decides which is which?

## **Shift in attitude towards the scientific**

Rather than talking about science, I think it would be more helpful to talk about an attitude or culture which characterises science, but which is by no means exclusive to it. Below we list some aspects of the change in attitude that might be required if psychoanalysis were to decide to adopt a more “scientific attitude” in the hope of addressing some of its epistemic problems.

### *Strengthening the evidence base of psychoanalysis*

Most psychoanalytic theorising has been done by clinicians who have not tested their conjectures empirically. Not surprisingly, therefore, the evidential basis of these theories is often unclear. In asking for evidence, I believe we are not returning to operationalism, verificationism, or other discredited residues of logical positivism (see, for example, Leahey, 1980; Meehl, 1986). By placing the focus of explanation into a domain incompatible with controlled observations and testable hypotheses, psychoanalysis deprives itself of the interplay between data and theory which has contributed so much to the growth of 20<sup>th</sup> century science. In the absence of data, psychoanalysts are frequently forced to fall back upon either the indirect evidence of clinical observation or an appeal to authority.

The validation of variables implicated by psychodynamic theories poses a formidable challenge to the researcher. Most of the variables are private; many of them are complex, abstract and difficult to operationalise or test with precision. Psychodynamic accounts focus on very remote etiological variables which are unlikely to be readily encompassed within an empirically based psychological model. Even when constructs are apparently operationalisable, they are rarely formulated with sufficient exactness so that they could be submitted to disproof. For example, concepts such as splits in the ego, masochism and omnipotence, are rarely defined with the exactitude necessary for operationalisation.

There is a further major logical problem with the reconstructionist stance adopted by most clinicians (see Perron’s overview). At the simplest level, clinical theories of development are based on the accounts of currently symptomatic individuals who attempt to recall events that occurred during early childhood, the most relevant part of which covers the pre-verbal stages of development.

Psychoanalysis has contributed significantly to our current sophistication about sources of bias that can distort memories of early experience (see Brewin, Andrews, & Gotlib, 1993). The clear danger is of a logical fallacy of assuming that something must have gone amiss during childhood, otherwise these individuals would not be in such difficulties. Thus most psychoanalytic developmental theories make recourse to various errors of omission or commission on the part of the mother that would be hard to verify. The converse is also true; the presence of “healthy” aspects in an otherwise severely disturbed individual, may lead clinicians to postulate moderating factors such as the presence of “a good object” in an otherwise devastated interpersonal environment. As we have seen, there is a confirmatory bias inherent to enumerative inductivism, which clinical theories of development find hard to circumvent.

Clinical illustrations have enormous value in summarising central and recurrent themes emerging in a particular patient group. They are also useful in generating hypotheses that can be examined through more formal investigative techniques. Clinical insight, however, is unlikely to be helpful in resolving theoretical differences concerning developmentally remote variables that are considered to place an individual at risk of a disorder. The reason for this, as we hope this chapter has illustrated, is that the observations of perceptive and experienced clinicians do not always converge on common interpretations.

It should not, however, be too readily assumed that the empirical data which are most useful in the context of justification, which allow optimal control of variables, minimise threats to internal validity

and maximise the possibility of causal inference, are also most helpful in the construction of a psychological theory. Westen (1991) points to the relative paucity of rich theories within current psychiatry and psychology that are based on controlled studies. Indeed, many psychological theories of psychopathology explicitly acknowledge their indebtedness to psychoanalytic ideas, which have inspired specific lines of empirical investigation. Clinical data clearly offer a fertile ground for theory building, but not for distinguishing good theories from bad or better ones. The proliferation of clinical theories currently in use is the best evidence that clinical data are more suitable for generating theories and hypotheses than for evaluating them. The convergence of evidence from several data sources (clinical, experimental, behavioural, epidemiological, biological etc.) will provide the best support for the theories of mind proposed by psychoanalysis (Fonagy, 1982).

Thus, future psychoanalytic work should move away from enumerative inductivism and develop closer links with alternative data gathering methods available in modern social and biological science. To gather such data, without obliterating the phenomena which such investigations aim to scrutinise, is an important challenge to the current generation of analysts.

### *Moving from global to specific constructs*

Speaking broadly, psychoanalytic constructs lack specificity. For example, psychoanalytic developmental models have aimed at a level of abstraction where a one-to-one relationship could be identified between a particular pattern of abnormality and a particular developmental course. Thus within each of the major theoretical orientations there is a singular model for borderline personality disorder, narcissistic pathology, antisocial personality disorder and so on. Within modern psychopathology and psychiatry the trend is towards differentiation and specificity. Evidence is rarely found linking entire classes of disorders with particular pathogens, but rather specific pathogens linked to specific sub-classes within diagnostic groups. The single case orientation of clinical research has not served psychoanalysis well in this context. It is hard to generate a specific nosology using many single cases, all observed from slightly different vantage points. Studying case series with reference to a single schema may be more productive in this regard. John Clarkin's (1994) work at Westchester looking at sub-classifications of borderline personality disorder from within a combined DSM-IV and structural object relations theory framework is an excellent example of the value of this approach.

There is a further sense in which psychoanalytic constructs are often overly global. For example, object relationships are often treated as a singular phenomenon yet clearly, even at a descriptive level, they encompass a number of subservient functions. These include empathy, the quality of self-object representations, the affect tone of relationships, the ability to maintain these and to invest emotionally in them, understanding interpersonal interactions and so on. It is understandable from a clinical viewpoint, but probably counterproductive from the point of view of research, to conceive of object relations and similar constructs in such a global way. The meaningful categorisation of forms of pathology will be compromised unless we are able to be more specific about the particular aspects of object relations pathology which we see as common to a specific disorder.

Many current theories fail to distinguish between components of a process and a developmental course and thus create potential ambiguity. It is a regrettable general characteristic of our theories that they rarely explain the specific disorders which an individual is likely to develop given quite general characteristics of early experience. Our models do not regularly identify specific remote or proximal variables which account for the emergence of specific symptoms or the nature of the interaction among predisposing variables and other contributory factors. Thus we are rarely able to comment meaningfully on demographic trends such as recent increases in the prevalence of eating disorders or the varying prevalence of disorders across the life-span – for example the spontaneous improvement in borderline personality disorder in middle age (Stone, 1993). Psychoanalytic concepts, as we have seen, often have multiple referents (e.g. narcissism). Some of these pertain to the developmental course (e.g. inadequate experiences of mirroring and soothing) others to underlying mental states (e.g. a fragile sense of self) and yet others to manifest presentation (e.g. a grandiose view of the self). Stating this in more general terms, it would seem desirable to aim at shifting from an interest in global constructs and towards a greater concern with individual mental processes, their evolution, their vicissitudes, and their role in pathological functioning. There may be a trade-off between explanatory

power on the one hand and differentiation and exactitude on the other. That is to say, analyses at a global level offer an apparent power of explanation. This will be lost if the level of analysis is shifted to a specific mental process. However, the inexactitude of global-level analysis ultimately causes fragmentation and precludes the possibility of integrating findings across reports.

It seems then, that as part of the scientific attitude the preferred level of analysis of the psychoanalytic researcher should be groups of individuals (series of cases) and specific mental processes rather than global descriptive characterisations. A more scientific attitude would require us to be more developmentally and culturally specific about risk factors as well as suggest working in collaboration with other disciplines to address the problems of symptom specificity and specificity across the life course.

### *The routine consideration of alternative accounts*

Again speaking generally, in current clinical research there is a notable lack of serious consideration of alternative accounts when relationships are proposed between clinical observation and theory. It is very rarely that authors genuinely consider how the observations they report may be accounted for by theoretical frameworks other than the one they espouse. There is no tradition of “comparative psychoanalytic studies”, where alternative frameworks are considered side-by-side in a specific context. In fact, it is generally, if informally held that those who have not been trained in a specific tradition might be on shaky ground when using constructs rooted in that tradition. It is hard to imagine how this could lead to anything but fragmentation. Instead, each framework, once established, tends to take on the challenge of incorporating all new data, gradually making them unwieldy and contrasts between theories of little practical relevance.

There are two facets to this problem. The first is that the principle of parsimony (Occum’s razor) is hard to apply as explanations are rarely placed side-by-side. For example, the concept of splitting has been widely used since Freud’s introduction of the notion (Breuer & Freud, 1895; Freud, 1923) and Fairbairn’s (1952) popularisation of the idea. As a behavioural phenomenon, splitting is readily observed in most severe psychopathology, particularly borderline personality disorder (American Psychiatric Association, 1994; Perry, 1992; Westen, 1997). Accounts of the concept, however, vary, from ones tracing its origins to infantile mental states and the need to protect the good object from internal attack to others where any separation of mental state from consciousness is considered under this heading (Rousillon, 1998). The conceptual framework within which splitting is considered profoundly influences the range of phenomena which it is used to explain. Yet since Hartmann’s (1964) description of the “genetic fallacy” we understand that the origin of an ego defence has no implication for its current function and use. The most parsimonious account of the phenomenon of splitting might be that it is a normally and naturally occurring cognitive response to extreme levels of conflict and stress (Linehan & Heard, 1993). The extensive use of splitting as a defence may have less to do with a past history of unresolved ambivalence or inaccessible traumata and more to do with the current stress which borderline individuals experience.

The second aspect is the identification of the best-fitting account amongst rival accounts. For example, hostility and destructiveness in borderline patients has been attributed at various times to constitutional aggression, experiences of unempathic caregiving, self-protective defensive manoeuvres etc. It is not clear if these competing accounts should be applied to the same individual at different times, to different individuals, or if just one of these accounts is correct and applies to all individuals in the category.

The challenge for the future must be more fully to explore alternative accounts, identify the appropriate sub-population to which they are best suited or discontinue their use having replaced them with a better-fitting alternative. Such an endeavour requires systematic scrutiny.

### *Increasing our sophistication concerning social influences*

Psychoanalytic theories vary in the extent to which they show concern about the impact of the environment. However, generally speaking, they suffer from a lack of sophistication in considering the impact of the external world. In some respects this is understandable as the focus of

psychoanalysis is explicitly upon the intrapsychic. It is this lack of sophistication which leaves psychoanalysis vulnerable to accusations of mother-blaming and the unrealistic over-emphasis on external influences during the first years of life.

It is now generally accepted that influences between the child and the environment are reciprocal. Constitutional and parental risk factors interact in the generation of risk (Rutter, 1993). Such interactional models suggest that risk and trauma are processes rather than events and problems arise when a constitutional vulnerability is combined with a sub-optimal environment thus generating a maladaptive response which in turn might undermine further the adequacy of environmental provision and so on. A scientific psychoanalytic attitude would suggest the elaboration of current psychoanalytic developmental models in the direction of increased specificity concerning transactional aspects of the process of traumagenesis.

There is a further respect in which psychoanalytic views of environmental influences lack sophistication. The wider social and cultural context within which object relations develop are often ignored by psychoanalytic theorists. This observation is only partially accurate in that many individual theorists have paid specific attention to cultural factors (see for instance, Erikson, 1950; Lasch, 1978; Sullivan, 1953). However, the impact of race and culture on development and pathology is rarely a focus for psychoanalytic theorisation, perhaps as a residue of the biological origin of psychoanalytic ideas.

A particularly dramatic example of the influence of cultural factors may be found in approaches to self-development. Psychoanalysts have traditionally emphasised, in their general theories of development, the individuated self (see, for example Kohut & Wolf, 1978; Mahler, Pine, & Bergman, 1975). In generalising these models to other cultures, we may be ignoring the extent to which these ideas are rooted in Western thought. In non-Western cultures, the relational self is far more widely represented than the individuated self (Sampson, 1988). The relational self is characterised by more permeable and fluid self-other boundaries and by an emphasis on social control where this includes but reaches far beyond the person. The unit of identity for the relational self is not an internal representation of the other or its interaction with an ego ideal, but rather the family or the community. In traditional psychoanalytic theories a person who is over-dependent upon, and influenced by, moment-to-moment changes in their inter-personal experience might be considered immature or even pathological. Yet there is nothing universal about this view of the self. These ideas have emerged only gradually even in the Western world over the past 200-300 years (Baumeister, 1987). The well-known gender asymmetry in the diagnosis of borderline personality disorder may be interpreted as a consequence of the greater challenge experienced by women than by men when faced with the Western ideal of an individuated self (Gilligan, 1982). Placing the individuated self implicitly or explicitly at the peak of a developmental hierarchy may risk ethnocentrism as well as pathologising a mode of functioning which may be highly adaptive given specific social contexts.

The lack of psychoanalytic sophistication concerning the social environment represents a major challenge to the evolution of psychoanalysis beyond the issue of its scientific status. Given the intensive nature of psychoanalytic treatment, its influence will always be restricted to the relatively few individuals who have the benefit of this intensive form of psychotherapy. The decline of the social influence of psychoanalysis since the Second World War may have more to do with the extension of concerns about the mental health to a larger section of the population. Given the numbers now involved, psychoanalysis is bound to be seen as less relevant as a treatment approach. For the discipline to survive and flourish, it is essential that our theories are made relevant to the community at large and that we are able to offer input with problems of concern to our local community. Certainly at the present state of knowledge, such input should never be didactic but rather offered with the aim of learning at least as much as teaching. There are several projects in this spirit already underway in major cities in the US including Michigan, New Haven, Los Angeles and New Orleans. Traditionally our discipline has been highly ethnocentric. For example, psychoanalytic studies of multi-generational traumata have principally focussed on survivors of the Holocaust (Bergmann & Jucovy, 1982; Kogan, 1995). Yet perhaps we could learn as much or more about this process from the study of African-American communities in the US, many of whose current problems could be seen in the context of our failures in terms of their history in North America as an enslaved group (e.g. Belsky, 1993).

In brief, with regard to social influences, psychoanalysis should develop an improved categorisation system to describe environmental influence. Transactional models of development pay more attention to cultural factors, show greater awareness of their cultural context and step beyond ethnocentrism.

