International Journal of Applied Psychoanalytic Studies
Int. J. Appl. Psychoanal. Studies 5(4): 238–256 (2008)
Published online 5 September 2008 in Wiley InterScience
(www.interscience.wiley.com) **DOI**: 10.1002/aps.174

Dodo Birds, Doctors and the Evidence of Evidence

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ABSTRACT

This paper asks what is evidence-based psychology and looks at the ways in which psychoanalysts have sought to engage with the challenges posed by this method of deciding best science. The paper examines the evidence for the evidence-based method and looks at how psychoanalysts have attempted to shift the scientific debate from evidence-based models to practice-based, outcome driven models and, ultimately, to a patient-therapy approach that seeks to marry best practice with a spirit of open-minded assessment of alternatives. Copyright © 2008 John Wiley & Sons, Ltd.

Key words: evidence-based, psychoanalysis, psychotherapy, research, treatment-manual

INTRODUCTION

The relation of psychoanalysis to empirical testing has always been of interest and of controversy. The debate is currently taking place within the context of a revival of psychodynamic psychiatry in North America, exemplified by the publication of the Psychodynamic Diagnostic Manual (PDM) by the Alliance of Psychoanalytic Organizations (PDM Task Force, 2006), which aims to complement the internationally recognized standard for evidence-based psychiatry: the Diagnostic Statistic Manual (DSM), now into its fourth edition (APA, 1999). The PDM includes an increased interest in manualization of psychotherapy/psychoanalysis, and spearheads the move to reintegrate psychoanalysis into mainstream psychiatry. The PDM is designed to raise the status of psychoanalysis within the mental health field. These moves raise questions about the psychoanalytic ethic which, in its purest form, has historically been antagonistic to standardization of treatment and other perceived "compromises" in clinical practice. Can the psychoanalytic ethic resist dissolution in this progressive détente with biological psychiatry and academic psychology? Fonagy and Target (2002) recently commented on this very problem. In the context of new research

DOI: 10.1002/aps

at the Anna Freud Centre that required the intervention during treatment of video recorders, they noted the ethical concerns raised by therapists at the commencement of the project, but found that, over time, the status of such interventions were not to be seen as an ethical compromise, but rather a tension that needs to be carefully managed (see Fonagy and Target, 2002).

The task then for psychoanalysis is to validate its method and/or practice in the terms set by other psychological disciplines, even when the methods of those disciplines may seem alien to the psychoanalytic method. This paper will examine the evidence for and against psychoanalysis and psychotherapy in the evidence-based paradigm. An unavoidable and unfortunate conclusion that follows from a review of this literature is that very little of the "evidence" changes anyone's mind on fundamental differences. This is the so-called "dodo bird effect" – a product of researcher allegiances (see later). Many of the studies reviewed in this paper address this problem. Nonetheless, practitioners cannot remain aloof from economic and sociological considerations that have drawn psychoanalysis into an increasingly integrated and multi-disciplinary mental health approach, which is why it has become essential – aside from the questions of "truth" in evidence – to rend psychoanalysis valid in terms comprehensible to other mental health professions. The language of psychoanalysis must change to meet this situation. This problem is not in itself new: psychoanalysts have had the challenge of explaining and justifying psychoanalysis to the unanalyzed. At times, the analysts have retreated from this engagement, but that approach is clearly outdated. Freud himself, often resorted to works of literature to explain the workings of analysis to the lay public. Today, we are obliged to make use of the language – as well as the practice – of empiricism and evidence-based medicine when debating the science with colleagues from other fields, with governments, and with the public.

EVIDENCE-BASED MEDICINE: WHAT IS IT?

According to a recent North American commentary (Straus and McAlister, 2000), "evidence-based medicine" is simply a more systematic description of a practice which has always been fundamental in medicine. Evidence-based practice has grown geometrically since the early 1990s. One quasi-official definition of evidence-based practice is: "The process of systematically finding, appraising, and using contemporaneous research findings as the basis for clinical decisions" (Evidence-based Working Group of the American Medical Association, cited in Straus and McAlister, 2000: 837). While this sounds eminently reasonable there is a significant challenge to health professionals in acceding to this model. While it has always been the case that the conscientious medical practitioner has "critically appraised" his own practice and that of his colleagues in the improvement of his craft, the new paradigm introduces an additional layer of evidence testing. This is the "Randomized Clinical Trial" (RCT) which, according to Straus and McAlister, is "usually considered the 'gold standard' for establishing the effects

DOI: 10.1002/aps

of an intervention" (2000: 839). And yet, we are told, RCTs "are not the best sources for answering questions about diagnosis, prognosis or harm". This explains the direction to practitioners that it is still possible that "several sources may inform clinical decision-making" (2000: 839).

