

### DEAD SOULS: CRISIS INTERVENTION IN GERIATRICS

DEAR SIR,

The claim in the paper by Dr Ratna (*Journal*, September 1982, **141**, 296–301) to practise psychogeriatrics with fewer beds and with a lower mortality would, were it true, be of great significance. We therefore examined his figures closely, but first may we comment on some other parts of his article.

We tried not to be put off by tendentious phraseology, such as repeated claims that the author, by contrast with others, operates a “socially orientated” service rejecting a “mainly medical model”. Since none of these phrases is defined in a way that would permit testing, they are best disregarded. But one cannot disregard that the paper offends an important principle of evaluation of services for the elderly—that claims to use fewer resources in any one part of the inter-dependent system are always suspect when an equally detailed review of the effect on the other parts is not reported. A psychogeriatric service which is known to be unwilling to admit is likely to cause referrers to turn from the beginning to the local geriatric and social services (on referrals to which no data are presented)—just as a gynaecologist who is well known to be “hard” on abortions, can usually rightly claim that he gets little demand for them.

We note also in passing that no evidence at all was presented to support claims that “symptoms of depression and dementia were often altered or reversed without recourse to drugs”—presumably the claim is of some special success in this respect, for reversal of symptoms without drugs is hardly in itself news.

We examined therefore the figures and the author’s use of them, and the comparisons which he makes with other reports. We find serious errors in calculation, quotation, and interpretation. Since the matter is of some importance for public policy in this field, we hope you can find space for our comments. We find, in short, that the author’s claims are in part not established, and in part simply wrong.

The rates of admission during the follow-up period which the author presents in Tables III and VI have been incorrectly calculated. The rate of admission over

the whole two year period shown for Barnet has been calculated with members of the cohort who died *included* in the denominator. The Chichester and Salisbury two year rates of admission have been taken from Sainsbury’s (1965) paper in which the denominators *exclude* those who died during follow-up. The “N” values Dr Ratna shows in Table III are wrong and should read 48 and 19, not 85 and 34. If the author had re-analysed the Chichester and Salisbury data in a way consistent with his handling of the Barnet data he would have included dead patients in the denominators. The admission rate for Salisbury would then be shown not as 79 per cent, but as only 44 per cent. This approach is clearly not the best, as dead patients are not “at risk” of admission to hospital and should be excluded from the denominator when it is no longer possible for them to appear in the numerator; but for claims based on comparison like must be set against like.

This error is serious enough and its correction substantially alters the results shown. However, an additional inconsistency further confounds the results. Sainsbury *et al*, when they presented their data on patients ever admitted during the two year follow-up counted not just admissions to mental hospital but also admissions to nursing homes and other institutions. It is not possible to distinguish between the two types of admission from the data provided by Sainsbury on Chichester (in Salisbury all admissions were to mental hospital). In order to compare the three services Dr Ratna should have counted admissions to Part III accommodation in Barnet, as well as admissions to mental hospital.

In the Table below we show an “ever admitted” rate for Barnet calculated in the same way as the Chichester and Salisbury rates.

We have assumed that the 42 Barnet patients admitted to mental hospital or Part III accommodation on referral and the seven (5 per cent  $\times$  141) subsequently admitted to mental hospital died at the same rate as the rest of the Barnet patients and have accordingly excluded 14 of them from the equation. The 57 “ever admitted” Barnet patients consist of these 35 assumed survivors of patients admitted on referral or subsequently admitted to mental hospital,

TABLE  
*Patients ever admitted to mental hospital or other institution during two year follow-up*

	Barnet		Chichester		Salisbury	
	n	%	n	%	n	%
“Ever admitted” and still alive at follow-up	58	(57)	25	(52)	15	(79)
Total alive at follow-up (100%)	101		48		19	

plus 22 of the 23 shown to be in Part III accommodation at the end of the follow-up period. (We have assumed that the 23rd Part III resident and the patients in mental hospital at the end of follow-up are included among those already counted).

The numerator (numbers ever admitted and still alive after two years) and denominator (number still alive at follow-up) of the Barnet "ever admitted" rate is thus in line with the Chichester and Salisbury rates taken from Sainsbury's paper.

It can be seen that the proportion of patients admitted to Barnet during the two year period is slightly higher than the proportion admitted in Chichester sixteen years earlier, although lower than the Salisbury figure.

Apart from presenting data on admissions *during* a two year period, Dr Ratna also gives some figures in Table IV of his paper on outcome *at* two years. These have been "consistently" calculated—that is to say dead patients in all three areas have been included in the "at risk" population. What is interesting about the outcome data is the remarkable *similarity* of the proportions of patients in institutions at follow-up. In Barnet 32 patients (32 per cent of the survivor population) were in hospital or another institution two years after referral compared with 18 (36 per cent) in Chichester and 7 (37 per cent) in Salisbury.

Dr Ratna also seeks to compare the three services by looking at mean length of stay and at chronic patients in hospital for two years. Unfortunately, the figures which he presents are of questionable validity. The mean length of stay data provided by Sainsbury *et al*, first of all only applied to patients still alive at follow-up, so patients who died in hospital shortly after admission, for example, would not have been included. Secondly the Sainsbury figures refer to "average number of weeks spent in hospital *during two years*" (original italics). There is no indication that the mean length of stay shown for Barnet has been calculated in the same way. A comparison between the proportion of chronic patients who spent the whole follow-up period in hospital is not possible, for the simple reason that Sainsbury *et al*, did not publish figures on this. The percentages for Salisbury and Chichester shown by Dr Ratna in Table VI of his paper refer not to chronic patients in hospital *throughout* the follow-up period, but to patients in hospital *at* follow-up two years after referral, a group which could include patients who had been in hospital for less than 24 hours!

From the data presented by the author it does seem that there is one respect in which patients referred to the Barnet service in 1976–78 fared better than those referred to the Chichester service in 1960–62. The two year mortality rate for the Barnet patients (29 per cent)

is significantly lower than the rate (44 per cent) experienced by the Chichester and Salisbury patients (SND = -2.49,  $P < .05$ ). However, even here the author is not justified in suggesting that the lower mortality rate observed among Barnet patients was due to a lower admission rate.

No convincing evidence whatever is presented to show that high hospital mortality rates are the result of hospital procedures rather than of the selection of hospital patients, and no data are provided to show whether it is the patients the author admitted or those he refused to admit who had the higher mortality. It is far more likely that the lower mortality in Barnet is largely a reflection of the sixteen year gap between the two studies. Recent papers have noted apparent increases in the survival of dementia patients in America (Rabins, 1982), in Newcastle (Blessed, 1982) and Dumfries (Christie, 1982), and between 1960–62 and 1976–78 average expectation of life at age 65 increased by six months for males and 1.3 years for females (O.P.C.S., 1980).

The author thus fails to take any account of the probably confounding effect of the difference in time between the two studies, nor does he consider the effect of other variables such as, for example, social class. However, the effect of these theoretical weaknesses is probably minor when compared with the errors in calculation and interpretation, which invalidate Dr Ratna's claims for the outcome of his service by comparison with others.

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