Letters and Comment

Contributors to this section are asked to make their comments brief and to the point. Letters should comply with the Notice printed on the inside back cover. Tables and figures should be included only if absolutely essential and no more than five references should be given. The Editor reserves the right to shorten letters and to subedit contributions to ensure clarity

A systematic review of the effectiveness and safety of laparoscopic cholecystectomy

I am writing to express a few of my concerns about both the methodology and the conclusions drawn in the above review.

It is wrong to ignore all foreign language literature and assume only meaningful studies are published in English. Furthermore, the online databases are notoriously incomplete as is reflected in the bibliography. Not only will this have missed articles published in dedicated laparoscopic journals, which were not initially listed but, as the BMA point out, up to half the relevant articles are missed. A quick search through my own articles revealed several series which were not included (for example *(1–3)*).

The executive summary and final conclusions are at odds with the conclusions presented at the end of each section. There was strong evidence that laparoscopic cholecystectomy (LC) was associated with fewer pulmonary complications, less postoperative pain and shorter hospital stay and return to normal recovery times than open cholecystectomy (OC) and probably also mini-cholecystectomy (MC). There was no evidence that these important benefits were realised at the expense of increased complications and, indeed, mortality may be reduced. Therefore, one cannot fail to conclude that LC is safe and effective with proven benefits over other operations and *must* be recommended as the treatment of choice for patients with symptomatic gallstones.

The review does not scientifically address the issue of training and it is wrong that it should focus on LC experience alone. The personal views of the authors presented as conclusions are out of date and out of touch with reality. They have not addressed training for OC or MC. There is increasing concern about trainees' ability to perform OC rather than LC *(4)*!

Of course there are regional variations in standards of LC. There are regional variations in standards of all procedures, as has recently been highlighted for colorectal cancer. I am sure the same applies to OC and MC. This is not an argument against LC. Quite the contrary. LC standards are easier to assess and compare than most other operations and as a consequence easier to 'police'.

I am surprised that in the current economic climate cost-effectiveness was not considered sufficiently important to review.

This review attempts to 'shut the stable door after the horse has bolted'. The benefits of LC are recognised by both surgeons and patients around the world and highlighted in the text of this review. I think on the data presented it would be unethical to have a randomised controlled trial.

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References

1 Wilson P *et al.* Elective laparoscopic cholecystectomy for 'all-comers'. *Lancet* 1991; **338**: 795–7.
2 Martin IG *et al.* Laparoscopic cholecystectomy as a routine procedure for gallstones: results of an 'all-comers' policy. *Br J Surg* 1992; **79**: 807–10.
3 Widdison AL, Lonstaff AJ, Armstrong CP. Prospective study of combined endoscopic and laparoscopic treatment of gallstones and bile duct stones. *Br J Surg* 1994; **81**: 595–7.
4 Widdison AL, Norton S, Armstrong CP. Open cholecystectomy in the age of the laparoscope. *Ann R Coll Surg Engl* 1995; 77: 256–8.

Authors' reply

Mr Widdison needs to read our review more carefully as none of his criticisms is relevant.

First our review covered literature in all languages, not just English.

Second, we did not rely solely on bibliographic databases but used several other techniques to ensure we identified all comparative studies that had been published or were in progress.

Third, we made no attempt to cover case series comprehensively as these studies offer little or no meaningful evidence of effectiveness.

Fourth, our conclusions are based on an interpretation of the scientifically most rigorous evidence. This led us to the conclusion that laparoscopic surgery offered little or no advantage over mini-laparotomy. Mr Widdison is entitled to his opinion but it is not supported by the best scientific evidence.

Fifth, if he looks at our objectives he will see that we did not aim to consider the issues of training. All we have suggested is that training is likely to have an important effect on the risk of complications. We felt this was a fairly uncontentious comment.

Like Mr Widdison, we were quite surprised by the finding in our review as we shared his view about the supposed benefits of laparoscopic cholecystectomy. Unfortunately, the best scientific evidence does not support this belief. Hence our conclusion that surgeons should not be encouraged to replace mini-laparotomy with laparoscopy. We recognise that this attempts to 'shut the stable door after the horse has bolted', but such a message may help the development of research-based clinical policies in other areas of surgery in which the horse has not yet bolted.

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