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**Impact of portal vein embolization on long-term survival of patients with primarily unresectable colorectal liver metastases**  
(*Br J Surg* 2010; 97: 240–250)

Sir

We read with interest this report of a large experience with portal vein embolization (PVE) before liver resection for colorectal hepatic metastases. The key to extrapolation of the message of these data lies in the authors' definition of the term 'initially unresectable' liver metastases. In their study, with an inclusion period spanning 16 years, there is likely to have been some variation in interpretation of this term, based not least on the increasing choice and sophistication of imaging modalities with time. Given that 100 per cent of patients who underwent PVE had initially unresectable disease (78 per cent bilobar), it would be useful to know whether adjunctive staging modalities such as [<sup>18</sup>F]fluorodeoxyglucose positron emission tomography (PET) were used in the later patients in this series<sup>1</sup>. If so, does the use of PET reduce the number of patients coming to PVE by demonstrating extrahepatic disease in individuals with bilobar disease? The demonstration of equivalent survival curves in patients coming to resection after PVE is encouraging but the number of patients at risk 2 years after PVE is small at 14. Although these findings support the current concept of modification of the future remnant liver before hepatic resection for extensive colorectal hepatic metastases<sup>2</sup>, more tightly defined prospective comparative data are required to precisely assess the benefit and role of PVE.

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**Authors' reply: Impact of portal vein embolization on long-term survival of patients with primarily unresectable colorectal liver metastases**  
(*Br J Surg* 2010; 97: 240–250)

Sir

We would like to thank Dr Jamdar and colleagues for their interest in our report. At our institute, initial unresectability of colorectal liver metastases is divided into technical and oncological reasons. Technical unresectability concerns the inability to completely resect all metastases while leaving sufficient normal liver parenchyma, and this definition has been used by our team since our first experience with advanced liver metastases<sup>1,2</sup>. The presence of extensive extrahepatic disease determines oncological unresectability.

Indeed, positron emission tomography (PET)/computed tomography (CT) is now a useful imaging modality for the diagnosis of extrahepatic disease sites. Nevertheless, it provides similar findings in terms of detection of intrahepatic metastases as contrast-enhanced CT only<sup>3</sup>. The majority of patients in our current study were

treated before PET/CT became available and it was therefore not routinely used in the preoperative staging of these patients. In recent years, PET/CT was used only in case of any doubt about the presence of extrahepatic disease for which conventional imaging modalities had insufficient diagnostic value. However, progression of extrahepatic disease excluded surgery only in eight (8 per cent) of 99 patients selected for portal vein embolization (PVE). The standard use of PET/CT would therefore have influenced the treatment strategy in only a minority of our patients.

Our study is one of the largest series describing the results of PVE on long-term survival of patients with extensive colorectal liver metastases. Of course, larger patient cohorts will allow more precise assessment of the role of PVE in the multimodal treatment of these patients. However, indications for PVE should remain strict, owing to the risk of tumour progression and associated unresectability, limiting the possibilities of prospective comparative trials for ethical reasons.

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**Implementation of a short-stay programme after breast cancer surgery (*Br J Surg* 2010; 97: 189–194)**

Sir

We read with interest the recent article by de Kok and colleagues, which reports on the introduction of a care programme to facilitate short-stay admission following breast cancer surgery. In line with previous national increases in short-stay breast cancer surgery we have previously conducted an audit of 162 breast procedures performed at the Princess Anne Hospital, Southampton, UK, from 1 to 24 October 2009. For this audit a day-case procedure was defined as a patient being admitted and discharged for operation on the same calendar day<sup>1</sup>. By this definition, 54 (89 per cent) of 61 breast-conserving procedures compared with four (10 per cent) of 39 mastectomies were performed as day-care procedures. In total, however, 84 per cent of patients who had a mastectomy went home on the day of surgery or the following day. Drains and axillary surgery did not influence successful day care. However, only a quarter of patients aged over 70 years, irrespective of operation, managed a day-care procedure. Adequate social support appeared to be the reason for a prolonged stay as an equal proportion of patients aged over or under 70 years had breast-conserving surgery.

