Correspondence

The Editors welcome topical correspondence from readers relating to articles published in the Journal. Letters should be no more than 250 words in length and should be typed on A4-sized paper in double spacing.

Experimental study of the effect of intraperitoneal heparin on tumour implantation following laparoscopy

Sir

We read with interest the article by Neuhaus *et al*. (*Br J Surg* 1999; **86**: 400–4) in which the authors show that heparin may reduce tumour cell implantation. As well as directly decreasing tumour cell adherence, there are numerous other ways in which heparin may act, including inhibition of the formation of thrombin, which has a role in enhancing tumour growth and metastasis. Thrombin may play a role in the activation of progelatinase A[1], of which the active form, matrix metalloproteinase 2, is thought to play an important role in angiogenesis[2] and tumour invasion[3]. Thrombin may also have a direct proangiogenic effect by inducing differentiation of endothelial cells into capillary structures[4].

The effects of thrombin may also, therefore, be inhibited by the use of intraperitoneal heparin during laparoscopic surgery and decrease the incidence of abdominal wall metastases as observed in this department[5].

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1 Zucker S, Mirza H, Conner CE, Lorenz AF, Drews M, Bahou WF *et al*. VEGF induces tissue factor and matrix metalloproteinase production in endothelial cells: conversion of prothrombin to thrombin results in progelatinase A activation and cell proliferation. *Int J Cancer* 1998; **75**: 780–6.
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Effect of training on the incidence of nerve damage in thyroid surgery

Sir

I read with interest the article by Lamadé *et al*. (*Br J Surg* 1999; **86**: 388–91) which highlights important aspects of surgery and surgical training.

I compliment the authors for carrying out this study. In a changing world with periods of surgical training shrinking (particularly in the UK) and the ever-increasing climate of litigation and compensation, both the trainers and the trainees find themselves under considerable duress. Articles such as this and others[1–3] facilitate surgical training and encourage trainers to overcome long-held views as to what a trainee can safely perform under supervision without compromising patient safety and eventual outcome.

It would have been interesting to know the time taken for similar procedures by the three groups as the time factor is the often-quoted constraint that denies possible surgical opportunities to the trainee.

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Current management of acute leg ischaemia: results of an audit by the Vascular Surgical Society of Great Britain and Ireland

Sir

We read with interest the article by Campbell *et al*. (*Br J Surg* 1998; **85**: 1498–1503) and appreciate the work done by the Vascular Society of Great Britain and Ireland. We do not have such data in our country and the use of trials creates valuable effects in our practice. We would appreciate clarification of two issues.

First, the delay period mentioned in *Table 4* is very important. The extent of the delay is critical in deciding any intervention or surgery for limb salvage. Does the mortality of the procedure have a correlation with the delay period. No data are given on the delay period.

Second, the terms 'dubious', 'viable' and 'non-viable' (*Tables 6* and 7) need further explanation in the understanding of the type of ischaemia. The further comments of Campbell *et al*. will help us to understand the pitfalls of limb-salvage surgery.

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Author's reply

Sir

We thank Dr Akgun and colleagues for their interest in our paper. Their questions expose the acknowledged limitations of this kind of survey. It did not seem reasonable to analyse the data on delay in presentation for two reasons. First, our definition of acute ischaemia left the time of onset uncertain. Case selection was left to the judgement of individual surgeons and included patients with a sudden deterioration in blood supply up to 2 weeks before presentation. This meant that we did not know how long the acute ischaemia had been present before the patient presented. Second, whether or not any delay had occurred was also left to the subjective judgement of those involved. Any analysis of outcome in the context of perceived delay would, therefore, have been most suspect, and we did not feel that this was fair or scientifically proper.

The terms 'viable', 'dubious' and 'non-viable' were used simply as the best way of allowing surgeons to make a global judgement on the state of the limb. We recognized that, in addition to limbs that were definitely viable and definitely not viable, there were likely to be some that were still in a critical state and that might well be lost shortly after the end of the 30-day observation period. As with the perception of 'delay', these were subjective judgements designed to accommodate a miscellany of patients and surgeons in a large study involving nearly 200 hospitals.

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Leucocyte count and C-reactive protein in the diagnosis of acute appendicitis

Sir

I read with interest the paper by Grönroos and Grönroos (*Br J Surg* 1999; **86**: 501–4). Any effort to reduce the incidence of unnecessary appendectomy is to be commended. However, I feel the use of C-reactive protein (CRP) in the diagnosis of acute appendicitis cannot be justified with the given data. If one looks at the overall numbers presented, only one-quarter of the patients operated on, with an uninflamed appendix, would have been saved an operation if CRP levels had been taken into account. Of the patients studied, this represents only 7–8 per cent and most were women. It is these patients in whom there should be a high clinical suspicion of other diagnoses. They would be suitable for diagnostic laparoscopy to help make definitive diagnosis[1].

