From I.L. Coverdale, Cheshire County Council
R.J. Gibbs, Cabinet Office, London
K. Nurse, Civil Aviation Authority, London

Dear Sir,

Independent research in public policy

In the October 1981 issue of the Journal, Ashford et al.¹ argue that independent research into issues of public policy may become stifled as a result of defensive behaviour by 'in-house' researchers in central government. They illustrate this argument by describing circumstances surrounding a paper² by us which was published in the September 1980 issue of the Journal. They suggest that we, as O.R. scientists working in the Department of Health and Social Security (DHSS), deliberately ignored their alternative methodology, and they imply that we were party to a decision in DHSS to discourage their research because it constituted a threat to our own. Not only do we doubt the validity of the general argument of Ashford et al., we also wish to assert most strongly that it has no validity in our case.

Ashford et al. asked why our paper did not examine alternative modelling approaches such as theirs. The answer is twofold but simple. First our paper was deliberately written for the Journal as a case-orientated paper rather than a theoretical paper. In other words, we described some of the work we had done in DHSS and we indicated how the results had been used in a practical policy-making context. Had we wished to discuss the technical merits of different types of model, then we would have had to omit most of the policy-orientated material, and the paper would have become a theoretical one. (In common with many other members of the O.R. Society, we believe that the Journal needs more papers of the case-orientated variety.) Second, at the time when our work was carried out, which was during the period 1970–78, no account of the model of Ashford et al. was generally available. Indeed we suspect that their model was first developed at around the end of this period. Ashford et al.¹ state that their model was first mentioned in a paper³ published in 1977 and that a full account of the model was published⁴ in 1979. Unfortunately the mention in the former paper was far too oblique to give any insight into the form and merit of their model - "A technique has been developed based on simple statistical procedures to estimate the costs of individual specialities from these data sources." - whilst the latter paper appeared after our work was completed and after we had submitted our own paper for publication. It is true that Ashford et al. provided a description of their model during discussions with DHSS staff in 1978 and made strong claims for its merits vis-à-vis our own. However they failed to present their own results on a comparable basis to ours and so were unable to substantiate their claims. In effect they compared the power of a primitive version of our model to explain variations in cost per case with the power of their model to explain variation in total hospital cost. Furthermore they appeared to be unwilling to remedy this failing when it was brought to their attention - which did not inspire confidence in their approach. This failing persisted in their 1979 publication⁴ but has been remedied in the 1981 publication.¹ Thus even if we had wished to write a theoretical paper, as opposed to a case-orientated one, we would have not been able to say anything constructive about the relative merits of their model vis-à-vis ours.

We now turn to some of the strictly technical issues raised by Ashford et al. in their 1981 paper.¹ We find instructive their comparison of the A-B and C-G-N models on the same basis. We ourselves experimented with several different forms of the C-G-N model, including some that were rather similar to

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the A-B model. We found that many forms had almost equally good explanatory power. Therefore we are not surprised that Ashford et al. find that the A-B and the C-G-N models have very similar coefficients of determination (0.98 and 0.97 respectively) for non-psychiatric hospitals (which were the main field of our concern). We are surprised that they should regard as important the very slightly better performance of the A-B model. Nor are we impressed by their claim that the A-B model is more parsimonious in terms of number of parameters (10 rather than 12). The (slightly) larger number of parameters in the published version\(^2\) of the C-G-N model was caused by the larger number of specialty groupings which was required by the nature of the policy application (e.g. for specialty costs in the RAWP formula) not by the quest for explanatory power; in fact the C-G-N model performs quite well with no specialty disaggregation, i.e. only three parameters.

In the light of our experiences with alternative formulations, we were content to choose an approach in which a structure was imposed on the model rather than the more purely statistical approach of Ashford et al. The choice between these two approaches is by no means clear cut. Each has its strengths and weaknesses. The former approach has the advantage that insights from other researchers can be built into the model. Also the model can be designed to concentrate attention on the variables relevant to the policy issues being considered (e.g. in our study of optimum hospital size we included a quadratic beds term). It is, however, open to the charge of prejudging the issue by excluding potentially relevant variables. The latter approach has the advantage that it can normally achieve a better fit to the data (or a similar fit with fewer variables) by not being shackled by a predefined model. Its disadvantages are practical ones. One cannot collect information on all possible explanatory variables. Thus in practice, the models produced still reflect the judgements of their creators. Certain variables will have had to be excluded. A second difficulty with models developed in this way is that they may differ in structure from one data set to the next, making comparison and interpretation difficult.

Ashford et al. criticise our apparent neglect of the problem of heteroscedasticity. In fact we recognised the problem in our analysis (see Table 2 in our paper\(^2\), and we experimented with ways of overcoming it (e.g. by partitioning our sample into different size bands and examining the stability of the coefficients). We concluded that the problem was not a serious one. However, in this respect we concede that the procedure of Ashford et al., in which observations are weighted by an appropriate factor, may be superior to our own and we would commend this procedure to the attention of any future researcher in this field.

We are intrigued by the "component model" described by Ashford et al. Like them we would expect it to provide greater insight into an explanation of hospital costs than either of the other two models, albeit with the additional expense of disaggregated cost data and loss of parsimony. Ashford et al. claim that the component does indeed provide a better fit, and yet they quote an \(R^2\) value of 0.98, which is no better than that which they quote for the A-B model. Some clarification would be welcome here.

We conclude that the work of Ashford et al. may represent a useful addition to the understanding of hospital costs but that they have exaggerated the size of this achievement. Moreover, we claim that they have confused what should be an objective technical discussion with misleading suggestions about the background to the work.