### *Collaboration with other disciplines*

For some psychoanalysts, the separateness of the psychoanalytic discipline from others whose subject matter overlaps with ours has been a source of pride to the extent that analysts have been criticised for including too many bibliographic citations to non-psychoanalytic work amongst their references (Green, 2000). The fear appears to be that fields adjacent to psychoanalysis have the potential to destroy the unique insights offered by clinical research. Whilst this is not a dominant view in psychoanalysis, and most psychoanalysts welcome the insights which knowledge from related areas can bring, instances of active collaboration with neighbouring disciplines are patchy, unsystematic and usually focussed on specific findings, discoveries or ideas which are already consistent with a particular author's preconceptions (c.f. Wolff, 1996).

Contrary to the suggestion that closer proximity to sciences with similar interests to ours may destroy psychoanalysis, Kandel (1998) made a strong case that the rich insights from psychoanalysis are most likely to be preserved through closer integration with biological psychiatry. He based his argument on three general principles:

All functions of the mind reflect functions of the brain. This principle may be maintained even if it is found that, for many aspects of behaviour, a biological analysis may not prove informative. Psychoanalysts may have a certain unease about the notion on two counts. First, that a biological account is invariably reducible to genetics, and second that genetic transmission leaves no space for environmental causation. Kandel, however, convincingly demonstrates that the ability of a given gene to control the production of specific proteins in a cell is subject to environmental factors and the fact that only 10-20% of genes are transcribed or expressed in each cell leaves plenty of room for social factors: "social influences will be biologically incorporated in the altered expressions of specific genes in specific nerve cells of specific regions of the brain" (p. 461).

Genes contribute importantly to mental function and can contribute to mental illness but behaviour itself can also modify gene expression. Twin, adoption and pedigree studies have provided ample evidence that genes determine about 50% of what we traditionally call personality. Variables such as tastes, religious preferences, and even clearly environmentally determined neurotic disorders such as post traumatic stress disorder have substantial genetic components. On the other hand, studies of learning in simple animals have demonstrated some time ago that experience can produce lasting changes in the effectiveness of neural connections by altering gene expression. These interactions suggest that the traditional distinctions between organic and functional disorders are unsustainable. All mental disease is organic since functional imaging techniques can reliably demonstrate that the biological structure of the brain is altered (Jones, 1995). This observation is a trivial consequence of the previous principle. The outstanding two-fold question is how biological processes modulate mental events and how biological structure is modulated by social factors. It is in answering the second of these questions that a scientific psychoanalysis has a clear role to play.

Alterations in gene expression as a consequence of learning impact on the brain by causing changes in patterns of neural connections. By the same token, psychological interventions such as psychoanalysis must also produce changes in gene expression which alter the strengths of synaptic connections. It is possible to argue that both pharmacological and psychotherapeutic interventions produce functional and structural changes in the neural circuitry. The former may be more non-specific than the latter and therefore more effective for some mental disorders than others. Alternatively, the two may function synergistically - each acting on slightly different systems but enhancing the benefit to be derived from the other. The evidence from combined pharmacological and psychotherapeutic interventions implies that there is considerable benefit from an integrated treatment approach (Roth & Fonagy, 1996).

The same set of arguments could be made for the further integration of psychology and psychoanalysis. As long ago as 1982, I proposed that much that has been learned in psychology about mental processes was applicable to psychoanalysis and should be integrated with it (Fonagy, 1982).

Since that time, together with a number of colleagues, I have been working on integrating the mental function associated with the representation and understanding of mental states with psychoanalytic ideas. This is just one of a wide range of mental processes or modules (Fodor, 1983). Systematic study could achieve a high level of integration and a great deal of increased sophistication in the way that psychoanalysts talk about remembering, imagining, speaking, thinking, dreaming and so on.

All that is required for both these integrative initiatives is a more scientific attitude, a broader range of methods and an openness to and excitement about new ideas.



## Section C: Response to Peter Fonagy by Roger Perron

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I am in agreement with Peter Fonagy on certain basic principles: the need to clarify and be more precise about our concepts, in an effort to really attain a common language, and to be more convincing (both to ourselves and to others) about the validity of our theoretical corpus and the efficacy of our work. I second his regret about the chilly isolationism of many of our colleagues towards other disciplines that could be valuable for us, particularly psychology in its modern developments: I have often remarked that, when our psychoanalytical society organises interdisciplinary discussions, it invites immunologists, sociologists, anthropologists, historians, etc., but never psychologists. At the present time in France, psychologists are far more receptive to psychoanalysis than psychoanalysts are to psychology.

However, there are also topics on which we differ or disagree. I shall limit my response to four points:

A. Is psychoanalysis a science or not? This question roused much controversy among some of our French -speaking colleagues some years ago, and produced some important publications. As a scientific researcher who teaches the methodology of clinical psychology, I must confess to being sometimes irritated by these discussions, because the question was too often discussed according to an implicit model of science which is the one that reigned -and reigns- over physics, chemistry, optics, cinematics, electronics, etc. where the looked for laws are shaped in terms of mathematical propositions. This forces us to discuss the problem in terms of the criteria prevailing in these sciences (quantification, repeatability, etc.).

But there are other models. The main one is the functional model, as laid by Claude Bernard and largely used by Freud. But we must also take into account the taxinomy model used by botany and zoology to produce, from the middle of the 18th century, an enormous bulk of knowledge; the developmental -more widely diachronic -model, in paleontology, embryology, linguistics, child psychology, etc.; formal models as used in anthropology, sociology, linguistics, some currents of cognitivist studies, etc. In all these cases nobody contests that scientific work is being done, even if in many cases there is no quantification of the observed phenomena, or only an accessory quantification. There are plenty of examples. For example, lunar cartography is a scientific enterprise: even if measures of distances, altitudes, etc., are taken, this is clearly not what defines the object and the method. Paleontology draws developmental lines by arraying along time forms of skeletons: even if carbon-14 dating is utilized, the real scientific work is done by the researcher's mind when he looks for similarities and differences and puts them into temporal order.

If we enlarge the discussion to consider the so-called "human sciences" (the very term being disputable) I think that many serious historians, for instance, would be shocked if somebody declared that their work was "unscientific" because they do not measure anything, and cannot deduce mathematical laws...

What is the status of psychoanalysis in these disputes? Psychoanalysis is an endeavour to understand something about mental functioning. But passions are roused when we try to consider the functioning of our own minds: mathematics about other people, yes, but not about me ! (It can be very entertaining to hear eminent colleagues proud of their position in scientific psychology arguing, with much passion and ire, that the human mind can be understood only without passion).

In fact, what is at stake is the question of human liberty. We are in a double bind: on one side, the resolute determinism of Freud, which we generally adhere to, and on the other, the evident aim of the psychoanalytic treatment, which is more personal liberty. How to lean on determinism to promote liberty? I have no clear answer to this question, but I think that it is worth pondering. In fact, I think that the question must be posed in this form if we are not to be trapped in what appear sometimes to be distressingly naïve discussions of the question "is or is not psychoanalysis a science".

I cannot concur with Peter Fonagy's discussion in terms of deductive or inductive methods. Of course, psychoanalysis cannot be deductive in the sense of "if A... then B". Happily so, because this would totally suppress any liberty. I far prefer: "If A, B, C... X, then perhaps Y": this is precisely what we observe, not because our knowledge is coarse, but because things are so. The statement on p. X that:

"Psychoanalytic practice bears no logical relationship to theory" implies that the deductive method is the only really "logical" method, but there are other ones. As a psychoanalyst I try to be "logical", i.e. to avoid contradiction. To seek for contradictory observations and hypotheses is an essential principle in every objective approach, including clinical and theoretical work in psychoanalysis. But, this admitted, the question of the relations between contradiction (at the knowing mental apparatus level) and conflict (at the known apparatus level) is an important one for psychoanalysts.

B. The diachronic approach. Here I differ widely from the position elaborated by Peter Fonagy. As a clinician and a research worker, I have worked in the fields of developmental psychology and child psychopathology. To make predictions in these fields is always highly dubious: happily so, as this is a good proof of human liberty. This is striking in the case of families with an autistic child: why this particular child, and not their brother or sister? Usually we find no credible answer when we try to point to a constellation of factors and conditions prior to and external to the subject himself, considering the psychic functioning of the subject as a mere result of this constellation. This is so because, by adopting such an approach, we ignore a basic principle of psychoanalysis: the psychic dynamics of the subject have in themselves a causal value, proceeding from internal laws. We must therefore strive to grasp the history of the construction of this particular mental apparatus (in this case the mental functioning of an autistic child). Of course, it remains important to know also the history of events and surrounding conditions, but they are to be considered more as an array of conditions than of causes in the strict sense.

From this perspective two points may be underlined. First, we cannot and must no longer accept the model of causality that prevailed in 19th century physics (if A, then B). Even in physics, models of recurring or circular causality, chaos theories, etc., have upset this simplistic approach to understanding causality. In this respect, chaos theories are very fruitful for us. It has been clearly demonstrated that a very large number of non-predictable phenomena occur in the field of events relating to matter and energy (the subject matter of physics, chemistry, biology and other natural sciences). Moreover, the latter are non-predictable not because of a lack of knowledge on our part but by their very nature: a tiny random variation at any given point of the causal chain may lead to a totally different long-term result. Remarkably, this has been very clearly demonstrated mathematically; however, what has been demonstrated is not that a specific event will necessarily occur but, on the contrary, that it is impossible to predict which event will occur. We can therefore no longer define a science on the basis of its capacity to predict phenomena. Psychoanalysis, which is interested in psychic development and its pathologies, often falls into this category; moreover, the same applies to a course of treatment with an adult – while one may have hopes and predictions and seek to establish and direct the analytic process as effectively as possible, the outcome is not – and can never be – certain.

Second, in the psychoanalytic field, it seems essential to consider the role of deferred actions. André Green (who knows a lot about English-speaking authors) has often underlined the fact that we differ widely in our understanding of this term. In French, "Nachtraglichkeit" is translated as "apres coup" (effet en causalité rétrograde, with effects on what came before, or more precisely on what is left of it) whereas the English speak of "deferred" or "differed" action, in a one-way time arrow. On both the clinical and the theoretical level, no matter how credible a patient's account of an event (for example a violent action of the father against the mother when the patient was 8) we are faced, not of course with the event itself, as it would have been observed by an objective witness (but even such an objective witness would have seen and experienced the event with the same dynamics, the same identifications, projections, etc., as anyone else), but with a memory of the event. The question is therefore: why and how was this event seen and experienced by the child at 8, and how and why did it afterwards affect the history of his psychic functioning, his identifications, his drives and anxieties, etc., if it is now being presented in this way? Throughout this history, the original event has been remodeled and rebuilt; it has contributed to assimilate, and sometimes to provoke, other events, other experiences, etc. Only the present event is open to our work, which is: such a thing is said to me, here, now, in this session, in this moment of the treatment: how is it said, why now, why to me? It follows that the questions of how the original event happened (at 8 in my example), and even whether it happened, are secondary. The evolution of Freud's theory from a theory of "real seduction" to the assumption of fantasy is pertinent here. The history of the patient is the history as it is deconstructed and rebuilt in the course of the treatment; and the very aim of the treatment is precisely to build a new

personal history. Serge Viderman, who had a great influence on French psychoanalysis, discussed this with great momentum (Viderman, 1970).

C. Polysemia (multiple sense). On p. 31 it is asserted that I deny the possibility of unequivocal definitions for our concepts. I do not deny this possibility, but aim to raise a question. I agree that the more a concept can be defined without any ambiguity, and the more we can articulate the relationships of these concepts in an unambiguous manner, the better it is for our discipline. But what is lost of psychoanalysis if we go too far in this way? This question may be discussed at two levels.

The first is plainly a semantic one. Yes, we should be in a better situation if we could use words that would not provoke misunderstandings among psychoanalysts. But different words create a trend to think that things are wide apart. Two examples. "Fantasy" is a word that covers all levels of this psychic type of productions, from unconscious to clearly conscious ones. I maintain that the term "fantasy" must be used to point to the continuity of the field, but qualified, to be clear, with adjectives such as "conscious fantasy", etc. "Identification" is a protean term, and it seems better to qualify it with adjectives (hysterical, adhesive, etc.), but to keep the term "identification" in order not to split the field in tiny pieces.

The second level of the question is far more difficult. It pertains once more to the problem of the relations between the knowing and the known; in other words, to the fact that we know the psychic apparatus by the means of the psychic apparatus. Objectivity of course may be looked for and attained, but only by considering transference and counter-transference. As to the question and to this answer, I think that the status of psychoanalysis is unique (and difficult to understand by non analysts). A consequence is that perhaps some plasticity of our knowledge means (words and concepts connoted by words) is necessary. I fear that in a cure the exclusive use of unequivocal terms (one meaning only for each) would blind the analyst to what s/he ought to see. The model of the computer (which stalls when confronted with ambiguous terms and/or ambiguous relations) is a fallacious one. It is evident that we work with multiple senses of the "same" material, as well in the analyst as in the patient; the productive moments of a cure are those when the senses diverge, and the analyst, thinking about it, finds and suggests something new to the patient. One may add that linguistics (we work with and in language) has demonstrated that each word has several meanings, of which one is selected from its place in a syntagmatic net, according to the context (verbal and non verbal). If this is ignored, perhaps there is no more psychoanalysis. This of course is largely open to discussion. This idea would ground some objections I could raise about some forms of atomising check lists, grids, etc. (but some forms only). Physics has clearly demonstrated that conventional mechanics does not apply in all cases since some processes are irreversible, in both nature and in thought. Consider the example of the mixture of gases and ash produced by a burning cigarette: it seems impossible to use the spatial distribution of these molecules afterwards to accurately determine the spatial distribution and structure of the cigarette's constituent molecules before combustion. (cf Atlan, H, *Entre le crystal et la fumee. Essai sur L'organisation du vivant*, Paris, Seuil, 1979; Prigogine, I, Stengers, I, *La nouvelle alliance. Metamorphoses de la science*, Paris, Gallimard, 1979). The same applies to psychoanalytic research: it seems to me extremely unlikely that by using psychic or behavioural fragments, each allocated a place in an analytic framework, we can create a picture of how the whole psyche functions, regardless of how sophisticated our methodology is.