The decision to operate or to observe is often made soon after admission and on clinical grounds alone. Given the prolonged turnaround time for what are considered non-urgent laboratory tests in an average hospital the decision to operate would be delayed to at least the next day.

In conclusion, the addition of CRP to the diagnostic decision making in patients who may have acute appendicitis is currently impractical in British clinical practice. The current trend towards laparoscopy in women with right iliac fossa pain makes the measurement of CRP obsolete.

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1 Laine S, Rantala A, Gullichsen R, Ovaska J. Laparoscopic appendectomy – is it worthwhile? A prospective, randomized study in young women. *Surg Endosc* 1997; **11**: 95–7.

Author's reply

Sir

I thank Dr Taylor for his comments but disagree, and the difference between our views may be due to the fact that in Finland the measurement of CRP is a laboratory test which is easily performed and available at all times in hospitals with a surgical unit. I understand this is not the case in the UK.

I trust measuring CRP level is worthwhile in excluding acute appendicitis. One-quarter of the negative appendectomies can be avoided if both leucocyte count and CRP are not elevated and I believe that laparoscopy is unnecessary in this situation. If clinical symptoms and signs continue and leucocyte count and/or CRP value increases above the upper limit of the reference interval, the patient should undergo laparoscopy to establish a precise diagnosis. Otherwise, careful clinical follow-up should be continued and additional blood samples should be taken for leucocyte count and CRP measurements. We have changed our own treatment strategy accordingly and experience so far is encouraging.

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Interstitial laser coagulation for hepatic tumours

Sir

The excellent review of interstitial laser coagulation (ILC) by Heisterkamp *et al.* (*Br J Surg* 1999; **86**: 293–304) offered a comprehensive insight into potential applications of this modality for irresectable liver tumours. However, we feel that there are unanswered questions.

The major limitation of laser coagulation is the small lesion size generated by this technique[1,2]. The authors indicated that increasing the power would result in an increase in the area of coagulation, but with an increase in power there will be an increase in resistance and a reduction in heat conductance due to charring effects leading to a reduced lesion size[3]. The characteristics of the tumour are crucial factors. We have found that laser ablation was ineffective for patients with tumours with a high fibrous component such as cholangiocarcinoma (unpublished material). Heat is lost from tissue during laser coagulation via conduction and low-resistance shunting[4,5]. Thus, to achieve effective tumour coagulation some additional methods should be available when treating perivascular lesions[5] . Although the immediate effect of laser coagulation on tumour tissue can be revealed clearly by ultrasonography following tissue heating, the appearance of tumours changed very little after ILC because of the charcoal effect. This could make postoperative interpretation of the effectiveness of the treatment misleading. Visceral angiography may be valuable[5].

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1 Bosman S, Phoa S, Bosma A, Van Gemert MCJ. Effect of percutaneous interstitial thermal laser on normal liver of pigs: sonographic and histopathological correlations. *Br J Surg* 1991; **78**: 572–5.
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Author's reply

Sir

The valuable comments of Messrs Jiao and Habib allow us to emphasize the aspects of interstitial laser coagulation (ILC) essential for the destruction of large volumes of tissue. Resistance of the tissue, pivotal in radiofrequency coagulation, does not cause the carbonization found with conventional laser fibres in ILC. Charring is the result of concentrated light emission at the tip with further absorption of light by the blackened tissue and preventing further light emission into the tissue. The argument that the characteristics of the tumour are crucial is not supported by evidence in the literature. The advantage of ILC is the induction of symmetrical and reproducible coagulation which makes the technique particularly suitable for solid intraparenchymal tumours. The lack of success in the patient with cholangiocarcinoma might be related to the diffuse spreading pattern of the tumour rather than the high fibrous content. As stated in the review ultrasonography has no place in real-time monitoring of ILC as the echogenic area does not correlate with the coagulative damage. This is not because of charring but because the formation of gas bubbles results in a heterogeneous image.

We emphasized that the use of a cylindrical light-diffusing tip in an internally water-cooled sheet (combined with hepatic vascular inflow occlusion during laser treatment) results in coagulated volumes of 5 cm diameter. We hypothesize that this combination leads to a higher percentage of successful treatments in a single session when compared with the results of current application of ILC for colorectal metastases and percutaneous ethanol injection for hepatocellular carcinoma. In an ongoing study the safety, feasibility and initial response rate of percutaneous ILC with vascular inflow occlusion is being assessed in patients with irresectable colorectal metastases or hepatocellular carcinoma (largest diameter 4 cm). Triphasic contrast-enhanced spiral computed tomography is used to evaluate tumour response[1]. In nine patients, 11 laser treatments have been performed for 13 tumours. Minor complications were noted in five treatments, but complete coagulation at 24 h was 70 per cent with no failures in the last seven treated tumours. These results encourage us to determine the duration of response in a larger group of patients.

