

outcome, because the extent to which the patient's attitude can determine his own outcome needs bringing forward more emphatically.

However, their research requires to be put into perspective. They start their presentation with a quotation from Hooley & Teasdale (1989) that "it is surprising that no study has sought to obtain data directly from patients themselves concerning their perceptions of criticism from family members" (p. 230). They also continue in their paper to say that "Patients' perceptions of their key relatives have only recently been studied, and it now appears that they may also have prognostic significance".

This in fact is not correct. Since the 1970s Scott and his co-workers have published a number of reports on their research into specific factors in the dyadic and triadic relation between schizophrenic patients and their parents which relate to outcome (Scott & Casson, 1970; Scott, 1973; Scott & Alwyn, 1978; Scott *et al.*, *Journal*, July 1993, 163, 62–68). They used the Family Interpersonal Perception Test (FIPT), a self-rating questionnaire containing terms ranging from the very negative and critical (such as emotionally inadequate, interfering), to the very positive (such as secure, self-confident). The FIPT is scored by the patient and parents at the same session. They score how they see themselves, how they see each other, and how they expect to be seen by each of the others. From the scoring, factors were identified which have a well defined relation to outcome.

It is interesting that the essential findings from the research of Scott *et al.* and Lebell *et al.* have much in common. In both a poor outcome is associated with the patient expecting a negative view from their relative(s), and with the relative(s) taking a negative view of the patient. In both there is a correlation between how the relative sees the patient and how the patient thinks he/she is seen (indicating that the patient has an accurate awareness of the relation), although in Scott *et al.*'s studies this is so only for poor-outcome patients. Thus both groups of researchers give the patient a key role in influencing his/her own outcome, with the patient's scoring predicting it better than that of his/her parents (Scott *et al.*, *Journal*, July 1993, 163, 62–68).

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Down's syndrome, dementia, and superoxide dismutase

SIR: Drs Dickinson & Singh (*Journal*, June 1993, 162, 811–817) discuss the possible relationship between cognitive decline and raised mean cell volume (MCV) in people with Down's syndrome (DS). We recently investigated 92 people with DS (Mean age 44.2 years, range 19–72, s.d. 12.3) for an ICD-10 psychiatric diagnosis for dementia in Alzheimer's disease with early onset and for a diagnosis of a depressive episode. All cases were investigated for raised MCV (> 98 fl, normal range 80–98 fl).

For an association between raised MCV and clinical dementia to be valid, the changes in MCV must be due to increasing age *per se*. For our sample, no statistically significant association was found for these two parameters (Pearson's coefficient = 0.448).

Twenty-two subjects (mean age 54.0 years, range 42–72, s.d. 8.09) fulfilled ICD-10 criteria for dementia. A statistically significant association, using χ^2 tests, between the diagnosis of clinical dementia and a raised MCV was found ($P=0.01$). Two patients fulfilled ICD-10 criteria for depression, but neither had raised MCV values.

We support previous findings (Lansdall Welfare & Hewitt, *Journal*, April 1986, 148, 482–483) of an association between intellectual deterioration in people with DS and macrocytosis, and tentatively rule out that changes in MCV are secondary to mental illness *per se*.

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Home-based acute psychiatric services

SIR: I read with interest the two papers by Burns *et al.* (*Journal*, July 1993, 163, 49–54, 55–61) on a controlled trial of home-based acute psychiatric services.

At Dingleton we have routinely seen out-patients at home, preferably with a co-therapist for 25 years. Up until now this model has not been evaluated in a well controlled trial, although several new services have made favourable comparisons between themselves and previous or adjacent hospital-based services. Punukollu (1991) reported reduced bed occupancy after introducing a crisis intervention team to one of the sector teams in Huddersfield. Pullen & Gilbert (1985) and Dean & Gadd (1989) reported fewer admissions if the initial emergency assessment was done at home rather than hospital.

