LETTERS TO THE EDITOR

Grading system for inflammation in ulcerative colitis

EDITOR,—Geboes et al described a grading system for inflammation in ulcerative colitis and carried out rigorous assessment of the reproducibility of this system (Gut 2000;47:404–9). This is a very useful study which fills a void in the histopathology assessment of ulcerative colitis. However, now that this system has been described, its use in clinical practice and clinical trials needs to be considered.

Many of the features that Geboes et al have used in their grading system are described as continuous spectra—for example, chronic inflammation assessed from no increase through to marked increase—but are divided into discrete groups (for example, mild, moderate, marked). This means that these features are ordinal categorical variables rather than continuous real numbers—that is, they have a numerically labelled order but the distance between adjacent numbers will not be the same through the whole range and there are no non-integer values.1 The consequences of this are that these grades cannot be used in procedures which require continuous variables, such as linear regression.2 The authors already seem to have made this mistake themselves as they give mean grades of the system in table 2 (to two decimal places), when they should have given frequency distribution histograms or possibly median grades with centiles as an indicator of spread. They do not state which method they used to measure the correlation between location of neutrophils in the epithelium, crypt destruction, erosions, and ulcerations (table 4 and last paragraph of results section).

The nature of ulcerative colitis as a chronic relapsing condition means that many studies and trials require a measure of inflammatory activity and need to relate this to other measured parameters. It is likely that this new grading system will be used in clinical trials of new treatments. The ordinal categorical properties of the new grading system means that measures such as mean grade should not be used in comparing groups of patients before and after treatment or between groups of patients receiving different treatments.

S S CROSS
Section of Oncology and Pathology, Division of Genomic Medicine, University of Sheffield, Medical School, Beech Hill Road, Sheffield S10 2RX, UK

Reply

EDITOR,—We appreciate the comments of Dr Cross on our paper in which we presented the results of a reproducibility study of a grading system for inflammation in ulcerative colitis. We agree that certain features used in the grading system in reality present as continuous spectra. Therefore, the scoring system is composed of major grades and subgrades. The features which represent the major grades such as architecture and infiltration of round cells are clearly different from each other. The continuous spectrum exists within the grades, especially for architectural changes and chronic inflammation. Major grades are divided into different subgroups (for example, mild, moderate or diffuse) and these are indeed ordinal categorical variables. The situation is even more complex. Indeed, the inflammatory cell population in the lamina propria is heterogeneous. It includes T and B lymphocytes, plasma cells, and CD68+ monocytes. The cells can synthesise cytokines or immunoglobulins, or express markers such as LFA-1 or ligand-receptor pairs such as CD40-CD40L which might be important for disease activity. In the past it has been shown for instance that there is a correlation between disease activity and immunoglobulin containing cells.3 Hence changes in “chronic inflammation” do not have only a continuous spectrum. There are changes in subtypes of cells, and these changes show a continuous spectrum. Analysis of routinely haematoxylin and eosin stained sections is therefore obviously limited. The aim of our study was to construct and evaluate a scoring system which can be applied routinely. In this system, the distinction between the major grades (for example, structural change, chronic inflammatory infiltration, infiltration of neutrophils in the epithelium, crypt destruction, and erosion or ulceration) is much more important than the subgrades. The differences between these major grades are clearly defined and do not present as a continuous spectrum. A change from one grade to another is a major difference, which can indicate an important effect, while changes within a grade from mild to moderate are far less important. Furthermore, the distinction between active disease (neutrophils and epithelial damage) and inactive disease is clearly defined. For evaluation of neutrophils in the epithelium, the number of crypts involved was counted.

The reproducibility study presented in table 2 as mean grades were meant to show an example of interobserver agreement. Frequency distribution histograms of the same data are available but were not included, perhaps wrongly, because we had to limit the data which were submitted for publication to keep the paper within a reasonable length. The score allows a good comparison for each individual patient as well as a comparison for the major grades and numbers of patients within each grade. The latter allows comparisons between patient groups. The scoring system is under prospective evaluation in clinical trials and has so far been easy to use with a good assessment of microscopic inflammation. The results will be published in due course.

We realise that the distinction between different groups within one grade is not rigorously correct but we feel that it can be useful, especially as we decided to use the worst aspect for the grading, rather than an average aspect. The correlation between location of neutrophils in the epithelium and occurrence of crypt destruction, erosions, and ulcerations was studied using Spearman’s correlation coefficients.

