Gallbladder Cancer: The Role of Laparoscopy and Radical Resection

To The Editor:

We read with great interest the article by Shih et al1 in which they concluded that patients discovered with gallbladder carcinoma (GBC) during a laparoscopic cholecystectomy do not need to be converted immediately to open resection and should be referred to a tertiary care center for further exploration after completing cholecystectomy.

We agree with the authors’ contention that if the diagnosis of malignancy is made on the examination of the resected gallbladder or on histopathological examination and the expertise for radical resection is not available at the center, delayed resection at an expert center is useful and advocated. This has also been shown in previous studies.2,3 However, we do not agree with the authors’ view that surgeons who are not expert in liver resection can proceed with cholecystectomy even on intraoperative suspicion of malignancy and refer them to a more advanced center for resection at a later date.

They concluded this on comparing 6 patients who underwent resection at the time of cholecystectomy with 33 patients who underwent resection after laparoscopic cholecystectomy. There was no significant difference in survival between the 2 groups (5-year survival 60%, median survival not reached versus 5-year survival 49%, median survival 35 months). But, we think that the number of patients undergoing potentially curative resection at the time of cholecystectomy (only 6) is too small to compare the 2 groups. Also, all 6 patients underwent resection outside of Johns Hopkins Hospital. So, one cannot be confident about the expertise, radicality and adequacy of these procedures. What was the stage of these 6 patients? Were the 2 groups comparable in terms of stage? Why were the 3 patients diagnosed to have GBC at the time of cholecystectomy at Johns Hopkins Hospital itself, not offered curative resection at first instance?

There are many disadvantages in proceeding with cholecystectomy in a patient with GBC. There is risk of tumor cell dissemination and port site metastasis especially if the tumor is ≥T2, because the subserosal plane of cholecystectomy will be going through the tumor.4 Second, there is always a risk of dissemination due to bile spill and tumor perforation at the time of cholecystectomy. Third, postoperative inflammatory changes in the hepatoduodenal ligament and hilar area make the later dissection difficult. Sometimes it is difficult to differentiate these postoperative changes from the tumor and this leads to increased incidence of major hepatic resection and common bile duct excision.2 This could be one of the causes of higher incidence of extra hepatic bile duct excision in the incidental group as compared with nonincidental group in the present study (67% vs. 18%; P < 0.001).

Thus, we think if there is suspicion of GBC on laparoscopy itself, further management is decided by the availability of local expertise of radical resection and index of suspicion. If local expertise is not available, one should not proceed further for cholecystectomy and refer the patient to a higher level center. If the local expertise for radical resection is available, one can proceed depending on the degree of suspicion of malignancy to segment 4b + 5 resection directly or open cholecystectomy with small liver wedge resection, so as to avoid breaching the tumor. If the index of suspicion is high, one should directly proceed with segment 4b + 5 resection. If the index of suspicion is minimal, one can do open cholecystectomy + small wedge resection and send it for frozen section/imprint cytology. If it returns positive, one should do formal segment 4b +5 resection + standard lymphadenectomy.

It is important to make a distinction between the patients who are diagnosed at the time of cholecystectomy from those who are diagnosed on histopathology. The authors included the patients who were either diagnosed at the time of cholecystectomy or on histopathology in the incidental GBC group.1 But, other authors have included patients diagnosed on histopathology alone in incidental GBC.3–5 This is important as the 2 might represent different stages of the tumor and so different prognoses and outcomes. We would like to know how many patients in the incidentally diagnosed group were diagnosed on laparoscopy and how many on opening the gallbladder specimen or on histopathology alone.

Also, we would like to highlight that 7 of 13 patients who underwent conversion at the time of cholecystectomy for curative resection were found to be unresectable. These we think were most probably missed GBC because such an advanced tumor should have been picked up on preoperative ultrasound. Have the authors tried to review the ultrasound of these patients done at the time of cholecystectomy for any finding suspicious of GBC. In our experience, we have found that many of these so called “incidental GBC” are actually missed GBC and these present some pointers toward malignancy on preoperative ultrasound such as focal thickening, diffusely thick walled (>5 mm), sessile polyp, or lymph nodes in hepatoduodenal ligament.

We believe that there are 4 patient groups that actually represent a continuum of the same disease with increasing stage and decreasing prognosis: (1) Incidental GBC–Malignancy diagnosed only on histopathology with no suspicion preoperatively or intraoperatively including the examination of opened up gallbladder; (2) Unsuspected GBC–Malignancy suspected intraoperatively, but not on cut section (no preoperative suspicion); (3) Missed/Overlooked GBC–Patients in whom there were some indicators of malignancy on ultrasound but were missed, misinterpreted or overlooked by the surgeon, and (4) Primary GBC–Malignancy diagnosed or suspected on preoperative imaging.

Shih et al, have also concluded that the radicality of the resection offered a survival advantage only in stage II, but not in stages I, III, and IV. However, they have not provided the data for stages other than stage II. Other studies have shown that radical resection offers a survival benefit even in stage T1b.2–6 It is difficult to draw a definite conclusion for stages I and III, as there were very few patients undergoing resection in these groups, with 8 patients in stage I and 10 patients in stage 3.

We have a few other queries for the authors, (a) In the nonincidental group, (1) Why 17 of 28 patients, undergoing surgery with curative intent, underwent cholecystectomy alone. Simple cholecystectomy alone is curative in patients with T1a only.2,6 Why lymphadenectomy was done in only 32% of patients. (3) What were the criteria for unresectability on laparotomy in this group? (b) In the incidental group, (1) What was the time interval between the cholecystectomy and radical resection. (2) Why was port excision done in only 20 patients of 33 undergoing potentially curative resection in the incidental group.

Shivendra Singh, MS, MCh
Anil K. Agarwal, MS, MCh
Department of Gastrointestinal Surgery
GB Pant Hospital and Maulana Azad Medical College
New Delhi, India
aka.gis@gmail.com

REFERENCES


Reply:

We are pleased to have the opportunity to respond to the letter to the editor, and thank Professors Agarwal and Singh for their interest in our work.

The letter written in response to our article “Gallbladder Cancer: The Role of Laparoscopy and Radical Resection” states “…we do not agree with the authors’ view that surgeons who are not expert in liver resection can proceed with choledochoectomy even on intraoperative suspicion of malignancy and (then) refer (patients) to a more advanced center for resection at a later date.” Our position may have been misinterpreted based upon the following statistical conclusion in the results section that “Staged curative resection (ie, referral to a tertiary center following discovery of an incidental gallbladder cancer during or after laparoscopic cholecystectomy) did not adversely affect survival.” The data underlying this conclusion indeed have the inherent weaknesses described by Professors Agarwal and Singh. In other words, the data are not sufficiently powerful to prove that referral in parts either a benefit or a disadvantage in this group. Our understanding of the weakness of the data on this point, combined with our clinical experience and a knowledge of the literature, led us to state the following opinion in the discussion section: “aggressive resection of gallbladder cancer may be of benefit in carefully selected patients and they should be referred to a hepatobiliary center if the initial team is not comfortable with the required procedure.” So, we basically agree with Professors Agarwal and Singh that “…if there is suspicion of GBC on laparoscopy itself, further management is decided by the availability of local expertise of (sic) radical resection and index of suspicion.”

