LETTERS

Systematic reviews: a cautionary note

Melville *et al*¹ present a systematic review of treatment options for benign prostatic hyperplasia. The review considered the question of whether or not transurethral resection of the prostate (TURP) has higher long term mortality than open prostatectomy. Recently I too have considered this issue² and I became very familiar with the studies on this topic. Consequently I was concerned to see that the systematic review included the following statement:

"A large retrospective study³ found that open prostatectomy was associated with lower long term mortality than resection, but subsequent observational studies⁴ suggested that differences in the severity of comorbidity may account for the apparent superiority of open prostatectomy".

The rationale of systematic reviews, unlike ordinary reviews, is that they should consider all relevant studies, critically appraise them, and provide a fair and balanced summary of their findings.⁵ Although I agree with their conclusion, the above statement does not accurately summarise evidence from all the relevant studies. Their conclusion happened to be right but it was based on inadequate appraisal of the literature.

The reference given in support of the claim that differences in comorbidity may account for differences in long term mortality after TURP and open prostatectomy was the study by Concato *et al*.⁴ In this study, statistical adjustment for comorbidity did, indeed, have an effect: the point estimate for the relative risk of death after surgery for TURP versus open prostatectomy was reduced from 1.30 to 1.03 (95% confidence interval 0.57 to 1.87).

However, the study had insufficient power to discount the possibility of 50% higher mortality after TURP (the value found in the original study by Roos *et al*³). Moreover, this study⁴ was not the only one in which adjustment for comorbidity was performed. Other studies include the original one by Roos *et al*³ and a subsequent study by Andersen *et al*,⁶ both of which found that adjustment for comorbidity had little effect on the estimate of relative risk. Melville *et al*,¹ therefore, based their assertion on results from only one of the relevant studies and an even smaller proportion of the available data.

(The study by Concato *et al*⁴ only included data on 252 patients. By contrast the study by Andersen *et al*⁶ included data on 38 000 patients.) Furthermore, Melville *et al*¹ did not undertake any critical appraisal of the studies, surely one of the key elements of systematic reviews.

It is not an easy task to obtain an exhaustive list of studies covering a wide field, to critically appraise them, and to provide a balanced overview of the evidence. Readers have to trust that this has been carried out in systematic reviews. I have given one example of a systematic review in which this was not done. There may well be other examples. Maybe it is time for a critical appraisal of systematic reviews. In the meantime, reader beware.

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- 1 Melville A, Donovan J, Sheldon T, Peters T. Benign prostatic hyperplasia. *Quality in Health Care* 1996;5:111-9.
- 2 Seagroatt V. Mortality after prostatectomy: selection and surgical approach. *Lancet* 1995; 346:1521-4.
- 3 Roos NP, Wennberg JE, Malenka DJ, Fisher ES, McPherson K, Andersen TF *et al*. Mortality and reoperation after open and transurethral resection of the prostate for benign prostatic hyperplasia. *N Engl J Med* 1989;320:1120-4.
- 4 Concato J, Horowitz RJ, Feinstein AR, Elmore JG, Schiff SF. Problems of comorbidity in mortality after prostatectomy. *JAMA* 1992; 267:1077-82.
- 5 Chalmers I, Altman DG, eds. *Systematic reviews*. London: BMJ Publishing Group, 1995.
- 6 Andersen TF, Bronnum-Hansen H, Sejr T, Roepstorff C. Elevated mortality following transurethral resection of the prostate for benign hypertrophy! But why? *Med Care* 1990; 28:870-81.

AUTHORS' REPLY — We thank Seagroatt for her comments. Firstly, to deal with the specific point she raises about studies included in the review, we would point out that the study of Concato *et al* includes the results of each of the three main observational studies comparing the outcomes of TURP and open prostatectomy, including that of Anderson *et al*, and for this reason we did not refer to them separately. Although it is true that these studies were adjusted for the presence of comorbidity, the distinguishing feature of the study by Concato *et al* was that it attempted to control for the severity of comorbidity.

We agree, however, that the study of Concato *et al* was underpowered. The *Quality in*

Health Care paper was a summary of *Effective Health Care Bulletin*, vol 2, no 2, where we stated that "this study was too small to be definitive". It is always difficult, when producing shortened versions of earlier publications, to be confident that important information is not being left out. If we had been making a clear practice or policy recommendation, our reports of the studies would have been more detailed, but in this case we were simply noting the debate. The Seagroatt paper was not included because it was published after the *Effective Health Care Bulletin* went to press.

We agree wholeheartedly with the author's sentiments about systematic reviews. Our experience at the Centre for Reviews and Dissemination of quality assessment of reviews has taught us that many are not as reliable as they might seem at first sight. Important studies can be missed and conclusions may not accurately reflect the data. Every review should be read critically, whatever its provenance.

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DIARY

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Evidence based healthcare: a great experiment? 15 April 1997 London: Royal Society of Medicine. This conference sets out to review progress so far in making the NHS more evidence based, and to explore what has been learned about the challenges involved in making the NHS more evidence based, and to explore what has been learned about the challenges involved in making a reality of evidence based health care. A new NAHAT research report titled *Acting on the evidence: progress in the NHS* will be launched at the conference. It presents the results of a study of the progress of evidence based health care in the NHS undertaken for NAHAT and the ABPI by the Health Services Management Centre at the University of Birmingham. Further information from: Nicola Fogarty, Academic Department, The Royal Society of Medicine, 1 Wimpole Street, London W1M 8AE. Tel: (0171) 290 2987. Fax: (0171) 290 2989.