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Letters to the Editor

To the Editor

Reading the article about the role of interleukin-6 (IL-6) in healing of intestinal anastomoses [1] by Ishimura and colleagues (Kagawa, Japan) with great interest, we believe some thoughts must be added. The messenger field, such as cytokines and other mediators of infection, is a complex area that has been studied by many groups around the world and of course with special attention from surgeons. Problems related to trauma, surgical trauma, sepsis, and burns bring questions to the surgeon's mind, and the role of these messengers seems to be an ever more crucial one.

Ishimura and colleagues evaluated the role of IL-6 in colonic anastomosis healing, especially in the presence of a septic condition. From their data it can be seen that modification of IL-6 expression strictly associated with sepsis or postsurgical trauma could impair healing of the intestinal anastomosis, perhaps influencing fibroblast activity. The authors stressed the importance of a local inflammatory reaction for successful wound healing mediated by IL-6.

In 1994 we became curious about the healing of intestinal anastomosis and particularly why colonic anastomoses have a greater risk of dehiscence than small bowel anastomoses. We therefore decided to compare the anastomosis tracts of the small and large bowel of the rabbit with particular respect to oxidative phosphorylation [2]. Previous studies had demonstrated massive collagen deposition in the large bowel anastomosis but with higher sufferance and more difficulty in collagen maturation versus that in the small bowel. We demonstrated different bioenergetic behaviors of the two cell types. Ileal mitochondria seemed to utilize glycolytic metabolism, rather than the oxidative metabolism utilized by colonic cells. After surgical stress and the critical organic condition with impaired peripheral oxygenation, colonic mitochondrial functions appeared to be poor, and the risk of ineffective anastomosis healing was often associated with an alteration of these already reduced fundamental functions.

We therefore believe that the factors that influence successfully healing are the local oxygen tension and oxidative phosphorylation modulators such as coenzyme. For every anastomosis we must consider laparotomy as an impaired but basic factor of the procedure. In contrast, septic status is a concomitant situation that influences the healing process negatively, as every surgeon knows. Nobody considers performing a colonic anastomosis in the presence of peritonitis. We and others believe that failure in these cases must be related to high general oxygen consumption with a significant reduction in oxygen tension available for intestinal cell metabolism. We do not believe, as Ishimura et al. wrote, that administration of anti-IL-6 antibodies could improve the anastomotic healing process.

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Reply

I thank Dr. Mario Testini and Dr. Giuseppe Piccinni for their valuable comments on our study. As probably anybody knows, my coauthors and I believe that the mechanism of wound healing could not be totally understood by any specific point of view because the process has many aspects. As they pointed out, we also believe that sufficient oxygen supply is needed for successful healing of an anastomosis, as cited in some of the references in our paper.

However, many other factors participate in anastomotic wound healing. For example, platelet-activating factor can accelerate healing by demonstrating a greater cellular infiltration [1]. Plasminogen activation by urokinase-type or tissue-type plasminogen activator stimulate the replacement of extracellular matrix [2]. Moreover, several cytokines, such as platelet-derived growth factor, tumor necrosis factor-α, and basic fibroblast growth factor also contribute to the wound healing [3–7].

In this study, we investigated anastomotic healing with respect to local IL-6 expression. Although IL-6 expression is not the only key phenomenon, our results suggested that local expression of IL-6 may play an important role in wound healing. Based on our results, there is a possibility of an effect of anti-cytokine antibody. We believe that further investigations are necessary to prove this hypothesis.

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To the Editor

We read with interest and with much surprise the article of Yilmaz Akgun and coworkers [1] because we have been involved in the management of amebic hepatic abscess (AHA) for years [2, 3]. We have insisted on routine puncturing of these abscesses and injection of air to replace the material aspirated. Only with smears and cultures is it possible to differentiate between "septic," "amebic," and "infected amebic" lesions, on which depends the choice of treatment. Injection of air allows simple, inexpensive imaging follow-up. Among our 93 patients, none had an elective open procedure, and we have had zero mortality. Only on occasion has it been necessary to use laparoscopy for guidance during percutaneous needle aspiration. This was discussed at the ISS meeting of 1993 in Hong Kong, at the Australian meeting in 1987, and at the Hungarian Surgical Association Congress in Budapest in 1984. Drs. Alton Ochsner and M. DeBakey since 1938 had recommended conservative treatment, not open surgery, for AHA.