Further, we stated in the discussion that “it is certainly possible that an early stage gallbladder cancer patient could suffer dissemination during an initial operation in which the cancer is unrecognized, and therefore the resection is less than optimal. Surgical technique is critically important in situations where the preoperative profile and the intraoperative findings suggest the possibility of malignancy. Thus, if the dissection is difficult, or the risk of gallbladder rupture appears high, conversion to an open procedure may avoid the intraoperative spread of malignant cells.” If on preoperative imaging there is a high degree of suspicion for gallbladder cancer, that patient should be referred to a center with expertise. If intraoperatively, upon laparoscopy, there is a new high suspicion of gallbladder cancer and the surgeons do not have the expertise to deal with it, and the dissection to that point has not committed to cholecystectomy, then that patient should probably be closed and sent to a referral center. If however, the dissection has proceeded to a point where the patient is committed to having a laparoscopic cholecystectomy, and at that point they become highly suspicious of gallbladder cancer, and they do not have the expertise to deal with open radical resection, then the laparoscopic cholecystectomy should be completed and the patient should then be referred. Obviously, the ideal situation is recognition before attempting laparoscopic cholecystectomy, or that during laparoscopic cholecystectomy, if suspicion of gallbladder cancer surfaces and the team has the expertise, it can convert to an open radical procedure.

Professors Agarwal and Singh have posed additional questions regarding the analysis of data and the evaluation of our cohort of patients. Since this was a retrospective review, covering a period from 1995 to 2003 in which a variety of approaches were taken to this vexing disease, the data analysis was limited. The analysis required the grouping of some patients that in a larger series would have comprised their own group. Currently, more consistent protocols regarding evaluation with ultrasound, etc, are being used, as is the case with cancer care in comprehensive cancer centers throughout the world.

Professors Agarwal and Singh also ask, “We would like to know how many patients in the incidentally diagnosed group were diagnosed on laparoscopy itself and how many on opening the gallbladder specimen or on histopathology alone.” In reference to the “7 out of 13 patients who underwent conversion at the time of cholecystectomy for curative resection—found to be unresectable . . .” Professors Agarwal and Singh ask if we tried to review the ultrasound of these patients done at the time of cholecystectomy for any finding suspicious of GBC.” As our manuscript states, 50 of the 53 incidentally diagnosed patients had their initial operation outside Johns Hopkins and were subsequently referred. As a result, their records from outside institutions were not complete enough to answer these questions.

Again, we thank Professors Agarwal and Singh for their interest in our work, and their attention to important details. Clearly, a retrospective review of a rare disease at a referral center over a substantial number of years managed by multiple surgeons often results in less than perfect data analysis. Despite these flaws, we believe the manuscript makes important contributions to the emerging literature regarding this stubborn and often devastating disease.

Laparoscopic Versus Open Colonic Resection: Better Design and Results
Presentation are Required for Sufficient Interpretation

To the Editor:

Klaarenbeek et al recently presented a double-blind trial comparing laparoscopic versus open colonic resection for elective diverticulitis surgery. Although the authors should be congratulated for undertaking such a double-blinded trial, where the primary endpoints were morbidity, it is unfortunate that the design and data presentation are made with many inadequacies that hinder sufficient interpretation. Thus, the duration of hospital stay was with no clear-cut discharge definitions, or analyses on why the patients stayed in hospital, although in the discussion it is mentioned that the long hospital stay (5–7 days) reflected the European health care system. Are the authors studying traditions or the effect of laparoscopic versus open surgery? Also, pain scores were not presented (measured at rest or function?) and neither duration nor opioid requirements during PCA or the overall pain management was in accordance with current evidence (available at: www.postoppain.org). Some patients received epidural analgesia, but there is no information on the type and content. There is no information about nausea and vomiting or why the patients did not tolerate early solid food or when defecation occurred. In the method section, it is mentioned that nasogastric tubes were removed at the end of operation, but in Table 4 between 44% and 58% had a nasogastric tube that is difficult to understand as the tube reinsertion rate was only between 8% to 10%. Finally, it is a little surprising that the discussion does not refer to the concept of fast-track surgery where several trials (randomized and nonrandom-
ized as well as double-blind trials in elderly patients) have demonstrated much faster achievement of discharge criteria and shorter hospital stay—all evidence-based and with a positive effect on morbidity.\textsuperscript{2–4} Therefore, comparison of postoperative outcomes in relation to surgical technique needs to be combined with updated evidence-based care principles to allow sufficient interpretation.\textsuperscript{5} We hope that such methodological issues will be considered in future trials, also to achieve the optimal potential for minimal invasive colorectal surgery.\textsuperscript{3,6}

Henrik Kehlet, MD, PhD
Section for Surgical Pathophysiology
Rigshospitalet Copenhagen University
Denmark

REFERENCES

Reply:
W would like to thank Professor Kehlet for his useful comments in his letter to the editor about the Sigma-trial. His questions concern to what extent the comparison of postoperative outcomes in relation to surgical techniques (laparoscopic vs. open sigmoid resection in elective diverticulitis) need to be combined with updated evidence-based care principles.

This randomized study was performed as a multicentric trial between 2002 and 2006. During this period, progressive implementation of fast-track principles have been adopted. Moreover the primary aim of the study was not to demonstrate the advantages of perioperative care, in terms of the length of nasogastric tube, pain management, and hospital stay, but the differences in major morbidity, in a double-blinded way, after laparoscopic or open surgery.

The obtained hospital stay in our study reflects the European standards of laparoscopic and open surgery at the start of the trial in 2002 and probably even today. In the Sigma-trial clear discharge criteria were established. Patients were discharged after having a bowel movement, tolerating solid food, and once they were able to walk properly, and felt comfortable with oral analgesia. Our results, 5 versus 7 days (statistically significant in favor of the laparoscopic group) is clearly longer than the data obtained by Basse et al and Kehlet et al of 2 days, but quite comparable with the 5 and 7 days obtained by King et al, comparing laparoscopic and open colonic resection within an enhanced recovery program.\textsuperscript{1,2,3}

Nevertheless some questions concerning the use of nasogastric tubes and pain management need explanation. Professor Kehlet addresses the high percentage of nasogastric tubes used postoperatively. This may be considered, indeed, as a protocol violation as our study stressed its removal at the end of the procedure. An explanation might be a liberal policy of nonremoval and reinseration of nasogastric tubes at the recovery room by the anesthesiologists. Furthermore the “erroneous” nasogastric tubes were removed very soon after return from the recovery room, with a median duration of 0 days in the laparoscopic group versus 1 day in the open group.

Concerning pain score presentation, visual analogue scale (VAS)-pain scores were analyzed by repeated measures analysis, showing a significant lower level of pain in the laparoscopic group over the 5-day postoperative period (1.6 points on average; \( P = 0.003 \)). This significant reduction in VAS-pain score is presented in the results section. Moreover duration of systemic analgesia is given in table 4, indicating some benefit of the laparoscopic approach. On the other hand opioid requirements, which would have been helpful in adequate pain assessment, were not registered. Furthermore, in our study, 16% of the included patients received epidural analgesia instead of patient controlled analgesia and this may also be considered a violation of the protocol. To date thoracic epidural analgesia is favored over patient controlled analgesia, even though this technique seems to offer no advantages in combination with fast-track programs and there is no reduction of major morbidity rates in patients undergoing colorectal surgery.\textsuperscript{4,5} Moreover the current recommendations of the PROSPECT workgroup were not clear at the start of the trial in 2002. Since then, several studies on enhanced postoperative recovery programs have been published, showing improvements in terms of pain, mobilization, and hospital stay. In contrast, no significant reduction in major morbidity has been proven.\textsuperscript{5}

In conclusion, we will try to answer the main question of this letter: “Are the authors studying traditions or the effect of laparoscopic versus open surgery?”

