Celebrating a Unique Achivement: Commentary on the LAC Study

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Much of the controversy in relation to the relative value of psychodynamic psychotherapy is fought in relation to short term psychodynamic psychotherapy, prototypically in the treatment of depression (P. Fonagy, 2015; Leichsenring et al., 2015). A recent review by Cuijpers and colleagues suggested that Hans Eysenck was correct in his evaluation of psychotherapy; when adjusted for several sources of bias, psychotherapy for depression had at best small effects on the outcome in terms of symptomatic benefit (Cuijpers, Karyotaki, Reijnders, & Ebert, 2018). But nothing is straight forward in this field. When four key methodological parameters were controlled for (adequate generation of allocation sequence, the concealment of allocation to conditions, the prevention of knowledge of the allocated intervention - masking of assessors - and dealing with incomplete outcome data - whether or not intention-to-treat analyses were conducted) and waiting list control studies were excluded psychodynamic psychotherapy based on five studies had moderate effect size (g=.43, 95% CI .10-.77) compared to CBT (g=.36, 95% CI .27-.45). Importantly, despite rumours to the contrary, in weeding out high risk of bias studies, the cull had least impact on the psychodynamic modality, leaving 50% of the original sample designated as 'low risk of bias'.

There is little point entering the methodological horse race debate. Real life, 'outcome based evidence' observational studies rarely yield meaningful differences between therapeutic modalities (Pybis, Saxon, Hill, & Barkham, 2017). In any case, the conceptual frame of reference for psychodynamic psychotherapy is not in a 10-15 session implementation, but in observations associated with specific benefit of long term interventions relative to short term less intensive psychotherapies. In this domain, the most recent meta-analysis has provided relatively encouraging findings (Woll & Schönbrodt, 2018, August 18). The meta-analysis reports 14 studies and an effect size of .24 on psychiatric symptoms (95% CI .06-.42) and a larger effect of .35 on social functioning (95% CI .11-.59). So there is evidence for long term psychoanalytic psychotherapy, but is it good enough?

The work of Professor Leuzinger-Bohleber at the Sigmund Freud Institute and Professor Martin Hautzinger from the University of Tubingen falls into the category of carefully conducted studies with minimal risk of bias yielding important results for clinical practice but without dramatically modifying our understanding of either the nature of depression or its effective treatment by two of the most commonly used therapeutic approaches. The study is unique in comparing long term psychoanalytic psychotherapy with cognitive behaviour approach offered for an effectively unlimited duration. I know of no other study where this comparison has even been attempted let alone delivered with magnificent clarity and rigor. The companion papers send a number of clear explicit and implicit messages to clinicians embattled with cases which present as severe or very severe on the depression spectrum. It is explicit that an approach which shows its commitment to the client by offering relatively long term therapy is worthwhile. To be able to confidently state to patients that in almost two thirds of cases (61%) persistence will yield remission is a highly significant contribution. It is implicit that psychoanalytic therapy is not the

only approach that will achieve long term benefit. Psychoanalysts have to give up their claim to be unique in providing a therapy with lasting benefit and have to be modest in terms of the length of time they to achieve benefit relative to other orientations. Nevertheless, they do as well as CBT therapists and there is no evidence of superiority associated with CBT even if the primary measure of outcome is likely to be slightly reactive (the BDI measure slightly favours the CBT approach because it focuses on cognitions as primary indicators of depression).

It is also important that in a complex design which intended to adopt a naturalistic stance of allowing a proportion of the patients to choose their preferred mode of treatment, being given treatment choice appeared to have little influence on therapeutic outcome. Randomisation certainly did not interfere with outcomes, so the study may be a poster child for showing no negative impact accompanies participation in a clinical trial. The findings also help overturn a prior unrepresentative result which suggested that psychodynamic psychotherapy, but not cognitive behaviour therapy, was restricted in its effectiveness to client groups which declared a preference for this mode of therapy (P. Fonagy, 2010; Watzke et al., 2010). It is interesting to note that many patients recruited for this study had a treatment preference. It is also interesting that notwithstanding the incessant drumbeat for CBT as the treatment of choice, many expressed preference for the psychodynamic approach when this was clearly described to them. However, the large number that had a preference in combination with the naturalistic design which allowed assignment by preference, had the unfortunate consequence of preventing the trialists from formally evaluating an equivalence effect in relation to those whio were successfully randomised to CBT or psychoanalytic psychotherapy. A limitation in one way, but it also made the study findings more generalisable to a real world setting where real patients with real depression have real preferences. The generalisability of randomised controlled trial methodology, when administered in its purest form, is undoubtedly exaggerated and the design is overvalued (P Fonagy et al., 2014). The external validity of RCTs has been repeatedly questioned (Cartwright, 2011; Cartwright & Munro, 2010) and the value of a systematically performed comparison of treatment modalities, balanced for preference represents a far more readily convertible currency of knowledge than a large scale demonstration of treatment equivalence might have been.

