

stage through to full expression of the disorder. A problem here may be that the snapshots are too widely spaced at 18 months. In addition, the estimated ultimate rate of transition is still quite low, around 15%. The costs for the design, and hence the funders, the subjects and the research team themselves, are that a large sample size and an extensive follow-up period are required. The study is, therefore, expensive and labour-intensive. Farmer (1999), in her commentary, estimates that only half of those making the transition will have done so within five years and Johnstone (1999) acknowledges that her co-workers may not be around for long enough to reap the fruits of their labours.

Hence, the main strength of the study is the modern assessment using imaging and other methodologies from asymptomatic stage through to full syndrome, a latter-day, enhanced version of earlier high-risk studies. The most interesting finding in the study to date is the high functioning observed in a subset of the young people at risk. A final quibble is the notion expressed in the author's response (Johnstone, 1999) that those ultimately expressing the disorder were 'destined' to do so. This is too deterministic. We think that the expression of risk is more dynamic and that the group who ultimately express the phenotype is not fixed at the start of a prospective study like this. It may be more like 'musical chairs' with risk factors such as substance use and stress operating to select the final sample.

An alternative design which addresses these problems, known as the 'close-in' strategy (Bell, 1992), has been applied by our group (Yung *et al*, 1996, 1998a) to the challenge of predicting, clarifying and trying to delay or prevent the transition to psychosis in a high-risk sample. Building on the concept of indicated prevention (Mrazek & Haggerty, 1994), we focus on cases with early clinical features which are associated with a high risk (approximately 41%) of transition to psychosis within 12 months (Yung *et al*, 1998b). In a proportion of these, there is a first-degree relative with a psychotic disorder, while others are defined on subthreshold or attenuated psychotic symptoms alone. It is important to emphasise that over half of these patients do not develop psychosis, though they do have other axis I disorders. This design does not allow study of the process of transition from as early as the asymptomatic period, but it does seem to address the two key

weaknesses of the traditional high-risk approach.

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**Farmer, A. (1999)** Too early to publish results of high-risk subject research. Invited commentaries on: People at risk of schizophrenia. *British Journal of Psychiatry*, **174**, 554.

**Hodges, A., Byrne, M., Grant, E., et al (1999)** People at risk of schizophrenia. Sample characteristics of the first 100 cases in the Edinburgh High-Risk Study. *British Journal of Psychiatry*, **174**, 547–553.

**Johnstone, E. (1999)** Author's response. Invited commentaries on: People at risk of schizophrenia. *British Journal of Psychiatry*, **174**, 556–557.

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### Comparing ICD–10 and DSM–IV

**Sir:** We wish to comment on First & Pincus's (1999) editorial, which itself is in part a response to the earlier editorial by Andrews *et al* (1999) on the comparability of ICD–10 and DSM–IV classifications. First & Pincus make much of the fact that Andrews *et al* have not compared DSM–IV to the 1992 clinical guidelines version of ICD–10, which influenced the development of DSM–IV. Instead, Andrews *et al* compared ICD–10 to the Diagnostic Criteria for Research version published in 1993 (World Health Organization, 1993). First & Pincus point out that there are subtle differences between the 1992 clinical guidelines and 1993 diagnostic criteria for research versions of ICD–10. Although this is true, comparing DSM–IV to the 1992 clinical guidelines version of ICD–10 would not be appropriate, and is probably not possible, since the clinical guidelines are descriptive only and are not in the same operationalised format as DSM–IV. In order to compare like with like, the

1993 operational definitions version of ICD–10 must be used.

We disagree that in "the world of research, the DSM system of specified diagnostic criteria is the *de facto* standard". Although the DSM system from the third edition onward has had an important influence in improving the reliability of psychiatric diagnosis, it remains a *national* system. Despite the aims of its authors to be 'atheoretical' the DSM must inevitably reflect the current culture and ideology of North American rather than world psychiatry. On the other hand, the ICD–10 classification has been derived on the basis of field trials and debate internationally, and is the official classification for many countries. ICD–10 can arguably be considered to have greater universality in terms of its international acceptance and use.

We accept that the literature review process and reliability studies that accompanied the most recent revision of the DSM system probably represent certain advantages over the field trial method undertaken for ICD–10. However, the latter were carried out in the late-1980s (our own in Cardiff was undertaken in 1988), whereas DSM–IV was published nearly a decade later. One would hope for an improvement in methodology over such time. Also, it is considerably easier to arrange detailed and costly studies of a nosology in a single rich country, than to undertake such testing in many countries, with associated differences in economy, language, custom and religion.

Finally, a point we have reiterated many times, is that no classification as yet has proven validity, since the causes of most mental illnesses remain uncertain. Only when the aetiology of psychiatric disorders is properly understood, will it be possible to identify the most valid classification. Until then all classifications must be considered as working hypotheses. Thus, there is more than a hint of diagnostic imperialism in First & Pincus's assertion that DSM–IV should be the accepted standard, and that "the introduction of the ICD–10 criteria is the main source of confusion among researchers . . . [and that] many (if not most) of the differences seem to exist for no good reason". Others besides ourselves have written about the comparability and differences between operational criteria (Farmer *et al*, 1991a,b). The current arguments relating to whether DSM–IV is 'better' than ICD–10 merely induce a state of *déjà vu*, and hopefully will not need to be

revisited with the publication of ICD-11 and DSM-V.