The aim of this study was to demonstrate the possible benefits of the laparoscopic approach in terms of major morbidity. The Sigma-trial has shown a significant reduction in major morbidity if elective diverticulitis is approached laparoscopically instead of with an open approach. Moreover Professor Kehlet is right that the fast-track perioperative principles have contributed to a great extent to the enhancement of the quality of life of our patients and to the shorter hospital stay. Probably the combination of different factors of the fast-track treatment and the laparoscopic approach will provide the best outcome for our patients. Soon the LAFA trial will define the role of laparoscopic surgery as part of the fast-track principles.\textsuperscript{6}

Bastiaan R. Klarenbeek, MD
Donald L. van der Peet, MD, PhD
Miguel A. Cuesta, MD, PhD
Department of Surgery
VU University Medical Center
Amsterdam, The Netherlands
br.klarenbeek@vumc.nl

REFERENCES

Emergency Tourniquet Use

To the Editor:
I read with interest the important article by Kragh et al, \textit{Survival With Emergency Tourniquet Use to Stop Bleeding in Major Limb Trauma}, published in the January 2009 issue of the Annals of Surgery.

Emergency Tourniquet Use

To the Editor:
I read with interest the important article by Kragh et al, \textit{Survival With Emergency Tourniquet Use to Stop Bleeding in Major Limb Trauma}, published in the January 2009 issue of the Annals of Surgery.
I note that it states that 5 (3 American and 2 Iraqi) casualties with isolated limb injuries—who had indications for tourniquet application, but had no tourniquet used, all died. A survival rate of 0% versus 77% for those casualties with indicated tourniquets used \( (P < 0.007) \) is then quoted in the body of the article. However, the summary/abstract states that 0% versus 87% \( (P < 0.001) \). Which percentage is correct?

The 5 individuals who were alive in the field and did not have tourniquets applied apparently bled out in the presence of their fellow soldiers. This subgroup was then limb-matched with 13 others, all of whom had tourniquets applied, of whom 10 survived. The mean age in the nontourniquet group was 40.6 years (median: 46, range: 37–50) and that in the tourniquet group was 26.9 years (median: 25, range: 21–35). This is a significant difference in age profiles \( (P < 0.004, \text{student} \ t \text{ test}) \). Could the authors comment on this difference as a potential confounding variable given the physiological effects of aging? Also, were these soldiers or contractors? The ages of the 5 dead would seem to be at odds with routine military service. Finally, was there an increased limb fasciotomy rate in the early hospital care of the survivors?

Lt Col Paul Parker, FIMC, FRCS(Ed)(Orth), RAMC
Department of Orthopaedics, British Army Academic Department of Military Surgery and Trauma
RCDM, Selly Oak Hospital
Birmingham, England
Paul.Parker@stees.nhs.uk

REFERENCE


Reply:

I thank Lt Col Parker for his interest and the Annals of Surgery for the opportunity to reply.

Both percentages are correct. As explained in the text, one deals with use and the other with indication.\(^1\) Regarding age, the work was an observation, not an experiment, and so the cases were not randomized. That they did not have statistically similar ages is common in such situations. Our experience with age is evidently broader than Lt Col Parker’s. The associations among age, hemorrhage, shock, and tourniquet use are inadequately evidenced in clinical practice and are infrequently studied in translational research. We saw servicepersons, contractors, and civilians. We did not emphasize job categorization since it was often unreliable and ultimately unproductive of knowledge.

Fasciotomy was beyond the scope of the work and was addressed elsewhere.\(^2^3\)

John F. Kragh, Jr., MD
US Army Institute of Surgical Research
Fort Sam Houston, Texas
E-mail: john.kragh1@us.army.mil

REFERENCES


To the Editor:

We read with great interest the systematic review “The Current Role of Radiofrequency Ablation in the Management of Hepatocellular Carcinoma,” in which Lau and Lai summarized the published data and clearly defined the role of radiofrequency ablation (RFA) as a treatment option for hepatocellular carcinoma (HCC).\(^1\) As the authors pointed out, patients with smaller HCC (tumor diameter: ≤30 mm) have significantly better local control compared with patients with larger tumors. However, given the absence of randomized controlled trials, there is no consensus on whether to use the percutaneous, laparoscopic or intraoperative approach. In addition, we would like to emphasize the importance of operator-related expertise and hospital volume on the outcome of RFA for HCC treatment, as many clinicians are embarking on this therapy without paying much attention to its associated learning curve.

In patients with smaller tumors, we and others have found that the intraoperative approach appears to be superior to the percutaneous route.\(^2^3\) The open approach offers the advantage of direct examination for intra-abdominal extrahepatic disease and permits more precise positioning of the needle, resulting in lower local recurrence rates. A surgical approach also allows the use of a Pringle maneuver (temporary occlusion of both the hepatic artery and portal vein), which has been shown to result in larger coagulation zones when compared with treatment during normal hepatic flow.\(^4^5\) Despite the benefits of a lower complication rate, less invasiveness, and lower costs that the percutaneous approach offers, they may not outweigh the long-term risk of local recurrence. Future studies should focus on comparing the ultrasound-guided percutaneous approach, the computed tomography-guided percutaneous approach, and the intraoperative approach.

If the number of peer-reviewed publications over the last several years is an indication of its application in clinical practice, we can conclude that there is a widespread increase in the use of RFA for the treatment of HCC. Using the keywords “radiofrequency ablation,” “liver,” and “carcinoma” in a search on PubMed, the number of articles published in 2000 was 21, gradually increasing to 148 articles last year. As many centers report their initial experience, the confounding effect of a learning curve needs to be kept in mind when interpreting data of RFA treatment in the published data. Experience from laparoscopic surgery, another minimally invasive treatment modality, has emphasized the importance of a learning curve when introducing new surgical procedures.\(^6^–^8\) Although such a learning curve is also likely to apply to the use of RFA for the treatment of HCC, RFA is often misperceived as a simple and safe technique, especially in contrast to hepatic resection.

Two groups previously described the importance of operator’s experience with RFA on the outcome for the treatment of HCC. In 2004, Poon et al demonstrated that there is a significant learning curve when they compared their first group of 50 patients with a second group of 50 patients that underwent RFA treatment for malignant liver tumors (HCC, n = 84).\(^9\) The results showed a lower complication rate and a higher complete ablation rate in the latter group. In 2006, Hildebrand et al published a similar study comparing their first group of 42 patients with a second group of 42 patients treated with RFA for hepatic malignancies (HCC, n = 6).\(^10\) Although the complication rate for both groups was comparable, the complete ablation rate and the overall survival in the latter group were significantly higher. Both of these studies, therefore, showed a significant learning curve in the use of RFA.

Although we agree with the authors on the proposed indications for the use of RFA in the treatment of HCC, we would like to stress the importance of centralization of this treatment modality. As available published data demonstrates a considerable learning curve for RFA, an experienced and specialized RFA team could improve the patient’s outcome significantly. We would advocate that the use of RFA as a treatment modality for HCC should be restricted to high volume, experienced hepatobiliary centers with a dedicated, multidisciplinary approach.

