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Making the most of interpreters

SIR: The editorials on the war in the former Yugoslavia by Deahl *et al* (*BJP*, April 1994, **164**, 441–442) and O'Brien (*BJP*, April 1994, **164**, 443–447) provide timely reminders of the extent of suffering and the long-term psychological effects of war, torture and other forms of state-organised violence.

The majority of war casualties are now civilian. Increasingly, psychiatrists in the UK are being asked to assess and treat refugees, be they political or medical, with traumatic stress reactions or other psychological difficulties. The assessment of such patients is made more complicated in the bi-cultural setting, and, as Deahl *et al* point out, difficulties in communication between interpreter, client and therapist can give rise to inaccuracies in assessment and effective treatment.

Vasquez & Javier (1991) point out five common errors in communication made by the untrained interpreter: omission, addition, condensation, substitution, and role exchange. Such errors are compounded when the survivor seeks refuge in a foreign country, often without family and friends, where problems such as social and cultural isolation, financial and housing worries and concern over immigration status add to distress and vulnerability.

Rather than being a hindrance, in our experience the use of properly trained interpreters or bi-cultural workers are key components in effective assessment and therapy. Although helpful, it is not essential that they have knowledge of mental health issues, but it is essential that they have a particular knowledge of the political and cultural background from which the survivor comes. It is often ignored that the interpreter should be socially, ethnically and politically acceptable to the survivor. The interpreter should not merely act as translator, but as a cultural bridge between therapist and client (Turner, 1992). They can provide insight into the cultural significance of what is being said and into the way in which emotions are expressed, both verbally and non-verbally. Local belief systems can be explored, and knowledge of the prevailing political and economic environment can contribute to the overall assessment. In our clinic, we encourage the interpreter to explore developing themes during the session and not to rely exclusively on the therapist to supply questions. With careful supervision and training, the interpreter becomes more skilful at probing for relevant information. We would encourage the use of trained interpreters as links between two different worlds, not merely as translators. In this way, patients' needs will be met more effectively.

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Prodromal symptoms of schizophrenia

SIR: There are two possible reasons for Malla & Norman's failure to demonstrate prodromal symptoms in the majority of their schizophrenic subjects (*BJP*, April 1994, **164**, 487–493). Firstly, their definition of a prodromal symptom is rather restrictive. While purporting to examine only 'non-psychotic' symptoms, they have omitted disturbances of action, cognition and perception which have been reported prodromally elsewhere (e.g. Gross, 1989). Although psychotic-like symptoms such as these are included among the 'basic symptoms' of schizophrenia in the German literature, Gross (1989) considers them to be non-specific, occurring also in schizoaffective psychoses, 'cyclothymic depressions', and organic brain disease. Similar phenomena have been reported as prevalent within normal populations, with follow-up showing them to predict a range of psychoses and not exclusively schizophrenia (Chapman & Chapman, 1987). Finally, basic symptoms may relate more closely to the development of particular psychotic (e.g. first-rank) symptoms (Klosterkötter, 1992), and their lack of specificity to schizophrenia does compare with that of the first-rank symptoms. Therefore, in view of their prevalence, lack of specificity to schizophrenia and non-psychotic (although psychotic-like) nature, such phenomena fulfil Malla & Norman's criteria for prodromal symptoms. It

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would appear premature to exclude them from research into the prodromal period.

The second issue relates to their sampling interval of one month, which they justify with reference to the literature. Birchwood *et al* (1992) have reviewed such studies, and more frequent measures have often been used. They report that up to 50% of schizophrenic subjects studied have been found to progress through a prodromal period to psychotic breakdown within four weeks. Many such prodromes would have been missed had a sample interval of one month been used.

Frequent prospective measures of a wide range of phenomenological experience are required before one is able to conclude that "a large proportion of psychotic episodes appear to occur without identifiable prior prodromal symptoms."

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High-dose antipsychotic medication

SIR: As a rehabilitation psychiatrist, I am frequently required to manage patients who are discharged from acute wards on doses of neuroleptics which are multiples of those I have been accustomed to use. I therefore initially welcomed the consensus statement on the use of high-dose antipsychotic medication (*BJP*, April 1994, **164**, 448–458).

However, the consensus panel's advocacy of rapid dose reduction after acute treatment is based on too simple a model of courses during the recovery phase (Drury, 1992; Weiden *et al*, 1993) and is supported by a mistaken reference to Cookson (1987), who deals with relapses after a 50% reduction in high doses, and not on lower than routine doses as stated.

My own experience is of relapses, during gradual dose reductions in depot medication, at dose levels much higher than those on which the same patients were previously maintained well for long periods before admissions caused by medication refusal. I am puzzled by this pattern and wonder whether some form of rapidly developed drug tolerance may be involved (Sramek *et al*, 1990) or whether there are other so far unrecognised factors.

It is misleading and indeed potentially dangerous to include recommendations in consensus statements without either the clinical or the research data on which to base them. I would suggest that the consensus panel now systematically gathers and collates information about the effects of different strategies used by clinicians during this phase of treatment, as an initial step which may need to be followed by controlled trials.

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SIR: As psychiatrists in the adolescent subspeciality, we question the basis on which two main assertions are made in the consensus statement. First, it is asserted that "the natural history of early-onset psychosis is for the first few episodes to remit spontaneously". This corresponds neither with our clinical experience, nor with recent research studies (e.g. Werry *et al*, 1991; Gillberg *et al*, 1993). Typically, schizophrenia with an onset in adolescence follows a course similar to that in adults, and spontaneous remissions are rare.

Secondly, it is asserted that "High-dose antipsychotic medication should rarely be necessary in children and adolescents". Anecdotal clinical experience does not bear this out. Psychotic disorders in children and adolescents are not uncommonly refractive in their response to antipsychotic medication at standard doses (Green *et al*, 1992), and adolescents may tolerate adult doses with less risk of adverse side-effects (Garralda & Ainsworth, 1987).

While we welcome the general spirit of the views expressed – it is important not to overmedicate children and adolescents, and expert treatment is always important – the views appear to us to be flawed in their details. Since no references are

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