Commentary: How experienced should a colorectal surgeon be?

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A question that a patient with colorectal cancer will naturally ask is, "Will my chances of being alive in two years' time be enhanced by my choice of surgeon?" Intuitively we feel that this is so, and that the patients of a more experienced surgeon will have less chance of dying or having complications after the operation and greater long term survival. If this is the case, there are wider implications in terms of training in the subspecialty of colorectal surgery and the concentration of treatment in larger centres with high volumes of patients. Such concentration of activity, however, has its downside because these centres may be more remote, making treatment less accessible and increasing the cost for patients and relatives.

Most, but not all, publications support a positive relation between increased activity (volume) and outcome for colorectal cancer. Not all studies, however, adjust for case mix or severity of illness, but Kee et al's paper attempts to do so.

The relation between the number of operations that a surgeon performs and the outcome is unlikely to be linear throughout. The threshold for experience to influence outcome (learning curve) must vary according to the operative procedure—lower for colonic cancer and higher for rectal cancer, where the surgical requirements are more demanding, to preserve anal sphincters in low lesions and to reduce the chances of local recurrence by mesorectal excision. The authors found no relation between survival and the site of the tumour.

The authors are the prisoners of their experimental design because inherent variability is such that there is little chance of showing statistical differences between the groups and subgroups that they analyse. For example, by focusing on mortality at 2 years the authors have combined the 30 day hospital mortality (which will be largely influenced by the type of surgery and postoperative complications—for example, anastomotic leakage) and longer term survival, which will probably be influenced by such factors as the biology of the tumour and the use of adjuvant treatment, of which radiotherapy, in the case of rectal cancer, is important. Combining surgical and non-surgical factors, which could influence mortality at 2 years, introduces too much variability into the analysis. The fact, however, that no statistical difference can be shown does not mean that a real difference does not exist.

No information is given on how many of the surgeons had received specialist training in colorectal surgery. Indeed, since in each case it was the name of the consultant surgeon in charge of the case that was recorded, we do not know if the operations were performed by consultants, by surgeons in training, or by both. This could be a factor of importance in emergency operations.

The experience of the surgeons was measured by the number of years on the medical register, surely a rather crude indicator of specialist experience. The number of years as a consultant surgeon would perhaps be a more accurate marker of post-training experience and could presumably be easily obtained in a circumscribed community such as that in Northern Ireland.

The worst survival in those hospitals that treated 33-54 cases a year defies explanation and must be a statistical oddity. The value of analysing hospital data is questionable. A separate study is required to determine if hospital volume can serve as a surrogate for surgeon volume for achieving good outcomes in colorectal cancer.

The problem with Kee et al's study is that, with the comparatively small numbers in the various groups and high variability, it becomes statistically impossible to show that surgeons with small caseloads are better or worse than those with higher caseloads. The results must not give the green light to those who wish to defend small volume workload or to encourage the surgeon who does colorectal surgery only occasionally.

The paper is interesting because it tackles an important subject and invites further study. Much depends on the resolution of the volume-outcome controversy, not only for the individual patient but also for surgical training and the provision of colorectal cancer services.

Corrections and clarifications

Double blind, cluster randomised trial of low dose supplementation with vitamin A or β carotene on mortality related to pregnancy in Nepal

This general practice paper by Keith P West Jr and colleagues (27 February, pp 570-5) contains some minor errors that do not affect the validity of the conclusions. The last paragraph of the results section (p 573) should have read: "The maternal mortality ratio was 630 [not 645] (42 deaths/6670 live births), 407 (29/7120 [not 29/7074]), and 346 [not 361] (23/6643) per 100 000 live births in the placebo, vitamin A, and β carotene groups, respectively (P = 0.11 [not 0.08] for vitamin A and 0.04 for β carotene *v* placebo). The ratio for women receiving either vitamin A or β carotene was 378 [not 385]." In the section of table 5 (p 574) labelled "infection" the values for the column for vitamin A should have been 2, 5, 3, 4, 14, 181, and 0.87 (0.39 to 1.96) [not 2, 5, 5, 3, 15, 194, and 0.94 (0.42 to 2.05)]; the values for the column for β carotene should have been 3, 3, 1, 3, 10, 139, and 0.67 (0.28 to 1.62) [not 3, 2, 1, 3, 9, 125, and 0.60 (0.24 to 1.51)]; the values for the column for vitamin A or β carotene should have been 5, 8, 4, 7, 24, 161, and 0.78 (0.39 to 1.58) [not 5, 7, 6, 6, 24, 161, and 0.78 (0.39 to 1.58)]; and the footnote for "Other" infections should have also included tuberculosis.