Home assessment was introduced by Querido in Amsterdam in the 1930s in an effort to save money by reducing admissions (Querido, *Journal*, 1968, 14, 293–302). Burns *et al* appear to have given further support to his claims by showing that home-based services are cheaper and reduce bed use. It is important to remember that Querido felt that there were other advantages in having the first assessment at home where “a picture is unfolded which I am convinced never can be obtained in any other way.” This theme is repeated throughout descriptions of home assessment services and there is a general belief that home assessment allows a better assessment of social and family circumstances. All the home-care studies quoted by Burns *et al*, with the exception of Dean & Gadd (1989), involved initial contact with the patient at hospital. For example, Muijen *et al*'s (1992) patients were randomised after initially being assessed as needing admission in the Maudsley 24-hour walk-in clinic. This is a home-treatment rather than a home-assessment service and I feel it is important to emphasise the difference.

Home assessment also improves access to care. Dingleton's failure-to-attend rate runs at around 5%. This is similar to the 7% in Burns *et al*'s experimental group while their 25% control-group rate is similar to rates in other hospital-based services reported recently. Dingleton's success can no longer be dismissed because it has not been systematically evaluated.

DEAN, C. & GADD, E. (1989) An inner-city home treatment service for acute psychiatric patients. *Psychiatric Bulletin*, 13, 678–669.

MUIJEN, M., MARKS, I., CONNOLLY, J., *et al* (1992) Home-based care and standard hospital care for patients with severe mental illness; a randomised controlled trial. *British Medical Journal*, 304, 749–54.

PULLEN, I. & GILBERT, M. A. (1985) Description of an extramural service for psychiatric emergencies. *Health Bulletin* (Scotland), 38, 163–166.

PUNUKOLLU, N. R. (1991) Huddersfield (West) crisis intervention team: four years follow-up. *Psychiatric Bulletin*, 15, 278–280.

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Small babies and schizophrenia

SIR: The story of obstetric complications and schizophrenia continues to intrigue. The finding of McNeil and his colleagues (*Journal*, April 1993, 162, 517–523) that preschizophrenic babies had small heads on lean bodies is, on the one hand, a curious echo of Kretschmer's link of asthenic body build to schizophrenia, but on the other it is further evidence pointing to an anomaly of early development.

McNeil *et al* could find no association between reduced head circumference and documented pregnancy complications. The problem (as ever) is one of definition: reanalysing the stratified data using our operational scale (Lewis *et al*, 1989) yields a significant association ($\chi^2 = 13.2$, 2 d.f., $P < 0.005$).

McNeil *et al*, comparing 70 schizophrenic patients with a matched control group, found both birthweight and head circumference to be significantly smaller in the preschizophrenic group. Repeating their analysis after removing 'preterm' infants, they found that only head circumference was significantly smaller and then focused only on head circumference differences between the two groups.

There are problems with this approach. Firstly, it is unclear what their preterm cut-off was. They excluded all infants born “at least two weeks before term”. If term means 40 weeks then this could mean that all infants of 38 weeks or less were excluded. This is very much above the accepted preterm cut-off and appears fairly arbitrary. This cut-off also meant that twice as many preschizophrenic as control subjects were excluded.

The choice of body length as the “body size control variable” is also problematic. Body length is notoriously difficult to measure at birth with wide variations reported between different people measuring the same infant. In addition, body length was the only measure which was not significantly smaller in preschizophrenics compared with controls on paired *t*-tests, and it is thus not surprising that preschizophrenics' head circumferences remained smaller after controlling for body size with a measure that was not different in the two groups. It would seem to be more appropriate to use birthweight, a measure which is reliable, was significantly smaller in the preschizophrenic group, and which has been shown in other samples to be smaller in preschizophrenics (Lane & Albee, 1966; Woerner *et al*, 1973).

Potentially the most informative approach would be to convert the absolute values of both head circumference and birthweight to Z-scores, standardised by charts of head size or birthweight. One could then compare the various birth measures in the entire sample, and by looking at the head circumference:birthweight ratios, could determine whether