age when dementia is uncommon. In two studies, when the authors' rates of severe and moderate dementia are added together, the rates increase to 5.3% and 7.7%, the latter being higher than any other rate in the Table. In the study by Maule *et al* (1984) the rate of severe + moderate dementia is only 2.9%, but the *overall* rate is 8.6%, which is higher than the overall Newcastle rate of 6.2%.

This leaves only the London–New York cross-national study of Gurland *et al* (1983), in which the London rate of 2.5% compares with the New York rate of 5.8%, a striking difference for which there is no ready explanation; but as the authors point out, replication is needed. Finally, in the overall prevalence Table, the Melton Mowbray survey rate of 4.5% in a population aged 75+ is lower than expected (Clarke *et al*, 1986), but is based on one brief questionnaire.

There seems to be no good reason to think that dementia among the elderly at home is appreciably rarer than the Newcastle rates of 5–6%; in Newcastle, institutions added less than 1% to the rate, but the ratio of cases at home and in institutions will vary. In Finland, for instance, *severe* dementia is found in 3.8% of people aged 65+ at home and in 6.7% when institutions are included (Sulkava *et al*, 1985). The problem is that in the community the range of cognitive impairment is wide and degrees of severity are not yet adequately defined.

In fact, Kay elsewhere (Kay, 1972) reported the Newcastle rates as 6.2% ('severe') and 2.6% ('mild'). This just goes to show the state of confusion the grading of dementia is in.

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History of Depressive Disorder

Sir: The recent article by Berrios (*Journal*, September 1988, **153**, 298–304) suggests that depressive disorder became conceived as an emotional disorder in the 19th century. Not all the descriptions from that time would concur with this. Those of Griesinger (1867), Lewis (1889), and, a little later, Mendel (1908) suggest that a disturbance of energy may be the basis of the condition.

My own personal experience of the disorder over a period of 20 years and objective view of it during the past 10 years in general practice has led me to the conviction that depression is primarily a disorder of energy, both qualitative and quantitative, with mood changes being secondary. I favour the term dysenergia, which was used by Dioscorides in the 1st century AD, and which is no longer extant, to describe this.

In the early 20th century, when the term 'affective disorder' was gaining usage, caution was nevertheless exhibited by some writers. Craig (1912) observed that, "as with other disorders the mistake was made of naming the disease according to its most prominent symptom"; Bleuler (1923) wrote: "the disturbance of the affect represents merely the most conspicuous symptom of a general transformation of the psyche that cannot as yet be comprehended".

Over the past 40 years the idea that depression is a single system disorder of mood has become increasingly accepted. The biochemical evidence is conflicting, and I can find no phenomenological evidence to support it. I wonder if it is a long-standing assumption which needs examination.

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Supportive Analytical Psychotherapy

Sir: Holmes (*Journal*, June 1988, **152**, 824–829) proposes a carefully planned admixture of dynamic and supportive therapy which I suspect could only be

https://doi.org/10.1192/S0007125000225361 Published online by Cambridge University Press

carried out by an extremely skilled and experienced practitioner. As he points out, supportive psychotherapy is most often carried out by the multidisciplinary team rather than specialist psychotherapists, and often there is no supervision or training available. An inexperienced therapist might be able to practice the distributive psychotherapy referred to by Holmes, that is, to spend a part of the session questioning and history-taking, and the rest in a more passive, listening and understanding mode. However, judging the niceties of when to draw attention to transference issues, or at what point to challenge instead of contain the patient, are no matters for a novice therapist, especially when faced with the type of patient thought to require supportive rather than dynamic therapy – in other words, the more damaged and vulnerable patient.

I suspect that Dr Holmes' own training and skills are not shared by the majority of those medical and non-medical practitioners of supportive psychotherapy who might well come to grief if they attempt to blend supportive and dynamic features in the way he suggests.

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Ethnic Density and Schizophrenia

Sir: It is doubtful if 'ethnic density', as determined simply by population size within an area (especially one as large as a Regional Health Authority), can have any useful meaning. This measure used by Cochrane & Bal (*Journal*, September 1988, **153**, 363–366) may be taken less as a determinant of 'density' than as denoting the presence or absence of a particular community within the area. The daytime density of psychiatrists in Belgrave Square is high compared with Eaton Square, but this tells us little about the individual motivations and psychology of these particular individuals, or their patterns of settlement within the Square.

Minority groups are hardly free to 'diffuse' throughout an area for reasons purely associated with individual psychopathology, as contrasted with external constraints associated with a racially biased provision of housing and employment. The actual pattern of distribution within a smaller local area may be a more significant indicator of those demographic factors possibly associated with psychopathology, such as 'social drift', group cohesion, and community support, let alone nosocomial factors which we now suspect account for much of the differential 'pathology' between ethnic groups.

To take the case of Nottingham: the 'foreign-born' community has settled predominantly in the inner-city areas where it also has the highest rate of schizophrenia, arguing against the ethnic density hypothesis of Faris & Dunham (Giggs, 1986). While higher rates of schizophrenia for each group taken separately are found among European and South Asian migrants in the outer-city council estates than in 'inner-city, low status' areas, the highest rates among West Indians occur when they live in the 'inner-city, high status' areas. For each of the three groups, however, the highest rates of settlement are in the inner city. Thus the ethnic density hypothesis seems to fit Europeans and South Asian migrants, but not West Indians.

It is in the specific pattern of settlement within a particular area, with all its political, economic, and psychological constraints, that explanations of differential pathology and access to services can be found. While I am sure this was not the intention of the authors, the bald 'ethnic density' argument by itself comes perilously close to ethological and thus biological explanations (e.g. Esser & Deutsch, 1977).

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Treatment of Suicidal Manic Depression

Sir: I read with great interest the paper by Schou & Weeke (*Journal*, September 1988, **153**, 324–327). In their guidelines for improving suicide prevention in manic depressive illness they state that "continuation or prophylactic treatment with antidepressants should presumably be carried out with full therapeutic dosage". They base this argument mainly on their finding that 13 patients who were treated prophylactically with sub-therapeutic doses of antidepressants committed suicide. However, 10 patients who received full therapeutic doses of antidepressants also committed suicide. In Modestin's (1985) study too, 21% of the patients had received full therapeutic dosage, compared with 26% who were

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