Radiofrequency Ablation in the Treatment of Hepatocellular Carcinoma: The Need for Centralization

© 2009 Lippincott Williams & Wilkins
www.annalsofsurgery.com | 497

Copyright © Lippincott Williams & Wilkins. Unauthorized reproduction of this article is prohibited.
Letters to the Editor

Vincent E. de Meijer, MD, MSc
Department of Liver and Transplantation
Surgery
Erasmus MC – University Medical Center
Rotterdam
The Netherlands

Jan N.M. Ijzermans, MD, PhD
Department of Liver and Transplantation
Surgery
Erasmus MC – University Medical Center
Rotterdam
The Netherlands

Reply:

Radiofrequency ablation (RFA) for hepatic neoplasms can be accomplished using an open, laparoscopic, or percutaneous approach. Laparoscopic and open approaches increase the chance of detection of unknown intrahepatic and extrahepatic tumors because they allow complete abdominal exploration and intraoperative ultrasound assessment. The additional advantages of open and laparoscopic approaches are the accurate placement of probes, and the possible treatment of tumors in percutaneously inaccessible areas of the liver and tumors in close proximity to or invading adjacent organs. This is further enhanced by the freedom of the probe insertion at different angles in the open or laparoscopic approaches, with mobilization of the liver if necessary. As one moves from open to laparoscopic to percutaneous techniques one loses the advantages of the open approach but gains the advantages of the minimally invasive approach.1

Tumor size is an important determinant of outcome after RFA, but there is no consensus on the treatment approach in relation to tumor size. The rate of complete ablative necrosis decreases with the size of the tumor, particularly those larger than 3 cm.2 There is a general consensus that complete response of RFA therapy in patients is associated with improved outcome. There are several advantages to the open and laparoscopic approaches over the percutaneous approach that might contribute to the superior outcome in patients with large tumors (>3 cm). First, the open and laparoscopic approaches allow placement of probes without any restriction to achieve with a percutaneous approach. Finally, large tumors more frequently have irregular borders and satellite nodules as compared with small tumors, and these might be better delineated by intraoperative real-time ultrasonography than preoperative imaging.1–3

Long-term regular surveillance and an aggressive treatment strategy are required to optimize the benefits of RFA. Prompt treatment of residual or recurrent tumors is necessary to ensure a better outcome after RFA. Promising results of residual or recurrent tumors are achieved with a percutaneous approach. Finally, large tumors more frequently have irregular borders and satellite nodules as compared with small tumors, and these might be better delineated by intraoperative real-time ultrasonography than preoperative imaging.1–3

Differences in hydroxyproline concentration do not necessarily mean absolute forming complex procedures. However, about 80% of all hepatocellular carcinoma are found in Asia.4 As a result, tertiary units in Asia, particularly in China, Korea, and Japan, experience a high volume of case load even without particular methods made to centralize patients in these units.

REFERENCES

To the Editor:

In the elegant experimental study by Marjanovic et al.,7 Wistar rats were randomly assigned to 3 groups according to fluid volume administered during an ileo-ileal anastomosis. Among the groups, the volume restriction group received 3 mL/kg/h of a balanced crystalloid solution, whereas the volume overload group received 36 mL/kg/h of the same solution. Hydroxyproline concentration was determined to compare structural stability and bursting pressure was measured to compare functional stability between the groups. Hydroxyproline concentration in the volume overload group was significantly lower than that in the volume restriction group. Mean bursting pressure on the fourth postoperative day in the volume overload group was 77 mm Hg, which was significantly lower than the 112 mm Hg in the volume restriction group. Histologic examination showed a sign of submucosal edema in all animals with high volume load. The authors think that submucosal edema resulting from fluid distribution is closely related to anastomotic instability in the volume overload group.

Fluid Regimen for Intestinal Anastomosis

REFERENCES

Wan Yee Lau, MD, FACS, FRCS, FRACS(Hon)
Vincent E. de Meijer, MD, MSc
Department of Surgical Oncology
Daniel den Hoed Cancer Clinic
Rotterdam
The Netherlands

c.verhoef@erasmusmc.nl
j.ijzermans@erasmusmc.nl

© 2009 Lippincott Williams & Wilkins

Published online in Wiley InterScience (www.interscience.wiley.com). DOI: 10.1002/asia.200902432

498 | www.annalsofsurgery.com

Copyright © Lippincott Williams & Wilkins. Unauthorized reproduction of this article is prohibited.
as much as possible to protect lung and maintaining the patient in a state of zero fluid circulating blood volume and adequate tissue tation is essential to maintain effective tion or perforation, aggressive fluid resusci-trointestinal surgery for bleeding, obstruc-tions of hydroxyproline may be caused by different mechanisms but we should not forget one in particular, namely the change of extracellular fluid depending on the periop-erative fluid management. Therefore, in our opinion, the relative amount of hydroxyproline is more important to be determined in early anastomotic healing than the absolute amount of collagen. However, one could discuss the significance of the quality of synthesized collagen in the anastomotic area during early anastomotic healing as an addi-tive parameter to explain differences of anas-tomotic stability. When examining the heal-ing of intestinal anastomoses, at least 2 parameters, ie, functional (bursting pressure) and structural (hydroxyproline concentra-tion) stability, seem to be very important to be able to compare the results to those of other studies. In our study, we therefore chose the 2 parameters as mentioned above, but we measured neither the absolute content nor the quality of collagen of the anastomotic site. Certainly, measurement of the hydroxyproline concentration at fourth postop-erative day only depicts a “snap shot” of the process of anastomotic healing, but a very important one. Anastomotic healing pro-ceeds in different phases, in which collagen is being degraded on the one side, but syn-thesized at the same time, resulting in a relatively weak anastomotic stability be-tween postoperative day 3 and 5.7 When considering further anastomotic healing, sta-
bility of intestinal anastomoses rises rapidly, as it could be demonstrated in bursting pres- sure values, which may reach values of un-touched small bowel without any resection just 1 week after operation.8

The second point raised by Dr. Fujita is the development of significant extracellular fluid sequestration in volume overload animals and the role of inflammatory reac-tions during the healing process. In our opinion, the submucosal edema demonstrated rel-ates strongly to the administration of the crystalloid infusion in an unphysiological amount, since it was not seen in control animals or animals with fluid restriction. Furthermore, the development of bowel wall edema in untouched, not resected, small bowel after an excessive infusion of Hart-mann’s solution had already been shown by Chan et al,5 but edema persisted for 5 days at the anastomotic site while it resolved earlier in normal bowel wall. Thus, anastomotic edema seems to develop every time an intesti-nal anastomosis is performed, but it also seems to be highly dependent on the periop-erative fluid regimen.

Indeed, our experimental animal data are not directly transferable to human practice but they fit in our daily clinical experience and may be a convenient explanation for the clini-cally assumed higher rates of anastomotic complications in liberal fluid regimes. We greatly appreciate further clinical or experi-mental studies which on the one hand investi-gate an optimal perioperative fluid regimen in bowel surgery and on the other hand examine intestinal anastomotic healing on a more cellular/molecular basis since data are lacking in this area of research.