Our findings demonstrate that breast-conserving surgery, including axillary surgery, can be performed successfully as a day-case procedure. In women who require mastectomy the increased emotional impact and the requirement for solid social support necessitates a longer (overnight) stay for most women. This is supported by the recent call for protective legislation against 'drive-thru mastectomy' in the USA<sup>2</sup>.

Finally, we note that in the study by de Kok the proportion of patients undergoing breast-conserving surgery was greater in the group treated after implementation of the care programme and wonder whether this

might have confounded their results. For example, the documented increase in short-stay admission may have been mainly due to the increased proportion having breast-conserving surgery rather than a result of the care programme that they implemented.

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**Author's reply: Implementation of a short-stay programme after breast cancer surgery (*Br J Surg* 2010; 97: 189–194)**

Sir

Thank you for your comments about our paper and congratulations on the positive results of your audit. One of the main aspects of our structured care programme is repeating patient information about the perioperative phase starting from the first outpatient visit following the diagnosis. In that first educational visit, lasting 1 h, the patient is not only informed about all aspects of surgery and admission by the breast nurse, but informal care and home-care nursing facilities are also discussed. In doing so, the patient and her relatives are well prepared for discharge from hospital as soon as her physical condition allows.

In our programme, there are no exclusion criteria for day-case operations, including age as this parameter

does not provide information about the physiological status of the patient. Informal care at home for at least 24 h after surgery is, however, a criterion for day-care treatment which, as noted above, is discussed repeatedly starting soon after diagnosis. For a detailed description of our protocol we refer to a previous paper<sup>1</sup>.

The increased emotional impact of ablative surgical procedures you describe was certainly observed in our study cohorts but this was not and, in our opinion, should not be a reason to exclude patients undergoing mastectomy from being treated as a day case. If the care team starts early with the explanation about the perioperative phase, we think that the patient is prepared for early discharge home, especially emotionally. This is also why home-care nurses received special training on emotional care following surgery, in addition to wound and drain care.

We have noted the developments in the USA, but believe that it is more important to assess each patient individually for her suitability for breast cancer treatment in a short-stay admission rather than denying all patients the opportunity to recover in their familiar home situation. In conclusion, we still believe that introduction of a care programme incorporating breast cancer surgery performed in day care or during a 24-hour admission is feasible and safe.

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**Randomized clinical trial of laparoscopic gastric bypass versus laparoscopic duodenal switch for superobesity (*Br J Surg* 2010; 97: 160–166)**

Sir

I would first like to congratulate the authors for their randomized clinical trial of laparoscopic gastric bypass versus laparoscopic duodenal switch for superobesity.

Determining the 'best' surgical treatment of superobesity is an important task facing the bariatric surgical community<sup>1</sup>. To date, there is no consensus on how to match a particular operative procedure to an individual patient. The most common factors considered include demographics (age, sex) as well as the degree of obesity (body mass index), amount of weight loss, extent of commonly associated comorbid conditions and possible resolution after surgery, cost-effectiveness and surgical risks<sup>2–4</sup>. However, of more concern in my view is the impact on quality of life (QOL), including social, cosmetic and employment issues, and the patient's wishes. So, the priority to be considered when making surgical choices should be individualized. Whether the operation is successful or ideal should depend mainly on patient satisfaction, instead of merely the degree of weight loss. Thus, the authors could ask patients to fill out a QOL questionnaire before surgery and at each follow-up visit. Besides the factors discussed in this study, analysis of patients' responses may assist in determining which type of surgery is more beneficial to which type of patient in the future.

Superobesity is associated with a greater burden of obesity-related comorbidities<sup>5</sup>. To lower the surgical risk of laparoscopic duodenal switch, Gagner and Matteotti<sup>4</sup> proposed a two-stage procedure; their mortality rate decreased from 6.5 per cent in the prestudy population to zero, with a reduction in complications from 23 to 5.6 per cent. The authors in the present study listed co-morbidities in

*Table 1*. Would it have been more reliable to use the five-category physical status classification developed by the American Society of Anesthesiologists to evaluate the risks instead of using co-morbidities? This might help in comparison with other series. Meanwhile, what criteria do the authors think patients should meet to receive a two-stage laparoscopic duodenal switch procedure?