D. Recording data from sessions. Peter Fonagy objects on p. 41 to: "the prescriptive tone of Perron's analysis and the certainty which it implies". Yes, I am prescriptive and certain that I should be unable to tape one of my sessions with a patient. Because: either I do not tell him, and this would be an intolerable betrayal of his faith (and could I then be indignant if he himself secretly taped some sessions, as happened some years ago to a colleague, who was publicly denounced as a quack in magazines); or I ask his agreement. Whatever I say and think, he will think that other people will hear or read what he said, so a third party will be from the very beginning and constantly with us, intruding in our relationship. All transference and counter transference processes will be affected. Fonagy says, "I do not believe that we know to what extent audio-taping might or might not interfere with the analytic process". But can we find that out from a statistical study? (which would be from the very onset distorted). I know, on the basis of all my analytic experience, that between me and my patients this would interfere, in a way I could not properly analyse. All my French colleagues think that way. If one of my trainees told me that he intended to tape a patient, I would tell him not to do so. All the training analysts in our Institut de psychanalyse share this position.

I must add that this is for psychoanalytic sessions proper (armchair-couch). I think it would be possible to tape under other settings (face to face, psychodrama. etc., with some patients (not all, of course). This is to be discussed. But the following question is: With what methods should these data be treated? This is another big chapter, for another day.

## Section D: The justification of effectiveness studies in psychoanalysis

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In this section we shall consider the current climate in health care services which is largely responsible for the drive for effectiveness research and briefly overview some of the methodological issues that confront these studies. In the last part of this section we shall overview studies of psychoanalytically orientated psychotherapies.

### Evidence based medicine and its justifications

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#### Reasons behind the insistence on evidence

Psychoanalysis is a clinical intervention. Its aims and ambitions, at least from the point of view of most patients, are clearly associated with those of other healing arts such as surgery, physiotherapy and osteopathy. Admittedly, this is just one aspect of the psychoanalytic enterprise, but one that is crucial to its standing within most of the cultures where it is practised. Over the last ten years, all aspects of medicine have come under scrutiny, where increasingly both commissioners and funders of medical intervention, as well as those managing and directing clinical services, have embraced the values of “evidence based medicine” (Sackett, Rosenberg, Gray, Haynes, & Richardson, 1996). Clinical judgement is no longer accepted as sufficient grounds for offering medical treatment. Recommendations at national policy as well as at local health care provider level are expected to be based upon evidence of effectiveness. What factors account for this change?

#### *Ostensible reasons*

Evidence based medicine is founded on an ideal – that decisions about the care of individual patients should involve the “conscientious, explicit and judicious use of current best evidence”. Much is claimed in favour of this approach, particularly in North America and Western Europe. The arguments in favour of it include (a) the more effective use of resources, (b) improvements in clinician’s knowledge, and (c) better communication with patients (Bastian, 1994). From an ethical point of view, the strongest argument in support of evidence based medicine is that (d) it allows the best evaluated methods of health care to be identified and enables patients and doctors to make better informed decisions (Guyatt, Sackett, Cook, & the Evidence Based Medicine Working Group, 1994; Hope, 1995). All these are good reasons but all were as relevant to medicine in the past as at the moment. So why the current emphasis?

#### *The political background*

The real driving force behind evidence based medicine is unlikely to be a genuine concern for the quality of care. The movement appears to be largely driven by financial consideration and the hope of health care organisation to be able to reduce escalating costs by focussing on the most cost effective option given a range of treatments. Governments and health funds find the notion of allocating health resources on the bases of evidence quite attractive. In North America, D.K. Eddy in an important editorial suggested that healthcare funds should be required to cover interventions only if there was sufficient evidence that they can be expected to produce their intended effects (Eddy, 1996). The Australian Health Minister, Dr Michael Wooldridge, adopted a very similar position stating “[we will] pay only for those operations, drugs and treatments that, according to available evidence, are proved to work” (Downey, 1997).

While we believe that evidence for psychoanalytic interventions are important to derive, we are sceptical about the pressures brought on psychoanalytic clinicians as it seems to us unlikely that even in the face of overwhelming evidence as to the benefits of this relatively expensive treatment, the resources would be available to provide psychoanalysis for a significant proportion of those who require it. We shall consider the specific issue of cost effectiveness separately. In this context it is

important to review the philosophical basis of the search for evidence for psychoanalysis in order to gain perspective on the entire enterprise of outcomes research. Perron's critique has covered some of these issues from a more general epistemological standpoint; here some additional conceptual and practical concerns will be briefly explored.

## **Philosophical concerns**

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Evidence based medicine represents a practical example of "consequentialism". Consequentialism refers to the proposition that the worth of an action may be assessed by the measurement of its consequences. There are at least three problems with the consequentialist argument, all of which apply to psychoanalytic outcome research: (a) the difficulty in measuring outcomes, (b) the ownership of outcomes (whose interest should be considered?), (c) consequentialism may lead to unethical conclusions. We shall take these in turn.

### **Philosophical questions concerning the measurement of outcome**

The first concern is with the measurement of outcome. It is indisputable that many important outcomes of any medical treatment are unmeasurable. Evidence based medicine claims to provide a simple logical process for reasoning and decision making: (a) systematic scrutiny of the available evidence, (b) drawing appropriate conclusions leading to (c) a clinical decision as to the appropriateness of a treatment. Within this framework, for any decision to be balanced, all relevant consequences of a treatment must be considered. Unfortunately, in the current state of methods of psychological measurement, many important outcomes can only be very inadequately measured. Psychoanalysis concerns complex internal states such as the degree of distress or pain experienced by an individual. Often these complex states are reduced to simpler, easily measurable ones such as depression (Beck, Ward, Mendelson, Mock, & Erbaugh, 1961), anxiety (Spielberger, Gorsuch, & Lushene, 1970) or total symptomatology (Derogatis, 1983). A valid objection to such measures (if used without sophistication) is that they are reified and researchers may conflate the measure with the phenomena they were aimed at quantifying. Thus, the BDI score is not depression and the total symptom distress score of the SCL-90 is not equivalent to mental pain. By having these measurements we have not at all done justice to the complex cognitive, affective and physiological processes which are implicated by these terms.

Even if better measures were found for some of the domains of outcomes entailed in psychoanalytic treatment, other aspects of the process, such as an ethical life, a sense of purpose or social justice, may be inherently unmeasurable. Even more troublesome are key domains which are not even well defined, let alone measurable. One such is the "quality of life". Attempts have been made to provide a metric for this, yet in the absence of a consensus as to what a reasonable quality of life might entail, it is hard to imagine how measurement is possible.

The philosopher Bernard Williams (1972) noted that values that can be quantified in economic terms, may require comparison with values which are not quantifiable. His comments may be easily extrapolated to the current situation of psychoanalysis in some countries: "Again and again defenders of such values are faced with the dilemma of either refusing to quantify the value in question, in which case it disappears from the sum altogether, or else of trying to attach some quantity to it, in which case they misrepresent what they are about and also usually lose the argument, since the quantified value is not enough to tip the scale" (p 103). Some outcomes of psychoanalysis may indeed be costed, but these may be some of the least important. The cost saved may not "tip the balance" in favour of psychoanalysis.

### **The ownership of outcome**

The second common criticism concerns the ownership of outcome: "Whose outcome is the outcome of psychoanalysis, anyway?". It may be in principle impossible to decide between the competing claims of different individuals. For example, a treatment that enhances the quality of life of one person may be deleterious to a spouse or an employer. This is particularly evident in the case of the psychoanalytic treatment of children where the treated child's desired outcome may be in conflict

with that of the parent's, or indeed that of the sibling. Ideally, notwithstanding the insurmountable practical problems, all individuals significantly concerned with an analysis should be assessed as part of the outcome study. The research enterprise itself is clinician led. It is the clinician-researcher that decides whose outcome will form the basis of evidence based practice. Thus all outcome investigations, perhaps particularly that of psychoanalysis, will be arbitrary, and limited by the selection of the individual(s) on whom outcome is measured.

An extension of the arbitrariness problem of outcome ownership concerns the status of client choice as an indication of outcome. It could be argued that the client is in a privileged position relative to the investigator in determining whether the treatment is helpful. Interestingly, when user groups are asked they tend to strongly favour approaches to most mental health problems which are psychologically rather than pharmacologically based, or at least they plead for a greater emphasis on psychological help. When individuals perceive their difficulties arising out of psychosocial causes, they understandably seek redress in the same domain i.e. the interpersonal. It is also worth noting that psychoanalytic therapy often has greater *prima facie* acceptability than exposure-based cognitive behaviour therapy (for example with patients with OCD, Apter, Bernhout, & Tyano, 1984). Yet the desire of the user, "client satisfaction" is not generally acceptable as an adequate criterion for outcome. By this criterion, many treatments known to be ineffective and even harmful, (e.g. recreational drugs such as nicotine counteract anxiety) could be selected.

Psychotherapy researchers are particularly conscious of the danger of imposing ethnically rooted cultural biases on what is designated as "needing treatment" and to be a "good outcome" (Bernal, Bonilla, & Bellido, 1995). For instance, the achievement of selfhood through the separation-individuation process is one of the cornerstones of psychotherapeutic interventions. Yet is Lasch (1978) correct that the emphasis on individual achievement in Western culture is excessive and that an appropriate submission to the goals of the family and community (Kagan, 1984) may be a far better indicator of healthy adaptation? Such differences are particularly acute in the area of child development and parenting. Rogler (1989) outlined some of the practical steps which culturally sensitive outcome research requires. In particular, it is important to ensure that interventions are consonant with the subjective culture of the ethnic group to which it is applied and that instruments used are able to integrate cultural meanings with the pertinent scientific categories. In reality, this is an ideal to strive for, but it is rarely achieved.

## **Ethical concerns**

Finally, it is commonly asserted that a uniquely evidence based treatment approach can lead to activities which are at odds with common morality. A good example of this is the success of aversive conditioning and other punishment based techniques in behavioural control of individuals with "challenging behaviour". The fact that there is evidence supporting the efficiency of these techniques cannot and does not make them right.

More generally, ethical concerns arise out of the implementation of randomised control trials. While such trials have the potential to prevent the propagation of worthless treatments, for example insulin coma therapy, they raise major ethical issues in the context of subject selection, consent, randomisation and the continuing care of subjects once trials are complete. Randomised control trials require the clinician to act simultaneously as physician and research scientist. Patients are simultaneously invalids and research subjects. It is questionable if the physicians' moral responsibilities towards patients can be consistent with the recommendation that the patient should participate in a randomised control trial, principally because of this conflict of interest (Hellman & Hellman, 1991). It has been suggested that such trials may be recommended by the physician if clinicians are in a state of "therapeutic equipoise", that is they are genuinely in doubt about the value of different interventions (Lilford & Jackson, 1995). Such equipoise may be achieved in the case of treatments with moderate affects which might otherwise be obscured by bias and random effects. However, equipoise may not be achievable when interventions have great benefits and risks and then alternative clinical procedures to be investigated by other methods.

Is therapeutic equipoise applicable to the recommendation of psychoanalytic treatment? Interestingly, neither psychoanalysts nor the opponents of psychoanalytic treatment believe that this is the case.

Psychoanalytic clinicians are so firmly convinced of the appropriateness of 4 or 5 times a week treatment that they tend to consider it unethical to recommend less intensive alternatives. Sceptics, on the other hand, feel that the sacrifice demanded of the patient and his/her family is such that randomisation to a psychoanalytic arm is normally ethically unacceptable. In principle, the existence of these opposing views might somehow be combined to construct an attitude of therapeutic equipoise, but in reality it is simply tantamount to what may be an insurmountable obstacle facing a randomised controlled trial of psychoanalysis.

## **The status of concerns about evidence based medicine**

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Many other concerns could be raised about the appropriateness of subjecting psychoanalysis to outcome evaluation. We raise some concerns here in part to demonstrate our awareness of the issues and in part to underscore that the clamour for evidence should be met with caution and sophistication. It needs to be recognised that objections to research will not win the day. It is unlikely that the prevailing view which places controlled studies at the top of the hierarchy of evidence will change no matter what the pressures of arguments. The complexities of issues surrounding resource allocation, the drive to seek certainty and simplicity at the level of policy making are such that alternative formulations will not be heard.

Psychoanalysis is not alone among medical treatments with a weak evidence base. Evidence to the standards required is available for relatively few medical interventions (Kerridge, Lowe, & Henry, 1998). The drive for an evidence base for the selection of treatment interventions will inevitably mean a biased allocation of resources to those treatments for which rigorous evidence of effectiveness is relatively easily collected or where funds are independently available to carry out more lengthy and complex effectiveness research. Brief therapy benefits from the former, pharmacotherapy from the latter. Psychoanalysis is further disadvantaged by the opposition to many of its fundamental propositions among fellow mental health professionals and influential leaders (Crews, 1995; Grünbaum, 1984; 1986; Webster, 1995). These kinds of considerations drive us to override our concern and accept the imperfect solution of outcome research with the overriding objective of preserving the discipline.

The best strategy available to us is to collect all the data available rather than enter an epistemological debate amongst ourselves. The debate is inaudible to those outside the discipline. Further, it would sap our energies when this is required for a collaborative effort to make the best case possible for psychoanalysis as a clinical method. Even those of us who are engaged in collecting evidence for the effectiveness of this discipline have major methodological as well as epistemological concerns. These should not be set aside, forgotten about, but nor should they become an alternative focus.

It should be remembered that the debate over the effectiveness of psychoanalysis is one of pragmatics not of principles. There is a clear danger that the therapy that is “without substantial evidence” will be thought by all to be “without substantial value” (Evidence Based Care Resource Group, 1994). Once this idea is allowed to flourish, a cultural change becomes inevitable, a change which at least temporarily has the power to stop the development of our discipline – through the rejection of psychoanalysis as the therapeutic choice, through discouraging young people from entering the profession and through bringing psychoanalytic contributions to mental health disciplines and other subjects into disrepute.



# Section E: Methodological considerations in evaluating the outcome of psychoanalysis

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## Methodological problems inherent to evaluation research

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Research into psychoanalysis is inevitably a compromise between usual clinical procedures and the demands of scientific influence. Clear thinking about the applicability of research findings rests on an understanding of the nature of these compromises. In this section we shall briefly list some of the issues which must be taken into consideration in interpreting and evaluating evidence for the effectiveness of psychoanalysis. While these issues are well known and obvious to some, they may be less familiar to others. More important, we list them here in part to show that researchers are well aware of these problems and while not necessarily able to resolve the issues, at least it should be clear that they are working towards this end.

