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.

COOKSON, I. (1987) The effects of a 50% reduction of cis(z)-flupenthixol decanoate in chronic schizophrenic patients maintained on a high dose regime. *International Clinical Psychopharmacology*, **2**, 141–149.

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SRAMEK, J. J., GAURANO, V., HERRERA, J. M., *et al* (1990) Patterns of neuroleptic usage in continuously hospitalised chronic schizophrenic patients: evidence for development of drug tolerance. *DICP, The Annals of Pharmacotherapy*, **24**, 7–10.

WEIDEN, P., SCHOOLER, N. R., SEVERE, J. B., *et al* (1993) Stabilisation and depot neuroleptic doses. *Psychopharmacology Bulletin*, **29**, 269–275.

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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

provided for the assertions described above, we remain uncertain about what they are based on. Since these views are part of the consensus statement, which presumably will be used as a benchmark for clinical practice, for second opinions, and so on, this is a serious matter. In its present form the consensus statement is profoundly misleading in its comments on adolescents and we urge that it be revised in the light of current research and informed clinical opinion.

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WERRY, J. S., MCCLELLAN, J. M. & CHARD, L. (1991) Childhood and adolescent schizophrenia, bi-polar, and schizo-affective disorders: a clinical and outcome study. *Journal of American Academy of Child and Adolescent Psychiatry*, **30**, 457–465.

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Epilepsy in Down's syndrome

SIR: McVicker *et al* (*BJP*, April 1994, **164**, 528–532) describe the associated features of epilepsy in adults with Down's syndrome. In a similar study of a hospital population of people with Down's syndrome in Bristol, we found some markedly similar as well as contrasting results.

In a population of 43 patients with genetically proven full trisomy 21 (23 women, 20 men, mean age 51.5 years), 11 (26%) had epilepsy, which is the same percentage as in McVicker's hospital subsample. Other similarities were the dramatic increase in prevalence of epilepsy with age, being 38% for those over 50 years and only 6% for those under 50 (c.f. McVicker's 46% and 7%) and the strong association between epilepsy and dementia, with eight of the ten epileptics over 50 years dementing, and only two patients showing marked functional decline without evidence of epilepsy.

In contrast to McVicker, only 2 out of 11 epileptics (18%) had clearly secondary generalised seizures (McVicker reported the "majority" to have them) and only 6 (55%) showed paroxysmal features on electroencephalography (c.f. McVicker's 80%). The commonest seizure type was generalised tonic–clonic, but notably three had myoclonic, one atonic, and two a mixture of absence and tonic–clonic seizures. This mix of seizure type explained our finding that, as in McVicker's study, sodium valproate was the most commonly used anti-epileptic medication. I would therefore question the McVicker group's assertion that, on the basis of seizure type, carbamazepine is in general a more logical choice of anti-epileptic.

In the Bristol study, neurological examination was done on all patients except two who refused. A striking feature was the high prevalence of clinically increased muscle tone (19 out of 43 (44%)) in a group that is typically hypotonic. Prevalence increased with age, with no patients below the age of 40 years and 11 of the 14 over 60 years (78%) showing increased tone. Hypertonicity appeared to be associated with dementia and epilepsy: only one dementing and one epileptic patient had normal muscle tone, and all patients with both dementia and epilepsy had increased tone. This raises the possibility that increasing muscle tone is an important precursor of the dementia complex in Down's syndrome. A prospective study with more precise measurement of muscle tone is needed to evaluate this hypothesis.

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Defeating depression in Zimbabwe

SIR: Abas *et al* (*BJP*, March 1994, **164**, 293–296) describe how mental health research and service