In general, we agree with Dr Cross that a correct scoring system is needed. On the other hand, such a scoring system should be simple and easy to use. We have tried to find a balance between the different needs and have shown that such a system can be applied with fair interobserver agreement.

K GEBOES
G I Pathology Unit, KU Leuven, Belgium

R RIDDELL
McMaster University, Medical Center, Hamilton, Canada

A ÖST
Malmö AB and Karolinska Institute, Stockholm, Sweden

B JENSFELT
T PERSSON
Aörs Zengil
R LÖFBERG
Department of Gastroenterology, Karolinska Institute, Huddinge University Hospital, Sweden

Correspondence to: K Geboes, Department of Pathology, University Hospital, KU Leuven, Mind- erbroedersstraat 12, 3000 Leuven, Belgium. Karel.Geboes@uz.kuleuven.ac.be


Insulin and gall stones

EDITOR,—In showing for the first time that raised serum insulin is a risk factor for incident gall stones, independent of body mass index, Misciagna et al (Gut 2000;47:144–7) have made an important contribution. However, they do not seem to realise that we had similar findings in the East Bristol Gallstone Study (population based like theirs)—namely, that raised plasma insulin is a risk factor for prevalent gall stones, at least in men.1 In our study, another significant factor was abdominal fatness or central obesity, but not body mass index (as is usually the case in men), and abdominal fatness probably explained the hyperinsulinaemia as the association of insulin with gall stones disappeared when we controlled for waist-hip ratio. Abdominal fatness is a well known determinant of fasting plasma insulin and it is a pity that Misciagna et al did not include any measure of it in their study.

Should Misciagna et al continue this line of enquiry, they will be well advised to measure the insulin response to eating because in our experience, postprandial as well as fasting levels of insulin are raised in men with gall stones.2 I fully agree with Misciagna et al’s conclusion that “hyperinsulinaemia may play an important role in the aetiology of gall stones”.3 I also suggest that future studies of gall stone aetiology should include measures of insulin sensitivity and of its determinants. One such determinant is physical fitness4 and this may be relevant because, in our study, there was a hint that loss of muscle bulk may be associated with gall stones in men. Men with gall stones had not gained weight during adult life more than controls, despite having more abdominal fat, suggesting they had lost more lean body mass.1

K W HEATON
University of Bristol, Division of Medicine, Bristol, UK

P M EMMETT
University of Bristol, Division of Child Health, Bristol, UK

Correspondence to: Dr WK Heaton, Claverham House, Claverham, N Somerset BS49 4QD, UK. kenheaton@compuserve.com


Heparin as an anti-inflammatory agent: it's no GAG to forget about chemokines

EDITOR—We approached with enthusiasm the report by Salas and colleagues (Gut 2000;47:88–96) showing that heparin prevented tumour necrosis factor induced leucocyte rolling, adherence, and migration in vivo, as demonstrated using intravital microscopy. The novelty and potential importance of the report were emphasised by the appearance of an accompanying commentary by Perretti and Page (Gut 2000;47:14–15). Our enthusiasm was tempered, however, by the authors' selective invocation of potential explanations for heparin as an anti-inflammatory agent. While an effect of heparin on the neutrophil integrin adhesion molecule CD11b was described in elegant experiments, heparin almost certainly exerts its anti-inflammatory effects through a range of activities beyond an adhesion molecule target. One of these targets is the superfamily of cytokines known as chemokines, metabolite of which have now been identified.

Chemokines are small, basic, chemotactic cytokines that mediate leucocyte recruitment to sites of inflammation and immune responses. In addition, it is now clear that they are crucial to routine immune surveillance and homeostasis. They have a capacity to bind selectively to a range of glycosaminoglycans, or GAGs, including heparin, in tissues and on the surface of both endothelial cells and leucocytes. This interaction heightens the chemokine mechanism for the anti-leucocyte migration activity of heparin. We ignore chemokines at our peril though, as their sheer number and abundance, and the intensity of the effort being directed at discovering pharmacological inhibitors of their function, highlight their critical role in inflammation.