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1 Amin Z, Donald JJ, Masters A, Kant R, Steger AC, Bown SG *et al.* Hepatic metastases: interstitial laser photocoagulation with real-time US monitoring and dynamic CT evaluation of treatment. *Radiology* 1993; **187**: 339–47.

Risk scoring in surgical patients

Sir

We read with interest the review by Jones and de Cossart (*Br J Surg* 1999; **86**: 149–57) who concluded that the Physiological and Operative Severity Score for enUmeration of Mortality and morbidity (POSSUM) is the most appropriate scoring system for the general surgical population while briefly mentioning the Portsmouth modification of POSSUM (P-POSSUM) as an alternative scoring system. They stated that if an exponential analysis was used[2], the original POSSUM system was more accurate[3].

In the original POSSUM paper the methodology used to analyse results was not given and it was, therefore, not open to critical review. In a recent article the exponential model of analysis used for POSSUM clearly demonstrated the unwieldy nature of this analysis[2]. POSSUM was used to predict the probability of death for a patient group but could not be applied to individual patients.

P-POSSUM uses a well validated statistical method, which is similar to the linear model described by Wijesinghe *et al.*[2], but easier to apply. A distinct advantage of P-POSSUM compared with POSSUM is the use of discriminant analysis that can be used to ascribe risks to individual patients as opposed to groups of patients. This may make the P-POSSUM analysis more useful in every day surgical practice.

Both systems appear to be equally effective in predicting operative mortality provided the correct methodology is used. The most recent NCEPOD (National Confidential Enquiry into Perioperative Deaths) report[4] suggests the use of POSSUM to highlight 'poor risk cases' in oesophageal surgery, but investigators should be aware of the limitations associated with the exponential analysis.

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1 Whiteley MS, Prytherch DR, Higgins B, Weaver PC, Prout WG. An evaluation of the POSSUM surgical scoring system. *Br J Surg* 1996; **83**: 812–15.
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Author's reply

Sir

We thank Mr Tekkis and colleagues for their interest. The statement that the original POSSUM is more accurate when the exponential analysis is used is making a comparison using the exponential analysis to predict outcomes based on the P-POSSUM-derived risks. It is not making any comment on the accuracy of the two methods overall. We would not differ with the conclusion of Wijesinghe *et al.*[1] that 'both POSSUM and P-POSSUM are good predictors … if the correct analysis is used for each system'.

We are somewhat concerned by the suggestion that P-POSSUM be used in day-to-day surgical practice for predicting an individual's risk. The POSSUM system has been designed as an audit tool. Neither it, nor P-POSSUM, is sufficiently accurate or specific to predict the risk of an individual and to be safely used as the basis for clinical decision making. It may give an indication that a patient is 'high-risk' after operation (the risk cannot be calculated before operation) and guide appropriate placement of the patient, for example in a high-dependency unit. We feel it would be unwise to allow these predicted risks to outweigh clinical judgement in determining any individual patient's treatment.

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Intraoperative lymphatic mapping and the sentinel node concept in colorectal carcinoma

Sir

We read with interest the recent article on lymphatic mapping and sentinel node biopsy in colorectal carcinoma by Joosten *et al.* (*Br J Surg* 1999; **86**: 482–6). Although we share the view that a cautious approach is required in extrapolating the technique to colorectal carcinoma, we must disagree with the comment that the concept is only applicable in a limited subset of skin derived cancers as this is not supported by presently available evidence. The sentinel node is defined as the node(s) receiving direct lymphatic drainage from a primary tumour. Joosten *et al.* have retrieved all nodes (up to 16) that took up blue dye and called them sentinel nodes, which is contrary to the concept. It is well recognized that the blue dye moves progressively from one node to the other resulting in staining of most nodes in the lymphatic basin.

We, with others[1,2], have reported that the false-negative rate of sentinel node histology increases in the presence of gross involvement of lymph nodes in the lymphatic basin; due to the diversion of lymphatic flow as a result of replacement of the sentinel node with metastatic carcinoma. Therefore, seven of 12 patients who had a false-negative sentinel node biopsy were not suitable for this technique.

The authors describe two patients with micrometastasis which we believe is significant. Liefers *et al.*[3] showed that molecular detection of micrometastasis is a prognostic tool in stage II colorectal cancer.

This field is evolving and further studies are required to confirm these data before the concept of sentinel node biopsy in colorectal carcinoma is completely disregarded.

