declared. Why should a cognitive–behavioural therapy-trained researcher or anyone with a particular leaning not declare that interest? It is not complicated to state succinctly that there is a potential bias. It is simple to do and aids transparency. Let the readers decide! The authors' distinction between 'conflicts of interest' and 'perspectives of interest' is splitting hairs and appears pedantic and defensive. Declare, declare!

Declaration of interest

Peter Bruggen and I worked together in 1990 and have been good friends since then. I subscribe to www.healthyskepticism.org.

1 Cook CHC, Dein S, Powell A, Eagger S. Declarations of interest. *Psychiatrist* 2010; **34**: 259.

2 Bruggen P. Declarations of interest. *Psychiatrist* 2010; **34**: 259.

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Meaning-centred approaches: what about psychodynamics?

In Wallang's tour de force[1] (history of Western philosophy in four pages) arguing for a narrative-based approach to psychiatric consultation, there was a striking omission: nowhere was psychodynamic/psychoanalytic psychiatry mentioned. Yet this etiolation of psychodynamics underpins the aridity of diagnosis-focused psychiatry that he bemoans.

Psychodynamic approaches enlarge semiotic space in two main ways.[2] First, they bring into the field all the communications – verbal and non-verbal, conscious and unconscious – that arise between patient and professional, not merely stated symptoms. Wallang himself illustrates this via his 'noticing' his patient's diagnostically 'irrelevant' Taoist bedside reading; this brought into focus a different, non-pathological dimension of the patient's life. Second, they offer a set of developmental meanings which help understand how it is that this individual finds herself or himself in this particular dilemma at this particular juncture in her or his life. Wallang's 'personal meanings' are invariably illuminated by this developmental perspective. His last-ditch drug-addicted patient who found solace in the thought that there is 'motion in inertia' might be referring to a childhood experience of a depressed 'inert' mother, his own 'motions' (pleas for attention?, 'shitty' feelings?) towards her, and the later discovery of drugs as a short cut to assuagement of longing.

Was Wallang's lacuna tactical (don't frighten the horses) or technical (psychodynamics still not fully evidence-based)? Either way, despite this conspicuous absence, his piece was a welcome change from standard psychiatric journal fare.

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1 Wallang P. Wittgenstein's legacy and narrative networks: incorporating a meaning-centred approach to patient consultation. *Psychiatrist* 2010; **34**: 157–61.

2 Holmes J. *Exploring in Security: Towards an Attachment-Informed Psychoanalytic Psychotherapy*. Routledge, 2009.

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Author's reply I would like to address some of the points raised by Professor Jeremy Holmes in his letter.

He asks whether my omission of the psychodynamic approach was 'tactical' or 'technical'. It is the case that psychodynamic psychotherapy has been unable to demonstrate any convincing evidence supporting the explanatory basis of the psychodynamic approach. This does not necessarily mean that evidence cannot be found. It is a problem experienced in all science to differing degrees,[1] the question being: how do we derive scientific knowledge, how do we know that what we know is right?

These evidential problems are bound up with another question raised by Professor Holmes, namely the general validity of all 'meaning' statements. The history of psychiatry reveals the evolution of the meaning-centred approach. Porter & Berrios[2] detail its development: the confinement of reason during the Enlightenment, through the liberation of the 'hysterical' patient with Freud as 'interpreter'. An extrapolation of these developments ultimately presages the next phase of evolution: a reappraisal of what an acceptable interpretation of the 'patient voice' should be. Inevitably, there will always be a degree of interpretation; the question is how much interpretation is plausible without supporting evidence? The narrative method adopts a stance which attempts to liberate the patient perspective by laying down the fetters of possibly invalid interpretations which up until now have been lacking in evidence and may ultimately remain so unless we can design a process which demonstrates their validity as explanatory statements. Ultimately, the level of evidence demanded is dictated by the claims of a theory. The narrative approach is an adjunct to facilitate communication; it makes no claims to diagnostic or explanatory validity, unlike psychoanalysis or psychodynamics. The explanatory statements within psychodynamics are often stretched beyond the limits of plausibility in a search for meaning without any adequate supporting evidence. Narrative aims to liberate the patient's own voice from overly speculative interpretations, it promotes patient equality and transparency, valuing what helps the patient in their suffering.

