

## RESEARCH ARTICLE

### The Research Agenda: The Vital Need for Empirical Research in Child Psychotherapy

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#### ABSTRACT

Some possible reasons for the historical absence of psychoanalytic outcome research are examined, particularly the incompatibilities in the world view espoused by psychoanalysis and that of most of current science and the assumption of privileged knowledge on the part of many psychoanalysts and their training institutions. It is argued that the view that psychoanalysis offers an alternative epistemology to that of scientific research maintains psychoanalysis in its inferior position. The existing evidence for the effectiveness of adult psychoanalytic psychotherapy is reviewed and the relevance of these findings to child psychotherapy outlined. Finally the author proposes a research agenda for both child and adult psychotherapy and outlines the changes that psychotherapists will need to make if they are to implement this agenda.

In a significant exchange in last week's BMJ, Nick Tarrier, representing the views of the four thousand-strong British Association for Behavioural and Cognitive Psychotherapy, responded stridently to a balanced article by Jeremy Holmes in which Holmes expressed some reservations about the apparent dominance of CBT in the field of NHS adult psychotherapy. Holmes' article did not attempt to recreate the antagonism of past decades, but cautioned that the evidence did not warrant making CBT the first and only psychosocial approach to mental disorder and that premature commitment to one approach could seriously erode the skill base of NHS practitioners. Tarrier replied, "Holmes relies on the specious old adage that absence of evidence is not evidence of absence [of effectiveness]. [...] I would have more enthusiasm for this argument if traditional psychotherapy were new. It has been around for 100 years or so. The argument, therefore, becomes a little less compelling when psychotherapy's late arrival at the table of science has been triggered by a threat to pull the plug on public funding because of the absence of evidence" (p. 292). Tarrier concludes that while all kinds of psychotherapy may be just sticking plasters over the wounds of social inequality, "cognitive behaviour therapy is the best plaster available".

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Freud himself was uncertain about the effectiveness of psychoanalysis (see e.g. Freud, 1904, p. 254; 1937, p. 228). Given his loss of enthusiasm for the therapeutic aspects of analysis more than half a century ago, what hope can there be for psychoanalytic therapies in the era of empirically-validated treatments, the era that prizes brief structured interventions, what hope for a therapeutic approach that defines itself in terms of freedom from constraint and preconception (Bion, 1967)? But there can be no excuse for the thin

evidence base of psychoanalytically oriented treatments. In the same breath that we often claim to be at the intellectual origin of other talking cures, we also seek shelter behind the relative immaturity of the discipline to explain the absence of evidence for its efficacy. Yet the evidence base of 'derivatives' of psychoanalytic therapy (e.g. systemic therapy, cognitive behaviour therapy) has been far more firmly established than evidence for psychoanalytic therapy itself.

The most likely reason for the absence of psychoanalytic outcome research lies in the fundamental incompatibilities in the world view espoused by psychoanalysis and most of current science. In a recent paper, Paul Whittle (in press) describes a 'chasm' between psychoanalysis and psychology in a brilliant overview with which I heartily agree. While the method of psychoanalysis was developed to fill gaps in self narrative and self awareness, inevitable because of the limitations of conscious reflection, psychology has a minimalist theory building tradition which Whittle elegantly describes as 'cognitive asceticism'. The kind of narrative making which psychoanalysis entails is so central to the experience of personal meaning, that it will probably always be vital to understanding human nature. The complexity of psychoanalytic theorization is defensible because the content of the mind is irreducible and because any assertion of a singular reality is inherently suspect. At a more impressionistic level we might say that the world-view that is normally created by working intensively and long-term with disturbed individuals is incompatible with the ethos of tightly controlled studies.

Those who work at close quarters with the human mind will inevitably have an impression of reductionism when they see the full complexity of an individual's struggle with internal and external experience reduced to a single 100-point scale (Endicott, Spitzer, Heiss, & Cohen, 1976; Shaffer et al., 1983) or even 12 five-point ones (Wing, Curtis, & Beevor, 1996; Wing, Lelliott, & Beevor, 2000). Can psychoanalytic therapy ever show its effectiveness, let alone cost-effectiveness in this way? After all, is it not a qualitatively different form of therapy that needs a qualitatively different kind of metric to reflect variations in its outcome? Symptom change as an indicator is crude in relation to the complex interpersonal processes that evolve over the hundreds of sessions of psychotherapeutic treatment. No wonder that most psychotherapists are sceptical about outcome investigations.