S J CONNOR
M G GRIMM
Department of Medicine,
St George Clinical School and
Inflammation Research Unit,
School of Pathology,
University of New South Wales,
Sydney, Australia

Correspondence to: M C Grimm.
M.Grimm@unsw.edu.au


Management of varical haemorrhage in cirrhotic patients

EDITOR—We have serious concerns about several of the recent UK guidelines for the management of varical haemorrhage in cirrhotic patients (Gut 2000;46(suppl 3 and 4):iii–iii).115), particularly those that contra-indicate published evidence. We highlight below the ones we feel are the most important.

In the management of acute varical bleeding, varical bleeding is the method of first choice which was given an AI recommendation. Meta-analysis of all trials of acute bleeding of banding versus injection sclerotherapy have shown no statistically significant difference between the two treatments for either control of bleeding or survival (data derived from 12 studies with 419 patients), with no statistical heterogeniety. The implication of recommending ligation for acute bleeding is that double intubation would be necessary in a patient who is actively bleeding so as to attach the ligation device after the initial diagnostic endoscopy. Although in our view, this would create more risk to the patient; it is common sense that a single intubation would be preferable and would take less time. At best the recommendation should be that either endoscopic technique could be used as first choice, dependent on operator expertise and facilities.

Secondly, there is evidence from randomised studies of vasoactive drug therapy combined with endoscopic techniques that combination therapy is superior in terms of control of bleeding. This is based on five randomised studies with 610 patients (pooled odds ratio 0.42, 95% confidence interval 0.29–0.6).1 Publication bias assessment has shown that 29 null or negative studies would be necessary in a patient who is actively bleeding so as to attach the ligation device after the initial diagnostic endoscopy, demonstrating their utility during the period of resuscitation before endoscopy could be safely performed, which in practice may be several hours after admission. This goes against the recommendation that drugs can be used if endoscopy is not available. Drugs should be used first followed by therapeautic endoscopy.

As regards the prevention of rebleeding from sources due to portal hypertension, the treatment of first choice, unless there are contraindications, is either non-selective β blockers as they are superior to sclerotherapy, or banding ligations. Fully published randomised studies are available with regard to β blockers versus banding. If banding is not available, β blockers should be used, not sclerotherapy, as recently published in meta-analyses with contraindications or intolerance to β blockers, banding should be used. One can argue cogently that as non-selective β blockers are cheap and do not involve repeated endoscopy sessions, they always should be considered the treatment of first choice.

The recommendation of measuring hepatic venous pressure gradient (HVPG) in patients given β blockers cannot be one for current practice. Only two Spanish groups have suggested this, and it is unclear when a repeat measurement should be performed. Moreover, both a 20% reduction from baseline HVPG or an absolute reduction of less than 12 mm Hg are “protective” from rebleeding, so both end points, and not just the absolute reduction, need to be mentioned if this management strategy is used.

In any case, the randomised studies of endoscopic therapy used non-selective β blockers empirically to the maximum tolerated by patients so that use of drugs without pressure measurement was effective. Lastly, if the recommendation of using drugs with re-measurement of pressure is taken to its logical conclusion, all patients should be tried on drugs first, as those who respond have less rebleeding (10% or less) than patients who receive banding, and secondly, a recommendation of what to do next would need to be made for those who do not reduce their portal pressure (for which as yet there is no evidence).

Lastly, two meta-analyses comparing TIPS with endoscopic therapy do not make it a first choice treatment for rebleeding, even in centres with expertise such as the authors’ own, as stated in the guidelines. Thus the AI recommendation grading is particularly inappropriate.

With respect to primary prevention of portal hypertension bleeding in cirrhosis, we

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recommendation that nitrates should be used if neither β blockers nor banding are available or contraindicated is potentially dangerous. A long term randomised study has shown that at least in elderly patients, nitrates cannot be recommended as a substitute therapy.

Finally, the guidelines should have included some issues of general management—for example, association with fluids, early assessment of portal vein patency, and presence of hepatocellular carcinoma—and an AI recommendation for the use of prophyactic antibiotics in acute bleeding based on the meta-analysis of the authors quoted. A corrected and improved update of these guidelines is needed soon.

