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EDITORIAL

Psychotherapy research: do we know what works for whom?

Peter Fonagy, PhD, FBA

Freud Memorial Professor of Psychoanalysis, University College London

Chief Executive, The Anna Freud Centre, London

Sub-Department of Clinical Health Psychology, University College London,

Gower Street, London WC1E 6BT, p.fonagy@ucl.ac.uk

Editorial

Peter Fonagy PhD FBA

Psychotherapy research rarely addresses the question of the 'fit' between person and treatment. In this issue, Watzke and colleagues highlight the scarcity of knowledge about the individual characteristics that may make someone suitable for a particular form of therapy [1]. Yet as clinicians we spend a considerable amount of time attempting to identify what might work best for whom. We often make research-based inferences from data about someone's 'suitability' for an intervention on the basis of (a) effect sizes calculated from meta-analytic sensitivity analyses, (b) post hoc analyses of variables moderating treatment response rates in randomized controlled trials that compare two treatments, and (c) individual differences in response rates in trials that include only treatment-no treatment randomizations. Even more problematic are inferences based on response rates from correlations of outcomes observed in cohort studies. In most contexts statistical power is insufficient for a meaningful examination of moderator variables. Post-hoc analyses are treacherous. Trials are costly and pertinent replications are rare. Predictors of good outcome in follow-along studies are not necessarily related to the treatment concerned. Much of what is known about relative treatment effectiveness is focused around major diagnostic conditions. Even gross moderators such as gender, age and co-morbidity are rarely the subject of systematic, statistically valid studies. Clinical judgment of suitability therefore continues to have an important role to play in this field.

But are such judgements worthy of the name? Mental health professionals tend to assume that they know what works for whom. Given that the vast majority of psychotherapists believe themselves to be above average in terms of therapeutic effectiveness [2], conviction about competence cannot be considered sufficient grounds for accepting such judgements. Personalised medicine is becoming extremely influential as we see treatments interacting with individual differences in tackling a disease process. Psychotherapy may be light years away from such sophistication, given our lack of understanding of the therapeutic mechanisms by

which treatments have their effects [3]. Yet, most therapists would claim to know intuitively what type of treatment is likely to lead to the best outcome, basing their judgements upon a constellation of demographic features, psychological capacities, clinical history including previous treatment response, contextual factors, personal goals in relation to treatment, and implicit naïve theories in relation to both treatment and treatment process that a particular individual presents to the referring clinician. Given this range of parameters, let alone the practically infinite number of combinations in which they might occur, the chance of clinical decision-making of this sort having practical value may seem quite small.

Notwithstanding the obvious barriers to informed judgement, the paper by Watzke and colleagues appears to find value in giving thought to what might work best for whom. At least in the case of psychodynamic therapy, there appears to be a valid if implicit clinical algorithm that identifies some patients as more suitable than others for this type of treatment. The findings suggest that this goes beyond superficial judgement of demography, although clearly demography does play a part, and points to indicators such as psychological-mindedness, a wish to target the treatment beyond symptom removal and a concern with the antecedents as well as the relational contexts of the presenting problem. These characteristics make psychodynamic psychotherapy an appropriate choice, at least for relatively short-term treatments assessed in terms of symptom distress six months after termination.

Two further issues cry out for commentary. First, that CBT does as well or better when patients are randomized to this treatment arm

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as when patients are systematically selected for it (pre-post *ES*: $d_{(STS)}=0.50$, 95%CI: 0.20-0.80 and $d_{(RTS)}=0.54$, 95%CI: 0.11-0.96). In other words, even

when psychodynamic therapy appears particularly appropriate, it is no more effective than CBT, but when individuals are randomly allocated to this treatment, the outcomes can suffer. By contrast, there appears to be no loss of effectiveness in randomly assigning individuals to CBT, even though presumably some of these individuals might initially have preferred another modality of treatment. On the face of it the best outcomes appear to be associated with random assignment to CBT. This is despite the fact that CBT in this arm loses some individuals who might be particularly suitable for a cognitive-behavioural treatment approach. The relative benefit CBT patients receive from being randomised as opposed to assigned to that treatment, despite the reduced number of particularly well suited individuals, would be consistent with systematic treatment selection favouring psychodynamic treatment because the implicit algorithm 'cherry picks' for PDT those patients who are somewhat more likely to improve in symptom distress in any case.