Metabolic and nutritional deficiencies might be serious in patients undergoing bariatric surgery. However, body mass index, mentioned in *Table 4*, could not be regarded as a reliable tool for nutritional evaluation. Nutritional status and its change after surgery should be evaluated in more detail with a universal tool because it has an important impact on QOL and long-term follow-up.

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**Authors' reply: Randomized clinical trial of laparoscopic gastric bypass versus laparoscopic duodenal switch for superobesity (*Br J Surg* 2010; 97: 160–166)**

Sir

As Dr Chiu correctly points out, although several factors may be relevant when deciding on a bariatric surgical procedure in morbidly obese patients, there is no consensus about which procedure should be used in different patient subgroups. More data from randomized studies are needed to guide this clinical decision making. In our recent paper, we report 1-year safety and weight loss outcomes after gastric bypass and duodenal switch in superobesity. Evaluation of other important aspects of the operations, including impact on quality of life and cardiovascular risk factors, will be reported after longer follow-up.

The American Society of Anesthesiologists scores of the patients in our study are shown in *Table 1* in our report. We do not usually employ this score in the triage of patients for surgery. The duodenal switch is performed as a two-stage procedure in patients perceived to be high risk with a body mass index greater than 60 kg/m<sup>2</sup>. If considered feasible, we perform the duodenal switch as a single-stage procedure. The present study adds to the literature supporting this practice also for patients with a high body mass index<sup>1</sup>.

Dr Chiu further points out that nutritional deficiencies may cause serious problems after bariatric surgery. Patients in our randomized trial followed a predefined vitamin supplementation protocol<sup>2</sup>. After gastric bypass, concentrations of most vitamins were stable or increased, whereas there was a significantly greater decline in several fat-soluble vitamins as well as in thiamine after duodenal switch procedures. The significantly greater weight loss after duodenal switch is likely to inspire more patients to request this operation. It is, therefore, important to point out that duodenal switch might be linked to a greater risk of nutritional complications.

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**Randomized clinical trial of symptom control after stapled anopexy or diathermy excision for haemorrhoid prolapse (*Br J Surg* 2010; **97**: 167–176)**

Sir

We read the article by Nyström and colleagues with interest. There are few points that need further clarification.

This randomized controlled trial was well conducted, but would have been more informative if the authors had further classified the patients into the following categories: grade III, grade IV, and with or without skin tags. The statement in the last paragraph ‘The large external component is better served with diathermy excision, whereas stapled anopexy is suitable for a simple prolapse’, could only be made once the patients had been classified separately and the results studied accordingly.

In the haemorrhoidectomy group, a larger proportion of patients underwent skin tag removal (60 per cent) than in the anopexy group (12 per cent). This may be the reason for higher pain scores reported in the haemorrhoidectomy group. Furthermore, the criteria for removing skin tags is not mentioned.

Another important factor is the cost of the stapler used in anopexy. The cost of both procedures should have been compared. The article mentions the pain score difference, but time to return to work is not mentioned. It would also be interesting to have details of long-term follow-up of these patients and to know whether any required further treatment for persisting or recurrent symptoms.

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**Author’s reply: Randomized clinical trial of symptom control after stapled anopexy or diathermy excision for haemorrhoid prolapse (*Br J Surg* 2010; **97**: 167–176)**

Sir

We thank Dr Khan and colleagues for their remarks. All the information is stated in the article but we have attempted to look at the problem of classification from a different angle.

We deliberately did not ask surgeons to grade haemorrhoids in the traditional way. It is quite obvious from the literature that the mode of grading is not universally agreed. Among the published randomized clinical trials of stapled anopexy, the proportion of grade IV haemorrhoids ranges from 0 to 100 per cent; this may reflect a deliberate selection process or misclassification. A previous study found no correlation between the frequency of preoperative symptoms and degree of mucosal prolapse for operated patients<sup>1</sup>.

