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Research issues in psychoanalysis

By Peter Fonagy

Foreword

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

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.[1] 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.

[1] 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.

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 20th 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 7ians 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-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 (2001), in his incisive and erudite analysis of epistemology in the 2nd edition of the ODR, 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

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 (1912e) 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, 1992).

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 et al.,, ~~Kernberg, & Yeomans~~, 1999; Greenson, 1967; Kernberg et al., ~~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, 1917). 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, examplars 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

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, 1999). 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 et al.,, ~~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 et al., ~~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 et al., ~~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 et al., ~~Oliver, Collins, & Carson~~, 1995; Johnstone & Zolese, 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 et al., ~~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, 1958) – 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 et al., ~~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 et al., ~~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 (2001). 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, 1909b; 1909d) 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 21st 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 (2001) 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, 1926e). 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 (2001) 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, Scheflin, & 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 studies of facial expressions of affect in face-to-face psychotherapy (Krause & Lütolf 1988) and Beatrice Beebe's et al. (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?

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 (2001) 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 20th 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, 1923e) 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. 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). 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, 2005).

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.

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