to establish the relation between the structure or process of care and the outcome, and thereafter it is structure and process rather than outcomes that should be audited.⁵

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Further studies are needed to assess current outcome indicators

EDITOR,—Richard J Davenport and colleagues question the validity of the clinical outcome indicators published by the Scottish Office.¹ In fact, the results that they present suggest that the outcome indicators published in Scotland do their intended job—raising meaningful questions, nothing more—reasonably well.

The indicators in the authors' study—that is, the study data standardised for age and sex—suggest that mortality associated with standard care is higher than that associated with the care provided in a stroke unit. In a real context, such a result might well lead to a call for further analysis or audit to establish whether this difference remains after adjustment for case mix. The authors follow this logic and conclude that, "because of the imprecision of the corrected data, the results are still consistent with a moderate but non-significant beneficial effect." Thus the indicator is shown to raise a valid issue even in artificial circumstances far removed from those for which the indicators were designed, involving a comparison over time (with an associated major shift in case mix) rather than between hospitals at one point in time, and also involving a number of cases well below the threshold for publication.

The authors' study is not an appropriate way to test the validity of the Scottish outcome indicators. Even according to the rule of thumb that the authors use to justify their statistical procedure, the analysis may well have "overcorrected" by forcing the inclusion of 19 explanatory variables. Eighty nine deaths within 30 days does not represent five outcome events for each explanatory variable.

Davenport and colleagues, in collaboration with the producers of the Scottish outcome indicators, are engaged in a major study to assess the feasibility of routinely monitoring outcomes of stroke by looking at enhanced administrative data. This study will soon make possible a linked comparison, on a case by case basis, between outcomes derived from Scottish hospital discharge data as they currently stand and outcomes derived from data enhanced to enable adjustment for case mix. Such a comparison, based on adequate numbers and across five hospitals, will help us to come to a much more precise and balanced understanding of the strengths and limitations of the current outcome indica-

tors and how to improve them. We look forward to collaborating in this analysis.

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- 1 Davenport RJ, Dennis MS, Warlow CP. Effect of correcting outcome data for case mix: an example from stroke medicine. *BMJ* 1996;312:1503-5. (15 June.)

Study should have had more patients or longer time scale

EDITOR,—We welcome Richard J Davenport and colleagues' reminder of the importance of correcting outcome data for case mix.¹ Given the size of their study, however, they should not be surprised or disappointed that they were unable to show a beneficial impact of their stroke unit on mortality.

The 12 month mortality before the introduction of their stroke unit in Edinburgh was 39%.¹ A meta-analysis of the effect of stroke units reports a 23% (95% confidence interval 4% to 37%) reduction in the odds of death at final review (median follow up one year) in the intervention group.² Such an effect would lead to a lowering of the death rate in Edinburgh to 33%. To be able to detect this change with 80% power and at a (two sided) significance level of 5% would require a study of 2066 patients (1033 before the introduction of the stroke unit and 1033 after its introduction). Davenport and colleagues' study was of 468 patients identified over 27 months. To be confident that the introduction of their stroke unit had a smaller impact on mortality than that predicted by the meta-analysis, Davenport and colleagues' study would have needed to be much longer (119 months as opposed to 27).

The study is a clear illustration of why it can be impractical to try to compare performance among or within providers by monitoring outcomes, even when they are adequately adjusted for case mix: large numbers of patients or long time scales may be necessary to overcome the play of chance.³ In areas of health care in which the results of randomised controlled trials are available it is possible to ensure that clinical practice reflects this evidence by the use of appropriate measures of the process of care. In this case, there is good evidence that introducing a stroke unit saves lives. Purchasers would thus do better to ensure that their providers offer "well organised services for acute stroke patients which provide comprehensive care centred on an integrated multi-disciplinary team who have a specialist interest in stroke rehabilitation"² rather than seek to interpret their case fatality rates.

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Authors' reply

EDITOR,—All of the authors of the letters about our paper seem to agree with its central tenet. David Barer and colleagues and Steve Kendrick and Marion Bain suggest that our regression models may have overcorrected the data, and we discussed this possibility in our paper. While multiple logistic regression modelling is far from perfect, we thought that attempting to correct for case mix in this way was a more sensible approach than simply ignoring the problem. As discussed, we were reassured that we obtained almost identical results when we applied other prognostic models, which had been derived from and validated on different stroke datasets.¹ We had developed these considerably simpler models (which rely on five or six variables, such as age and systolic blood pressure on admission) because we realised that collecting the amount of data included in our study is beyond the resources of most hospitals, at least in routine clinical practice. We are currently testing the applicability of these models in several hospitals (as referred to by Kendrick and Bain). We agree with the suggestion that the structure and process of care may be more appropriate measures of quality of care, although there are still considerable problems associated with this approach.^{2,3}

We have conducted a similar study to that of Barer and colleagues, comparing the accuracy of routine hospital coding statistics with our stroke register.⁴ Of 566 patients registered as having a stroke, 84 (15%) were not given a code (*International Classification of Diseases*, ninth revision) for stroke; although better than Barer and colleagues' experience, this is far from perfect.

Finally, while we agree with Kendrick and Bain that the purpose of the Scottish outcome indicators is to raise meaningful questions, we are aware that they have been interpreted by many people as comparative league tables of hospitals' performance, and the purpose of our paper was to show the dangers of using the data as a direct measure of quality of care. As an example of this problem, the *BMJ* recently published a news item which suggested that the clinical outcomes initiative could identify hospitals which "seemed to perform badly."⁵ We were interested to note that this report also raised doubts about whether the outcomes initiative would continue.

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Measuring health outcomes

Negative values should be allowed in ratings of quality of life

EDITOR,—Ability to assess treatment and the outcomes of diseases continues to improve, and the schedule for evaluating individual quality of