Furthermore, the rapid progress of technology and biological science has offered what many see as the only viable solution to the challenge of treating mental disorder – biochemical rehabilitation. Statutory funding for psychological therapy is in many countries threatened by the readiness with which pharmacological treatments can be made available to relatively large groups. Popular views concerning the causes of mental illness have shifted with research during 'the decade of the brain', sometimes powerfully supported by far sighted pharmaceutical companies, to the point where commonly held

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theories of psychological disorder have shifted towards the constitutional, and antidepressants are bizarrely accepted as appropriate means of addressing social difficulties, notwithstanding strong resistance from user groups (Cornwell, 1996). Behaviour genetics research has not helped (Fonagy, in press), and has cast doubt in the minds of many about psychodynamic claims concerning the causal significance of shared early family environment, the bread and butter of psychotherapeutic narrative (Rutter, 2000).

Add to this already toxic mixture for psychoanalytic thinking the undeniable arrogance of many psychoanalysts and the training institutions that they have created, who until recently have too often treated non-analytically trained colleagues with a condescending assumption of privileged knowledge, and you have the background to the current crisis for our approach<sup>1</sup>.

So can we think of psychoanalysis as offering an alternative epistemology to that of scientific research? I believe that such an attitude maintains psychoanalysis in its inferior position. And seeing scientific research and psychoanalysis as at opposite ends of an epistemological continuum risks shielding us from appropriate criticisms. Child psychotherapy needs to change. Gathering further evidence for psychotherapy through outcome studies is important, not simply to improve support for existing practices but far more to generate a change in our own attitudes from a culture of knowing and certainty to one of questioning, uncertainty and progress. We were far too complacent about our technical knowledge and its application, and this may not yet have changed enough.

Given this unpromising context, the strength of the evidence for adult psychoanalytic psychotherapy is surprising. Although there are not too many studies, and many of these have limitations, brief psychodynamic treatments appear to have effects of comparable size to those of other therapies, with manualised treatments showing larger effect sizes, and a slight superiority of some psychodynamic therapies on follow-up (Anderson & Lambert, 1995). Further, there has been a backlash against treating the findings from randomised controlled trials as 'gospel' (Markowitz & Street, 1999; Weisz & Jensen, 1999). There has been a heartfelt outcry for effectiveness rather than efficacy research, so that research findings would more truthfully represent the value of a treatment in the field (Wells, 1999).

There is increasing evidence, chiefly from the adult literature, for three propositions that are very relevant to evaluations of child therapy. These are:

(i) for certain conditions, longer-term therapy is more effective than short-term therapy. These findings emerge in part from naturalistic 'follow-along' studies of progress in therapy, such as the Consumer Survey in the US (Seligman, 1995) and the multi-site eating disorder study in Germany (Kachele, Kordy, Richard, & Group, 2001). Some results from long-term follow ups of short-term therapies have been spectacularly disappointing. With severe disorders, problems persist regardless of therapy type. For example only 20% of MDD (major depressive disorder) patients treated with CBT or IPT (interpersonal therapy) were free of depressive episodes 18 months after termination of 16 week psychotherapy (Elkin, 1994; Shea et al., 1992).

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(ii) many of the benefits of psychoanalytic psychotherapy seem to emerge after the termination of treatment (sleeper effect). In our randomised controlled trial of psychoanalytic day treatment of borderline personality disorder, compared with treatment-as-usual (Bateman & Fonagy, 2001), Anthony Bateman and I found major symptomatic differences on self-report measures at discharge, differences which significantly increased over the follow-up period. Self-harming behaviour similarly declined further over a follow-up period. Most importantly, substantial

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<sup>1</sup> Fortunately, those trained by them often shed those values once working in multi-disciplinary settings.

differences in health-care costs began to emerge only after termination of treatment.

(iii) the intensity of therapy, i.e. number of sessions per week, appears to make a difference. Many of you will be familiar with Mary Target's and my finding that non-intensive therapies appeared to be less helpful in more complex cases of emotional disorder at the Anna Freud Centre. Long-term follow-up from that study suggests that the results of successful treatments may be maintained into adulthood, whereas unsuccessful treatments – or lack of treatment - mark a lifetime of difficulties. The combination of sleeper and intensity effects were demonstrated by the Stockholm Outcome of Psychotherapy and Psychoanalysis project (Blomberg, Lazar, & Sandell, 2001; Sandell & Blomberg, 2001). Sandell and colleagues found little difference in outcomes at termination between once-weekly and four or five-times weekly therapies, but self-report measures of symptomatic improvement over a three-year follow-up period showed more intensive therapies to have a significant advantage over non-intensive work. [By contrast, short-term treatments in the Stockholm study had only minimal effects in the long run.]