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Author's reply

Sir

We thank Messrs Keshtgar, Amin and Taylor for their reaction. We would like to see the sentinel node concept apply in tumours other than melanoma, breast, penis or vulva, but no proof for this has been published. We agree with the definition of the sentinel node, were fully aware of it at the time of writing the article and never called the blue nodes sentinel nodes, as the attentive reader will have noted.

Being aware of the problems when there is massive lymph node involvement (as pointed out in the discussion), we decided to include the patients with enlarged nodes as this was one of the first published studies to explore this concept in colonic cancer.

The relevance of micrometastatic disease is still a matter for debate, but we too considered it important enough to search for micrometastases.

Undeniably, only the first word has been said on the sentinel node concept in visceral tumours. We look forward to other studies.

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Randomized clinical trial of laparoscopic *versus* open inguinal hernia repair

Letter 1

Sir

I read with interest the paper by Juul and Christensen (*Br J Surg* 1999; **86**: 316–19) reporting a randomized trial comparing transabdominal preperitoneal (TAPP) with open inguinal hernia repair.

In the discussion the authors point out that 'in a previously published multicentric trial the laparoscopic procedure was compared with a suboptimal open surgery group'. Notwithstanding, in 23 patients belonging to the open group in the present authors' series simply a 'sutured closure of the internal ring' was performed. As these patients were adults, can this be regarded an adequate repair? One wonders how many of the three early recurrences seen in the open surgery group belonged to this subgroup. A previous prospective randomized study has demonstrated the long-term recurrence rate after high ligation of sac combined with closure of the internal ring to be as high as 34 per cent[1].

Furthermore, the authors have chosen to use polydioxanone for the Shouldice repair in preference to a non-absorbable suture. Again, in a prospective randomized study this practice has been shown to be associated with a higher recurrence rate when compared with the use of non-absorbable suture material[2].

Comparing a group of patients having open hernia dealt with by less than optimal methods with a group having laparoscopic preperitoneal mesh repair amounts to comparing apples and oranges. The technical considerations may not matter when comparing early outcome measures such as postoperative pain and length of recovery, but may influence the comparison of long-term recurrence rates between the groups.

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1 Beets GL, Oosterhuis KJ, Go PM, Baeten CG, Kootstra G. Long term follow-up (12–15 years) of a randomized controlled trial comparing Bassini–Stetten, Shouldice, and high ligation with narrowing of the internal ring for primary inguinal hernia repair. *J Am Coll Surg* 1997; **185**: 352–7.

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Letter 2

Sir

The trial by Juul and Christensen compares laparoscopic repair with a muscle-stitching operation ('modified Shouldice') in healthy adults. There is, however, abundant evidence that tension-free repairs using mesh reinforcement of the posterior wall provide results in many respects superior to the Shouldice repair[1]. A more meaningful trial would be a comparison of laparoscopic and open mesh repairs. Moreover, the series is selective: patients over 75 years and those with serious cardiopulmonary or neurological illness are not included (10 and 7 per cent respectively in our series), groups in whom local anaesthesia is specifically indicated.

The reported advantages of laparoscopic repair in this study were reduction of immediate postoperative pain and shorter time (13 days) before return to normal activities. The former is widely reported[2], the latter is no better than our experience of office and manual workers having open mesh repair who returned to work in 7 and 13 days respectively[3].

Thus, the advantage of laparoscopic repair is marginal, less pain in 25 per cent of patients in the first 2–3 days. We question the justification for undertaking an expensive and invasive operation taking significantly longer to complete, using general anaesthesia and with well documented risks of serious visceral or vascular injury, when the overall results of open mesh repair under local anaesthesia are significantly better.

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Author's reply

Sir

Dr Bhandarkar questions the use of a 'sutured closure of the internal ring' as an adequate repair in adults. As stated in the paper, it was used in 23 patients who each had a small indirect hernia and an intact inguinal floor. None of these patients developed a recurrence. Dr Bhandarkar also comments on the use of an absorbable suture, but we doubt that a recurrence rate of 2 per cent after 1 year of observation could be improved by using non-absorbable suture materials.

Mr Kark and colleagues find it more meaningful to compare laparoscopic with an open mesh repair. The first patient in our trial was included in 1994 and then the Shouldice repair was regarded as the gold standard. We, therefore, compared the new procedure with the best proven conventional method. Other prospective studies have compared laparoscopic with open mesh repair and found that the time for return to normal activity and work are shorter in patients receiving laparoscopic repair[1,2].

We do not agree that there is substantial evidence to justify the statement from Kark *et al.* that open mesh repair is a significantly better operation than the Shouldice and laparoscopic repairs. These methods need evaluation in clinical trials to find the optimal treatment for each hernia in different patients. We have to master more than one technique.

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