So we can see from this first foray into the logic of evidence-based practice as applied to medicine that there is the possibility, if not the presence, of a disjuncture between evidence-based practice and good medical practice.

Ostensibly, the idea that drives "scientific" medicine (as distinct from what is designated as "folk" medicine) is the quest for better health outcomes based on evidence of what works. But, like folk medicine, scientific medicine was, until recently, based on the inherited lessons of previous generations of practitioners. In other words, knowledge retention (i.e. evidence of "what worked") was transferred from master to pupil. The introduction of evidence-based practice has shifted the authority for this best practice from the individual clinical judgement (i.e. of the "master") to the RCT, which aims to provide relevant evidence from either basic or applied research to "close the gaps" where currently, "clinical experience and reasoning (based on principles derived from basic scientific research) must be applied to traverse many grey zones of practice" (Straus and McAlister, 2000: 838). Implicit in this ideology is a scientific optimism that suggests once these gaps have been traversed, there will be no more gaps in practice. So by informing practitioners of best-practice based on the authority of evidence from randomized controlled trials, one can expect superior treatment outcomes than from the older method of training the practitioner's independent clinical judgment – psychoanalysts beware!

The question that needs to be asked is whether evidence-based practice improves health outcomes. Surprisingly, this is not as clear as one would think. The evidence-based advocates continue to tell us that they do not yet have enough evidence to answer this question. They argue for more evidence, as if there is only evidence and lack of evidence; neither of which allows for quality of evidence. At least one meta-analysis of evidence-based practice has already shown (Kirsch and Sapirstein, 1998), unrecognized assumptions underlying the methodology of RCTs are capable of corrupting the evidence that is produced. Thus, an examination of around 1500 RCTs testing the effects of various depression medications revealed that one cannot be sure of any correlation between the pharmacological intervention and the alteration in the psychological state claimed to be a consequence (Kirsch and Sapirstein, 1998).

It was perhaps inevitable that once evidence-based ideology gained a foothold in medical practice, that the mental health sciences – psychoanalysis included – would too be subjected to this paradigm. This has occurred and it has demanded a different kind of response from psychoanalysts to that of the positivist and hermeneutic critiques which dominated the mid-century challenges to psychoanalysis. The positivist and hermeneutic attacks at some level engaged with the intellectual basis of psychoanalysis; that is, with the presuppositions of its science. This is not the case with the evidence-based challenge because this method has

DOI: 10.1002/aps

the potential to influence scientific practice by volume of research outcome. This is to say that financial and institutional weight can drastically influence what research outcomes are produced, because they influence what research is conducted. Thus the debate can sometimes pivot around the status of those who make the competing claims. And analysts have been an easy target in these debates, being largely private-practice based and even anti-institutional. The response of much of the psychoanalytic fraternity to this challenge has, historically, been silence. Part of the profession has argued for the hermeneutic understanding of psychoanalysis and of science generally, so side-stepping the problem of evidence. Peter Fonagy commented on this problem recently,

> There is a clear danger that a therapy that is without substantial evidence will be generally thought to be without substantial value. Once this idea is allowed to flourish, as it has already shown ominous signs of doing, cultural change becomes inevitable, a change that at least temporarily has the power to stop the development of our discipline by encouraging potential patients to reject psychoanalysis as a therapeutic choice, discouraging young people from entering the profession, and bringing psychoanalytic contributions to mental health disciplines and other subjects into disrepute. (Fonagy and Target, 2002: 54)

The burden of proof now lies squarely with the analysts in what must surely be understood as a marketplace environment where public policy makers encourage "consumers" to shop around for their choice mental health outcome. It cannot be denied that the public perception of psychoanalysis influences, to some degree, the type and number of people turning to analysis for assistance and entering analytic training programs. However, one might ask has psychoanalysis ever enjoyed favor in the public consciousness, and does this really matter? Should we assume that the educated and socially mobile elite that has traditionally constituted the core of psychoanalytic practice will no more be able to exercise judgement and choose analysis for themselves? We might also remember that the mental health professions at large have long been the subject of popular ridicule and that psychiatry, in many places, remains one of the lowest paid of the medical specialties.

THE EVIDENCE OF THE EVIDENCE

That said, some analysts have responded differently to these challenges by engaging with the evidence-based psychology on its own turf. One initiative has been the inauguration of annual training programs in research methodology under the auspices of the International Psychoanalytic Association (IPA). This is designed to train analysts in the research methodologies of academic psychology in order to make analytic research more readily comprehensible to funding bodies and public policy makers (see Emde and Fonagy, 1997). It appears this initiative has been successful in increasing funding to analytic researchers (see later).

