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Author's reply: I would like to make some comments on the points raised by Kirov & Korszun and Anderson & Haddad. They both cite evidence from continuation and maintenance studies, but this is likely to be more flawed than evidence from acute treatment studies. In studies of long-term treatment, patients who have responded to acute treatment are randomised to continue active drugs or to be withdrawn to an inert placebo. However, it cannot be assumed that the state of having had treatment withdrawn is equivalent to never having had treatment in the first place. It is known that there is a discontinuation reaction with all classes of antidepressants (Haddad *et al*, 1998). The symptoms of this reaction may themselves be mistaken for relapse, or they may unblind participants and predispose them to relapse because of fears of discontinuing treatment. This is likely to be a particular problem given that the initial sample of patients comprises people responsive to treatment who are therefore likely to have high expectations of the benefits of treatment.

In addition, the evidence on antidepressant effects and severity is complex. The majority of studies that show that increased efficacy correlates with increased severity are studies of out-patients. In in-patients, more-severe depression has been shown to respond less well to antidepressants than moderate depression does, independently of the presence of psychotic symptoms (Kocsis *et al*, 1990). In our meta-analysis we found no significant differences from placebo in in-patient studies (Moncrieff *et al*, 1998), which is in line with results from other large landmark in-patient studies such as the Medical Research Council study and the National Institute for Mental Health study described in my editorial (Moncrieff, 2002).

Finally, if the benefits of antidepressants are so obvious, it seems surprising to me that we have little evidence that the burden of depressive illness is reducing in line with the vast expansion in antidepressant prescribing. In contrast, long-term incapacity related to depression has been rising rapidly both in absolute terms and in relation to other conditions (Moncrieff & Pommerleau, 2000).

Haddad, P., Lejoyeux, M. & Young, A. (1998) Antidepressant discontinuation reactions. *BMJ*, **316**, 1105–1106.

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Talking about cognitive analytic therapy

Isaac Marks' review (Marks, 2003) encapsulates the reciprocal roles expressed in so much of the comparative debate in psychotherapy: dismissing : dismissed, contemptuous : contemptible. To contemptuously attack the review would simply be to continue the dance and to encourage further polarising responses. I have great respect for Isaac Marks' work and would invite him to join in a dialogue with cognitive analytic therapy. It was thought-provoking to consider the role of Pavlov in the developmental understanding of symptoms.

Cognitive analytic therapy has its devotees among therapists and clients. It is a tremendously human therapy where the strengths of cognitive theory and object relations theory have more recently begun to incorporate strikingly original ideas on human development, dialogue and the construction of interpersonal meaning from the Russian tradition. For many this represents an exciting evolution of thought concerning the nature of the psychotherapeutic relationship and the process of change in psychotherapy.

Cognitive analytic therapy has attempted to integrate the cognitive and the analytic as well as the dialogic Eastern approach to development with the reductionist Western scientific tradition. A more challenging task is to bring into dialogue the entrenched culs-de-sac of psychotherapy theory and their defenders. So, let's start to talk and engage in some positive role-play – valuing : valued, respecting : respected, giving : receiving.

Declaration of interest

J.H. is a member of the Association for Cognitive Analytic Therapy and has published in the field.

Marks, I. (2003) Book review: *Introducing Cognitive Analytic Therapy. Principles and Practice*, by A. Ryle & I. B. Kerr. *British Journal of Psychiatry*, **182**, 179–180.

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Preserve psychoanalysis from too much neuroscience

Professor Hobson (2003) argues admirably for the continued relevance of psychoanalysis in a mainstream psychiatric journal. But is his suggested rapprochement between psychoanalysis and contemporary neuroscience really desirable?

Contemporary neuroscience as illustrated by his example of 'mirror neurons' typically assumes an 'empiricist' worldview. In brief, imitation is assumed to be an acquired process in which information is abstracted from experience using associative learning. The current focus is on the anatomical location of the associative learning responsible for imitation (Rizzolatti *et al*, 2001).

In contrast, psychoanalysis derives from an older, rationalist philosophical tradition. It assumes the existence of both innate beliefs, such as persecutory anxiety, and distinct mental mechanisms, such as introjection or Klein's paranoid–schizoid position, that do not rely on associative learning.

These two philosophies have been in tension for centuries. One option is to make psychoanalysis more empiricist by downplaying the innateness and divergent mental mechanisms of classical theory. This is seen in attempts to incorporate 'theory of mind' deficits into a psychodynamic understanding of mental states (Fonagy, 1991).

But will associative learning form the secure basis for understanding the mind that empiricism proposed? Practical

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