Goran Marjanovic, MD Christian Villain Robert Obermaier, MD Department of General and Digestive Surgery University of Freiburg Freiburg, Germany goran.marjanovic@uniklinik-freiburg.de

REFERENCES
8. Chan et al,5 but edema persisted for 5 days at the anastomotic site while it resolved earlier in normal bowel wall. Thus, anastomotic edema seems to develop every time an intesti-nal anastomosis is performed, but it also seems to be highly dependent on the periop-erative fluid regimen.

Tetsuji Fujita, MD
Department of Surgery
Jikei University School of Medicine
Tokyo, Japan

REFERENCES
CLOSING THE LOOP AROUND ESOPHAGECTOMY AFTER NEOADJUVANT THERAPY

To the Editor:

I read with interest the article by Schneider et al evaluating the ability to predict histopathologic regression after neoadjuvant therapy for esophageal cancer with routine staging studies including upper endoscopy, endoscopic ultrasound, CT and PET scans.1 The authors concluded that none of the tests were accurate enough to reliably exclude the presence of residual disease. This is an important finding, but it needs to be understood in a broader context. First, one of the rationales for trying to identify complete pathologic response is to then spare these patients an esophagectomy. However, complete pathologic response is a moving target. If routine histology is used most studies suggest that complete pathologic response occurs in 20% to 30% of patients after neoadjuvant chemoradiotherapy. When immunohistochemistry is added, Prenzel et al showed that 41% of patients thought to be node negative had micrometastases.2 Thus, the pool of patients who are truly free of all disease after neoadjuvant therapy is likely very small, and in the overwhelming majority of patients who receive an esophagectomy after neoadjuvant therapy, residual disease will be removed whether it is identified in the routine pathology report or not.

The second important issue that must be considered is whether resection of residual disease in patients that have not had a complete pathologic response improves survival. This is a critical issue given the common belief that patients with residual disease after neoadjuvant therapy are not curable and have a poor outcome after esophagectomy. This pessimism led to the concept that no one should receive resection after neoadjuvant therapy since those with a complete response do not need it, and those with residual disease will not benefit from it. Thus, a critical corollary to the findings by Schneider et al is evidence that esophagectomy improves survival in patients with residual disease. We have shown that the type of resection influences survival in patients with residual disease, and in particular long-term survival was only seen in patients that had an en bloc resection.3 Can the authors comment on their perspective of the outcome after esophagectomy in patients with residual disease, and the influence of the type of resection on survival?

The authors are to be congratulated for an important contribution and a clear message that current restaging studies can not rule out the presence of residual disease after neoadjuvant therapy for esophageal cancer. Now we must close the loop and demonstrate that patients with residual disease experience improved survival with an esophagectomy.

Steven R. DeMeester, MD
Keck School of Medicine
The University of Southern California
Los Angeles, CA
sdemeer@scsurgery.usc.edu

REFERENCES

Reply:

We thank the editors for the opportunity to respond to the letter by Dr. DeMeester regarding our recent published manuscript.

In his letter, Dr. DeMeester correctly concluded that in considering routine histology of endoscopic biopsy as well as the immunohistochemistry of the surgical specimen, the percentage of patients with true complete response is small after induction radiochemotherapy for esophageal cancer.1,2 However, the purpose of neoadjuvant therapy is mainly downsizing rather than complete eradication of the cancer as the dosage of radiation or chemotherapy is limited. Our results have shown that endoscopy, rebiopsy, or endoscopic ultrasound are not reliable means to decide which patients after induction therapy will have a major response and could go on with definite radiochemotherapy or which patients should have surgery because of a minor response.3,4 Therefore we are convinced that all patients after induction therapy need esophagectomy. This is supported by the results of the randomized controlled trial of Stahl et al comparing definite radiochemotherapy to neoadjuvant radiochemotherapy plus surgery.3,4 Not only did the patients in the surgery group experience significantly better local control and a better 5-year survival rate (28% vs. 17%) than the definite radiochemotherapy group, but considering the group with clinical response after induction therapy only, the patients with surgery had a better prognosis than those after further chemoradiation (56.5% vs. 37%).

As for the question about outcome after esophagectomy in patients with residual disease, we have shown that those patients with less than 10% vital residual tumor cells experience a significantly better survival rate than those with more than 10% vital residual tumor cells (71% vs. 18%) 5-year survival rate).5 At least the patients with minor residual disease have a clear benefit from esophagectomy after induction therapy. The patients in our series had only en bloc esophagectomy with 2-field lymphadenectomy and no transhiatal resection like a subgroup of the series of Rizzetto et al. Therefore we cannot comment on the influence of the type of resection on survival. We thank Dr. DeMeester for raising these important issues in the discussion of our results.

Arnulf H. Hölscher, MD
Daniel Vallböhmer, MD
Elfriede Bollschweiler, MD
Department of General, Visceral and Cancer Surgery
The University of Cologne Medical Center
Cologne, Germany

REFERENCES

LAPAROSCOPIC SIGMOID RESECTION FOR DIVERTICULITIS DECREASES MAJOR MORBIDITY RATES: A RANDOMIZED CONTROLLED TRIAL

To the Editor:

With great interest we read the article on laparoscopic versus open sigmoid resection (LSR vs. OSR) in the January 2009...
issue of *Annals of Surgery*. In this report, Klarenbeek et al analyzed the outcome of 104 patients undergoing a laparoscopic or open procedure for Hinchey I, IIa, and IIb diverticulitis. Patients having open surgery were more likely to develop major complications. Although laparoscopic surgery took longer to perform, blood loss was reduced, patients suffered less pain, experienced an improved quality of life, and hospital stay was reduced.

It is our opinion that this study is of accurate methodological quality and we would like to congratulate the authors. However, we feel some issues need to be clarified before the conclusion that laparoscopic surgery decreases major morbidity in patients with diverticulitis can be justified. First, the eligibility criteria state that patients having previous colorectal resection or median laparotomy (except for gynecological or obstetrical reasons) were excluded from analysis. These criteria seem to be in contrast with the characteristics of patients given. Table 1 shows that 46.2% of the patients in the laparoscopic group and 48.1% of patients in the open group had previous abdominal surgery.

Second, the authors reduced surgeon bias to perform all procedures by surgeons skilled in both techniques stating that 15 laparoscopic sigmoid resections indicate the end of a learning curve. However, the referred published data uses total operative time as an indication of learning. More recent published reports suggest the assessment of a learning curve should not be limited to measurement of a decrease in operation time, but should also include conversion and complication rates. Steady states are then reached at 70 to 80 laparoscopic interventions. All surgery was done in teaching hospitals where residents often perform this kind of surgery. It is of interest to know if surgery was performed by residents as well and if their representation was equally distributed in both groups.