For the present study, we asked the surgeons to state whether prolapsed haemorrhoids were present at clinical examination. Almost all patients had at least one prolapsed pile. However, the most commonly stated definition of grade III haemorrhoids is the need for manual repositioning of the prolapse at defaecation. Only the patient can know

about this. Hence, we asked the patients to confirm or deny this need in a written questionnaire that was submitted independently of the surgeon’s examination. Some 68 and 71 per cent of patients in the anopexy and haemorrhoidectomy groups respectively acknowledged this need, which constitutes the proportion of grade III haemorrhoids in this study. Patients who deny such need would have grade II haemorrhoids according to this definition.

The real problem lies in defining grade IV haemorrhoids. Goligher’s seminal text states that skin-covered parts cannot be reduced. It seems clear that this refers to anodermal prolapse, which is usually termed skin tag or external component. It is not clear whether patients with grade IV haemorrhoids, in addition, must have a mucosal prolapse that needs manual repositioning. We think most surgeons would not require this for classification of haemorrhoids as grade IV. It leaves us with a subjective decision about how much of an anodermal prolapse is grade IV. We avoided this ambiguity by asking the surgeons to decide whether there was none, one or two skin tags, or many or circumferential tags. It does not entirely solve the ambiguity because an anodermal prolapse can be circumferential, but soft and reducible. The rate of circumferential anodermal prolapse before operation, as assessed by the surgeons, was 21 and 24 per cent, but only two patients in each group had such prolapse after 1 year.

For the trial, we left it to the surgeon’s discretion to remove remaining skin tags, but to record if this was done. The trial protocol did not stipulate any criterion for skin tag removal. I have personally been liberal with respect to excising skin tags and removed those that extended for 1 cm when gently tested with forceps. According to one study, removal of skin tags did not increase postoperative pain in stapled anopexy, with the proviso that the excision was not carried into the anal canal proper<sup>2</sup>. In the absence of other evidence, we believe it unlikely that removal of additional skin tags in the case of a diathermy haemorrhoidectomy would cause any extra pain.

With equal operating time and theatre time, and similar hospital stay, the cost difference amounts almost exactly to the cost of the stapler, which is hospital expenditure. However, the time on sick leave can be shorter for stapled anapexy because of the lesser degree and quicker resolution of pain. The time off work amounts to about 1 *versus* 2 weeks. To society as a whole, the cost of the stapler is offset by the shorter sick leave. However, the hospital pays for the stapler and money is usually not transferred to redress the balance<sup>3</sup>.

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**Open adhesiolysis is more effective in reducing adhesion reformation than laparoscopic adhesiolysis in an experimental model (*Br J Surg* 2010; **97**: 420–427)**

Sir

I read this original article by Prushik and colleagues with interest. It is generally believed that laparoscopic surgery inflicts less trauma to the peritoneum than open surgery<sup>1</sup>. The result of this study gives a contradictory conclusion. However, I feel that a

number of important issues require clarification and comment.

Precise definition of ‘open’ and ‘laparoscopic’ approach is needed in the manuscript. In the discussion, classification seems to be based mainly on use of carbon dioxide insufflation or not. Yet, the original main point of laparoscopic technique development is its minimal invasiveness via small wounds. The effects of carbon dioxide on the peritoneum have been discussed in several studies and its demerits have gradually been realized<sup>1–4</sup>. Furthermore, a ‘gasless’ laparoscopic technique using an abdominal wall lifter has been developed. It is suggested that these two independent factors (minimal invasiveness and use of carbon dioxide insufflation) should be studied and discussed separately.