DOI: 10.1002/aps

Strupp and Howard point to the maturation in psychotherapy research over the last 50 years or so, from a "process analysis" testing – including to a lesser extent "content analysis" – to that of a dual thrust "process and outcome research" (1992: 315–316). This is a development that has also been pointed to by Wallerstein (see Wallerstein 2006), former head of the Menninger Clinic Long-term Psychotherapy Project, and it is a key methodological plank in Peter Fonagy's recent efforts to transform the evidence-based agenda.

Fonagy et al. (2005) undertake an exhaustive review of the psychotherapy outcomes literature for short-term (i.e. around 20 sessions delivered usually once weekly) and long-term psychodynamic psychotherapy. This research arose out of earlier work done by the authors for the UK Department of Health on the effectiveness of psychotherapy (Roth and Fonagy, 1996). The authors ask if there is any psychological disorders for which either treatment option can be considered evidence-based.

But to answer this Fonagy first moves to discredit – and then reformulate – the current evidence based methodology. He draws on a "powerful critique" by Westen et al. (2004) of the research methods used to establish "empirically supported therapies". In summary, Westen et al. found that the underlying aim of these research methods was "maximisation of internal validity by random assignment, controlling confounding variables and standardising procedures" (cited in Fonagy et al., 2005: 2). According to Westen et al., research that is considered empirically supported tends to have three characteristics:

(1) the studies address a single disorder (usually Axis I disorder in the DSM) with diagnostic assessments to ensure homogeneity of samples;
(2) the treatments are manualized and are of brief and fixed duration to ensure the integrity of the "experimental manipulation";
(3) the outcome assessments focus on the symptom(s) that represented the declared priority of the study (and often of the intervention). (After Westen et al., 2004, cited in Fonagy et al., 2005: 2)

Based on this critique, Westen et al. next identify (according to Fonagy) four poorly supported assumptions underpinning the application of RCT methodology to psychotherapy research. The said presuppositions are that,

(1) psychotherapy is so malleable that a brief intervention is likely to change it permanently;
(2) most patients can be treated for a single disorder or problem;
(3) psychiatric disorders can be treated with psychosocial interventions without regard to personality factors that are less likely to change with brief treatments;
(4) experimental methods provide a helpful "gold standard" for evaluating these packages.

DOI: 10.1002/aps

Fonagy then sets out responses to each of these presuppositions, disarming their bases one by one. In response to the first assertion, he cites Kopta et al. (1994) for having established that, "most forms of psychopathology encountered in specialist centers are treatment resistant" (Fonagy et al., 2005: 2). On the question of whether patients can be treated for multiple disorders, Fonagy cites Kessler et al. (1999) for showing that most psychopathology as described by Kopta et al. (1994) is "comorbid with other disorders" (Fonagy et al., 2005: 2). This neutralizes the second presupposition. Fonagy next relies on Thompson-Brenner et al. (2003) who found that psychopathology needed "to be tackled in the broader context of the patient's personality structure", which argues strongly against the validity of the third basis of the RCT methodology (Fonagy et al., 2005: 2). Finally, on the question of the experimental methodology of RCTs, Fonagy points to Ablon and Jones (2002), when they concluded that, "... experimental methods need to be supplemented by correlational analysis to ascertain the effective components of treatment" (cited in Fonagy et al., 2005: 2).

Having hereby brought into question the fundamental conceptual bases of the RCT evidence-based methodology for evaluating psychotherapeutic treatment modes, Fonagy seeks to correct each of these four deficiencies by modifying the process for assessing outcomes. He states that his interest is to address public health policy concerns. The only language policy makers understand at present is that of evidence-based RCTs. So he must find some variation of the methodology that will support (i.e. make a just assessment of) his hypothesis that psychoanalytic psychotherapy is a superior form of treatment for certain conditions. And so he advocates a "best-practice" model of evidence to supplant "evidence-based practice" as the chief guide to clinicians in their choice of treatment, saying that, "substantial evidence does exist to guide clinicians, even if this evidence is not yet accessible through a mechanical process of review" (Fonagy, 2001b: 647).

THE ECONOMICS OF EVIDENCE

The challenges that psychoanalysis (and health-care more generally) face in Britain today are in some ways similar to those that confronted the US when Managed Health Care was rolled out in the early 1980s, after its introduction in the final days of the Nixon administration. All types of mental heath treatment have come under scrutiny. Hans Strupp followed the political debate in the US as it concerned psychoanalytic practice (see Strupp, 1982). Though couched in broader terms, the chief issue for American legislators at that time was the rapidly rising cost of health care. Psychoanalysis was an easy target. The case advanced against subsidizing psychoanalysis was summarized by Strupp:

(1) There is as yet insufficient evidence that any form of psychotherapy is effective and safe [and],

DOI: 10.1002/aps

(2) there is even less evidence that psychoanalysis, the most ambitious, expensive, and elitist form of psychotherapy, results in significant benefits to its consumers, at least benefits of the kind that the taxpayer should be expected to underwrite (Strupp, 1982: 123).