Third, overall morbidity in the open group was higher than in the laparoscopic group. It remains unclear how many complications were found in how many patients. Many patients having a complicated postoperative course have more than one complication. Therefore it is essential to report the number of patients with a complicated postoperative course to see whether the difference still reaches significance. Fourth, our most important concern regards the open technique. One should not ignore that both open and laparoscopic surgery are developing and we doubt if the open group received state of the art perioperative care. Enhanced recovery programs combine individual evidence-based elements in the care of patients receiving colorectal resection. The use of colon lavages, nasogastric tubes, and delayed diet after colorectal resection is proven obsolete and specifically applied to open surgery patients in this trial. Colonic lavage is proven to be harmful in patients undergoing open colorectal resection. Nasogastric tubes were frequently used although there is evidence that they are disadvantageous in patient undergoing abdominal surgery. A step-up diet delays restoration of GI function while restoring a normal diet as soon as possible and the use of prokinetic drugs enhances gastrointestinal function. Furthermore, all patients received a patient controlled analgesia pump. Epidural analgesia is known to provide better pain relief, reduce the surgical stress response, and may further reduce postoperative mortality and morbidity. Using epidural anesthesia not only reduces the stress response, but also diminishes the use of opioids that hamper GI function. The choice for transverse laparotomy in the lower abdomen instead of a midline incision facilitates pain management and is less likely to reduce pulmonary function. Enhanced recovery programs imply guidelines to prevent excessive fluid administration, which is thought to contribute to an increased complication rate. In our center, we reduced the incidence of complications after open colorectal resection to a third by applying enhanced recovery after surgery. Almost all patients received epidural anesthesia and had significantly less fluid intake. Regarding the major complication rate, there was a reduction from 30% to 16%. Hospital stay was significantly reduced from 9 to 6 days.

**Pascal H. E. Teeuwen, MD**

Radboud University Nijmegen Medical Center Nijmegen, The Netherlands

P.Teeuwen@chir.umcn.nl

**Marièke G. J. Schouten, MD**

André J. A. Bremers, MD,

PhDRobert P. Bleichrodt, MD,

PhD-Radboud University Nijmegen Medical Center Nijmegen, The Netherlands

**REFERENCES**


**Reply:**

We appreciate the “letter to the editor” from Dr. Teeuwen et al responding to our manuscript entitled “Laparoscopic sigmoid resection for diverticulitis decreases major morbidity rates: a randomized control trial.” We will attempt to clarify the issues mentioned by the authors.

First of all, the authors address the high percentage of patients included with previous abdominal operations despite the fact that previous colorectal surgery was one of the exclusion criteria. Similar numbers of previous abdominal surgeries were present in both groups (laparoscopic 46.2% and open 48.1%); these were interventions such as cholecystectomies, appendicectomies, and gynecologic operations.

During the design of the study in 2002, the available evidence suggested 15 laparoscopic sigmoid resections to be the minimum experience for surgeons to participate in the trial. Moreover, all surgeons participating in our study at that time already exceeded this number and thus would also have met the more recent requirements. It is also mentioned that some bias might have been introduced by an unbalanced number of procedures performed by residents, suggesting that more open surgery was performed by residents and more laparoscopic procedures by experienced staff. During the study, all procedures were performed by surgeons who had met the requirements of our protocol.

Concerning the analysis and presentation of the complications, we agree that you...
Letters to the Editor

Many questions arose about the possible benefits of perioperative fast-track care in this study. The Sigma-trial showed that major morbidity rates of 5 in the laparoscopic group and 13 in the open group represent 5 patients and 13 patients, respectively, with major complications.

The fourth comment in the letter is about the development of open surgery and perioperative fast-track care. A few statements are made on obsolete or inadequate care, that were actually standard procedures at the start of the trial. And even though the referred literature shows no benefit of step-up diet, bowel preparation or nasogastric tubes, it is not to be expected that there is a negative or harmful effect on our primary endpoints.2,3 The same can be taken into account concerning epidural versus patient controlled analgesia. Nowadays we also prefer epidural analgesia in colorectal surgery, yet this does not result in a reduction in adverse morbid outcomes.4,5 The use of epidural analgesia instead of patient controlled analgesia in 16% of the included patients could be considered as a protocol violation. In concordance with the authors we support the superiority of transverse incisions instead of longitudinal incisions, but also the superiority of the use of laparoscopic over open approaches. This has been established in the short-term outcomes of all randomized studies on colon cancer and the here discussed Sigma-trial.6,7 Moreover, in open elective sigmoid resection after diverticulitis, we have no experience with mobilization of the splenic flexure through a transverse incision.

Many questions arose about the possible benefits of perioperative fast-track care in this study. The Sigma-trial showed that major morbidity is lessened if elective diverticulitis is approached laparoscopically. Moreover, the progressive introduction of fast-track aspects in perioperative care will contribute to an enhanced quality of life for our patients and a shortened hospital stay.

Bastiaan R. Klarenbeek, MD
Donald L. van der Peet, MD, PhD
Miguel A. Cuesta, MD, PhD
VU University Medical Center
Amsterdam, The Netherlands
br.klarenbeek@vumc.nl

REFERENCES


Methylprednisolone Therapy in Deceased Donors Reduces Inflammation in the Donor Liver and Improves Outcome After Liver Transplantation

To the Editor:

In the early period following orthotopic liver transplantation (OLT), initial poor graft function (IPGF) is one of the complications leading to primary graft nonfunction (PNF) in serious cases. We read with interest the recent article regarding steroid treatment in deceased donors before liver transplantation surgery by Kotsch et al.1 This clinical trial is based on the authors’ previous study showing that brain death was associated with a systemic cytokine storm in which a significant upregulation of inflammatory cytokines in the graft leads to a deteriorated ischemia/reperfusion injury and impaired early graft function, such as higher rates of primary nonfunction and acute rejection episodes.2 Although they could not find any significant differences in the rate of occurrence of initial poor graft function and/or primary nonfunction in this clinical trial, a significant ameliorated ischemia/reperfusion injury seen in the pretreatment group was accompanied by a decreased incidence of acute rejection.3 They speculated that the recipients receiving marginal or fatty grafts in particular might benefit from steroid pretreatment.

Many clinical trials of pretreatment of donor liver have been carried out by applying ischemic preconditioning,4–6 epoprostenol,7 N-acetylcysteine,8 and intraportal glucose infusion9; however, there is currently no evidence to support or refute these treatments with special respect to IPGF and PNF. This may be partly explained by the fact that graft viability after reperfusion is influenced a great deal not only by donor-associated risk factors, but also by recipient-associated factors such as preoperative comorbid disease status10 and the extent of surgical insults, including retransplantation and operating time.11 The authors evaluated the recipient-associated risk factors using Meld score and warm ischemia time of the graft; however, surgical risk factors as represented by intraoperative blood loss and/or perioperative administration of blood products, which remains a significant problem affecting immediate outcome.12,13 were not evaluated.

We previously evaluated the perioperative systemic responses of endotoxin and proinflammatory cytokines during OLT surgery and investigated their relation to graft viability.12 We found that exaggerated systemic induction of proinflammatory cytokines and/or endotoxin at the end of the anhepatic phase during OLT surgery was accompanied by IPGF and subsequent development of PNF, and independent factors predicting intraoperative responses of inflammatory mediators were the preoperative serum level of bilirubin and the intraoperative blood product requirement.13 Surprisingly, the circulating levels of interleukin (IL)-6 in their brain dead donors obtained just before organ procurement (1500 pg/mL) in their study were almost identical to those in our recipients obtained at the end of the anhepatic phase during OLT surgery, in which recipients required more than 10 units of blood transfusion.1,12 Since the usual postoperative peak value of circulating IL-6 after gastrointestinal surgery is 100 to 200 pg/mL, it seems likely that both brain dead donors without steroid pretreatment and OLT recipients undergoing complicated OLT surgery are subject to the same levels of heavy cytokine storm.