My argument was not to be divisive or champion the pre-eminence of any one modality over another, be that biological, social or psychological. The jostling for authority between these camps is well known and in my opinion fruitless. My main aim in writing the article was to highlight the current dilemma we face as clinicians in trying to understand patient meaning, and argue (I hope) for a discussion about the integration of all strands of current learning leading to a comprehensive, multidimensional, meaning-centred approach. This would better reflect the complex aetiology of mental illness and surely help to create a humane working method which would promote a deeper understanding of our patients. It would also lead to the realisation that our patients are equal participants and allow us to move into the next phase of psychiatry, the overdue liberation of the patient's own voice, freeing them from any single interpretive or explanatory authority and allowing further recognition and hopefully alleviation of their suffering. The narrative approach is well equipped to facilitate this transition.

1 Ladyman J. *Understanding Philosophy of Science*. Routledge, 2002.

2 Porter RA, Berrios GE. *A History of Clinical Psychiatry: The Origin and History of Psychiatric Disease*. Athlone Press, 1999.

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Too much reality television

C. J. Jung, among others, commented that people cannot stand too much reality. Two recent admissions to our adult acute ward illustrate a related point regarding 'reality television'. Recent years have seen a marked increase in the quantity of such programmes, many of which contain content of a stigmatising nature.

The first case was a 24-year-old man who presented for the first time with persecutory delusions, including the belief that the hospital was a television studio. His family identified his appearance on a daytime talk show 3 months earlier as a contributory precipitant to this episode. During the programme the man had been exposed to a prolonged period of negative comments by the presenter in front of a live, as well as the TV, audience.

The second case was a 35-year-old man who had appeared on a talent show during which his audition performance had been severely criticised. He himself linked the subsequent deterioration in his self-esteem, and his feeling that people in his community saw him in a negative light, to the experience. His admission followed an episode of self-harm and he was admitted with predominantly depressive symptoms.

The British Medical Association has argued for the banning of boxing owing to the risks involved. I wonder whether the Royal College of Psychiatrists should take a similar view towards programmes that present public humiliation as entertainment.

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Variable practice complicates standard setting for PICU prescribing

Brown and colleagues[1] rightly point out that there are minimal reference data against which psychiatric intensive care units (PICUs) can measure their own performance. We welcome their study, which describes the clinical activity of seven such units in England. What is particularly striking is their finding that there was a huge variability in prescribing practices between the units studied, which reached statistical significance in 14 of the 16 prescribing measures described. This included a tenfold variation in the rate of combination antipsychotic prescribing ($P<0.001$) and a ninefold variation in the rate of high-dose antipsychotic prescribing ($P<0.005$). The authors acknowledged that the rate of high-dose prescribing may have been underestimated owing to potential calculation errors.

As pointed out by Brown and colleagues, PICU patients are at a particularly high risk of neuroleptic malignant syndrome. Therefore, it is difficult to justify deviating from the evidence base for the particular conditions being treated, and practices such as combination prescribing and high-dose prescribing should be avoided if at all possible.

We question Brown *et al*'s assertion that despite the wide variation in practice, and the potential calculation errors, their results are robust enough to serve as reference data for clinical governance purposes. Certainly, if these results are to be used as reference points, it needs to be clear which results should be used, i.e. the best results (e.g. 6% rate of combination prescribing) *v.* the combined percentages (23% rate of combination prescribing overall). Given the high variability between the units which participated in the study, perhaps other PICUs should be comparing themselves against the best results achieved, rather than the average.

A 6% rate of combination prescribing and a 2% (albeit an underestimate) rate of high-dose prescribing seem like standards that all PICUs should aspire to. Our experience is that such rates may well be achievable. We have achieved rates of 13% combination antipsychotic prescribing and 0% high-dose prescribing without any increase in our rate of violence (abstract in publication). We hope that the study performed by Brown and colleagues serves as a stimulus for further research and debate on the important issue of maintaining evidence-based practice, even when treating the most severely ill patients.

1 Brown S, Chhina N, Dye S. Use of psychotropic medication in seven English psychiatric intensive care units. *Psychiatrist* 2010; **34**: 130–5.

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CORRECTION

The dois for several items in last month's correspondence column were printed incorrectly; the online versions have been corrected post-publication in deviation from print and in accordance with this correction.

Divine intervention in mental health (letter). *The Psychiatrist* 2010; **34**: 258–259: doi: 10.1192/pb.34.6.258

Declarations of interest (letter). *The Psychiatrist* 2010; **34**: 259: doi: 10.1192/pb.34.6.259

Declarations of interest (authors' reply). *The Psychiatrist* 2010; **34**: 259: doi: 10.1192/pb.34.6.259a

BNF limits *v.* threshold dosing (letter). *The Psychiatrist* 2010; **34**: 259–260: doi: 10.1192/pb.34.6.259b

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