Strupp therefore concluded that the chief evidence against psychotherapy and psychoanalysis was economic. The application of this medico-economic model to psychotherapy served the purpose of restricting access to psychotherapy. Jeremy Holmes (2002) later wrote on the confusion of "lack of evidence" with "evidence of ineffectiveness" inherent in this critique. Indeed, a recent review of the evidence by a group of Australian psychiatrists found yet again that, "the most detailed and sophisticated outcome studies in all of psychiatry are in the field of psychoanalysis and the psychotherapies derived from it" (Buckle et al., 1995: 74). The authors drew on an historical survey of Bachrach et al. (1991) who examined long-term outcome studies as far back as the Berlin psychoanalytic clinic from 1930, which consistently found improvement rates for neurotic patients of the order of 80 to 90 percent of those treated.

Elsewhere, studies such as Luborsky et al (1988) have shown that improvement is maintained at seven-year follow-up (cited in Buckle et al., 1995: 74). This reflects a series of research projects and surveys by B. P Karon (e.g. Karon and Vanderbos, 1981; Karon, 1989). These in turn were reviewed by the Royal College of Psychiatry "Psychosis: psychological approaches and their effectiveness" (Martindale et al., 2000) for the "International Society for the Psychological Treatments of the Schizophrenias and other Psychoses". They found that the outcomes of psychotherapeutic treatment – for psychotic patients in this study – were greatly affected by the time at which the results were taken. For example, in Karon's Michigan trial of treatment of schizophrenia (Karon and Vanderbos, 1981), it was found that, "Two years after termination, psychotherapy patients had had half the number of hospitalization days compared with the medication control group" (Martindale et al., 2000: 16). Certain conclusions regarding cost were able to be drawn from this study: only 33 percent of the psychotherapy patients needed welfare, compared with 75 percent of the medication-only controls. The author of the study stressed that the cost benefit findings "…would have been completely the opposite if the evaluations were only done at six months of treatment" (Martindale et al., 2000: 17).

Similarly, Buckle et al. (1995) cite analyses which have established that improved physiological health outcomes, employment participation, and diminished need for social services are correlative to positive psychotherapeutic outcomes. These results point to the net long-term economic benefits of psychotherapy. The authors concluded that, "Legislating how much psychotherapy an individual requires is arbitrary and with a treatment with so much cost-effectiveness, restriction of treatment becomes costly" (Buckle et al., 1995: 75).[1]

DOI: 10.1002/aps

FROM ECONOMIC-BASED TO OUTCOMES-BASED EVIDENCE: THE PRAGMATICS OF THE PATIENT

In recent works, Professor Fonagy has advanced a solution to the impasse over the pros and cons of psychotherapy. He stipulates that we should seek the evidence of "what works for whom" (Roth and Fonagy, 1996), which he says has significant advantages for both therapists and patients alike. In this context, Fonagy is generally supportive of the psychotherapy treatment manual. He cites a raft of studies supporting this position. For example, "When research therapy programs are successfully transferred to a clinical setting, there is likely to be an overall increase in therapeutic gain and an overall adherence to psychotherapy protocol" (Weisz et al., 1995, cited in Fonagy, 2001b: 654). This makes testing of treatment alternatives more accurate.

Indeed, says Strupp, citing Crits-Christoph and Mintz (1991), who found that, manuals are useful in significantly decreasing variance attributable to the therapist, manualization should increase researchers' ability to infer that differences in outcome are attributable to the different therapeutic modalities being investigated. In a meta-analysis covering 20 years of research, it was discovered that treatment manuals had "significantly decreased the variability of the therapist as a contributor to the variance in outcome" (Strupp, 2001: 608).

"However," he adds, "a manual is not the treatment" (Strupp, 2001: 609). Strupp himself produced a treatment manual (Strupp and Binder, 1984), but later expressed reservations about its application in the clinic (Strupp and Anderson, 1997). He thought that treatment manuals will be of limited value for psychotherapy, because the aim of psychotherapy differs fundamentally from that of what he called the "Empirically Supported Treatments" (EST). The latter are treatment programs endorsed by the evidence-based psychology establishment. As such, they are primarily geared to symptom relief, whereas psychotherapy – according to Strupp – has as its basic objective personality growth consequent to the alleviation of unconscious conflicts and/or the modification of internal structures through constructive relational experiences (Strupp, 2001: 610–611). Thus, the outcomes of the two approaches would need to be assessed by very different criteria. If this cannot be realized because of the ideological biases of the EST programs, he suggests the use of the "Research-Informed Case History" as a compromise between the demand of managed care administration and the peculiar nature of the analytic situation. The Research-Informed Case History follows a proposal by Soldz (1990) who,