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Reply

I thank Dr. Herrera-Llerandi for his comment and interest in our article and for giving me the opportunity to emphasize a number of points. I do not agree with Dr. Herrera-Llerandi because of the reasons below.

1. We do not advise aspiration of amebic liver abscess (ALA) routinely in the regions where ALA is endemic because the clinical and radiologic findings are sufficient for its diagnosis.

2. Aspiration risks secondary infection. In addition to aspiration, air injection into the abscess increases the risk of secondary infection in these patients.

3. I do not understand the insistence on follow-up with radiographs. Ultrasonography is less harmful and provides more information than radiography. Because the injected air is reabsorbed by hepatic tissue around the abscess after 1 to 2 weeks, it is impossible to follow up these patients by radiography.

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To the Editor

I am writing in response to the Invited Commentary by Jonathan R. Hiatt, to our article recently published in the *World Journal of Surgery* [1]. I appreciate Dr. Hiatt's review and insight and would like to take the opportunity to respond to some of the points he has raised. I address them in order.

1. He points out that he believes that at the present time the ". . . techniques are applicable to simple and routine gallbladder surgery." In our first 50 cases this constituted 76% of the cases. These operations were performed without enlargement of any of the ports. At no point does initiation of the surgery with miniature ports prohibit conversion to larger ports if deemed prudent or necessary. One cannot, however, start with larger ports, recognize a "simple or routine" case, and "convert" to smaller incisions.
2. He points out our note of the difficulties encountered in some obese patients and in patients with difficult or inflamed gallbladders. In these cases these conditions led to enlargement of one or more of the port sites, with the worst-case scenario being conversion to a "conventional" port configuration. Again, no "bridges were burned," and the only downside to these converted cases is that you have the added cost of the extra ports. I see this as no different from having to convert a laparoscopic case to an open procedure when deemed prudent or necessary. This also adds to the total cost of care but does not preclude us, ideologically, from initiating cases laparoscopically.
3. The limitations of the quartz fiber video-laparoscopes when compared to glass rod lens systems are pointed out. These 50 operations were performed visualizing through the 2 mm quartz fiber laparoscope whenever possible. Indeed, one of the major goals of this study was to assess the ability of laparoscopic surgeons to use this viewing technology to perform therapeutic laparoscopy. We used this technique in 110 patients before deciding that we could safely conclude that performing therapeutic surgery visualizing through 2 mm laparoscopes could be carried out safely and efficaciously. At that point we switched back to operating while visualizing through a 30°, 10 mm glass rod lens laparoscope through the umbilical

port. We advocate that surgeons initiate their use of 2 mm laparoscopic cholecystectomy utilizing this later visualization technique.

4. The epigastric location for the camera port site is described as "unconventional and disorienting to all but the most experienced laparoscopists." I must strongly disagree with this statement. Baylor College of Medicine is a "teaching institution," and I feel a strong obligation to fulfill my role as Director of Minimally Invasive Surgery for the Department of Surgery at this institution by teaching laparoscopic techniques to trainees at our institution. Accordingly, 52% of the cases reported here were performed by level VI surgery residents with supervision by an experienced attending physician. The statistics reflect all of the cases residents performed. We are proud of the skills of our residents and fellows but would not consider them to be "the most experienced laparoscopists." They were, however, easily capable of successfully performing this operation.
5. Dr. Hiatt concludes that the technique is feasible but offers no quantifiable advantages and is therefore "perhaps not yet desirable." Acknowledgment that the procedure is feasible is appreciated by the authors. Although this paper demonstrated no quantifiable advantages over conventional laparoscopic cholecystectomy, there were no disadvantages of using this technique in our study. Our subsequent report [2] involving 100 cases showed that for cases completed successfully with the 2 mm instrumentation there was a quantifiable advantage in length of stay (LOS) when compared to our "conventional" laparoscopic cholecystectomy control group. The control group LOS was 1.5 ± 0.3 days versus 1.2 ± 0.2 days for the group successfully treated with miniature instrumentation ($p < 0.05$). Further studies are clearly needed. Collection of data on patient satisfaction and physiologic response to the surgery is desirable.