A K BURROUGHS
D W PATCH
Liver Transplantation and Hepatobiliary Unit, Royal Free Hospital, Pond Street, London NW3 2QG, UK
Correspondence to: Dr A K Burroughs andrew.burroughs@talk21.com


Reply

EDITOR—We thank Dr Burroughs and Dr Patch for their interest and helpful comments. Our UK guidelines in the management of variceal bleeding. A number of the points raised by them reflects the fact that it is not always possible to directly translate the evidence gleaned from clinical trials into clinical practice because of the subjectivity in the definition of evidence based medicine. There is a lot of argument in the literature about what constitutes research evidence. Indeed, there is ongoing debate whether the results of a good randomised controlled trial are more reliable than a meta-analysis on the same subject because the latter often suffers from problems introduced by heterogeneity between studies.1

For the preparation of the present “guide lines”, about 300 papers were reviewed and 208 have been referred to in the paper. It is clear that the vast majority of these studies were not adequately powered to detect differences in mortality and a number of points that have been raised by Dr Burroughs and Dr Patch represent alternative interpretation of the available data which are not necessarily in variance with the “guidelines”.

Before discussing the specific points raised by them, it is important to point out that:

● Although the guidelines were written by us, they have undergone several revisions based on peer review organisation by the British Society of Gastroenterology (BSG), Liver Section. This review process we believe was extensive and largely anonymous. The guidelines therefore represent the views of the BSG.

● The guidelines were first commissioned in 1996 but first published following several alterations in mid-1998. Some of the more important data added were into the text (the antibiotic prophylaxis section) during the proof stage.

With respect to the specific comments:

(a) We agree with Dr Burroughs and Dr Patch that there have been any significant differences between band ligation and sclerotherapy in their ability to control bleeding. Also, most patients who have had a varical bleed and are undergoing endoscopy are not bleeding actively. It is therefore relatively easy to band in these situations and a double intubation using the new multi-band ligation devices is not necessarily a problem. Studies have also that complications from endoscopic therapy in the form of oesophageal ulcers, mediastinitis, and pneumonia are significantly less in the group treated with band ligation compared with sclerotherapy. This is associated with reduced mortality in patients treated with band ligation. It stands to reason therefore that band ligation should be used where possible as there is no significant difference between treatments in their ability to control bleeding but the rate of complications has been shown to be significantly less in the band ligation groups.

(b) Interpretation of data regarding the combination of vasoactive drugs with endoscopic therapy in the setting of acute bleeding is fraught with difficulties and there is no clear evidence that its use improves the second line mortality. This is despite a large number of trials in this area. The meta-analysis that Burroughs and Patch published in 1999 refer to as a justification for the combination treatment shows no differences in survival between groups. The role of vasoactive drugs in the management of variceal bleeding is an area of intense research by a number of groups and more data are needed before the combination treatment can be recommended in routine clinical practice.

(c) With respect to secondary prophylaxis of varical haemorrhage, the literature suggests that N if only a β blocker is used we should ensure that this is having some effect on the most important parameter predictive of reblooding, a portal pressure gradient <12 mm Hg (about 30% of patients in different studies show inadequate portal pressure response to β blocker therapy). It has been shown in a prospective study that in patients being treated with β blockers, none with a hepatic venous pressure gradient <12 mm Hg bled and only 8% of those whose hepatic venous pressure gradient fell by more than 20% on therapy bled during follow up.2 However, if the β blocker studies are included in patients being treated with β blockers, this is likely to increase both the cost and invasiveness. We do agree that we should add to the guidelines that a reduction in portal pressure gradient by 20% or more from baseline is acceptable.

(d) The guidelines clearly state what Dr Burroughs and Dr Patch suggest in their letter: “TIPSS is more effective than endoscopic treatment in reducing varical rebleeding but does not improve survival and is associated with more encephalopathy”. Three studies have shown that TIPSS is safe but none have followed up for publication for more than six months after their initial varical bleed.3,4,5 Studies that have compared TIPSS with band ligation have not shown any significant differences in encephalopathy between groups.3,6 This has, however, not been borne out in a meta-analysis.4,6 But it is clear from individual trials and also from the meta-analysis that TIPSS significantly reduces the rate of reblooding.

(e) The recommendation grade for the use of isosorbide-5-mononitrate (ISMN) in case of failure of propranolol or band ligation is grade B1 and is based on the equivalence study of ISMN and propranolol by Angelico and colleagues.3 The paper that Dr Burroughs and Dr Patch refer to as a meta-analysis of data from a study that was first reported in 1993.7 A preliminary report of another study has not confirmed these findings and it is clear that more data are needed before nitrates can be considered as being dangerous in the primary prophylaxis of variceal bleeding.