> …carried out a number of N = 1 studies in which individual cases were selected for intensive study from a larger database collected under controlled conditions. The therapeutic process in these cases was studied by means of various rating scales including the Structural Analysis of Social Behavior System (Benjamin, 1974), aided by careful, naturalistic observation. (Strupp, 2001: 616)

Int. J. Appl. Psychoanal. Studies 5: 238–256 (2008)
DOI: 10.1002/aps

Fonagy is generally in agreement with Strupp, but continues to assert that treatment manuals have a place in psychotherapy, and he cites the evidence for his position: a meta-analysis of over 3000 RCTs (see Fonagy et al., 2002). The more recent studies have attributed poor therapeutic outcomes in psychotherapy to "countertherapeutic components" which Fonagy says are attributable to the aimlessness of some courses of psychotherapy and the confusion and disenchantment deriving there from (Fonagy, 2001b: 654–655). Treatment manuals would overcome this aimlessness and lead to better outcomes. Fonagy says the only reason why analysts/therapists do not follow the evidence and change their methods in accordance with best practice is a narcissistic concern about their own effectiveness. Fonagy does not mention how one might distinguish "aimlessness" in psychoanalysis from Freud's "free association" method or Strupp's "time-unlimited treatment" (Strupp, 2001: 610). Surely free association works only because there is no directive to the analysand other than to say whatever come to mind as honestly as possible? (see Freud, 1913/1948).[2] What is there about free association that can be given an aim? Does a therapeutic aim conflict with this fundamental principle of psychoanalysis? Indeed, the psychoanalysis versus psychotherapy debate that so pre-occupied post-World War II American psychoanalysis revolves around this very question. Clearly what Fonagy is pointing to is a directed treatment of an educative kind, but one that goes beyond the suggestive relief of symptoms. On this too, Freud had written: "Ambitiousness in the educative direction is as undesirable as in the therapeutic" (Freud, 1912/1948: 332). Freud's point being that sublimation of aim must be patient directed; the outcome of treatment cannot be known before it occurs. In this respect one could say that Fonagy is moving beyond the psychotherapy versus psychoanalysis debate, to an analytically-informed targeted growth therapy or maturational model. This would be consistent with his writings on psychoanalysis and attachment theory (see e.g. Fonagy, 2001a). Strupp's studies on this question had concluded that,

> Irrespective of its theoretical underpinnings, psychotherapy is anchored in, and is fundamentally inseparable from, a human relationship. Thus, whether or not relationship variables are specifically conceptualized as critical to the process and outcome of therapy, they are undeniably an integral part of the therapeutic enterprise. (Strupp, 2001: 611)

One long-term psychotherapy study in Michigan would seem to support this notion. For a sample of patients suffering from thought disorder (not medicated), those patients in therapy with the most experienced therapist showed superior improvement, while for inexperienced therapists, the improvements were less obvious (reported in Martindale et al., 2000: 16).

FROM OUTCOMES-BASED TO PRACTICE-BASED EVIDENCE

Lester Luborsky (2001) has made a rigorous critique of the outcomes-based approach and to the meta-analyses that have followed in its wake. He begins by

DOI: 10.1002/aps

pointing to the challenge set down by Saul Rosenzweig in 1936, when he "supposed that the common factors across psychotherapies were so pervasive that there would be only small differences in the outcomes of comparisons of different forms of psychotherapy" (Rosenzweig, 1936, cited in Luborsky, 2001). In 1975, Luborsky et al. had examined about 100 comparative treatment studies and found that Rosenzweig's hypothesis was essentially right: there was a trend of only relatively small differences from the comparisons of outcomes of different treatment modalities (Luborsky et al., 1975). Researchers began to call such small differences in outcome the "do-do bird's verdict" in deference to Rosenzweig who had made use of the incident in Lewis Carroll's *Alice in Wonderland* where the dodo bird announced at the end of the race: "everybody has won, so all shall have prizes!".

A more recent meta-analysis found the same result: "The most extensive review of the topic ever done" showed the dodo bird verdict to be very prominent (Luborsky et al., 1999):

> For a sample of 29 comparative treatment studies, the correlation between the researcher's allegiance and the outcome of the treatments compared was an impressive 85! The realization from this kind of analysis is that any conclusion about the relative worth of a particular form of psychotherapy must be limited because it is not possible to rule out the presence of researcher allegiance effects. (Lubowsky, 2001: 588)

When one examines the complex meta-analyses at the center of Fonagy's important 2005 paper, one finds the dodo bird's verdict present still. It seems, in summary, that those who think psychotherapy works, find that it works; those who think psychotherapy does not work, find that it does not. A recent spate of comparative studies has tried to show – at the very least – that psychotherapy does something more than basic nursing care by introducing a treatment modality designated "supportive therapy" to describe the basic nursing-type interactions, so as to try to correct for the positive transference factors not associated with a directed therapeutic approach.