### Efficacy versus effectiveness

The term efficacy refers to the results a treatment achieves in the setting of a research trial, while clinical effectiveness is the outcome of therapy in routine practice. The discrepancy arises because trials are required to show “internal validity” (Cooke & Campbell, 1979); that is, they permit causal inferences to be made on the basis of the observed relationship between the variables. In this context, the absence of a relationship must imply the absence of a cause.

Achieving internal validity normally requires modifications to clinical procedures, which are rarely seen in everyday practice. The most common of these are: (a) the selection of diagnostically homogenous patient groups, (b) the randomisation of these patients into treatments, (c) the employment of extensive monitoring of the patient’s progress, (d) the careful specification of therapeutic procedures to be used and (e) the monitoring of their implementation. These requirements clearly pose a threat to “external validity”, to the extent to which the inferred causal relationship between variables may be generalised. Thus demonstrations of efficacy are not necessarily demonstrations of effectiveness. The fact that a treatment is highly efficacious under strictly controlled conditions cannot be thought to mean that it will have the same value in the context of ordinary clinical practice.

This problem is by no means unique to the investigation of psychodynamic treatment. To take a simple example, a pharmacological agent with distinctly unpleasant but harmless side effects may be shown to have considerable efficacy in a double blind controlled trial. No one would be surprised that it proves to be ineffective in clinical practice since patients frequently and conveniently “forget” to take this pill. In the trial, serum levels were carefully monitored and subjects whose blood levels indicated that they did not take their drug were excluded from the analysis. The same applies in trials of psychological treatment. Frequently psychotherapy is not delivered in practice as well as it is in the context of a carefully monitored trial. By contrast trials may underestimate the effects of a therapy by randomly assigning patients to treatments they do not wish to have, whereas in clinical practice their preference would be carefully noted by their treating physician.

### Spontaneous remission

As relatively few of the individuals who suffer from significant psychiatric morbidity have the benefit of any kind of professional help, it must be obvious that there are many roots to recovery which do not involve psychoanalysis, psychotherapy or indeed any kind of systematic intervention. What any treatment needs to demonstrate therefore, is that it is more effective than the natural processes of healing which human society provides (note for example Freud’s famous comments about the therapeutic potential of Lourdes (Freud, 1933)). From a historical point of view, Hans Eysenck (1952) was the first to raise this issue in connection with psychoanalytic therapy. He claimed, on the basis of insurance statistics as well as Fenichel’s Berlin I Study of the outcomes of the Berlin

Psychoanalytic Institute, that more individuals recovered in a two year period when they were untreated than when they were treated in psychoanalysis. More recently, it was demonstrated that even using Eysenck's data a more sophisticated analysis reveals that whereas half of treated patients improved within a couple of months, only 2% of those untreated improved over the same time period (McNeilly & Howard, 1991).

Whatever the status of Eysenck's own figures, there is no doubt that spontaneous improvement rates are sizeable for most psychological disorders (Bergin, 1971; Lambert, 1976; Subotnik, 1975). For example, from naturalistic follow up studies we know that individuals with borderline personality disorder tend to "burn out" in middle age (Stone, 1990). Thus statements about the effectiveness of psychoanalysis cannot be made on the basis of clinical reports of individual cases, however successful – certainly not without unequivocal knowledge about the course of the disorder. Ideally the course of untreated individuals should be compared with those who receive treatment. It is impractical and unethical to withhold treatment from an individual for the duration of a longterm treatment such as psychoanalysis and this has posed major problems for those intending to carry out outcome studies. As psychoanalysis is not generally available it seems sensible to compare its effectiveness with either the best available alternative treatment or so-called "treatment as usual". The former has the advantage of offering an apparently meaningful comparison from the point of view of a referrer or referring agency, but equally has the potential of prompting meaningless comparisons where the aims of treatment are not comparable and apples are being compared with oranges. Such comparisons also require that the researcher has comparable expertise with both the methods of treatment, as well as large sample sizes as the difference between the two methods is likely to be small. The alternative contrast with a treatment as usual group, has the advantage of telling us how much difference a treatment might make were it to be added to routine care but has the disadvantage of potentially great heterogeneity in the control group and inadequate information concerning the treatment received by the control group (Roth & Fonagy, 1996).

## **Strategies of psychotherapy research**

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The choice of a particular research methodology will always be a compromise, reflecting the intentions, interests (and resources) of investigators. Some of the major strategies used in psychoanalytic research, together with their strengths and weaknesses, will be considered in turn. A full account of these issues in psychotherapy research is given in Kazdin (1994).

### **Single case studies**

The belief that knowledge based on groups of individuals is somehow more likely to be generalisable – that is, applicable beyond the specific locus of its discovery – than is the case for knowledge based upon individual cases, is fatally flawed (Fonagy & Moran, 1993). In single case designs the focus is on the individual patient rather than a group average, even where a group of patients were studied. Single-case studies may be descriptive or quantitative. The former group is well represented in the traditional psychoanalytic case history. The method has many strengths, including high communicative value, and the richness of description of particularly complex unconscious interactive processes between analyst and patient. There is no generally accepted format for these reports and the information included tends to be quite variable (e.g. Spence, 1994) which undermines generalisation. Attempts have been made to systematise such qualitative reports (e.g. Klumpner & Frank, 1991) but these have not met with general approval.

In comparison to descriptive accounts of single treatments, quantitative reports undoubtedly lack richness and depth but are more generally accepted because of the greater ease with which the reliability of the observation can be assessed. Within this latter group some are naturalistic reports of outcome or quasi-experiments (Cooke & Campbell, 1979), while others are reports of the experimental manipulation of interventions. In cases where appropriate baseline measures are taken, or where treatments are applied and withdrawn in a controlled manner, the patient acts as his/her own control. This methodology has been widely used by behavioural and cognitive-behavioural researchers (Morley, 1987; 1989), but is equally applicable to psychodynamic investigators (e.g. Fonagy & Moran, 1993) and to the investigation of process factors in therapy (e.g. Parry, 1986).

Single-case studies have a number of attractive features. They can be combined with the routine clinical practice of private practitioners, they do not (necessarily) require the research apparatus and personnel normally associated with group based research and can be conducted fairly quickly. While of great importance in the demonstration or refinement of clinical technique and especially in treatment innovation, the results of single case studies can be difficult to generalise to the broader clinical population (indeed the design is not intended for such a purpose). Patients are often highly selected (necessarily so where studies are aiming to show the effectiveness of a technique for particular clients). More fundamentally, however, interpretation of results is limited by the fact that (as will become evident in the body of this report) therapeutic interventions have both general and specific impacts on the welfare of patients. A contrast intervention is required in order to be clear that any demonstrated benefits are attributable to specific therapeutic techniques – a strategy adopted in the randomised control trial.

## **Randomised Controlled Trials (RCTs)**

In contrast to the single case study, RCTs explicitly ask questions about the comparative benefits of two or more treatments. Patients are randomly allocated to different treatment conditions, usually with some attempt to control for (or at least examine) factors such as demographic variables, symptom severity and levels of functioning. Attempts are made to implement therapies under conditions which reduce the influence of variables likely to influence outcome – for example by standardising factors such as therapist experience and ability, and the length of treatments. The design permits active treatments to be compared, or their effect contrasted with no treatment, a waiting list or a “placebo” intervention. Increasingly, studies also ensure that treatments are carried-out in conformity with their theoretical description – for example, ensuring that psychoanalytic treatments do not include cognitive-behavioural or supportive elements. To this end many treatments have been “manualised” (a process which specifies the techniques of the therapy programmatically), and therapist adherence to technique is monitored as part of the trial. There are obviously major problems in the manualisation of psychoanalytic treatment (Clarkin, 1998) but some progress has already been made on this front (e.g. Clarkin et al., 1999; Fonagy, Edgumbe, Target, Moran, & Miller, in press; Kernberg et al., 1989; Luborsky, 1984).

Though this design has the potential to distinguish the impact of treatments (and to provide a control for the effects of spontaneous remission), there are inherent limitations to this approach.

### *Problems of control groups*

Although the ideal design of a treatment would be to contrast treatment to no-treatment, it is rarely the case that this is either ethically or practically possible. The alternative of offering a placebo treatment – one which is considered inactive, at least from the point of view of the active treatments offered – is beset by the difficulty of finding an activity which could be guaranteed to have no therapeutic element, which controls for the effect of attention and which is also viewed by patients as being as credible as a psychiatric intervention. Many recent studies restrict themselves to the comparison of active treatments; as evidence has accumulated for the general efficacy of therapy, institutional review boards (ethical committees) have become unwilling to sanction trials which could be seen to deprive patients of help (e.g. see Elkin, 1994).

### *Length of therapy*

Setting up an RCT is a major undertaking, and consequently a great expense. Although there are exceptions, most trials limit the amount of intervention offered (frequently to around 16 weeks). While this may be appropriate for some therapies (principally behavioural or cognitive-behavioural approaches), psychodynamic therapists (e.g. Fonagy & Higgitt, 1989) could – and do – argue that the techniques they employ were never designed for delivery over such a short time-frame. Psychoanalysis is in most countries an open-ended treatment and it is hard to imagine forcing it into a frame where the number of sessions is determined independently of the individual treatment process.

### *Generalisability*

Few RCTs achieve the implementation of psychological therapies under conditions which might be obtained in routine practice. As noted above, because they are characterised by a concern to maintain internal validity, their applicability could be seen as limited. For example:

patients will have been selected to conform to diagnostically precise categories

patients will have been exposed to multiple assessments

therapies will be applied with some precision, often under supervision

researchers will often be particularly enthusiastic and particularly expert in the techniques they employ.

### *Patient preference and random allocation to treatment*

Patients are not passive recipients of treatment, and their preferences for differing forms of treatment may be critical to their participation in clinical trials (Brewin & Bradley, 1989). The bias introduced by consequent attrition from treatment is invisible within studies, but may be particularly relevant to clinical practice.

## **Open trials**

This methodology is intermediate between the single-case design and the randomised control trial. Although entry to treatment may be governed by strict criteria, there is no control group. Such designs often reflect a more naturalistic treatment protocol than is the case with RCTs. At the simplest level such studies offer important information concerning:

the likely benefit the average patient might derive from the treatment

what features of presentation are likely to be associated with relatively good outcome

how effective a particular service is in terms of outcome

which aspects of a patient's problems are likely to be addressed by a treatment

given a certain natural variability in treatment delivery, what aspects of treatment are associated with felicitous consequences and which are accompanied by equivocal outcomes.

Frequently two or more treatments for the same disorder, as practised in different settings, are contrasted. In principle, such a design could answer the question "what kind of patient benefits most from particular treatment protocols". In reality differences in case-mix and the failure to control specific components of treatment usually place drastic limitations on the implications which may be drawn from such studies. Given a sufficiently large data-set, it may be possible to derive conclusions about the relative value of treatments even in the absence of random assignment. However, studies on such a large scale are rarely possible.

## **Resolving conflicts between internal and external validity in research designs**

We have already noted that a major problem for outcome studies of psychoanalysis is the tension between satisfying the demands of internal and external validity when developing research strategies. Designs have to reach a compromise between these factors; bridging the gap between them requires innovative attempts at integrating an apparent incompatibility between scientific rigour on the one hand and generalisability on the other. Single-case designs may come to play a more important role in this respect, since external validity is not an inherent problem in designs of this type (Kazdin, 1994). When replicated across randomly sampled cases, they have considerable generalisability. They can be employed to answer most of the questions that concern researchers, such as the appropriateness of a particular form of treatment, the length of treatment required to achieve a good outcome, the relative

impact of treatment on particular aspects of the problem or the relevance of particular components of treatment. However, there is one critical exception: within this research strategy patient and analyst factors are difficult to study. If there is no replication across subjects (patients and analysts), the design will not yield information about their influence on outcome.

Thus methodology which is truly adequate to the task of simultaneously assuring internal and external validity in psychoanalytic research has probably yet to be developed. In the meantime, the best – though possibly inadequate – answer lies in reviews (such as the present one), which include critical appraisal of likely threats to external validity posed by current research.

## **Other considerations**

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### **Follow-up**

For most conditions the success of therapy may be measured by its ability both to improve patient functioning and to maintain that improvement after therapy ends. Although most trials report follow-up data, the length of follow-up can vary markedly between studies, sometimes being only a matter of weeks, sometimes years. The length of follow-up required to demonstrate a clinical effect is governed by the natural history of a disorder, which will suggest both the probability of relapse and the usual length of time between episodes. Therapeutic efficacy can only be demonstrated in the context of both factors and, for example, three month follow-up for a condition known to show greatest relapse over a period of one year would clearly be inadequate. This aspect of research design is particularly important for psychoanalytic investigations where so called “sleeper effects” have been frequently reported (e.g. Kolvin et al., 1981). The term refers to improvements observed after the termination of treatment. Termination is a complex time in psychoanalytic treatment with recurrence of the original complaints commonly reported.

Although this suggests that extended follow-up periods should be the norm, the longer a patient is followed-up the more difficult it is to ascribe change to their original treatment. In part this is because patients will might seek further treatment in the intervening period (e.g. Shea et al., 1992), and also because the relative impact of treatment in the context of life-experiences decreases over time. Ironically, the results of very prolonged follow-up, while desirable, may be difficult to interpret.

Finally, the stability of symptomatic change over the follow-up period may be an issue of concern in its own right. Monitoring of individual patients suggests that a proportion will change their symptom status more than once (e.g. Brown & Kulik, 1977; Shapiro et al., 1995). Reporting of group-averages tends to obscure this variability, leading to an over-estimation of longer-term outcomes in clinical practice.

### **Attrition**

All clinical trials will lose patients at various points in treatment; the point at which they are lost will have differing impacts on validity. Early loss from a trial may disrupt the randomisation of treatment, threatening internal validity. Even where there is no differential attrition from treatments, it may be the case that significant attrition could lead to results being applicable only to a sub-group of persistent patients, threatening external validity. Alternatively, attrition rates across treatment conditions may not be random, and may reflect the acceptability of therapies, suggesting that attrition may be an important variable in its own right.