(f) Our brief was to develop guidelines about the management of variceal bleeding and not about the detailed intensive care management. We have however included some pointers in the guidelines which we though were likely to be useful. We accept that the use of prophylactic antibiotics should be a grade 1A recommendation. This section on the use of antibiotics following a varical bleed was added during the proof stage following the availability of the meta-analysis by Bernard et al in 1999.8 We do agree with Dr Burroughs and Dr Patch that the treatment options in portal hypertension are continuously evolving and with the emergence of new data, “guidelines” should be revised to incorporate the advances that have occurred in that time.

R JALAN
Institute of Hepatology,
University College London Medical School,
London, UK

C P HAYES
Liver Unit, Royal Infirmary of Edinburgh,
Edinburgh, UK
Correspondence to: Dr R Jalani, Institute of Hepatology, University College London Medical School, 69-75 Chenies Mews, London WC1E 6HX, UK.
jalani@ielc.ac.uk

Table 1  Number (%) of patients who cleared HBsAg at different times in the four treatment groups

<table>
<thead>
<tr>
<th>Time</th>
<th>Group 1</th>
<th>Group 2</th>
<th>Group 3</th>
<th>Group 4</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(n=8)</td>
<td>(n=34)</td>
<td>(n=5)</td>
<td>(n=12)</td>
<td>(n=59)</td>
</tr>
<tr>
<td>End of treatment</td>
<td>6 (75%)</td>
<td>23 (67.6%)</td>
<td>3 (60%)</td>
<td>6 (50%)</td>
<td>38 (64.5%)</td>
</tr>
<tr>
<td>12 months after stopping treatment</td>
<td>6 (75%)</td>
<td>23 (67.6%)</td>
<td>3 (60%)</td>
<td>12 (100%)</td>
<td>38 (64.5%)</td>
</tr>
<tr>
<td>End of follow up</td>
<td>8 (100%)</td>
<td>34 (100%)</td>
<td>5 (100%)</td>
<td>12 (100%)</td>
<td>59 (100%)</td>
</tr>
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Long term follow up of interferon responder children with chronic hepatitis B

EDITORS,—We read with interest the commentary by Clària and Rodés (Gut 1999;45:639) on our published paper in Gut which re-examined the mechanisms of renal sodium retention in patients with preascitic cirrhosis. In our commentary, in our paper published in Gut, 45:639, we concluded that indirect evidence of expanded central vascular fluid volume compared with healthy controls and thought this physiopathological alteration was due to slight reduced values of lithium clearance as a marker of distal fluid reabsorption in the distal nephron. We also wish to report on the long term follow up of 59 IFN responder children with chronic HBV who received IFN alpha for three or six months in a randomized, controlled clinical trial. We read with interest the commentary by Clària and Rodés (Gut 1999;45:639) on our published paper in Gut, 45:639.

Renal sodium handling in preascitic cirrhosis

EDITORS,—We read with interest the commentary by Clària and Rodés (Gut 1999;45:639) on our published paper in Gut which re-examined the mechanisms of renal sodium retention in patients with preascitic cirrhosis. In our commentary, in our paper published in Gut, 45:639, we concluded that indirect evidence of expanded central vascular fluid volume compared with healthy controls and thought this physiopathological alteration was due to slight reduced values of lithium clearance as a marker of distal fluid reabsorption in the distal nephron. We also wish to report on the long term follow up of 59 IFN responder children with chronic HBV who received IFN alpha for three or six months in a randomized, controlled clinical trial. We read with interest the commentary by Clària and Rodés (Gut 1999;45:639) on our published paper in Gut, 45:639.

Gut: first published as 10.1136/gut.48.5.738 on 1 May 2001. Downloaded from http://gut.bmj.com/ on September 15, 2023 by guest. Protected by copyright.
With reference to the first methodological remark, to our knowledge the value of fractional sodium excretion (FENa) below which lithium reabsorption beyond the proximal tubule occurs is 0.2% and not 0.4%.\(^1\) Obviously, our non-azoetric preascitic cirrhotics displayed values of FENa well above this threshold (0.76 (0.39)%).\(^1\)