Second, patients randomly assigned to the psychodynamic arm of the protocol appeared to change little in terms of symptom distress, whereas those assigned to CBT showed substantial reduction of scores. One does not wish to steal the authors' thunder in relation to future publications, yet these findings, if robust, raise significant questions about a 'Dodo bird' verdict in relation to CBT and psychodynamic psychotherapy [4]. Unselected consecutive admissions appear better suited to a CBT approach, while psychodynamic therapy requires cases to be specifically chosen for this type of treatment. Certainly, while there is no difference between pre-treatment means, six months following the end of treatment the difference between the means has an *ES* of around half a standard deviation (bias corrected $d=0.52$, 95% CI: 0.22-0.82). This finding comes at a time when the movement behind gathering evidence for psychodynamic psychotherapy is gathering momentum to the point where it is hard to doubt the value of these methods when administered under reasonably controlled conditions [5, 6].

What makes Watzke et al.'s observations particularly important is the pragmatic nature of the trial - real clinicians working as they normally would with just a slight modification to their practice. There may be a minor complication to these findings in that the first level of randomisation was only partially successful (for whatever reason, patients in the systematic treatment selection group had significantly higher initial symptom distress scores) but the statistical control leaves only slight doubt about comparability of the extent of change. Unlike the frequently made claims that non-generalisable experimental methodology exaggerates claims for CBT, here naturalistic design evidently identifies weaknesses in PDT practice. Previous experimental findings showing comparable effects of PDT and CBT in RCTs appear not to be generalisable to an unselected sample of patients in a busy outpatient clinic. It is likely that the lacklustre mean treatment response to PDT in the RTS condition was composed of a number of positive treatment responses as well as some individuals whose response to PDT was one of worsening symptom distress. To put it bluntly, unless patients are pre-screened for suitability they may be harmed by PDT as practised, at least by some psychotherapists working at this clinic.

Is the need to be highly selective before referring for PDT the inevitable conclusion? I think not. It is my belief that the issue here is less of patient pre-selection and more of routine psychotherapy practice. The conceptual frameworks of PDT and CBT differ in that direct feedback about symptom change from patients forms the foundation of practice for clinicians practicing CBT but tends to be taken less literally by psychodynamic practitioners who are trained to focus on "process outcomes" (e.g. transference responses, insight) that are believed to bring about symptomatic benefit. Rather than making 'palliative' suggestions about the inappropriateness of the outcome measure used [which has been used in many successful PDT trials – 7] or the lack of appropriate training of the PDT practitioners involved [they had more intensive training than most of the therapists in our trials – e.g. 8] or insufficiency of treatment duration [average treatment was longer than in most trials of short-term

psychotherapy - 9] psychodynamic therapists should take these findings to heart as pointing to risks associated with the normal protocols for practising this (usually quite effective) therapy.

The study helps us focus on the need to optimize the effectiveness of routine PDT practice by (1) more rigorous specification of therapeutic methods, including loose manualisation of routine psychodynamic treatment procedures, (2) closer attention to symptomatic as well as process aspects of treatment response in the course of a treatment, (3) attention to the overall effectiveness of individual practitioners and the provision of supervision and support for those whose outcomes are regularly below average. One cannot help wondering if continuous (session by session) outcome monitoring by appropriate measures [such as the Schwartz, 10] might not have helped some of the patient-therapist pairs achieve better outcomes.

Given that patients largely approach therapy with the aim of receiving help in relation to symptom distress, there is little room for special pleading. We know that PDT is a highly efficacious treatment for a range of psychological disorders. However, when applied in the context of modern healthcare, uncritically implemented parameters of therapeutic psychodynamic practice inherited from the past century can yield disappointing results. Psychodynamic psychotherapists need to upgrade psychodynamic treatment protocols to address the needs of individuals who might otherwise appear to benefit only from a largely symptom-focused orientation.

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