**Andrews, G., Slade, T. & Peters, L. (1999)** Classification in psychiatry: ICD-10 versus DSM-IV. *British Journal of Psychiatry*, **174**, 3-5.

**Farmer, A. E., Jenkins, P., Katz, R., et al (1991a)** Comparison of CATEGO-derived ICD-8 and DSM-III classifications using the Composite International Diagnostic Interview in severely ill subjects. *British Journal of Psychiatry*, **158**, 177-182.

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**First, M. B. & Pincus, H. A. (1999)** Classification in psychiatry: ICD-10 v. DSM-IV. A response. *British Journal of Psychiatry*, **175**, 205-210.

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### Memory recovery among adults in therapy

**Sir:** Andrews *et al* (1999) base their study upon the British Psychological Society survey (Andrews *et al*, 1995) in which 4005 questionnaires were sent to chartered psychologists and 810 replies were received – a response rate of 20%. Of those who responded, 36% (291) had recovered memories in at least one client and 71% (208/291) identified themselves for future research, of whom 180 were interviewed by telephone. In this telephone interview 16/180 denied having a case of recovered memory, 9% could not remember the client, 10/118 did not have the case records, 17% declined to be interviewed and 2% could not be traced.

Thus, the study is based upon 118 from a population of 4005 eligible (2.9%) and they were found to be more likely to believe in the accuracy of recovered memories and to have seen satanic/ritual abuse cases.

The authors found that 36% of recovered memories were from the first five years of life and 9% fell within the period of infantile amnesia. Memory recovery techniques “seemed to be used more to help the clients to elaborate the memories than to facilitate their initial recovery”, but the authors do not comment on the doubtful validity of such techniques or, indeed, of early memories. They also observe that “it is difficult to know how representative

respondents were of professional psychologists who encounter recovered memories in their practice”; 2.9% of the original sample are hardly representative of anything other than the views of those psychologists questioned. They were discussing patients “40% of [whom] were no longer in treatment and notes were not consulted”, and it is not clear how systematic the notes were on the remaining patients.

In our experience many who have recovered memories have been involved with multiple therapists and often do not inform their current therapist of this. Some people have also read literature such as Bass & Davis (1988) and have been members of survivors' groups.

We are still appallingly ignorant of the way in which real memories of abuse are dealt with. Do some people push the events to the back of their mind and avoid distress by not thinking or talking about them, only to feel compelled to share their experience after some trigger? Do some people who create ‘false memories’ do so through autistic thinking in the isolation of distress? There are many unresolved issues in this area, which needs less debate and more research. The only certain thing is that memory cannot be relied upon without external verification. We trust that there is at least agreement that recommendations for good practice such as those published by the Royal College of Psychiatrists' Working Group (1997) should be observed by all clinicians.

**Andrews, B., Morton, J., Belorian, D., et al (1995)** The recovery of memories in clinical practice: experience and beliefs of British Psychological Society Practitioners. *Psychologist: Bulletin of the British Psychological Society*, **5**, 209-214.

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**Bass, E. & Davis, L. (1988)** *The Courage to Heal: A guide for Women Survivors of Child Sexual Abuse*. New York: Harper & Row.

**Royal College of Psychiatrists' Working Group on Reported Recovered Memories of Child Sexual Abuse (1997)** Recommendations for good practice and implications for training, continuing professional development and research. *Psychiatric Bulletin*, **21**, 663-665.

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**Author's reply:** Drs Brandon & Boakes' letter contains some misconceptions concerning our past and present research. The criticisms they raise involve the representativeness of our sample of therapists, and the validity of their reports. They suggest that the sample of interviewed therapists referred to in the paper constitutes 2.9% of our original sample and hence is unlikely to be representative of all 4005 ‘eligible’ British Psychological Society practitioner members from the original survey (we should add that the response rate in the original survey was 27% (Andrews *et al*, 1995), not 20% as stated). However, the 4005 practitioners were only ‘eligible’ in the sense that they were all sent the original questionnaire. Many will not have been working therapeutically with non-psychotic, non-organically impaired adult psychiatric patients, and hence the survey would not have been relevant to them. It is almost certain, therefore, that the response rate among respondents seeing the appropriate group of patients was considerably higher than 27%. The response rate among practitioners who had actually had recovered memory patients is likely to have been higher still, considering that the subject of the survey would have been of special interest to them. Without a 100% response rate to our original questionnaire survey, the number of such eligible practitioners can, unfortunately, not be specified with any greater precision.

Therefore, we did our utmost in the paper to examine the representativeness of the therapists we went on to interview, when compared with the rest of the eligible respondents who in the original questionnaire survey reported having had a patient reporting recovered memories in the previous year. We compared the original survey item responses of eligible respondents in terms of whether they identified themselves for research or not, as well as in terms of whether they were actually interviewed by us or not. Very few differences arose and these were fully discussed where relevant with regard to their possible impact on our results. Thus, it seems fair to conclude that our sample is in almost all respects representative of the 291 eligible respondents from the original postal survey. There is some uncertainty about whether the 291 respondents constitute a large or small proportion of the total number of British Psychological Society practitioners with patients reporting recovered memories. There is inevitably, at this stage in the research, a tension