But Fonagy simply will not accept where this research is leading, and he asserts the view – reinforced also by Holmes (2002) – that, "the absence of evidence of differential effectiveness does not mean all therapies are equally effective" (Fonagy, 2001b: 650). Fonagy claims that his own surveys are evidence of the capacity for well-informed therapists to differentiate what works for whom. So for instance,

> . . . a review of the literature on conduct disorder made it seem very clear that cognitive-behavioral approaches aimed at improving parenting skills are highly effective for young children with conduct problems, though far less so for older children, for children with multiple problems, and for children with multiple-problem families. I believe these kinds of conclusions are far more helpful to psychoanalysis than the more immediately comfortable dodo bird verdict is. It helps us to identify those specific clinical populations for which a long-term psychodynamic treatment may prove efficacious. (Fonagy, 2001b: 650–651)

DOI: 10.1002/aps

And importantly, such a review of the literature, "identifies where further outcome research is necessary and where the efforts of psychoanalytic therapy researchers should be focused" (Fonagy, 2001b: 651).

But while the psychoanalytic community works to reform its ways, the mainstream psychological/psychiatric community has cultivated more subtle ways of excluding psychoanalysis and psychotherapy from public access. They make use of the dodo bird verdict in concert with the occasional use of double standards. An example of this can – regrettably – be found even in such a well respected forum as the Cochrane Reviews, which produce recommendations for clinical practice that inform groups such as the Royal College of Psychiatrists in the UK and Australia. The review of "Individual psychodynamic psychotherapy and psychoanalysis for schizophrenia and severe mental illness", found that, "When compared to a more cognitive approach, psychodynamic therapy may be more acceptable, but data on possible harm were not recorded" (Malmberg and Fenton, 2001: 12). And thus psychodynamic psychotherapy cannot be recommended as best practice.

Yet the corresponding Cochrane Review for "Cognitive behavior therapy for schizophrenia" records that, "it is not possible to assert any substantial benefit for cognitive behavioral therapy over standard care or supportive therapies" (Jones et al., 2004: 13), from which proposition the review panel concludes that, "Cognitive behavioral therapy holds a promise of providing a useful adjunct to traditional treatment of people with psychotic disturbance" (Jones et al., 2004: 13), without the introduction of evidence to support the conclusion. Nor indeed, to support the related conclusion that "more recent therapies [meaning pharmacological and cognitive behavioral] have eclipsed the psychodynamic approach for schizophrenia" (Malmberg and Fenton, 2001: 13).

Only gradually is psychiatry as a whole coming to realize that drugs are not the whole solution. And the so-called "compliance therapy" and other forms of cognitive behavioral therapies designed to maintain pharmacological regimes are also being found unsatisfactory. As an example of the evident shortcomings of the current psychiatric approach, the most comprehensive survey to date of people living with psychotic illness in Australia (Jablensky et al., 1999) found that, of those living with psychotic disorder, 86 percent were currently taking prescribed medication for mental health problems. Eighty-four percent of those complicit with their medication regime reported specific side effects of the medication, and 75 percent considered the side effects of sufficient severity to impair their daily activities (Jablensky et al., 1999: 11 and 16).

Even more extraordinary was the report that 61 percent had experienced/were experiencing either delusions or hallucinations (or both) during the past month (Jablensky et al., 1999: 10). This reflected other data indicating that, despite medication being beneficial in many situations, only 29 percent of those surveyed reported a "good recovery" from a single psychotic episode or from the remission period between multiple psychotic episodes (Jablensky et al., 1999: 10). Figures from the Royal Australia and New Zealand College of Psychiatry

DOI: 10.1002/aps

(RANZCP) reported that only around 50 percent of schizophrenic patients were achieving "social" recovery five years after the first episode (RANZCP, 2005: 4). These figures were not compared with what might have been the percentage of spontaneous recoveries during the same period – a test often leveled against reports of analytic success.