At least three mechanisms operate in adhesion formation: mesothelial injury, local inflammation, and imbalance of fibrinolytic and fibrogenic activities<sup>4,5</sup>. When peritoneal trauma is created artificially, these mechanisms are initiated with different timing and degree. The authors of this study assayed only tissue plasminogen activator (tPA) and plasminogen activator inhibitor 1 mRNA, transforming growth factor  $\beta$ 1 and tumour necrosis factor  $\alpha$  mRNA levels in peritoneal adhesion tissue, and tPA activity in peritoneal fluid. However, other, currently unknown interfering factors may have roles in the process of adhesion formation and reformation. Is it better to take peritoneal biopsy specimens for pathological analysis and comparison? From histological changes it may be possible to demonstrate the final ‘net’ peritoneal response to all these mechanisms at different intervals, in addition to the adhesion score.

Primary adhesions were lysed with blunt dissection in both methods, but whether the same instrument was used in both methods to reduce possible bias is not mentioned.

The mechanism of adhesion reformation may be different from that of primary adhesion or it may also be multifactorial. However, until the processes are fully elucidated, we should prevent adhesion formation and reformation by meticulous haemostasis,

gentle and minimal tissue handling, and shortening operating time.

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**Authors’ reply: Open adhesiolysis is more effective in reducing adhesion reformation than laparoscopic adhesiolysis in an experimental model (*Br J Surg* 2010; **97**: 420–427)**

Sir

It is with interest that we acknowledge your response to our recent paper. Our results, showing that open adhesiolysis was more effective than laparoscopic adhesiolysis in reducing adhesion reformation, were as perplexing to us and other general surgeons who perform these procedures at our institution as they were to you.

Let us begin by clarifying your statement that 'it is generally believed that laparoscopic surgery inflicts less trauma to the peritoneum than open surgery'. This is a common assumption based on the untested hypothesis that minimally invasive translates into minimal peritoneal trauma; however, there are few studies to validate this supposition. We agree that the findings of our study were contradictory to what we expected. However, as we pointed out in the paper, the few published reports that took a 'second look' after laparoscopic adhesiolysis showed considerable adhesion reformation, suggesting perhaps that a minimally invasive approach may not be minimally traumatic to the peritoneum<sup>1,2</sup>.

To clarify your inquiry as to whether this study used classical 'open' and 'laparoscopic' methods, the open procedure comprised a standard laparotomy with a 3-cm midline incision closed in two layers. In contrast, the laparoscopic operation was representative of a typical minimally invasive procedure with 5-mm ports placed superior and inferior to facilitate peritoneal access as described previously<sup>3</sup>. Similar instruments were used during the blunt dissection of adhesions and did not appear to have an impact on the outcome.

To investigate the mechanisms that regulate adhesion reformation, we measured key pathways previously shown to regulate primary adhesion formation<sup>4</sup> and found none to be involved in the reformation process. This finding supported the notion that adhesion reformation occurs by mechanisms yet to

be determined. In addition, we closely examined adhesion development over time by histological techniques and were unable to discern a plausible mechanism of adhesion reformation.

Finally, although Dr Chiu raises several excellent points regarding adhesion reformation, we must disagree with his final statement that we should prevent adhesion formation and reformation by meticulous haemostasis, gentle and minimal tissue handling, and shortening operating time. Surgeons have been practising these surgical techniques for decades now, and we find ourselves entering the second decade of the 21st century still burdened by the rising rate of adhesion-related complications and readmissions despite advances in strategies to prevent adhesions<sup>5</sup>. All abdominal operations, regardless of the means of abdominal entry, induce trauma and inflammation as a result of cutting, electrocautery, ligation and cautery of bleeding vessels, etc., all of which can initiate adhesion formation. These injuries are inevitable consequences of abdominal surgery, as are adhesions. Clearly, new approaches are needed to prevent both primary adhesions as well as those reformed after adhesiolysis. For example, earlier work from our laboratory showed that a soluble neurokinin 1 receptor antagonist with proven efficacy in reducing primary adhesions<sup>4</sup> can also reduce adhesion reformation following laparoscopic adhesiolysis<sup>3</sup>. Until we better understand the mechanisms of adhesion formation and reformation, it is unlikely that effective preventative measures will be implemented by surgeons.

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