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To the Editor

The paper by Meier and Tarpley on typhoid intestinal perforations in Nigerian children [1] is an excellent description of the management of typhoid intestinal perforations in children in less developed countries. It is commendable that even with the minimum of facilities they observed a mortality rate of only 20%. However, we want to discuss some of their observations.

The authors describe the use of a lower midline incision for operation in these patients. In our experience, midline incisions are best avoided in children, as there is a high incidence of wound dehiscence and "burst abdomen" with the use of these incisions. In our patients we have routinely used either a transverse incision or a Rutherford-Morrison incision, and we have noted significantly fewer wound complications with the use of the latter incision without compromising peritoneal lavage [2].

The authors did not describe the method of peritoneal lavage they use. They may not be using metronidazole due to nonavailability at their center. We have been routinely using 0.5% povidone-iodine-warm saline solution, as povidone-iodine and saline are readily available at even the least developed centers. Moreover, this has been shown to enhance the peritoneal defense mechanism in these patients [3].

The authors would do well to list the postoperative complications they encountered and their management given their limited resources. Surprisingly, only one of their patients developed an enterocutaneous fistula compared to an incidence of more than 10% in most reported series [4]. Is there a possible explanation?

We agree with the authors that simple closure of the perforation is probably the best surgical procedure in these sick patients. In addition to the advantages listed by the authors, one should also realize that most of these patients are operated on as an emergency at odd hours, and most of the time the operation is performed by trainee surgeons rather than senior surgeons in less developed countries. Simple closure of the perforation may be easy and rapid under these circumstances compared to more extensive procedures.

It is surprising that the number of perforations, degree of peritoneal contamination, and the perforation to operation interval did not correlate with mortality. In our study, mortality rates rose as the duration between perforation and operation increased, although the perforation was single in all our patients [4]. Also, in our experience, the degree of peritoneal contamination is an important factor that determines the outcome in these patients.

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Reply

We thank our colleagues Drs. Talwar, Prasad, and Talwar for their laudatory comments in paragraph one and note their consensus in paragraph five of their letter regarding our report on typhoid intestinal perforations in Nigerian children.

Regarding the preference of abdominal incision (vertical midline versus transverse), many or most experienced pediatric surgeons prefer the transverse incision whenever possible, especially in nonemergent situations. For our general practice registrars the vertical incision seems easier and faster to create and close, allows ready extension to improve exposure if required, and generally causes less blood loss. We were gratified that our dehiscence rate was less than 2% in this group of ill, often malnourished, and usually distended children.

At the Clinical Congress of the West African College of Surgeons meeting in Cotonou, Benin in 1981, we presented a randomized, prospective, multiinstitutional study comparing normal saline versus povidone-iodine/normal saline lavage for suppurative peritionitis. We did not find a difference in mortality or morbidity between the irrigating solutions, but we did have one serious allergic reaction in a child in the povidone-iodine/normal saline group. In our area povidone-iodine must be imported and hence is neither inexpensive nor readily available. We lavage the peritoneal cavity with locally produced, sterile normal saline until the effluent is clear.

In our Results section we noted seven nonlethal complications in our 75 patients: a leak from a site of ulcer closure, a metachronous perforation, a pelvic abscess, a fascial dehiscence, an enterocutaneous fistula, and two late small-bowel obstructions. These patients with complications were managed in standard manner: relaparotomy for the leak and new perforation, drainage of the abscess, and laparotomy for lysis of adhesions for the obstructions; the dehiscence and fistula patients were treated nonoperatively.

We previously believed and taught that the interval from the time of perforation, as best one can surmise it, to time of operation was key. In this study, to our surprise, the perforation-to-operation interval did not predict outcome. Professor Archampong of Korle Bu Hospital in Accra, Ghana has advocated the four "Rs": recognition, resuscitation, repair, recovery. Time spent in active preoperative resuscitation to achieve normovolemia, initiate urine output, and hopefully decrease tachycardia and toxicity is a good investment. We seek to be ready for anesthesia and operation within 4 to 6 hours of presentation or diagnosis if possible.