And so we might not be surprised to find that, when Luborsky formulated his summary,

> few…studies comparing treatments include dynamic treatments, but also that few include long-term treatments, and almost none of these are psychoanalytic therapies. Yet, psychoanalysis may offer patients special benefits as compared with other forms of psychotherapy. Their benefits may derive from the achievement of (a) greater insight, (b) greater depth of changes in personality, and (c) longer lasting results. (Luborsky, 2001: 588)

These points are made also by Fonagy and is, in part, what leads him to conceive of "practice-based evidence" as the successor to the "evidence-based practice". If one were to accept the evidence-based paradigm – or rather the need for empirically tested evidence of best practice – Fonagy suggests that the RCT be not the sole empirical test of successful outcomes. An additional range of empirical tests should be applied. Fonagy et al. (2005) argued that,

> The criteria that are used to determine what counts as evidence-based practice must themselves be empirically tested. Their specificity and sensitivity should be established against a variety of other public health criteria. (Fonagy et al., 2005: 2)

Having distinguished which treatments are evidence-based, he suggests that these treatment modalities then be tested against other treatments on a range of additional "independent but relevant indicators", which might include theoretical coherence, public health impact, and user/consumer acceptability, etc. He seeks thus to construct a bridge to the evidence-based paradigm. Psychoanalysis must be empirically validated from without the field of study. He tells us that Alan Kazdin (1998) "proposed the ideal outcome research program…but because it would require us to rethink our entire approach to outcome studies and evidence-base practice, it is unlikely ever to be implemented" (Fonagy et al., 2005: 6). This approach,

> Suggests that treatment research should begin with the identification of key dysfunctions associated with a disorder and the empirical demonstration of these dysfunctions in a sizeable proportion of the clinical group. Further a conceptual link must be established between a proposed treatment method and the dysfunctional mechanism hypothosized to underpin the disorder.…(Fonagy et al., 2005: 6)

By this means, the process of treatment could be correlated to the outcomes, leading to a manualization of treatment. And so we find ourselves returned to the problems of diagnosis and prognosis that were the concern of, for example,

DOI: 10.1002/aps

the Menninger Psychotherapy Project, and all the complications discovered in the process of that extraordinary research venture.[3] Fonagy states that this research cannot be easily replicated – probably because of the scale and complexity of such a project – instead, he suggests analysts resort to comparative studies using manualized therapeutic techniques conducted in pragmatic trials of statistically significant samples. This, he says, would be beneficial *within* the psychodynamic therapies even if not in comparison with cognitive-behavioral therapy, etc. And so at least some forms of therapy can be differentiated, and the suitability of different disorders for different treatments be identified.[4] He calls this closing the gap between "efficacy trials" and "effectiveness studies". The issue was raised recently in the psychological discipline by Henggeler et al. (1995, after Sarason, 1981) who advocated that, "the conditions of research from which your findings derive approximate the conditions of actual social living". The authors commented that, "The ongoing failure of university-based treatments to be used successfully in community-based clinical settings suggests that treatment researchers have not often followed the dictim" (Henggeler et al., 1995: 715–716). In this way one sees a rift not just between psychotherapy and psychology, but between academic psychology and clinical psychology; a divide in which psychoanalysis is but a small component.[5]

THE POLITICS OF EVIDENCE

After a career of engagement with practical and theoretical aspects of research, Luborsky finally confronts the sociological status of psychoanalytic engagement with the evidence-based psychology. He writes that, "the empirically supported treatment movement needs to be taken seriously, certainly scientifically, but also even politically" (Luborsky, 2001: 599). If psychoanalysts are to succeed in the maintenance and growth of their profession, they need to overcome their inertia on contemporary scientific matters and acknowledge the politics of a multidisciplinary mental health field structured in relation to a public health agenda that – at present – understands only the language of "evidence". To work towards overcoming the inertia, Luborsky notes five factors that have influenced – and continue to influence – the engagement of psychoanalysts in research (after Luborsky, 2001):

(1) *The availability of recorded psychoanalytic cases.* It is suggested that the unavailability of a set of recorded psychoanalyses has hindered objective evaluation of outcomes. However, there now exists a collection of 17 cases of complete and tape-recorded psychoanalyses as a research resource (Luborsky et al., 2001). Initial examination of these tapes has found that most of the cases demonstrated an improvement on the Health–Sickness Rating Scale (cf. Luborsky, 1962) as judged by independent clinician evaluations of the patient's state early versus late in the psychoanalysis.

DOI: 10.1002/aps

(2) *The negative impact on research of the reputation of psychoanalysis for deficient research backing.*

(3) *The positive impact of increased research funds and of the discoveries by psychoanalytic researchers.* It is argued that work by analysts in recent years has built a momentum of psychotherapy research: the evaluation of the benefits of long-term treatments is on the agenda of more psychotherapy researchers. A major facilitation of research has come in recent decades from the continued support of the Fund for Psychoanalytic Research of the American Psychoanalytic Association and the IPA. Furthermore, The IPA research training courses have been linked to the increased success of psychoanalysts in obtaining grant awards from funding bodies outside the analytic institutes (see Emde and Fonagy, 1997).

(4) *The virtues of methodologic pluralism.* Methodologic pluralism in scientific research is the catch cry of the new IPA education programs.