The second paper in this companion set is even more unique, and may be of even greater interest to many of the readers of this journal. The LAC study attempted to do something no other substantial RCT has taken on to my knowledge. Using a considerable (almost 50%) subsample of participants, the investigators applied what is probably the most robust measure of ‘structural change’ currently available (Cierpka, Grande, Rudolf, von der Tann, & Stasch, 2007) to assess, at least from one perspective, the mechanisms of change that might have underpinned the therapeutic work in the two arms of the trial. In some ways the findings are clear and unequivocal. At one year improvements could not be with any greater confidence assigned to structural change in psychoanalytic psychotherapy than in cognitive behaviour therapy. The authors reach out to Lane and colleagues (2015) who postulate emotional change to be a common factor across modalities. At three years, by contrast, the authors report positive structural change to be uniquely associated with symptomatic improvement in the psychoanalytic treatment group alone. Given that both groups improved, the pattern of findings

suggests that there may be unique therapeutic mechanisms at work for the two treatment arms. Obviously this would be a particularly interesting finding if a comparable in depth process focused outcome measure was available for those who were randomised or chose to be in the CBT arm of the trial.

Nevertheless, I suggest there is something extraordinary and important to celebrate here. For the first time a dose-response relationship has been systematically established and clinically illustrated in a treatment trial of any kind of psychological therapy. As perhaps the most sophisticated reviewer of this field, Alan Kazdin has pointed out, psychological therapies singularly fail to demonstrate meaningful relationships between theoretically purported change mechanisms and observed therapeutic improvements (Kazdin, 2007, 2009; Kazdin & Blase, 2011). The authors are therefore to be warmly congratulated for delivering such a unique and important finding.

The LAC study is perhaps the best, most carefully mounted comparison study of psychoanalytic study available to us to date. Amongst its strong features are well trained therapists fully committed to the orientation under scrutiny performing in a naturalistic context with real patients but following a well-scripted process orientated treatment manual. There are other studies where CBT yielded comparable results to psychodynamic therapy, but often in these instances what the treatments had in common was relatively poor performance compared to generally expected outcome (e.g. Driessen et al., 2013). In other studies where relatively good outcomes were observed, the comparison group was an unmonitored treatment as usual (P. Fonagy, Rost, et al., 2015). LAC does well on all important counts.

Do I have a wish list in relation to the next stage of analysing the results of this unique investigation? I guess I do. I, along with many others in the field, feel impatient about using heterogeneous clinical categories such as depression in organising our scholarship about what we know in relation to treatment effectiveness. Currently colleagues and I are actively engaged in trying to break down categories such as major depression into constituent hierarchically organised components. As we all know, comorbidity is the rule not the exception in mental disorders (e.g. Barlow, Sauer-Zavala, Carl, Bullis, & Ellard, 2014; Cummings, Caporino, & Kendall, 2014; Ormel et al., 2015). We wish to understand the clinical significance of the co-occurrence of diagnoses by statistically isolating aspects of individual presentations that belong to a general vulnerability to mental disorder, what Caspi and colleagues (2014) termed the p factor, from the spectral level or syndrome level influences. It would be intriguing to see if psychodynamic psychotherapy is more likely to impact at the level of general psychopathology while CBT has syndromal or spectral impacts. I would also be intrigued to know if our prediction of changes, at least at one year, are associated with changes in epistemic trust and consequent social and contextual changes which could account for the limited impact of structural change at this phase of the therapeutic process (P. Fonagy, Luyten, & Allison, 2015).

But then there are so many questions and time is the enemy of us all.

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