Muller et al found that the level of IL-6 released into hepatic venous blood at reperfusion was $619 \pm 87 \text{pg/mL}$; then, it

502 | www.annalsofsurgery.com

© 2009 Lippincott Williams & Wilkins

Copyright © Lippincott Williams & Wilkins. Unauthorized reproduction of this article is prohibited.
increased rapidly, peaking at 2 hours after reperfusion, being followed by a rapid decline and normalization within 24 hours after reperfusion. In our previous study, the circulating level of IL-6 in patients undergoing OLT surgery that required more than 10 units of blood transfusion increased up to more than 10 times as high as those at the end of the anhepatic phase within 4 hours after reperfusion (up to 20,000 pg/mL), and then decreased rapidly within 24 hours after reperfusion. In addition, the circulating level of endotoxin in these patients showed a continuous increase after reperfusion until 48 hours after reperfusion (up to 200 pg/mL), suggestive of continuous damage to the liver owing to progressive impeditment of hepatic microcirculation. These findings suggest that early exaggerated increases in the levels of inflammatory mediators after reperfusion may not only reflect an increased release of inflammatory cytokines from the graft, in which a significant upregulation of gene expression of inflammatory cytokines occurs during cold preservation, but may reflect perpetuated liver allograft injury in which a cytokine storm in the recipients causes ongoing graft injury rather than promoting tissue repair. The recovery of liver allografts from ischemia-reperfusion injury may be seriously impaired when a large amount of inflammatory mediators circulating in the recipients bursts continuously into the hepatic circulation. We would like to ask the authors to show the systemic cytokine profiles of recipients before and after reperfusion to clarify whether steroid pretreatment of donor livers could protect liver allograft viability from ongoing reperfusion injury, even in the heavy cytokine storm environment of the recipients. If steroid pretreatment of the graft has some beneficial effects in protecting grafts from ongoing reperfusion injury, this treatment may useful not only for marginal donor livers and fatty grafts, but also for recipients with high Meld scores or patients awaiting complicated OLT surgery, as represented by individuals with retransplantation or previous history of hepatobiliary surgery.

Keichi Uchida, MD
Yasuhiko Mohri, MD
Masato Kusunoki, MD
Department of Gastrointestinal and Pediatric Surgery
Mie University Graduate School of Medicine
Mie, Japan

REFERENCES


Reply:

Organ shortage and the increasing acceptance of marginal donor organs are the most challenging issues in solid organ transplantation. As a consequence, there are ongoing efforts to improve organ quality and increase the number of acceptable organs by donor treatment. There have been several publications showing that pretreatment in the experimental setup may be useful. Clinical trials using several compounds as treatment of the donor organ as well as the donor itself failed to demonstrate substantially improved organ function after transplantation. However, our trial was the first prospectively randomized clinical trial showing that donor treatment with methylprednisolone has significant beneficial effects.

Miki et al comment on our article discussing some important issues. However, it is proven that both the donor organ as well as the recipient contributes to the overall levels of proinflammatory cytokines after transplantation. It seems also obvious that recipient associated risk factors as comorbidities; age and the stage of liver failure contribute. Miki et al also showed that blood loss and operating time are correlating with postoperative cytokine levels. Keeping these factors in mind, we prospectively randomized our trial and compared 2 homogenous donor and recipient groups. As outlined in the article, the MELD score as parameter for the preoperative recipient status did not differ significantly in the study. However, the study was finished 2006 before the MELD score was introduced in the Eurotransplant area and no patients with scores over 35 were included in the study. We agree and speculate that the results in patients with scores over 35 may be significantly influenced by the recipient condition and the effects of optimizing the graft may be of secondary importance.

Surely blood loss and operating time are significant risk factors. In our study, the blood loss and the operating time were comparable in both groups excluding a bias based on these parameters (PRBC, 3.1 ± 2.3 vs. 3.0 ± 2.0; operating time, 274 ± 59 vs. 287 ± 48 minutes, P = NS). As the MELD score, the underlying diseases as well as the operative details were comparable in both arms we did not measure recipients IL-6 levels and we can not comment on this issue. Nevertheless, it is interesting to mention that circulating levels of IL-6 in untreated brain dead donors in our trial obtained before organ procurement are almost identical to those measured in recipients determined during OLT surgery. This
underlines the massive systemic inflammation organ donors are exposed and proof
the efficiency of anti-inflammatory treatment in our trial.4,4

However, publications underline the importance of the postoperative donor liver function on one side as source of proinflammatory factors as endotoxins and IL-6 and on the other side, as metabolizing organ reducing the systemic concentrations of cytokines.5,7 One can speculate that both pathways are influenced by improved preoperative organ function and anti-inflammatory treatment. Assessment of the impact of donor treatment on the cytokine levels in the recipient seems an interesting approach; which can’t be answered with the limited patient numbers in our trial.

Our study shows significantly better outcome after donor treatment with methylprednisolone comparing 2 equally defined groups. Restrictions may apply due to factor outlined in this letter; nevertheless, donor treatment seems advisable to improve the donor organ quality.

Frank Ulrich, MD
Department of Surgery
Katja Kotsch, PhD
Department of Immunology

Johann Pratschke, MD, PhD
Department of General, Visceral and Transplantation Surgery
Charité, Campus Virchow Clinical Center
Universitätsmedizin Berlin, Germany
Institute of Medical Immunology
Charité, Universitätsmedizin Berlin
Berlin, Germany
johann.pratschke@charite.de

REFERENCES

Lichtenstein Hernioplasty Versus Totally Extraperitoneal Laparoscopic Hernioplasty in Treatment of Recurrent Inguinal Hernia: A Prospective Randomized Trial

To the Editor:

I read with great interest the study by Kouhia et al.1 Very few published randomized controlled trials (RCTs) have compared totally extraperitoneal laparoscopic hernioplasty (TEP) with Lichtenstein repair in recurrent inguinal hernia and authors should be commended for this study with a long follow-up that further establishes TEP as a procedure of choice for managing recurrent inguinal hernia.2

I would like to make several comments. First, the authors mentioned that they used tackers for attaching the mesh in the TEP group. However, they made no mention about the position where the tackers were applied and how many tackers were applied in each patient. This is important because the number of tackers has been shown to influence the incidence of pain in the postoperative period.3 Second, being a prospective RCT, it is unclear why postoperative pain was not kept as a primary end point. Moreover, even if it was a secondary end-point, it would have been better to measure such an important parameter objectively by visual analogue scoring rather than indirectly by pain medication needed in the postoperative period. Third, the authors did not make any mention of urinary retention rate in the postoperative period.4

To the Editor:

We thank Dr. Garg for his comments and interest in our work.1 He addresses several important questions about both our work and inguinal hernia surgery in general.

The number of tackers used to attach the mesh in place was not recorded in the protocol. In 3 patients, the tackers were not used, because the mesh automatically positioned correctly without attaching. Tackers, when used, were first attached to the Cooper ligament. The number of tackers was increased as necessary to yield a correct positioning of the mesh, when the preperitoneal space was deflated. The number of tackers, although not recorded, most likely varied greatly among patients. As Dr Garg points out, the recent publication shows that the use of tackers to attach the mesh is associated with increased inguinal discomfort or pain.5 However, this study only presents the data from the first clinical examination postoperatively, which took place 6 months after surgery. As we discussed in our article, in all methods of inguinal hernia repair, inguinal discomfort is often present until approximately 1 year after surgery. It will be interesting to see if the difference detected by Taylor et al at 6 months between the 2 groups persists at the second, 2-year examination. However, our original study was designed to compare open and laparoscopic mesh repair, and the laparoscopic group had better outcomes. Therefore, in our opinion, it is a matter of academic interest, whether we would have had a more significant difference in favor of the

Pankaj Garg, MBBS, MS
Fortis Super Specialty Hospital
Chandigarh, India
drgargpankanji@yahoo.com

REFERENCES

Reply:

We thank Dr. Garg for his comments and interest in our work.1 He addresses several important questions about both our work and inguinal hernia surgery in general.