> It seemed quite clear that we need multiple methods and windows of observation in order to answer complex questions that are relevant for psychoanalytic work… Exploratory methods, aimed at discovery, can be distinguished from hypothesis-testing methods, in the context of confirmation… Systematic description is highly valued and needs to be verifiable. (Emde and Fonagy, 1997: 647)

(5) *The special virtue of the method of comparison of patient types.* There is also another very different methodological option used for psychotherapy research studies: instead of comparing outcomes of different kinds of *treatments*, outcomes are compared for different kinds of *patients* within single treatments. Such a within-types-of-patients design was well demonstrated in Beutler et al. (1991) and in other studies such as the Menninger Psychotherapy Project (see Wallerstein 1986). These studies have shown successful efforts to find patient-therapy matches that "increase therapy effectiveness" (Luborsky, 2001: 597–598). Patient-therapy matching has been a serious concern since the widespread introduction of psychotherapy in the US, of which Robert P. Knight, the first Director of Psychotherapy at the Menninger Clinic in the 1930s, was an early proponent and investigator.

In summary, Luborsky makes a cogent case to say that the task for analysts in research is that of quantifying research outcomes that have been investigated qualitatively over many decades.

CONCLUSION

Having surveyed some of the key issues in contemporary psychology as they affect the psychoanalytic field, there remains only to mention the promise of neuroscience and studies of the brain. Recently, University College London (UCL) and the Yale Child Study Center in association with the Anna Freud

DOI: 10.1002/aps

Center have introduced a "Masters in Developmental Neuroscience and Psychoanalysis". It is an exemplorary model of psychoanalysis engaging in innovative ways with mainstream mental health research.

Peter Fonagy writes,

> In the past we [the psychoanalysts] have been surrounded by scientific disciplines that were hostile to psychoanalytic ideas and clearly lacking in sufficient subtlety to enable discourse across disciplines, so we might have been correct in maintaining some isolation. However, in the light of rapid advances both in the biological and social science methodology, this isolation can no longer be justified… Future progress in these fields – for example, the use of brain-imaging as a measure of therapeutic change – is likely to benefit our discipline as long as we have maintained a sufficient presence in the therapeutic and scientific fields to be offered funding and opportunities for collaboration. (Fonagy and Target, 2002: 56)

Not surprisingly he looks to the advancing sciences:

> Drugs and psychotherapy both work to the extent that they do because they both affect the functioning of the brain… And diseases of the mind are unequivocally diseases of the brain. The outcome of psychotherapy therefore should be as easy (or easier) to measure in terms of brain function as in terms of behavior or subjective reports… But the brain is not the final frontier of our knowledge about the mind… Psychotherapy is the crystallization of the principle of psychical causation… without understanding mental disorder psychologically, it would be impossible to understand the self-evident social pathways to mental disorder. (Fonagy et al., 2005: 9)

The question remains whether it is even conceivable that researchers could find evidence sufficient to prove psychoanalysis ineffective and psychotherapy no better than supportive therapy. I am not convinced that, for any but a minority of analysts, there could be uncovered evidence of a kind that would convince them that they should resign their practice. However, it is the case that patient-therapy research and other innovative research models are likely to better fit patients to the best treatment modes, and it is very important that therapists take note of these innovations. Yet it also important to note that therapists have an intuitive sense of the success of their own approach, and that the success or otherwise of many courses of treatment is in large part measured inter-subjectively between the analyst and patient, not by third party interventions. The task then for analysis, as I think much of this paper has demonstrated, is a combination of fitting the evidence to the practice and fitting the practice to the evidence.

ACKNOWLEDGMENTS

This research was supported under Australian Research Council Discovery Project Funding Scheme (project no. DPO343944).

DOI: 10.1002/aps

NOTES

1 For an excellent account of the political and economic background to the reception of psychoanalysis and other health initiatives, see Vanderbos et al. (1992).
2 cf. Freud's opening remarks to his patient: "...say whatever goes through your mind...never forget that you have promised absolute honesty, and never leave anything unsaid because for any reason it is unpleasant to say it" (Freud, 1913/1948: 355–356).
3 See Koch (1959) for a highly sophisticated and diverse range of conceptual approaches to resolving some of these problems at the theoretical level.
4 On this question see: Wallerstein (1964/1975, 1986) and Sargent et al. (1968) on the complex challenges of differentiating types of therapy and matching them to presenting symptom clusters, let alone assessing the success of such.
5 There is some recent indications of a longed for rapprochement between the clinical and academic psychologies, see, e.g. Stricker (1997). But it is a divide that has existed since the mid-nineteenth century, see, e.g. Cronbach (1957) for his Presidential address on "The two disciplines of scientific psychology".

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DOI: 10.1002/aps

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Int. J. Appl. Psychoanal. Studies 5: 238–256 (2008)
DOI: 10.1002/aps