The key is to "get the stool out of the drinking water." We have a long way yet to go.

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To the Editor

The report by Smith et al. [1] provides more evidence toward defining the place of laparoscopic approach for adrenalectomy. Despite the authors' claim to have reviewed the world literature through late 1997, they missed some interesting reports that would have added more relevance to their conclusions.

Their question about "who should undergo laparoscopic adrenalectomy?" raises once more the debate regarding indications for surgical removal of an incidentally found adrenal mass, the so-called adrenal incidentaloma. Since 1994 the Belgian group for endoscopic surgery (BGES) has been assessing the relevance of proposed criteria [2] for removal of these incidentalomas: a secreting adrenal lesion after complete endocrine workup; diameter larger than 4 cm or an increase in size at any reevaluation; computed tomogram of intratumoral necrosis, hemorrhage, or irregular margins; high concentrations of dehydroepiandrosterone (DHEA). The 1997 BGES report [3] demonstrated that adhering to those simple criteria allows us to reduce to 13% the debatable indications in 52 consecutive laparoscopic adrenalectomies. By comparison, a recently updated series concerns 50 new cases of laparoscopic adrenalectomies over less than 2 years in Canada [4], the population of which is the same as that of the Benelux countries. Such a rise of 50 new cases can certainly be explained by the superb technique of the surgeons, but we also question their 14% incidence of debatable lesions classified as "other or data not available," in addition to the 15% of adrenal lesions classified as nonfunctioning tumors found incidentally, which comprises up to 29% of their series.

There is always the risk that a laparoscopic adrenalectomy must be converted to an open operation. Consequently, the availability of laparoscopic adrenalectomy should not change the indications for advising operation for incidentalomas. The previous [3] and latest [5] BGES results provide data that substantiate the prudent statement by Smith et al. about recommending routine adrenalectomy for incidentalomas. Nevertheless, the specific willingness of the patient to have his tumor removed deserves consideration because observation alone entails costs, both monetary and psychological, particularly for young patients.

Magnetic resonance imaging (MRI) is another tool that can be used to distinguish between benign adenomas and malignant adrenal tumors by comparing the intensity of the lesion signal to the signal intensity of liver, striated muscle, or fat. Benign adenomas usually have a low intensity ratio, whereas malignant masses and pheochromocytomas have a high signal intensity ratio. Contrast enhancement after injecting gadolinium diethylenetriamine-pentaacetic acid and comparing intensity ratios before and after contrast enhancement have further aided the differential diagnosis [6]. The range of criteria vary, however, when using different MRI techniques and equipment; hence if a strategy using MRI is promising, it has still to be confirmed. In the meantime, we must rely on more classic criteria.

Concerning the question of Smith et al. of "who should perform laparoscopic adrenalectomy?" it is worth mentioning that the BGES experience [3, 5] shows that this operation performed—even sporadically—by surgeons experienced in laparoscopic surgery is as safe as the open approach, provided those surgeons are also familiar with the rules and potential drawbacks of open adrenal surgery.

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Reply

I thank Drs. Michel, Decanniere, and Donckier for pointing out their work, which was not referenced in our manuscript. As they have well detailed, the availability of laparoscopic adrenalectomy should not change the management algorithm for an adrenal incidentaloma. Although intuitively it is appealing to consider the one-time intervention of laparoscopic adrenalectomy as a more immediate and definitive route by which to determine the nature of an incidentaloma, and by avoiding serial CT scans and examinations, a more cost-effective means of dealing with these lesions, until data become available to support these intuitions, the indications for adrenalectomy for incidentaloma should not change.

We applaud their work establishing that laparoscopic adrenalectomy performed sporadically by skilled laparoscopic surgeons is as safe as open adrenalectomy. We again emphasize that the technical challenge of performing a laparoscopic adrenalectomy remains secondary to the appropriate diagnosis, preoperative management, and patient selection required to manage patients with adrenal pathology appropriately.

We again thank Drs. Michel, Decanniere, and Donckier for their supportive and enhancing letter regarding our recent publication.

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