The number of tackers used to attach the mesh in place was not recorded in the protocol. In 3 patients, the tackers were not used, because the mesh automatically positioned correctly without attaching. Tackers, when used, were first attached to the Cooper ligament. The number of tackers was increased as necessary to yield a correct positioning of the mesh, when the preperitoneal space was deflated. The number of tackers, although not recorded, most likely varied greatly among patients. As Dr Garg points out, the recent publication shows that the use of tackers to attach the mesh is associated with increased inguinal discomfort or pain.5 However, this study only presents the data from the first clinical examination postoperatively, which took place 6 months after surgery. As we discussed in our article, in all methods of inguinal hernia repair, inguinal discomfort is often present until approximately 1 year after surgery. It will be interesting to see if the difference detected by Taylor et al at 6 months between the 2 groups persists at the second, 2-year examination. However, our original study was designed to compare open and laparoscopic mesh repair, and the laparoscopic group had better outcomes. Therefore, in our opinion, it is a matter of academic interest, whether we would have had a more significant difference in favor of the
laparoscopic group in cases where the tackers were not used.

At the beginning of the study, it was decided to concentrate mostly on long-term follow-up, and therefore, pain was not chosen as a primary end-point. Dr Garg states that the use of objective pain score or scale such as VAS would have been better than measuring pain by the doses of analgesics used. It must be remembered, that pain measurement can never be fully objective, because the intensity of pain is experienced differently by each patient. The VAS scale is one way of possibly more accurately recording the amount of pain, and is of course useful within its own limitations. In our hospital, the VAS scale has not been routinely used at any time during the study or after that. Our method of providing pain medication has long been "on-demand." If the patient feels that the amount of pain he/she has is limiting the recovery, pain medication is administered. This is after all on the same basis that the patient is taking pain medication after discharge from the hospital. There may be problems in this approach, if the patient has an opioid misuse history or is otherwise prone to addiction. However, the same problems arise with these patients in using VAS scale.

Dr Garg expresses his concern about urinary retention and its possible effect in decreasing patient satisfaction. In our study, 2 patients in the Lichtenstein group were postoperatively single-time catheterized a maximum of 3 times at the ward. This did not lengthen their postoperative stay. It is to a large extent unclear, what factors contribute to postoperative urinary retention. Spinal anesthesia has been speculated to be an independent risk factor independent of the nature of the surgical procedure. Therefore it is difficult to estimate, whether these 2 patients experienced some level of urinary retention due to the spinal anesthesia or the surgical procedure. Late postoperative urinary retention (occurring after the discharge from the ward) could be present in our study without us being aware of it, if the patient had been treated at the health-care center. However, no patient complained about such a problem at the out-patient clinic during the first follow-up at 3 weeks. Therefore, in our opinion, rather than designing studies with highly specialized treatment protocols, the emphasis in designing future studies should be on direct adaptability on clinical practice.

Sanna Kouhia, MD
Heikki Ahtola, MD
Tapio Hakala, MD, PhD
Department of Surgery
North Karelia Central Hospital
Joensuu, Finland
sanna.kouhia@pkssk.fi

REFERENCES

Not All Minorities Are Underrepresented in Academic Surgery

To the Editor:

Butler et al add to the literature on health care disparities, but this is a unique study in that they do not focus on the disparities in access to patient care but instead investigate the ethnic disparities among those delivering care. They discuss how ethnic minorities are underrepresented in academic surgery positions and that postulate that better representation will increase studies and investigations highlighting minority needs. Specifically they point out how African Americans comprise 12.3% of the US population, 7.1% of the US medical school graduates, but only 4.7% of the US surgical residents, 2.9% of the US surgical faculty, and 1.8% of the tenured faculty. Latino Americans comprise 14.8% of the US population and 6.7% of the medical graduates, yet 5.1% of the surgical residents, 3.6% of the surgical faculty, and 2.7% of the tenured faculty. Asian Americans were studied, but their discussion concentrates primarily on African and Latino Americans.

To have better representation of minority academic surgeons for African-Americans and Latino Americans perhaps, we should study what is that allowed the minority group of Asian Americans to overcome some of these ethnic disparities and represent a proportion of US surgical faculty similar to their population. The answer may not be related to what occurs in medical school, but may be related to a host of other factors. One might bring up the age-old question of “nature versus nurture?” Who are the important influences in a person’s life that affect career choices? Are parents’ beliefs a factor in this or is this related to teachers early in life, professors in college, or mentors in medical school?

Somehow, Asian Americans currently comprise 20.4% of US medical graduates yet they only make up 4.3% of the US population. Asian Americans are also represented in surgery with 17.2% of the US surgical residents, 10.8% of the US surgical faculty, and 4.9% of the tenured positions. No studies have explored why Asian Americans choose a career in medicine directly or why an Asian American medical students might choose a surgical or academic career.

Asian-Americans do place a high value on education and academic success. According to the US Census data in 2004, 49% of Asians age 25 or older had a bachelor’s degree or higher and the highest proportion of college graduates of any race or ethnic group in the US. In addition, 20% of Asians, age 25 and older had an advanced degree (Master’s, PhD, MD or JD). Information from the 1988 National Education Longitudinal Study data showed that Asian Americans were more likely to “live in intact homes, spend more time doing homework, and attend more lessons and activities outside of regular school” and that “Asian American parents had higher educational expectations than did American children.” Another educational study on adolescent’s perceptions in American, Chinese American, and Chinese high school students indicated that Chinese students were more willing to accept their parents’ advice and cared more about fulfilling academic expectations than did American students. Another study by the same
author indicated that Chinese parents were more likely to set higher standards and to help their children learn science than did American parents.\textsuperscript{5}

It is difficult to know if any of these educational studies will have any relevance to Asian American physicians, but perhaps we need to evaluate all minority physicians to understand cultural values and environment that made them pursue a career in medicine in the first place. Perhaps physicians and surgeons need to reach out to all minority youth to encourage them to study science and pursue a career in medicine so the future generations of academic surgical faculty will include all minorities equally.

\textbf{Linda L. Wong, MD}
Department of Surgery
University of Hawaii School of Medicine
Honolulu, Hawaii
hepatoma@aol.com

\textbf{REFERENCES}


\textbf{ERRATA}

Parmentier H, Melodelima D, N’Djin A, Chesnais S, Chapelon JY, Rivoire M. \textit{Ann Surg}. 2009; 249(1):129–136. In the article “High-Intensity Focused Ultrasound Ablation for the Treatment of Colorectal Liver Metastases During an Open Procedure: Study on the Pig” the names of each of the first six authors are listed in reverse, with the surname first. The authors and the publisher regret the error.

In the article by Dubecz, et al. (\textit{Ann Surg}; 2009; 249(3): 535–540), the last five references (18 to 22) were inadvertently omitted from the printed version of the article. The publisher apologizes for the omission.