Though massive by contrast to that available for children, the evidence base available for adult psychotherapy is currently weak by the standards of health service administrators. However, a number of promising studies in progress are likely to provide compelling evidence for the therapeutic effectiveness of psychoanalytic psychotherapy. These include the Cornell comparison of TFP and DBT, (transference focused psychotherapy and dialectical behaviour therapy) the Munich Psychotherapy of Depression Study, the Helsinki Psychotherapy Study, and many others. Many of these will be reviewed at the forthcoming IPA Research Conference at UCL on the weekend of March 8<sup>th</sup>-10<sup>th</sup>.

The challenges facing child and adult therapy research are, however, more or less identical. My agenda for research is as follows:

1. There is an urgent need to develop a classification system that psychoanalytic clinicians can use to help them identify cases who are most likely to be suitable for psychotherapy.
2. Measures are required that can verify that psychoanalytic therapy has taken place. This involves two separate challenges: the measure itself, but before this, an agreement on what child psychotherapy might be. We need to agree on what the essential components of psychoanalytically orientated child therapy are. [There is no generally accepted operationalised description of psychoanalytic therapy.] This underscores the need for a systematic examination of what happens in therapy.

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3. Related to this, we need to identify the therapeutically active components of the treatment of children through closer observation of the therapy process. Process notes are insufficient. Most of the potentially pertinent components of clinical interactions are not available to introspection and self report. [For example, Rainer Krause (1997) demonstrated the almost invariable association of therapist's mirroring of the patient's smiling during early phases of treatment with poor outcome. It is not difficult to understand this phenomenon in terms of pathological projective identificatory processes. Controlling such unconscious reactions without knowledge of the problem presents an insurmountable clinical challenge.]

4. Measures of therapeutic outcome represent a special challenge. We need measures that reflect the kind of changes that psychoanalytic therapy aims to generate. The classical ego psychological concept of structural change reflects this ambition. A radical change of object representation in the direction of greater integration might be a comparable goal. These are likely to interface with broad quality of life measures (not necessarily taken immediately post treatment) which in turn will need to be related to the cost of offering the service. At issue here is the need to go beyond, both in time and in depth and breadth, but also to incorporate, 'superficial' measurement embodied in HoNOSCA or the Strengths and Difficulties Scale. Systematising our impressions of the child's subjectivity is what is clearly required.

To summarize, developing a research agenda has three overriding aims. First, we require evidence concerning the specific patient groups who uniquely benefit from our interventions, and related to this, assessment systems that help to identify these individuals, either in terms of diagnosis and symptomatology, or in terms of characteristic modes of mental functioning or even social conditions. Second, we need sensitive measurement systems to identify changes that may go beyond symptomatic improvement and indicate benefits that are either valued by clients (or carers) or can be shown to be predictive of relative freedom from future difficulties (prevention). Third, we need to develop new adaptations of psychoanalytic therapy that extend and improve upon existing applications in the direction of both greater generalisability across clinical groups and more powerful impact on them either in terms of symptom relief or prevention.

No one type of research methodology can fulfil these aims. Different research questions require different scientific methodologies. There has been a resurgence of interest in qualitative as opposed to quantitative data gathering (e.g. Mayes & Pope, 2000). Though few would argue that questions of cost effectiveness could be usefully addressed using narrative data, quantitative approaches to certain subtle questions (e.g. the process of change in therapy and the nature of therapeutic experience) are often self-evidently empty and puerile. Qualitative analysis has always and will always precede detailed quantitative study. The interactive probing and questioning methods used in qualitative research facilitates exploration of individual circumstance and experiences. However, qualitative research samples are not designed to be statistically representative of the researched population and this means that statements about incidence or

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prevalence cannot be sustained. It is not possible to identify statistically discriminatory variables from qualitative data. Most importantly for us psychoanalysts, relationships identified by qualitative research reflect explanations offered explicitly or implicitly by respondents and current qualitative methodology cannot readily go beyond that which is at the surface level of the patient's narrative.

There are at least five ways we will need to change if we are to implement this research agenda: (1) incorporating of data gathering methods from social and biological science that go beyond the anecdotal; (2) making our concepts more specific, to facilitate cumulative data gathering; (3) routine consideration of alternative possible accounts for observations, not just the psychoanalytic, but at all appropriate levels; (4) becoming more sophisticated about social and contextual influences on behaviour, and (5) ending our splendid isolation and undertaking active scientific collaboration with other disciplines.

Rather than fearing that fields adjacent to ours might destroy the unique insights offered by long term intensive individual therapy, we must embrace the rapidly evolving 'knowledge chain', focused at different levels of the study of brain-behaviour relationships. As the Nobel laureate, Eric Kandel (1998; 1999) pointed out, this may be the only route for the preservation of the hard-won insights of our mother discipline, psychoanalysis.

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