As a result of the distrust of psychological approaches, studies of CBT (e.g. Kuipers *et al*, 1997; Sensky *et al*, 2000) have invariably recruited patients whose symptoms are 'resistant' to medication. The fact that these studies have still shown significant improvement over either a control intervention or routine care is testament to the greater benefits that might be demonstrated if the patients enrolled in research were representative of those in clinical practice targeted for psychological intervention.

In any case, surely the question is not which is more effective, but how both pharmacological and psychological approaches could be combined for greatest effect.

Declaration of interest

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Thank you for the debate on CBT and schizophrenia (Turkington/McKenna, 2003). I would like to make the following points.

First, CBT is not a single treatment – it contains many complex components and skills, and therapist variables must be an essential issue for careful evaluation as with all psychological therapies.

Second, befriending fares significantly better than 'treatment as usual' in much CBT research. McKenna dismisses this as placebo or 'special treatment'. The fact of such significant improvement from befriending says something very serious about treatment as usual. Why should those suffering from psychosis *not* receive special treatment? The finding points to the need for more consideration of the (poorly termed) 'non-specific factors in psychotherapy' – factors clearly not treated as sufficiently important in basic care in psychosis (Paley & Shapiro, 2002).

Third, in the Sensky *et al* (2000) trial quoted, CBT patients maintained their (significant) clinical improvement at follow-up, whereas the befriending controls fell back towards previous levels. It seems that CBT gives the patients a thinking structure to help manage some of their symptoms in the longer term.

Fourth, many people believe that you cannot treat persons with psychosis as if they were suffering from something such as diabetes, for which a single remedy like insulin might be sufficient. McKenna's pronouncement on randomised controlled trials is, therefore, open to serious questioning. The need adapted approach is the antithesis of the randomised controlled trial method. In the former, the treatment is individualised and intentionally different (qualitatively and quantitatively) from one case to another and may well change over time. A randomised controlled trial, equally intentionally, eliminates individuality in the treatment. Because the idea of relationships can be especially disturbing to patients with psychosis, psychological therapies can be seen by patients as threatening; therefore, the therapy has to be very carefully 'administered' – individually and flexibly.

Fifth, there are other outcome measurements at least as important as psychiatric symptoms. The experience of treatment is very important, as well as quality of life measurements. Turkington emphasises the high take-up rate of CBT, far higher than uptake of medication in psychosis.

Sixth, thank goodness for CBT, just one of several ways for practitioners to re-discover some tools that enable them to relate to patients with psychosis. McCabe *et al* (2002) show how uncomfortable ordinary psychiatrists are without such tools when engaging with patients when the latter want to discuss symptoms.

Seventh, CBT and psychodynamic approaches overlap to a degree, at least as practised by Turkington (Martindale, 1998; Turkington & Siddle, 1998). Much has changed in psychodynamic therapy since the flawed studies of old. Modern psychodynamic approaches to psychosis have a much more flexible technique in engaging patients, and a greater and broader appreciation of mental mechanisms in psychosis.

Finally, relationship approaches in psychosis need encouragement, support and research. All psychiatrists need basic training in engaging with patients with psychosis. Research indicates that befriending might be a good place to start, but it is clearly not so easy – as the outcome of 'treatment as usual' indicates.

Declaration of interest

B.M. is Chair of ISPS (International Society for the Psychological Treatments of Schizophrenia) UK, a network the main objective of which is to promote psychological approaches to psychosis (treatment, education and research).

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Efficacy of antidepressant medication

The debate between Parker and Anderson & Haddad (2003) neatly summarised contemporary thinking on the question of antidepressant effect. It was a pity, though, that they provided no discussion of any historical perspective. The wonderfully clear account provided by David Healy (2002), for instance, shows how the marketing tail of psychopharmaceuticals now often wags the entire dog. The process by which this came about has been gathering momentum since the early 1960s. Healy explores its various causes and corollaries

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in detail. It is not, he argues, due to any uniquely pernicious qualities of drug companies since similar trends can be seen in relation to some other types of therapy.