Significant levels of attrition will restrict the conclusions that can be drawn from a study, and complicate reporting of results. A number of statistical solutions to this problem are available to researchers which utilise the last available data-point to estimate the likely bias introduced by loss of patients (e.g. Flick, 1988; Little & Rubin, 1987). Alternatively data can be reported on the basis of an “intention-to-treat” sample, including all subjects entered into the trial, as well as presenting separate data for those completing all or a specified length of therapy (e.g. Elkin et al., 1989).

## Meta-analysis

In the past 15-20 years, techniques have been developed to enable quantitative review of psychotherapy studies. Meta-analysis is a procedure which enables data from separate studies to be considered collectively through the calculation of an effect size from each investigation (Rosenthal, 1991).

Effect sizes are calculated according to the formula:

$$ES = \frac{M_1 - M_2}{S.D.}$$

where

$M_1$  = the mean of the treatment group

$M_2$  = the mean of the control group

S.D. = the pooled standard deviation

The terms  $M_1$  and  $M_2$  can stand for the means of any two groups of interest, such as psychotherapy contrasted against a waiting list control, or equally could be the comparison of two forms of psychotherapy. Because this technique converts outcome measures to a common metric, individual effect-sizes can be pooled. In addition to examining the contribution of main effects such as therapy modality, effect-sizes for any variable of interest can be calculated, such as the impact of methodological quality or investigator allegiance on reported outcomes (e.g. Robinson, Berman, & Neimeyer, 1990; Smith, Glass, & Miller, 1980).

Effect sizes refer to group differences in standard deviation units on the normal distribution. Their intuitive meaning is made clearer by translating them into percentiles, indicating the degree to which the average treated client is better off than control patients. Thus an effect size of 1.0 corresponds to a result where 84% of the treated group are better off than the average control patient.

Meta-analysis is a powerful research tool, but some have been critical of the technique (e.g. Wilson & Rachman, 1983). Common criticisms include:

the fact that reviews do not include single-case studies

the inclusion of studies of questionable methodological adequacy

the inclusion of studies not directly relevant to clinical issues, such as analogue studies, and trials of patients whose symptoms are not clinically significant or of great severity

the fact that analyses can multiply sample measures taken from the same patient and from the same study leads to effect sizes computed on the basis of dependent data

the fact that using average Z scores assumes that outcome measures are appropriately measured on an interval scale, and that their distribution may be assumed to have insignificant skewness and kurtosis

sampling of studies will be biased by the tendency for editors and authors to favour positive results

not all meta-analyses weight the means for sample size.

A major difficulty is, however, that the effect size statistic can only speak to treatment effects for the average client, and though this is informative of general treatment effects, further elaboration of therapeutic impacts is usually required to detail the more specific effects of treatment.

## **Problems associated with the use of statistical tests in psychotherapy research**

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### **Clinical and statistical significance:**

Much of this report is based on journal articles examining the truth of the null-hypothesis – in essence the proposition that psychoanalysis has no effect, or no effect greater than a control treatment. It is conventional to report the statistical significance of differences between treatments in terms of a confidence level of  $p < 0.05$  or  $< 0.01$ . However, researchers may be able to reject the null-hypothesis at relatively high levels of statistical significance without simultaneously demonstrating that this finding is worthy of clinical attention (Kukla, 1989). Demonstration of statistical effects may not be equivalent to a clinically significant therapeutic change, and there are a number of strategies which have been used to detect this (discussed further in Kazdin, 1994):

Comparison of patient change with normative samples

Measurement of the extent of individual change by reference to a criterion measure of change; for example, that treated clients should be 2 standard deviations from the mean of the untreated group (Jacobson & Truax, 1991)

The use of a criterion of recovery which enables categorical rather than continuous scoring of outcomes; for example, considering all individuals scoring as low as 75% of the normal population to have benefited from the treatment (e.g. Elkin et al., 1989).

The clinical significance of change is central to the evaluation of psychotherapy outcomes; though recent investigations are more likely to report data in this form, such measures are not always available.

### **Multiple data sampling and Type-I error**

Researchers frequently report numerous results of statistical significance without being clear how each test relates to the prediction they are examining. Dar and colleagues (Dar, Serlin, & Omer, 1994) illustrate this problem by suggesting a hypothetical study in which two treatments for flying phobias are contrasted, with levels of anxiety and coping skills being the dependent variables. In practice there may be a number of procedures for measuring these variables, all of which are likely to be intercorrelated. Each of these variables could be examined separately, though in reality there are only two hypotheses under investigation – the impact of the treatment on anxiety and its effect on coping skills. More than two statistical analyses are therefore redundant, and represent an overstatement of the data available to the researchers. A real-life example of this process is the much-cited National Institute of Mental Health study of treatments for depression (Elkin, 1994) which shows statistical significance on only some of a relatively large family of variables pertaining to dysfunctional emotional states. A consequence of multiply-sampling related data-sets is to increase the risk of Type I errors – rejecting the null-hypothesis when that hypothesis is false (in practice, for example, claiming that one treatment works better than another when in reality both work equally well).

Because it is well recognised that a series of measures tapping similar domains may be inter-related, investigators often employ multivariate tests, which permit some understanding of relationships between dependent measures. Though this procedure overcomes some of the problems noted above, problems can arise where multivariate tests which indicate overall significance are then followed by univariate tests. Not only does this increase the risk of Type I error, but results can be difficult to interpret, once again because of possible relationships among variables under test.

### **Atheoretical analysis**

Dar et al (1994), in a review of the use of statistical tests in psychotherapy research from the 1960s to the 1980s, note a high level of inappropriate significance testing, which they attribute to the pragmatic concerns of psychotherapy researchers. The determination to find statistically significant associations

is seen by them as motivated by "a flight from theory into pragmatics". As psychotherapy research frequently has very little theoretical guidance leading to meaningful hypotheses and testable predictions, there has been an explosion of exploratory procedures, leading to a state of affairs where, even in the best journals, "much of the current use of statistical tests is flawed". Psychoanalytic outcomes research is sadly no exception to this trend and many of the studies included in this review have undoubtedly over-exploited their data.

## Statistical power

Statistical power is the extent to which an investigation is able to detect differences between samples when such differences exist in the population – in other words when there is a true difference between the groups under test. Power is a function of:

the criterion for statistical significance, or alpha level

sample size

effect size, or the magnitude of the difference that exists between the groups.

Statistical power in perhaps the majority of trials of psychoanalysis may be relatively weak, primarily because of low sample sizes (Kazdin, 1994). Cohen (1962) distinguished three levels of effect size (small=0.25, medium=0.50 and large=1.0), and evaluated the ability of published studies to detect such differences at the conventional alpha level of  $p < 0.05$ . Power within these studies was generally low – for example, studies had a one in five chance of detecting small effect sizes, and less than a one in two chance of detecting medium effect sizes. Despite the cautionary note struck by Cohen's paper, and the date of its publication, Dar and colleagues (1994) found that a significant proportion of even recent research continues to neglect these issues. Most particularly, there continues to be a neglect of measures of effect size in favour of citing statistical significance. The problems inherent in this procedure can be readily illustrated by considering a study with a large sample but a small effect size; although statistical significance may well be achieved this does not speak to the magnitude of the effect, nor its likely reliability or validity. In psychoanalytic studies the reverse scenario is often more likely: too few subjects being compared reducing the likelihood of the demonstration of significant changes, even when such changes are present.

It should be clear that all of the above issues threaten the external validity of psychoanalytic research. Dar et al. (1994) detail a number of strategies for ensuring that such threats are minimised; for example, by employing theory-guided predictions, planned rather than post-hoc statistical decisions, reduced use of omnibus multivariate techniques, stricter control of type-I error rates by using single rather than multiple tests, employing "families" rather than a multiplicity of hypotheses, the avoidance of step-wise statistical procedures and testing of hypotheses not against a difference of zero but rather against a predetermined interval. While these suggestions are well taken, the opportunities for psychoanalytic research are at the moment so few that many of these methodological niceties will have to remain on a "wish list", awaiting implementation by studies currently underway.



# Section F: Psychoanalytic Assessment Instruments

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## Introduction

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One of the major difficulties facing psychodynamic psychotherapy researchers is the relative lack of developed instruments to assess both the characteristics of patients in terms of their psychodynamic difficulties and to monitor change from a psychodynamic point of view which is beyond behavior and symptom change. No study of psychotherapy process and/or outcome is better than the instrumentation that has been utilized.

There have been several recent reviews of instruments that can be used to characterize patients and their outcome in psychotherapy research (Strupp, Horowitz, & Lambert, 1997) and a large compendium of useful instruments (American Psychiatric Association, 2000). In contrast to the instruments described in those volumes, we focus here on a growing number of instruments that measure constructs relevant to psychodynamic thinkers. With the growing interest in research into the psychodynamic process and outcome, researchers have been forced to construct appropriate instruments for this use. We highlight some of these newly developed instruments here for multiple reasons. First of all it is useful to inform researchers of the existence of these instruments and how they can be obtained. Secondly, in order for an impressive, coherent body of psychotherapy research data to develop, the use of the same key instruments across studies, studies which are often costly and time consuming, will enable some comparison of results.

Instruments reviewed here cover concepts and constructs related to patient variables, various ratings of psychotherapy process, and ratings of facial movements. The patient variables of interest and measurement include psychological capacities, the quality of object relations, and attachment style. Finally, the ratings of psychotherapy process include an analyst assessment of the process, a rating of facial expression, a rating of patient referential process and a rating of the patient-therapist interaction.

## Measurement techniques

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### Requirements of measurement

There is some consensus in psychotherapy research (Kazdin, 1994) that single measures of outcome are unsatisfactory, that measures should be unreactive to experimenter demand and that they should be drawn from:

differing perspectives (such as the patient, close relatives or friends of the patient, the therapist or independent observers)

differing symptom domains (such as affect, cognition and behaviour)

differing domains of functioning (such as work, social and marital functioning).

One relatively comprehensive approach, which has been implemented at the Menninger Clinic is the Functional Analysis of Care Environments (FACE) (1999; Clifford, 1999). In child psychotherapy Fonagy (1997a) recommended that at least the following domains should be monitored:

psychiatric symptom measures and diagnostic criteria

adaptation to developmental and social demands

transactional aspects such as family relationships and the measurement of the impact of the child's pathology on the functions of the family and its individual members

mechanisms underlying the child's symptoms and adaptational problems either at the physiological or psychological level (e.g. affect regulation on attachment representations)

service satisfaction and alternative service usage.

There is however little consensus on the precise measures to be employed. This leads to some difficulty in comparisons between studies and, on occasion, to problems of interpretation within trials where measures assumed to converge on similar target areas give discrepant results.

For many analysts this reduction of outcomes to a series of scores is unsatisfactory because it clearly fails to capture the complexity of their work. There is undoubtedly merit to this objection, since the majority of current measures do not address the subtleties of individual presentations or the significance of particular changes to particular patients. There is no agreed set of capacities and attributes which an analyst would argue might reflect change across a group of patients. Many psychoanalytic clinicians are impressed by the way in which, in some patients, therapy promotes the unfolding of developmental processes, step-by-step, in an ordered and progressive way. This would suggest a developmentally rooted measure of some kind. However, the appropriateness of the developmental metaphor is by no means universally accepted by psychoanalysts (Mayes & Spence, 1994). In addition, psychoanalysts have noted that a focus on symptomatic change is inappropriate where personality change – which may be hard to measure – is the object of therapy. Techniques considered to measure this dimension have been developed (e.g. Malan & Osimo, 1992; Malan, 1976; Wallerstein, 1988) although the degree to which they are truly independent of symptomatic change is less clear (Mintz, 1981). The eschewal of existing reliable and valid measures by the psychoanalytic community is a regrettable fact, which will only be corrected by a concerted effort on the part of psychodynamic therapists to identify, in a consensual and measurable way, the outcomes which treatment aims to bring about, and to validate these against criteria that other stakeholders (such as patients, funders and other practitioners) see as important.

There are general objections to the quantification of therapeutic outcome. The uncritical use of quantification is pervasive in social science (Frosch, 1997). Quantification may be thought to inappropriately “fix” meanings where these are variable and renegotiable in relation to the context in which they are applied. The uniqueness of particular human experiences is denied if we obliterate internally structured subjectivities by externally imposed “objective” systems of meanings. It may be argued that the complexity and variability of human meanings is lost if we assume (through the use of quantification) a universal “true” meaning of human behaviour and experience. Psychological data (whether quantitative or qualitative) derived from humans requires interpretation as inevitably they are based on interactive, discursive processes. Undoubtedly, claims to a full knowledge of an objective, fixed reality are specious. This, however, is not a critique of quantification but rather its inappropriate reification, a problem that has already been touched on.

Finally, there may be legitimate concern that some measurement techniques may tap domains of change close to those targeted by a particular therapy, and may therefore indicate greater degrees of change than would be found using broader assessments. For example, the Beck Depression Inventory (BDI) assesses the level of depression largely through more cognitive representations of this disorder. In contrast the Hamilton Rating Scale for Depression (HRSD) has more of a focus on biological symptoms. It has been argued that trials of cognitive therapy could achieve better outcomes using the BDI, and trials of medication better outcomes using the HRSD, reflecting less the “true” outcome than the bias of scoring instruments. A similar argument might be made if psychoanalysts choose outcome measures too closely linked to the progress of therapy (e.g., a measure of transference or of analytic process). On the other hand, measures need to be relevant to the goals of a therapy – the problem is that the aims of psychoanalytic treatment remain controversial (Sandler & Dreher, 1997).

## **Developments in measurement**

There have been major developments in psychoanalytic measurement techniques. Some assessment instruments that have been used in the studies reviewed in this volume are described in detail below. Certain process measures are covered in detail in the Appendix to this review.