Concerning the second remark, although our patients displayed slightly lower values of creatinine clearance (CCr) with respect to controls, the calculated deliveries of fluid and sodium to the distal nephron were not lower but somewhat higher, even if not significantly, than in healthy subjects (30.7 (9.3) vs 27.5 (6.7) ml/min and 4.25 (1.23) vs 3.9 (1.0) mEq/min, respectively; all p<0.05).\(^1\) In effect, not surprisingly, we observed no correlation between values of CCr and distal delivery of fluid or sodium.\(^1\) Furthermore, because of the inverse correlation in the cirrhotic group between levels of plasma active renin and lithium clearance, we reaffirm a compensatory role for the proximal renal tubule as it seems capable of delivering more fluid and sodium to the loop of Henle during a progressive increase in circulating fluid volume, at least at this stage of disease.\(^1\)

In conclusion, we agree with Clària and Rodes that some uncertainty may be introduced when assessing renal function in cirrhosis by measurement of glomerular filtration rate using creatinine clearance. However, we consider that our results on inappropriate avidity of sodium reabsorption by the distal nephron are relevant in explaining the already demonstrated increase in central fluid volume in patients with preascitic cirrhosis.\(^1\)

J SANSOÈ
Gastroenterology Unit, Gradaengo Hospital, Corso Regina Margherita 16, 10153 Torino, Italy
A FERRARI
Chair of Gastroenterology, Department of Internal Medicine, University of Modena, Modena, Italy
Correspondence to:
Dr G Sansoe, giovannisan@iul.it


Reply

Editor,—In their letter, Sansoe and Ferrari make some excellent points on our accompanying commentary (Gut 1999;45:639) to their paper published in (Gut 1999;45:750–5). In that paper, Sansoe et al investigated the status of central blood volume and examined the distribution of sodium reabsorption along the segments of the renal tubule in a group of 12 preascitic cirrhotic patients. Whereas the results on central fluid volume were quite conclusive, the findings on renal function merit some discussion (Gut 1999;45:639). As precisely pointed out by Sansoe and Ferrari in their letter, the contention was mainly methodological and was related to the use of lithium and creatinine clearances for determination of distal sodium reabsorption and glomerular filtration rate, respectively.

Lithium clearance is a useful marker of proximal tubule sodium handling because in theory this ion is reabsorbed in proportion to sodium and water along the entire proximal tubule.\(^1\) However, the validity of this method is not widely recognised. In this regard, there is compelling evidence that lithium is actively reabsorbed along the distal tubule in conditions characterised by low fractional sodium excretion.\(^1\) In this regard, we support the estimated limit of fractional sodium excretion below which this problem has arisen as elaborated in our letter.\(^1\) Conversely, comprehensive studies of micropuncture have revealed that this value may vary from 0.8% to 0.65% in sodium depleted states.\(^1\) Finally, a value of fractional sodium excretion of 1% has been proposed as a safer limit by Koomans and colleagues.\(^1\) Thus, inasmuch as the value of fractional sodium excretion below which lithium clearance is disqualified as an index of proximal sodium delivery remains unresolved in cirrhosis, data derived from this method in cirrhotic patients should be interpreted with caution.

We should also point out that preascitic cirrhotic patients included in Sansoe et al’s study (Gut 1999;45:750–5) had significantly lower values than controls for glomerular filtration rate, as determined by creatinine clearance. These findings are not consistent with those previously reported in compensated cirrhotics using more sensitive clearance techniques such as inulin clearance.\(^1\)

In summary, it is gratifying to see that Sansoe and Ferrari state that a certain amount of uncertainty may be introduced in studies dealing with renal function by using creatinine and lithium clearances. We believe that their paper will undoubtedly foster new studies investigating the central fluid volume status and renal tubular avidity for sodium in preascitic cirrhotic patients.

J CLÀRIA
RODES
Liver Unit, Institut d’Investigacions Biomèdiques August Pi i Sunyer (IDIBAPS) Hospital Clinic, Barcelona 08036, Spain
Correspondence to: Professor J Rodes, Liver Unit, Hospital Clinic, Villarroel 170, 08036 Barcelona, Spain. rodes@medicina.ub.es

end and I personally do not like the idea of paginating in sections and chapters. With a book of this length it is surely easier to simply number the pages. However, these are minor complaints and on the whole I would recommend this book to anyone interested in liver disease and particularly to trainees in gastroenterology, or hepatobiliary or hepatology surgery who will come back to this book again and again.