If this additional, temporal dimension had been taken into account, one suspects that Gordon Parker might have placed greater emphasis on one of the factors that he identified as contributing to the current situation: namely that '"depression" is currently modelled as a single entity, varying only in severity' (p. 102). The term 'depression' is thus semantically equivalent nowadays to 'abdominal pain', not to 'appendicitis' or 'peptic ulcer'. If trials of an antacid, say, were undertaken on patients selected for 'abdominal pain' the results obtained would sometimes be favourable, sometimes not. Debate over antacid usefulness would exactly parallel that over the effectiveness of antidepressants.

How did we get into this situation? It seems likely that a lot of the blame can be laid at the door of DSM–III (American Psychiatric Association, 1980), which explicitly aimed for reliability of diagnosis. Unhappily, there was an implicit downside. The state of the art in psychiatry, when DSM–III was under development, was such that reliability could be attained only at the expense of validity. Partly as a consequence of choices that were made then, this problem still remains. It is no good blaming the failings of clinical trials, the machinations of drug companies, the uselessness of antidepressants or reporting bias, for our predicament. The main fault lies in the consequences of a bad choice of diagnostic system, made by our predecessors for what seemed, at the time, good reasons. The remedy must lie primarily in seeing DSM for the hindrance that it is, and one day replacing it with a system that separates the 'peptic ulcers' from the 'appendicitises'.

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Integrity and bias in academic psychiatry

The illuminating discussion by Drs Healy and Thase (2003) focuses on the magnitude of the pharmaceutical industry's influence on academic medicine. However, this discussion needs to be taken a step further, and evaluated in relation to patient care. From my perspective, the central question is: 'Does the influence of the pharmaceutical industry on academia result in biased knowledge?' Professionals are charged with serving the best interests of patients/clients. In order to accomplish this, professionals need unbiased knowledge that can lead to an accurate risk–benefit assessment and serve to guide clinical decisions. If available knowledge is biased, decisions will be affected and clients will suffer accordingly. The frequently touted disclosure of potential conflicts of interest in academic publications is a small step in addressing the much more difficult question of whether existing knowledge is biased. Recognising potential bias is an initial step towards assessing and removing it from the collective knowledge used to make decisions in practice. For example, registering clinical trials is an approach to reducing publication bias (Dickersin & Rennie, 2003). Meta-analysis is an approach to removing bias from expert reviews of the literature (Beaman, 1991), although expert reviews still retain influence in the formulation of some practice guidelines (e.g. American Psychiatric Association, 1997). As the field moves more towards the implementation of evidence-based practice guidelines, the importance of removing bias remains central to providing optimal clinical care. If the extensive financial arrangements between industry and academia resulted in no bias to knowledge, I would probably agree with Dr Thase that no new policies are necessary to 'safeguard our integrity' (p. 390). However a recent systematic review and meta-analysis of evidence bearing on this question found 'strong and consistent evidence ... that industry-sponsored research tends to draw pro-industry conclusions' (Bekelman *et al*, 2003: p. 463). The question now becomes, 'What safeguards should be implemented to remove this bias from the knowledge that guides clinical practice (cf. Bodenheimer, 2000)?' Commitment to our patients' well-being requires that we act from this integrity.

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Good practice in publication of clinical trial results

As the name implies, ghostwriting is often hard to detect, so Healy & Cattell (2003) have made a valuable contribution to our understanding of this important subject by their measurement and thoughtful analysis of the practice. It is also refreshing to see such a balanced account which discusses both the benefits and potential dangers of ghostwriting.

Perhaps their most alarming observation is that the papers sponsored by the manufacturer reported 'universally positive results', which implies the existence of considerable publication bias. Such distortions to the published literature probably exist across all therapeutic areas and have been shown to distort the outcomes of meta-analyses (Tramèr *et al*, 1997) and therefore to have serious implications for evidence-based medicine (Melander *et al*, 2003).

Readers may be interested to know that guidelines have recently been published which call on pharmaceutical companies to endeavour to publish results of all clinical trials of marketed products (Wager *et al*, 2003). The guidelines also provide recommendations to ensure that professional medical (ghost)writers are used appropriately so that their contribution can be beneficial rather than harmful. The Good Publication Practice (GPP) for pharmaceutical companies guidelines have been publicly endorsed by several drug companies and communications agencies. Further details are available at http://www.gpp-guidelines.org.

Declaration of interest

E.W. is an author of the GPP for pharmaceutical companies guidelines. He also makes a living as a freelance medical writer, which sometimes involves ghostwriting.

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