## Scales of Psychological Capacities (SPC)

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- DeWitt, K.N., Hartley, D., Rosenberg, S.E., Zilberg, N.J., & Wallerstein, R.S. (1991). Scales of psychological capacities: Development of an assessment approach. *Psychoanalysis & Contemporary Thought*, 14, 3-361.
- DeWitt, K.N., Milbrath, C., & Wallerstein, R.S. (1999). Scales of Psychological Capacities: Support for a Measure of Structural Change. *Psychoanalysis & Contemporary Thought*, 22, 453-480.
- Huber, D. & Klug, G. (2001). *Measuring Structural Change with the Scales of Psychological Capacities (SPC)--Further Psychometric Studies*. Presented at IPA Research Training Program, London, March 2001, and at IPA Congress, Nice, July 2001.
- Huber, D., Klug, G., & von Rad, M. (2001). Die Münchner Psychotherapiestudie. Ein Vergleich zwischen Psychoanalysen und psychodynamischen Psychotherapien unter besonderer Berücksichtigung therapiespezifischer Ergebnisse. In U. Stuhr, M. Leuzinger-Bohleber, & M. Beutel (Eds.), *Psychoanalytische Langzeittherapien*. Stuttgart: Kohlhammer.
- Morin, M. (2000). Une Methode Americaine d'Evaluation des Psychotherapies. *Psychiatrie Francaise*, 4, 92, 67-75.
- Sundin, E., Armelius, B-A., & Nilsson, T. (1994). Reliability Studies of Scales of Psychological Capacities: A New Method to Assess Psychological Change. *Psychoanalysis & Contemporary Thought*, 17, 591-615.
- Wallerstein, R. S. (1991). Assessment of Structural Change in Psychoanalytic Therapy and Research. In: Shapiro, T. (Ed.), *The Concept of Structure in Psychoanalysis*. Madison: International Universities Press.
- Wallerstein, R.S. (1988). Assessment of Structural Change in Psychoanalytic Theory and Research. *Journal of the American Psychoanalytic Association*, 36 (suppl), 241-261.
- Wallerstein, R.S. (1994). Psychotherapy Research and its Implications for a Theory of Therapeutic Change: A Forty-Year Overview. *Psychoanalytic Study of the Child*, 49:120-141.
- Zilberg, N.J., Wallerstein, R.S., DeWitt, K.N., Hartley, D., & Rosenberg, S.E. (1991). A Conceptual Analysis and Strategy for Assessing Structural Change. *Psychoanalysis & Contemporary Thought*, 14, 317-342.

## Aim

The aim of the Scales of Psychological Capacities (SPC) is to create a metric for one of the central tenets of ego psychology, and a central concept for the understanding of therapeutic change, namely structural change in the ego. The problem in creating such a metric resides in the theoretical diversity of psychoanalysis, with each psychoanalytic theoretical perspective conceptualizing structure and structural change within a different conceptual and linguistic framework--and each theoretical perspective is itself experience-distant. SPC was created to be an experience-near set of *psychological capacities*, that comprehensively describe character and psychic functioning, and that if changes occur in the configuration of these capacities, adherents of all theoretical perspectives in psychoanalysis would accept that changed configuration as reflecting underlying structural change, however differently they would then describe that structural change theoretically.

## Description

There are a total of 17 described psychological capacities, with most of them (13) having two directions of deviation, and one with only one, making a total of 36 subscales being assessed. It is possible to deviate from the norm in both deviating directions of a scale simultaneously, like the person very inhibited in impulse and affect expression, who can nonetheless, when pushed, have an explosive eruption of affect. There are 3 described degrees of departure from the norm on each subscale, with half-way judgments allowed, thus making 7 possible scale placements, from each norm

to the severest point of duration. Each point on each subscale is anchored by one or more descriptive clinical vignettes describing the kind of psychological functioning indicated by that point. The SPC ratings are (usually) based on a videotaped clinical interview aimed to elicit a picture of overall psychic functioning, supplemented by a series of semi-structured probe questions designed to elicit more specific information about those areas of functioning not sufficiently illuminated in the prior unstructured clinical session.

## Practical Issues

The basis for the rating of the scales is a clinical interview followed by a semistructured SPC-interview with a set of probe questions, lasting one to two hours. The audio- or videotaped material will be scored for each subdimension on a 7-point scale from 0 for normal or fully adaptive functioning to 3 for functioning seriously and obviously disturbed, with half points in between. The rating procedure requires an extensive manual with a detailed description of each subdimension together with one or more clinical vignettes to anchor each scale point. The manual for the SPC plus the list of probe questions for the semi-structured part of the interview may be obtained from Robert S. Wallerstein, M.D. The official German translation (with a verified back-translation into English) may be obtained from Drs. Dorothea Huber and Gunther Klug in Munich, Germany. A Swedish translation may be obtained from Dr. Eva Sundin in Umea, Sweden. There also are Finnish, French and Italian translations.

## Psychometric Properties

Inter-rater reliability (DeWitt, Milbrath, & Wallerstein, 1999; Sundin et al., 1994), content validity (DeWitt, Hartley, Rosenberg, Zilberg, & Wallerstein, 1991), and convergent validity (DeWitt et al., 1999) of the SPC have already been examined in English. A discriminant and convergent construct validity study with 41 SPC interviews of depressed patients was performed in German in 1997. The SPC were compared with instruments that measure interpersonal functioning in order to evaluate convergent validity, whereas discriminant validity was evaluated by comparing the SPC with instruments measuring symptoms.

The SPC was found to be independent of current symptoms and a relevant correlation between interpersonal problems and psychic structure could be demonstrated. For another proof of *convergent validity* experienced clinicians described a hypothetical, prototypical profile of a depressive patient before they rated the SPC. Compared with the empirically found profile of the 36 subdimensions, their prediction was correct for all but one subdimension (Huber, Klug, & von Rad, 2001b).

A *interrater-reliability study* was conducted after a rater-training according to the formal method (Mercer & Loesch, 1979) with the Wallerstein group in San Francisco before three German raters were trained. The inter-rater reliability between the three raters was calculated by means of Intra Class Correlation Coefficient (ICC, Shrout & Fleiss, 1979) for all subdimensions separately. The mean ICC was 0.82 within a range from 0.54 to 0.89. Using as a standard cut-off score a correlation level of .70, according to the recommendations of Lambert & Hill (1994) only 4 of the 36 subdimensions had reliabilities less than .70. All of the 36 subdimensions reached Cohen's cut-off point of .50 (Cohen, 1988).

On that basis an extended *validity replication study* was performed with a homogenous group of 47 depressed patients. Discriminant validity was assessed by means of the Symptom-Check-List (SCL-90-R; Derogatis, 1983) and the Beck-Depression-Inventory (BDI; Beck, 1961), both widely used self-rating symptom inventories. The Symptom Severity Score (BSS; Schepank, 1995) and the Global Assessment of Functioning Scale (GAF, DSM-IV axis 5; American Psychiatric Association, 1994) are both observer-rating instruments.

For convergent validity of the SPC, the Inventory of Interpersonal Problems, (IIP; Horowitz, 1988), the Freiburg Personality Inventory, a personality questionnaire (FPI; Fahrenberg, Selg & Hampel, 1989), the Questionnaire for Coping Strategies (FKBS; Hentschel, 1998), and in addition to these self-rating questionnaires, the psychic structure of the patient rated with the Operationalized Psychodynamic Diagnostics, OPD, axis 4: Structure (Arbeitskreis OPD, 1998) were used.

There were medium range, significant correlations between the FPI scales, the IIP scales, the FKBS scales, the OPD rating for axis 4: structure, and the SPC subdimensions. The data clearly showed no significant correlation between neither the SCL-90-R scales, the BDI, the BSS nor the GAF, and the SPC.

As another test of construct validity the mean SPC profile operated in the theoretically expected way for depressed patients showing highest means for the subdimensions Self-Depreciation, Over-involvement in Relationship, Internalisation, Surrender of Self, and Pessimism.

Summing up the SPC can be regarded as a reliable instrument provided that a clinical interview plus a semi-structured interview with probe questions are administered to yield an extensive data-base, and a medium-range rater-training and regular recalibration-sessions are performed even with raters without thorough psychoanalytic training. There is substantial evidence that the SPC validly gauge psychic structure, and, provided that its sensitivity to change is proven, are probably a suitable instrument for psychoanalytic process-outcome research.

## **Clinical Utility**

The SPC were designed as measures of structural change consequent to psychoanalytic psychotherapy. They should be used at least at the onset and at the termination points of therapy. They can, of course, also be administered during the course of therapy (if appropriate and useful) and at follow-up points.

## Object Relations Inventory (ORI)

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Blatt, S. J. (1974). Levels of object representation in anaclitic and introjective depression. *Psychoanalytic Study of the Child*, 29, 107-157.

Blatt, S. J., Chevron, E. S., Quinlan, D. M., Schaffer, C. E., & Wein, S. J. (1988). *The assessment of qualitative and structural dimensions of object representations* (revised edition). Unpublished research manual, Yale University, New Haven, CT.

Blatt, S. J., Stayner, D. A., Auerbach, J. S., & Behrends, R. S. (1996). Change in object and self representations in long-term, intensive, inpatient treatment of seriously disturbed adolescents and young adults. *Psychiatry*, 59, 82-107.

Blatt, S. J., Wein, S. J., Chevron, E. S., & Quinlan, D. M. (1979). Parental representations and depression in normal young adults. *Journal of Abnormal Psychology*, 88, 388-397.

### Aim

The ORI is used an open-ended projective method based on an integration of psychoanalytic and cognitive-developmental theories and designed to operationalize and systematically assess the structural organization and thematic content of individual's self and object representations (Blatt, Bers, & Schaffer, 1992; Blatt, Chevron, Quinlan, Schaffer, & Wein, 1988; Blatt et al., 1979; Diamond, Blatt, Stayner, & Kaslow, 1993). An open-ended projective measure developed based on the premise that responses to ambiguous stimuli will be "shaped by the organizing characteristics of the individual's representational world" (Blatt & Lerner, 1983, p. 195).

### Description

The ORI is an open-ended projective measure in which subjects are asked to describe without interruption various individuals, most commonly their mother, father, themselves, a significant other, and for clinical subjects, their therapist. Blatt and his colleagues (Blatt et al., 1992; Blatt et al., 1988; Blatt et al., 1979; Diamond et al., 1993) have developed two main scales to assess the structural and thematic aspects of these narrative descriptions: (1) *Qualitative and Structural Dimensions of Parental Descriptions*; and (2) *Differentiation-Relatedness Scale of Self and Object Representations*.

*Qualitative and Structural Dimensions of Parental Descriptions* (Blatt et al., 1988). Descriptions of people are rated on 7-point scales for the following 12 traits or personal characteristics: Affectionate, ambitious, benevolent, constructively involved, intellectual, judgmental, nurturant, punitive, strong, successful, positive ideal, and warm.

Each description is also scored for the subject's degree of ambivalence about the person being described, length of narrative, the degree of articulation (the number of the 12 personal characteristics included in the description), and the conceptual complexity of the description. Conceptual complexity, derived from psychoanalytic and cognitive developmental concepts (Blatt, 1974) is rated using a 9-point ordinal continuum of increasing complexity from a *sensorimotor-preoperational* level where the parent is described primarily in terms of providing need gratification (scale point 1) through a *concrete-perceptual* (scale point 3) and *external and internal iconic* (scale points 5 and 7, respectively) to a *conceptual level* representation where the parent is described as a unique individual with an integration of external and internal characteristics and traits (scale point 9).

*Differentiation-Relatedness Scale of Self and Object Representations* (Diamond et al., 1993). Drawing from theoretical formulations and clinical observations about very early processes of boundary articulation (Blatt & Wild, 1976; Blatt, Wild, & Ritzler, 1975; Jacobson, 1964; Kernberg, 1975; 1976), processes of separation-individuation (Coonerty, 1986; Mahler et al., 1975), the formation of the sense of self (Stern, 1985), and the development of increasingly mature levels of interpersonal relatedness (1996; Blatt & Blass, 1990), Blatt and colleagues identified two fundamental dimensions of self and object representation: (a) the differentiation of self from other and (b) the establishment of increasingly mature levels of interpersonal relatedness. To assess the degree of differentiation and

relatedness in descriptions of self and significant others, Diamond and colleagues (1993) developed the Differentiation-Relatedness Scale, a 10-point scale on which to rate the following points: *a lack of basic differentiation between self and other* (Levels 1 and 2); *the use of mirroring* (Level 3), *self-other idealization or denigration* (Level 4), and *an oscillation between polarized negative and positive attributes* (Level 5) as maneuvers to consolidate and stabilize representations; *an emergent differentiated, constant, and integrated representation of self and other with increasing tolerance for ambiguity* (Levels 6 and 7); *representations of self and others as empathically interrelated* (Level 8); *representations of self and other in reciprocal and mutually facilitating interactions* (Level 9); and *reflectively constructed integrated representations of self and others in reciprocal and mutual relationships* (Level 10). In general, higher ratings of differentiation relatedness in descriptions of self and other are based on increased articulation and stabilization of interpersonal schemas and an increased appreciation of mutual and empathically attuned relatedness.

## Practical issues

The ORI can be administered to large groups of subjects as a self-report measure or used with individual subjects as an interview measure. When using the ORI as an interview it should be audio-taped and transcribed for coding. The ORI does not require any specialized training to administer as self-report measure or as an interview; however, the interview version is best administered by those with clinical experience, especially with clinical subjects. Coding the ORI requires training that can, in most cases, be obtained using the manuals and scoring for reliability before coding one's actual data set.

## Psychometric properties

*Qualitative and Structural Dimensions of Parental Descriptions* (Blatt et al., 1988). Parental descriptions have been scored reliably for both content and structural variables (Blatt et al., 1979; Bornstein, Galley, & Leone, 1986; Bornstein, Leone, & Galley, 1990; Levy, Blatt, & Shaver, 1998). These variables are stable over time (Bornstein et al., 1990) and are unrelated to intelligence, verbal productivity or socioeconomic status (Blatt et al., 1979; Bornstein et al., 1986; 1990; Wilson, 1982). Blatt and colleagues (Quinlan, Blatt, Chevron, & Wein, 1992) report a stable three factor structure for the ratings of these 12 characteristics which they labeled as benevolent, punitive, striving. The Benevolent factor comprises the attributes affectionate, benevolent, warm, constructive involvement, positive ideal, nurturant, successful, and strong. The Punitive factor includes the attributes judgmental, punitive, and ambivalent. The Striving factor includes the attributes ambitious and intellectual.

