care: this relationship cannot be assumed to be so clear-cut. There is minimal description of how the average cost per patient was calculated, yet this is given inordinate prominence in concluding (wholly on grounds of cost) which patients are best treated in hospitals – which, by the authors' own admission, are in a state of "scandalous neglect" and presumably in need of significant capital investment. Indeed, no mention is made of whether costings differentiated between capital and revenue expenditure, an essential distinction in any cost analysis.

The system of mental health care analysed appears to be undergoing evolutionary rather than radical change, yet average cost per patient is used rather than marginal cost (cost per extra patient within an existing system). This is a particularly misleading omission, because the cost of maintaining a small number of highly dependent patients (8 people, or 6% of the cohort) in the community is compared with the average cost of in-patient care, despite the likelihood that such patients will incur above-average costs in hospital. In any case, Fig. 4 of the paper indicates that such patients could be incorporated within a community care system with a relatively small increase in average cost.

This paper has stepped into a methodological minefield, and we conclude that it has failed to support the justifiable caution about rushing headlong into community care. Nevertheless, it emphatically exposes the need for good economic evaluation in psychiatry.

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SIR: Drs Checinski & Goddard criticise the approach to cost analysis in our paper. If the analysis of cost-effectiveness of a mental health care system had been the central issue of study, this criticism would be justified. The purpose of our study, however, was to evaluate the effectiveness of a specific component of extramural care in a representative cohort of schizophrenic patients, on the basis of an observational study with a naturalistic design. We substituted for the lack of control by a methodological approach that by means of a specific model (Fig. 1 in our paper) permitted the partialing out of confounding influences – taking into account the time sequence – on the interesting independent and dependent variables. The description of average direct costs and costs per case was meant to demonstrate that in principle, the actual structure and politics of care form the background of calculating these values, which for this reason cannot simply be applied to a different system. In the system evaluated, providing mental health care for the Mannheim population, the situation is characterised by the fact that about 25% of the schizophrenic patients needing long-term (>1 year) residential care were still in continued hospital care. The monotonically increasing costs per case (Fig. 4 of our paper) illustrate this, inasmuch as the curve of increasing costs per case would presumably continue to ascend if these 25% of patients were reduced, and consequently the number of cases exceeding the threshold value of the comparable costs for in-patient treatment increased. However, for describing this issue the calculation of the marginal costs would be of minor significance.

The majority of studies do not consider the interdependence between the costs of extramural care and the proportion of patients in hospital and complementary care. Doubtlessly we have ventured into a methodological minefield, but only to attract attention to some unnoticed mines. We leave it to Drs Checinski & Goddard to sweep them.

Contrary to Drs Checinski & Goddard's postulation, we found that if we considered capital investment in a very small number of heterogeneous facilities serving our catchment area, which has a population of 300 000, this would have resulted in considerable distortion. The determination of the total direct costs of medical care, social care, etc. proved to be the only comparable index for case-related costs of care.

Indeed, the analogy drawn between monetary and non-monetary costs is not based on empirical data – the collection of which was not the subject of our study. By indicating the tendency of a parallel course for these two types of costs, we intended to point out that due to the reduction of numbers of psychiatric beds, severely disordered patients would also have to be discharged. Extramural care for them would not be cheaper, and presumably would be worse than long-term admission. There is no doubt that in principle, more severely disordered patients also cause higher costs when cared for in hospital. However, these increased costs did not arise in a measurable way, since they had no effect on the number of staff or on the equipment of the hospitals concerned. This means that additional care of the more severely disordered patients was provided at the expense of the less severely disordered. The problems involved could not be treated in our paper.

Finally, our critics find fault with the outcome criteria, 'length of stay in the community' and 'length of stay in hospital after readmission'. The two care-related measures furnish fine indices of possible changes in the care provided by setting up complementary facilities. They operationalise just those goals for changes in the pattern of mental health care that have often been formulated by planners, for instance by the Royal Commission on Mental Health (Rollin, 1977): optimum replacement of in-patient treatment by extramural care. Further to care-related outcome criteria, we used disease-related outcome measures, assessed by PSE interview, which Drs Checinski & Goddard failed to notice.

The harshest criticism, the statement that it is not correct to assume that the frequency of out-patient contact directly influences the chance of readmission (this is in fact the most significant finding of our study) remains unfounded. We critically described our model (Fig. 1 of our paper) for testing the causal association under consideration of the relevant intervening variables. Drs Checinski & Goddard give no indication which of the variables not considered by us or which deficits of our model jeopardise the conclusions drawn.

Nevertheless, we are grateful to them for their stimulating comments on our paper – in particular for having pointed out that many aspects were not sufficiently treated, for example the description of programmes for mental health care, which indeed is true; articles in journals must regrettably be shorter than authors, and obviously also critics, wish.

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Reliability of GHQ factor structures

SIR: The paper by Huppert *et al* (*Journal*, August 1989, **155**, 178–185) on the factor structure of the 30-item General Health Questionnaire (GHQ-30) is misleading on one specific point.

Factor analysis has been widely used not only in psychological and psychiatric research but also in other social sciences. When we perform factor analysis, we usually rotate the axes in order to simplify the concept of each factor/component, which is obtained from the allocation pattern of the variables with significant loadings on the factor. In an initial analysis, the eigenvalue of the first factor is always much greater than the others, and the results of initial unrotated factors are difficult to interpret. Thus, we carry out rotation of the axes in order to reallocate the item loadings.

The loadings following rotation are quite different from those of the initial analysis, the difference among the variances of rotated factors becoming smaller than that of the initial factors. As has been reported in recent factor analysis studies on the GHQ (Elton *et al*, 1988; Iwata *et al*, 1988), the result concerned with rotated factor structure involves the sum of squared loadings obtained after rotation as a variance of each rotated factor.

However, Dr Huppert *et al* appear to have miscalculated the variances. They present the proportion of each factor's variance accounting for the total variance as 28.9%, 7.6%, 6.2%, 4.1% and 3.8% from the factor A (greatest) to E (smallest), respectively (Table I of their paper). Thus, we can estimate the variances of these factors: the values are 8.67, 2.28, 1.86, 1.23 and 1.14 respectively.

In contrast to these values, based on the loadings demonstrated in the table, the sums of squared loadings (proportion of variance explained) are 5.40 (18.0%) for the factor A, 2.84 (9.5%) for B, 2.64 (8.8%) for C, 2.33 (7.8%) for D, and 1.75 (5.8%) for E, respectively. Taking into account the fact that the loadings given have been rounded off to two decimal places these become 5.30–5.50 (17.7–18.3%) for the factor A, 2.77–2.90 (9.2–9.7%) for B, 2.58–2.71 (8.6–9.0%) for C, 2.28–2.39 (7.6–8.0%) for D, and 1.71–1.80 (5.7–6.0%) for E, respectively.

These values are markedly different from those given by Dr Huppert *et al*, who appear to have regarded the eigenvalues derived from the initial unrotated factor solution as the variances of the rotated factors. The values for variances of factors displayed in Tables I and II, therefore, are in error. Also, although they state that, "Despite the large number of items with significant loadings on D'Arcy's first factor (13 items), it accounts for only 16% of the variance. This contrasts with the eight significant items in our anxiety factor, which accounts for 28.9% of the variance." (pp. 183–184), in truth, their value is not so different from that of D'Arcy (1982).

Although the errors mentioned above do not seem to affect strongly the main results or conclusions of the paper, in view of the spreading use of statistical