D H ADAMS


“A picture is worth a thousand words” is as applicable to teaching gastroenterology as in any other context now that gastroenterology has become a visual science. Any atlas must stand or fall on the quality of the photographs and here the reader will not be disappointed as the vast majority are of excellent clarity and content. The second edition of this Atlas of Gastroenterology provides the most comprehensive visual images in gastroenterology this reviewer has seen, covering the broad spectrum of gastroenterology—histology, endoscopic images, CT scans, radionuclide imaging, and magnetic resonance imaging, including MR cholangiopancreatography. However, there are no “virtual endoscopic” images, which is a surprise and disappointment.

The atlas has a user friendly format setting pictures in their clinical context making perfect sense and easy access. There is a series of chapters entitled “Approaches to common gastrointestinal problems” beginning with a chapter on physiology that precedes the sections on diagnostic and therapeutic techniques, all paginating in sections and chapters. With a large number of images the second edition of this Atlas of Gastroenterology is clearly a specialist book in upper gastrointestinal problems” beginning with a chapter on physiology that precedes the sections on diagnostic and therapeutic techniques, all paginating in sections and chapters. With a large number of images the second edition of this Atlas of Gastroenterology is clearly a specialist book in upper gastrointestinal diseases and a series of chapters illustrating diagnostic and therapeutic techniques, all written and compiled by acknowledged experts in their field. Reference lists are suitably brief and up to date.

The atlas seeks to provide more than a picture book of gastrointestinal surgery but perhaps goes rather too far by providing information that would normally be within a textbook of gastroenterology. For example, there is a chapter entitled “Advice to travellers” that gives information about required vaccinations in various parts of the world and drug treatment for traveller’s diarrhoea. There are also several chapters with extensive clinical information that is more than just an accompaniment to the images. In one chapter, there is a long list of drugs likely to induce liver disease—appropriate for a textbook but not for an atlas, particularly when this atlas is designed for use with its partner the Textbook of Gastroenterology by the same editors.

This atlas provides the most up to date high quality illustrative review of gastrointestinal surgery and could perhaps only be improved by the addition of a slide or CD version. Access to the images via the Internet will probably be the next step but I for one would miss the pleasure of leafing through a book.


This is a small textbook which looks at specific aspects of gastric surgery from a laparoscopic approach. The overall format is attractive in that a chapter on physiology precedes the section on laparoscopic surgery. It does, however, in view of the rather concise nature, fall between two stools in that it is a specialist book and therefore does not necessarily appeal to the general trainee, but it is too short and the referencing is too limited to be a definitive text. The book makes good use of the basis that the laparoscopic approach is correct and there is very little discussion on non-laparoscopic and open surgery. This may well be appropriate in the form of laparoscopic antireflux surgery and cardiomcyotomy but is certainly not in the form of antiobesity surgery or surgery for cancer. The impression that the laparoscopic approach is well established is inaccurate for these latter conditions and malignancy, where open surgery still holds sway. The discussion on laparoscopic antireflux surgery is limited to the 360° Nissen loose floppy wrap. The operation is described nicely with clear photographs which is a characteristic of the entire text. However, there is no discussion on the alternatives to a 360° wrap, namely a toupee 180° procedure or even the more modern partial anterior fundoplications. The various merits of these procedures would be an addition to the text as well as the role of the laparoscope in revisional surgery, and some comparison with open operations. Similarly, for cardiomcyotomy for achalasia, a success rate related to open cardiomDavidson would be beneficial. Preceding these two operative sections however are two good chapters on the pathophysiology of reflux and achalasia. It is a pity in laparoscopic antireflux surgery that more comment is not made on the significant increase in the incidence of such surgery with the advent of the laparoscope. Is this a good thing or not? The pros and cons of treatment could be better discussed. With regard to open surgery, there really has to be emphasis as being historical experience. Comparison with these success rates versus those of open surgery and a reflection on the reality of the situation, as seen in Western Europe where the disease presents at a more advanced stage, and the role of other modalities such as chemo/radiotherapy, would benefit the textbook and would expand it into a more comprehensive text. On the plus side however, the illustrations are superb and the intraoperative photographs explain the laparoscopic nodal dissection extremely clearly. It is not however a textbook of operative surgery. This book will appeal to the more specialist clinicians in upper gastrointestinal surgery and provides a cheaper and smaller alternative to the more weighty texts.

R C MASON

CORRECTION

An error occurred in the abstracts supplement Gastroenterology 2000;128(suppl I):A68. For abstract 254, PC Hayes was the senior author.

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