Previous research supports the construct and predictive validity of these measures of object representation. Conceptual complexity of descriptions of parents in non-clinical samples has been related to experiences of depression (Blatt et al., 1979), emotional awareness (Lane, Quinlan, Schwartz, Walker, & Zeitlin, 1990), negotiation strategies, attachment patterns (Levy et al., 1998), and self-reported acting out (Schultz & Selman, 1989). In clinical samples, psychotic and borderline patients gave less differentiated and less conceptually complex descriptions (Bornstein & O'Neill, 1992; Marziali & Oleniuk, 1990), and more negative representations of both parents, expressing significantly more ambivalence (Bornstein & O'Neill, 1992). Moreover, conceptual complexity is negatively related to degree of psychopathology (Global Assessment Scale--GAS scores), presence and severity of hallucinations, and the impairment index on the MMPI (Bornstein & O'Neill, 1992). Increases in conceptual complexity, and degree of articulation (the number of scorable attributes) were related to independent assessments of change in clinical functioning (GAS scores) in long-term treatment of seriously disturbed adolescents and young adult inpatients (Blatt, Stayner, Auerbach, & Behrends, 1996; Blatt, Wiseman, Prince-Gibson, & Gatt, 1990). Most importantly, predicted developmental differences were found between 5-6 year-olds and 9-10 year-olds (Priel et al., 1995). Thus, the content and structure of the representation of parents differ in clinical and non-clinical samples; they are related to independent assessments of level of psychopathology and clinical functioning in clinical samples and to aspects of general functioning in non-clinical samples; and follow a developmental model (see Fishler, Sperling, & Carr, 1990; Stricker & Healey, 1990).

*Differentiation-Relatedness Scale of Self and Object Representations* (Diamond et al., 1993). Initial validity studies for this scale in both clinical and non-clinical samples are encouraging (Blatt et al., 1996; Levy et al., 1998). The levels of differentiation and relatedness, particularly self representations, were significantly related to independent assessments of clinical functioning. Moreover, changes in scores of representations of mother, father, self, and therapist predicted therapeutic change over a two-year period (Blatt et al., 1996). The relationship between level of differentiation-relatedness of representations of self and other and levels of clinical functioning, as well as the degree of clinical change, were independent of socio-demographic (e.g., intelligence, age) and clinical variables (e.g., length of hospitalization, age of onset). In a non-clinical sample, the degree of differentiation-relatedness was significantly related to attachment patterns in theoretically congruent ways (Levy et al., 1998).

## **Clinical Utility**

In developing the ORI method and coding systems, Blatt and colleagues were attempting to advance our understanding of how the content and structure of mental representations are involved in normal personality development, psychopathology, and the assessment of therapeutic change. The assessment of the content and structure of mental representations can also provide a basis for differentiating among various forms of psychopathology (Blatt, Auerbach, & Levy, 1997; Blatt & Levy, 1998). Blatt and his colleagues have used this approach in case studies to provide new ways of understanding various forms of psychopathology like schizophrenia, borderline pathology, and depression (Auerbach & Blatt, 1996; Auerbach & Blatt, 1997; Blatt & Auerbach, 2001; Blatt, Wein, Chevron & Quinlan, 1979; Blatt, Stayner, Auerbach & Behrends, 1996; Diamond, Kaslow, Koonerty & Blatt, 1990; Diamond et al., 1999; Gruen & Blatt, 1990).



# Attachment Scanner

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Fonagy, P., Allen, J., Stein, H., Fultz, J., & Target, M. (submitted). Variations in adult attachment across attachment figures as assessed by Q-sort measure.

Allen, J.G., Huntoon, J., Fultz, J., Stein, H., Fonagy, P., & Evans, R.B. (in press). A model for brief assessment of attachment and its application to women in inpatient treatment for trauma-related psychiatric disorders. *Journal of Personality Assessment*, 76, 420-446.

## Aim

The measurement of adult attachment is a complex and controversial process. A number of questionnaire measures are currently available (Bartholomew & Horowitz, 1991; Collins & Read, 1990; Hazan & Shaver, 1987; Main & Goldwyn, 1994). A number of papers and chapters have reviewed the current status of adult attachment measures (Hesse, 1999; Stein, Jacobs, Ferguson, Allen, & Fonagy, 1998; submitted).

Assessing attachment in adulthood has been shaped by two disparate traditions: clinical, as represented by Main's Adult Attachment Interview (Main & Goldwyn, 1994), focusing on representational models of relationships with parents, and social-psychological, as represented by Hazan and Shaver's (1987) self-report approach, which spawned a spate of questionnaires assessing various facets of attachment in romantic relationships. The "Attachment Scanner" was designed to assess adults' attachments to a potentially wide range of attachment figures in adulthood, with particular attachment figures rated to be determined by the researcher. To ensure content validity, we selected items representing core attachment styles—secure, dismissing, and preoccupied—on the basis of an extensive study of expert consensus. Moreover, to ensure discriminant validity, attachment items were carefully matched with counterpart non-attachment items of comparable social desirability (i.e., secure attachment versus positive non-attachment; dismissing and preoccupied attachment versus negative non-attachment). These non-attachment items were also selected on the basis of expert consensus. Hence, for example, the measure can assess the extent to which secure attachment characterizes a particular relationship, controlling for global positive valence of the relationship. Thus the Attachment Scanner can measure both the extent and quality of attachment for any variety of relationships.

## Description

Questionnaires are vulnerable to bias wherein, for example, respondents may simply endorse desirable items at high levels regardless of content. Hence a Q-sort approach that maximizes discrimination between attachment and non-attachment aspects of relationships was chosen. The measure consists of 60 items divided among five scales: secure attachment, 20; positive non-attachment, 10; dismissing attachment, 10; preoccupied attachment, 10; and negative non-attachment, 10. For each defined target, respondents are instructed to sort each of the 60 items into 7 piles (distributed 3:6:12:18:12:6:3) with respect to the extent to which each item is true or untrue of the relationship. To date, data has been collected with respect to five targets: partner, best friend, mother, father, and acquaintance.

## Practical Issues

An initial group of participants used a magnetic board to complete sorts, and subsequent participants have done the sorts on a computer program developed by the researchers. The findings for the magnetic board and computer program were similar, and participants showed a slight preference for the computerized version (regardless of their familiarity with computers); hence the computer is now being used for all data collection.

Participants are able to complete the sorts after a brief demonstration followed by minimal instruction and supervision. The data are automatically tabulated. The computer program runs on Windows NT

and requires a 17" monitor. Each sort takes from 15-20 minutes, and the whole task usually takes about 1-1/2 hours.

## **Psychometric Properties**

Initial studies with a convenience community sample indicate that the Attachment Scanner shows adequate reliability (internal consistency and test-retest reliability over a two-week period), although there is some variability across scales and targets. The Attachment Scanner shows good convergent validity when compared with two self-report measures of attachment styles, the Adult Attachment Scale—Revised (Collins & Read, 1990) and the Relationship Questionnaire (Bartholomew & Horowitz, 1991), and it shows good discriminant validity with respect to different targets. The measure also shows promising construct validity in its relation to measures of psychiatric symptoms and satisfaction in dyadic relationships (e.g., secure attachment and positive non-attachment scores contribute independently to extent of satisfaction in close relationships).

## **Clinical Utility**

The Attachment Scanner is ideally suited to psychotherapy process and outcome research. For example, quality of attachment to various attachment figures might be employed to predict formation of a therapeutic alliance or to forecast treatment outcome. In addition, changes in quality of attachment across various attachment figures might be employed as an outcome measure. Furthermore, the psychotherapist or psychoanalyst could be rated as an attachment figure, and the extent and nature of the initial attachment and the changes in the attachment over the course of therapy could be contrasted with simultaneous assessments of other attachment figures.

# Patient Therapist Adult Attachment Interview (PT-AAI)

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Diamond, D., Clarkin, J., Levine, H., Levy, K., Foelsch, P., & Yeomans, F. (1999). Borderline conditions and attachment: A preliminary report. *Psychoanalytic Inquiry*, 19(5), 831-884.

## Aim

The PT-AAI is a semi-structured clinical interview, which has been adapted from the Berkeley Adult Attachment Interview (AAI; George, Kaplan & Main, 1985; 1996) in collaboration with Mary Main and Erik Hesse. The aim of the PT-AAI is to assess patients' and therapists' state of mind with respect to attachment in the therapeutic relationship. The PT-AAI is also designed to explore patients' and therapists' experience and representation of the therapeutic relationship, and their capacity to mentalize or reflect on that experience.

## Description

The PT-AAI follows the same format and order of questions, as does the AAI, with minor changes in the wording of questions to fit the context of the patient-therapist as opposed to parent-child relationship. The interview consists of 29 questions asked in set order, the first 17 of which parallel the questions on the AAI. Speakers are asked to describe their relationship with their patient/therapist generally and then to choose five words describe the relationship with the patient/therapist, supporting these descriptors with specific examples or incidents. Speakers are also asked what they did when upset, hurt or ill in the context of the therapeutic relationship. The interview also includes questions about the individual's response to separations from the patient/therapist, about times when the individual felt rejected by the patient/therapist, and about whether the individual has ever felt threatened by the patient/therapist in the course of the treatment. In addition, speakers are asked why they think the patient/ therapist acted the way he or she did in the course of treatment, and are asked to describe and evaluate the effects of psychotherapy. As is the case with the AAI, the technique has been described as having the effect of "surprising the unconscious" (George, Kaplan, & Main, 1985) in that it allows numerous opportunities for the interviewee to elaborate on, contradict, support or fail to support previous statements or generalizations.

## Practical Issues

The PT-AAI requires specialized training to administer and to score. The technique of administering and scoring the PT-AAI is parallel to that of administering and scoring the AAI. PT-AAI interviewers must be trained in the specific technique of administration by an individual who has taken the Adult Attachment Interview Training Institute certified by Mary Main or Erik Hesse; PT-AAI coders must have taken the AAI training institute and achieved reliability on an extensive set of AAI transcripts (30). The PT-AAI is transcribed verbatim for purposes of analysis, using the same transcription rules that apply to the AAI. An adult attachment classification of the patient and/or therapist may be derived from the PT-AAI using the five-way Adult Attachment Scoring and Classification System (Main & Goldwyn, 1998), which has been slightly modified to fit the context of the patient therapist relationship (Diamond, Clarkin, Stouvall, and Levy, 2001). The interviews are assigned to one of five primary classifications: Secure/Autonomous, Preoccupied, Dismissing, Unresolved, or Cannot Classify. These classifications are derived from three classes of subscale ratings which have been adapted to fit the context of the patient therapist relationship by Diamond, Clarkin, Strouvall, and Levy (2001). 1) Scales that are based on the rater's inferences about the individual's experience of the therapist/patient (e.g., the extent to which there was mutual liking in the patient therapist relationship, the extent to which patient/therapist was rejecting, neglecting, involving, or pressuring to achieve); 2) Scales that assess the individual's organized states of mind with regard to attachment information (e.g., coherence of transcript, idealization, insistence on lack of recall, active anger, lack of resolution of loss and trauma, and overall coherence of thought); 3) Scales that assess for Unresolved (disorganized/disoriented) states of mind (e.g. the extent to which the individuals are unresolved with regard to loss or trauma).

The PT-AAI, like the AAI, may be scored for reflective function by raters who receive training in applying the Reflective Function Scale (Fonagy, Steele, Steele, & Target, 1997), designed to assess the extent to which the speaker has the ability to think of others in mental state terms or to comprehend and conceptualize the mental processes such as feelings, beliefs, intentions, conflicts, motivations and other psychological states of self and others.

Coders for reflective function must receive training in the application of the Reflective Function Manual, Version 4.1 developed by Peter Fonagy and Mary Target (Fonagy et al., 1997).

## **Psychometric Properties**

Since the PT-AAI is an adaptation of the AAI, it is thought to have the similar psychometric properties to the AAI. The AAI has been subjected to stringent psychometric tests of its stability and discriminant validity which have been summarized in a number of articles including Hesse, 1999 ; Main, Kaplan and Cassidy, 1985 . In brief, the AAI has been shown to have a high level of test-retest stability (Main et al., 1985; Sagi et al., 1994), as well as stability (for three adult attachment categories tested over an 18 month to four year period (Ammaniti, Speranza, & Candelori, 1996; Crowell, Waters, Treboux, & O'Connor, 1996) including one study which indicates stability between a prebirth interview and the interviews conducted 11 months after the birth of the first child (Benoit & Parker, 1994). Reliability studies on the PT-AAI adaptation of the AAI subscales are currently in process, as are validity studies that assess the relationship between the PT-AAI attachment classification at one year with independent measures of symptomatology.

## **Clinical Utility**

In developing the PT-AAI the aim has been to advance our understanding of how attachment status might affect the quality and nature of the therapeutic relationship, including the formation and maintenance of the therapeutic alliance, and the transference-countertransference dynamics. Bowlby (1977) conceived of the therapeutic relationship at least in part as an attachment relationship, guided by the proclivity of humans throughout the life cycle to seek "proximity to some other differentiated and preferred individual...conceived as older or wiser" especially when the individual is "distressed, ill or afraid" (p.792). Further, like all attachment relationships, the therapeutic one was thought by Bowlby to be inherently bi-directional with attachment-seeking behaviors (proximity seeking, smiling, calling) tending to evoke corresponding adult attachment or caretaking behaviors (soothing, holding, protecting). The PT-AAI is designed to assess how the attachment behavioral system of both patient and therapist contribute to the configuration of therapeutic relationship. The PT-AAI may help to track the transference as it unfolds over the course of a long term therapy by providing a measure of state of mind with respect to the therapist which may be compared with the state of mind with respect to early attachment figures as assessed on the AAI.

## Reflective Functioning (RF) Scale

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Fonagy, P., & Target, M. (1997). Attachment and reflective function: Their role in self-organization. *Development and Psychopathology*, 9, 679-700.

Fonagy, P., Target, M., Steele, H., & Steele, M. (1998). *Reflective-Functioning Manual, version 5.0, for Application to Adult Attachment Interviews*. London: University College London.

### Aim

The term reflective function (RF) refers to the psychological processes underlying the capacity to mentalize. Mentalising refers to the capacity to perceive and understand oneself and others in terms of mental states (feelings, beliefs, intentions and desires). It also refers to the capacity to reason about one's own and others' behaviour in terms of mental states, i.e. reflection. Reflective functioning or mentalization is the active expression of this psychological capacity intimately related to the representation of the self (Fonagy & Target, 1995; 1996a; Target & Fonagy, 1996). RF involves both a self-reflective and an interpersonal component that ideally provides the individual with a well-developed capacity to distinguish inner from outer reality, pretend from 'real' modes of functioning, intra-personal mental and emotional processes from interpersonal communications. This formulation differs from most developmentalists in considering RF not to be a maturational cognitive capacity but rather a developmental achievement which is never fully acquired and is not consistently maintained across situations. It is important that RF is not conflated with introspection. Introspection or self reflection is quite different from RF as the latter is an automatic procedure, unconsciously invoked in interpreting human action. Procedural knowledge of minds in general, rather than declarative self knowledge, is the defining feature.

### Description

With the help of a manual, trained raters apply the RF scale to transcripts of the Adult Attachment Interview. The manual describes the range of possible scores that may be awarded, from -1 (negative RF) to 9 (exceptional RF). Codings are anchored at odd numbers: 1 signifies lacking in RF, 3 signifies questionable or low RF, 5 signifies ordinary RF, and 7 signifies marked RF. This is so that where a rater is confident that a transcript falls between 2 of the main categories, it may be assigned the corresponding even number. The manual gives detailed explanations and examples of what constitutes high and low RF, and describes with examples how to decide on the appropriate score. Codings are assigned to the different sections of the interview, and the transcript as a whole is then assigned a rating. The weight of ratings depends on the passage rated. Some passages are characterized, on the basis of the interview question as requiring a reflective response, and narratives that follow these questions are given a greater weighting. The rater has to consider the interview as a whole, alongside the ratings for individual passages. The rater should not take an arithmetic average of the ratings given to core passages or even to all passages in the transcript. Too little is known of the psychometric properties of the individual ratings to permit this simple expedient. The rater has to come to a judgment about the whole text on the basis of the manual and their training, and over time in relation to their experience of other narratives to which they have assigned ratings.

### Practical Issues

Raters must be trained by Professor Peter Fonagy, Dr. Mary Target, Dr. Miriam Steele or Dr. Howard Steele, or by persons who they have trained and found to be reliable. The manual describing the application of the scale is available on request from Professor Fonagy, Psychoanalysis Unit, Sub-Department of Clinical Health Psychology, UCL, Gower Street, London WC1 6BT, or email [p.fonagy@ucl.ac.uk](mailto:p.fonagy@ucl.ac.uk). The training process consists of a two day seminar with preparatory work and reliability takes about 4 weeks to reach. It takes approximately 2-3 hours to code an average AAI transcript.

## Psychometric Properties

The reliability of the measure was assessed with 100 AAI transcripts rated by 3 judges. The inter-rater agreements were high (.79-.89). On a sample of 200 subjects the RF rating was found to correlate moderately with IQ ( $r=.27-.33$ ) and slightly with education ( $r=.19-.35$ ). There were no correlations with either age or socioeconomic group. As part of establishing the discriminant validity of the Reflective Function Scale, the scale was related to a number of psychometric instruments. Epstein's Mother-Father-Peer Scale (Epstein, 1983) measures independence-encouraging versus over-protective and accepting versus rejecting mothers, fathers and peers, none of these scales related to RF scores (see Table 8). The Eysenck Personality Questionnaire (Eysenck & Eysenck, 1975), concurrently administered to the subjects, showed no relationships to extraversion, neuroticism or psychoticism. The Langner 22 (Langner, 1962), a screening measure for psychiatric caseness, also showed no correlation with RF ratings. The RF scale was also correlated with the Sources of Self Esteem Inventory (O'Brien, 1981). The RF Scale had no significant association with any of the 11 scales of the SOSE.

On the AAI the strongest relationship of the RF measure is with the Coherence scale (combination of coherence of mind and coherence of transcript). This association is expected to be .64-.74 in a low risk sample. There are a significant number of cases however where RF is high and coherence as measured according to Main & Goldwyn coding system is low. These individuals often had the harshest childhoods. There are also cases of low RF and high coherence, particularly in cases of relatively problem free backgrounds. Factor analytic studies of AAI scales showed RF to load with coherence of mind, subjective experience of good recall, lack of idealisation of mother, and lack passivity of thought. Overall, the psychometric analysis of the AAI, as reported in Steele (1991) revealed that ratings on RF were consistently the strongest contributors to judges' assessment of attachment security, and accounted for more than half of the variance in the secure/insecure distinction.

The validity of the RF scale was initially established in relation to the Strange Situation in a prospective study of parental predictors of infant security of attachment (Fonagy, Steele, Moran, Steele, & Higgitt, 1991). There was a strong relationship between scores on the RF scale and the Strange Situation behaviour of infants, whose mothers and fathers had been assessed using the AAI before the birth of the child (see Table 6). The point biserial correlation between secure classification in infancy and parental RF was highly significant ( $r=.51$  for mothers and  $.36$  for fathers,  $p<.001$  in each case). In a subsequent study on the same sample, it was found that RF was particularly predictive of secure attachments with mothers, in cases where mothers independently reported significant deprivation in childhood (Fonagy, Steele, Steele, Higgitt, & Target, 1994).

In a further study (Fonagy et al., 1996) 82 out of 85 consecutively admitted non-psychotic patient in treatment for severe personality disorder were matched on age, gender, socio-economic status and verbal IQ with 85 normal control participants recruited from an outpatient medical department. The Adult Attachment Interview was administered to all patients and controls and coded for RF by two raters. The agreement between the raters was .91. Axis I diagnoses did not distinguish high and low scorers on the scale with the exception of eating disordered patients, many of whom also carried an Axis II diagnosis (particularly BPD). Patients without Axis II diagnosis were rated higher on RF than those with ( $p<.05$ ). This was principally due to the low RF scores of patients with a diagnosis of BPD ( $p<.001$ ). Thirty-two of the 53 (60%) cases who reported abuse were independently diagnosed with BPD, compared with 4 of 29 (14%) who did not report abuse. The likelihood of reported abuse being associated with BPD was greater in the group of patients with low RF than those with RF ratings above the median. Only 4 of 24 (17%) patients reporting abuse in the high RF group were diagnosed with BPD, whereas 28 of 29 patients (97%) reporting abuse in the low RF group reporting abuse were so diagnosed. In the group not reporting abuse the prevalence of BPD was the same in low and high RF groups (2 of 17 for high RF vs. 2 of 12 in low RF). Thus RF is predictive of BPD only in the presence of abuse. In line with this argument, the three-way interaction component of the log-linear analysis was significant ( $\chi^2 = 8.67$ ,  $N=82$ ,  $p<.004$ ).

Mother's RF, assessed on the basis of the prenatal interviews, was found to be highly predictive of the child's success in the Belief-Desire Reasoning Task ( $r(90)=.32$ ,  $p<.001$ ). This correlation controls for both the child's and the mother's verbal ability. In a path analysis, which included mother's

attachment security, father's attachment security, mother's RF (metacognitive ability), infant-mother and infant-father attachment security, and child's verbal fluency as predictors of the child's performance on a cognitive emotion task, mother's RF was found to predict the child's performance, both via its influence on the child's attachment to the mother, and directly.

## **Clinical Utility**

RF is currently used in a number of studies to explore the impact of psychoanalytic psychotherapy on the capacity to envision mental states. Imre Szecsödy and the AHMOS group were the first to address this issue in a prospective study (see this volume). It is not assumed that all of the effects of psychotherapy are mediated via this capacity for all patients. Rather, the authors assume that some patients require developmental help in this domain in the context of certain relationships. In a number of clinically oriented papers, Target and Fonagy have explored the clinical applicability of the RF concept (Fonagy & Target, 1996a; 2000; Target & Fonagy, 1996). The measure may be a helpful focus for early interventions as the focus on mother's RF in relation to her infant might well increase the chances of secure attachment. David Oppenheim at the University of Haifa is engaged with such work. Arietta Slade and Mary Target have explored the RF coding of the mother's representation of the infant and her relationship with the child in the context of the Parent Development Interview. Although, this has not yet been explored, groups are obviously possible to code from the standpoint of RF.

# Person Representation Coding System

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Lemche, E., Grote, K. et al. (1999). Early parent-child interactions, parental representations, and emotion-regulatory patterns as measured through evoked play-narratives: Results from an exploratory study of 16 preschool children. 1st IPA Research Conference, Santiago de Chile.

Lemche, E. (2000). *Person Representation Coding System (5th revision)*. Dresden University of Technology, Dresden.

Lemche, E. (2000). *Guidelines for coding positive and negative self, mother and father action in play narratives. Addenda to the MacArthur Narrative Coding System (10/2000)*. Dresden University of Technology.

## Brief Summary of Approach

Although Freud did not use a notion of representations in the contemporary sense of a stratified network of cognitive entities constituting internal structures, his writings on the theory of the ego contain a number of proposals on how affect-releasing tendencies are inextricably linked to ideational content. A number of theorists from object-relations and ego-psychology traditions have contributed to a house of theory that is concerned with how interaction experiences form mental models of other persons and the self, and how, in return, these models guide expressive, relationship, and action patterns. Attachment research ultimately transposed this theory into experimental design. While attachment theory is hesitant to undertake conceptual elaboration of internal working models, psychoanalytic thinkers such as Jacobson and Fairbairn provide much speculative anticipation for a more thorough scientific investigation of the extent to which hedonic valences of mental representations may influence internal and external behavioral regulations, including symptoms.

With the advent of the play narrative method, it became possible to study intrapsychic processes in very young children by facilitating them with an age-appropriate expressive tool. The Attachment Story Completion Task (ASCT) originated by Bretherton in Boulder in the mid 1980s and the development of the MacArthur Story Stem Battery (MSSB) by the Emde Lab in Denver in 1990 opened up a new major route to conscious and unconscious mental processes in children, including defensive behaviors. Starting with the MacArthur Narrative Coding System (MNCS) in 1995 there are now a number of coding systems and rating schemes in use internationally that allow for the subtle registration of nonverbal expressive displays as well as enacted and/or uttered emotion contents. With the recent fifth version of the Person Representation Coding System (PRCS) (Lemche, 2000b) there are observation criteria at hand for a precise quantification of the differentiation of the self, mother, and father representation, as well as of positive vs. negative valence representations.

Because the preschool period is regarded in the developmental literature as critical for the emergence and stabilization of intrapsychic emotion regulation, they chose to study this developmental span, with the initial explicit intention to learn more about the emotional aspects of oedipal development. A volunteer sample of eight boys and eight girls in the three-to-six-years range was drawn from five nursery schools in Berlin. Children and mothers completed a free-play session for the assessment of dyadic Emotional Availability prior to the administration of the MSSB. A number of data reports were obtained for sociodemographic, linguistic, and family background aspects. Among the parent-report instruments was the Child Behavior Checklist (CBCL), whose evaluation revealed that half of the children exceeded clinical cutoff levels. The sample met middle-class SES, but was biased towards higher education of the parents. Presentation of the story stems was randomized, and both coding instruments and cases were systematically varied among the coder pairs in the team. The first analyses established test-psychological criteria for the instruments MNCS, PRCS, and Emotional Availability, as well as sufficient intercoder reliabilities.

## Major results

General strategy of the steps in data analysis was to test possible relations of the emotional and representational measures in the semi-experimental laboratory situation with various outside measures, and of the observational variables among each other. Four directions of analyses were



pursued: development of language and narration, depiction of emotion regulatory patterns, prediction of person and valence representations, and exploration of mediator functioning for emotions and psychological symptoms.

Analyses on the aspect of narrative pragmatics indicated that children's narrative-interactive behaviours with the experimenter are not representation-mediated, but rather reflect Emotional Availability, i.e. parent-child interaction quality. In general, narrative coherence as assessed with the MNCS was related to PRCS representations, for the case of the mother representation, however, not without control for measurement error. There was a main effect in coherence by clinical symptom status and a multivariate two-way interaction by Emotional Availability and behavior problems.

The multitude of emotion observation measures suggested making attempts to describe them as conflict-elicited emotion-regulatory patterns (rather than cognitive emotion-regulatory strategies, in contrast). Two factor analyses, one on emotion contents only, and one with inclusion of nonverbal displays indicated sufficient factoriability. Although positive displays and themes are the most prevalent affect expressions, these tend to load on one singular factor, while various facets of negative affect tend to differentiate in further factors. Again, mother, father and self-representation (PRCS) exhibited the largest variance account on four emotion-content latent constructs. In the natural six-factor solution including nonverbal displays, one latent construct "emotion control" emerged that explained highly significantly most pairwise comparisons among overt-behavior regulatory cluster groups.

Efforts to explain the degree of differentiation of person representations can be summarized in the way that parent-child interaction quality, mother-reported language developmental milestones and a number of sociodemographic background variables form the three most important groups of predictors. If, however, valence representations were added, gestural deixis (a joint-attention protosymbolic sign of reference) proved to be the only significant predictor both in canonical correlation and multiple-criterion regression models.

Both a generic canonical correlation and multiple regression models demonstrated the mediator status of representations, influencing the relationship between positive and negative emotion aggregates and internalizing and externalizing behavioral syndromes. Similar as in the case of coherence, a methodological difficulty seems to arise from the fact that representations and emotion topics are elicited from conflict stories; as mother representations are more associated with proximity, soothing and positive affect, they tend to show less variability in distressing context, in majority. However, in a subgroup of about 35.7% they found a strong association of mother and negative valence. The coincidence of this constellation predicted the occurrence of negatively toned hallucinatory-bizarre content, which in turn proved to be related above chance with externalizing behavioral syndromes. Further studies are planned to seek to replicate and extend the findings in larger scaled and longitudinal contexts.

## **Brief evaluation of the approach**

The representational world of the young child is the appropriate focus in the assessment of change in psychoanalysis. The story-stems originally designed and drawn up in Robert Emde's laboratory in Denver, represent a major step forward in creating a relatively standard form of semi-projective assessment for this domain. The material offered by the experimental situation is rich and the coding systems that have been developed are strong. The present studies are a significant contribution to this tradition.