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 evaluated.

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 processes 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 the location of neutrophils in the epithelium and occurrence of 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 treatment regimens. 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.

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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. These cells can synthesize cytokines or immunoglobulins, or express markers such as LFA-1 and leucin-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 infiltrate, infiltration of neutrophils in the epithelium, crypt destruction, and erosion and 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 as before 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 in diagnostic 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 felt 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 JENSPELT T PERSSON Astra Zeneca AB R LÖFBERG Department of Gastroenterology, Karolinska Institute, Huddinge University Hospital, Sweden

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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, Misiagna 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 Misiagna et al did not include any measure of it in their study.

Should Misiagna 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 Misiagna et al’s conclusion that “hyperinsulinaemia may play an important role in the aetiology of gall stones”. 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 fitness3 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.4

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they are crucial to routine immune surveil-

1. 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 highlights their critical role in inflammation.

2. Chemokines that mediate leucocyte recruitment and Page chose to mention an anti-

3. Neither Saadas and colleagues nor Perretti and Page chose to mention an anti-

4. The implication of recommending 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 rebreeding, 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 study showed that therapy used non-selective β blockers empirically to the maximum tolerated by patients so goes against the recommendation that drugs be used first followed by therapeutic endoscopy.

5. It is common practice to ensure that patients not reduce their portal pressure are considered for TIPS, as recommended. If there are contraindications or intolerance to drugs, 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.

6. 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 rebreeding, 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 study showed that therapy used non-selective β blockers empirically to the maximum tolerated by patients so goes against the recommendation that drugs be used first followed by therapeutic endoscopy.

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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 on their own decrease survival. Thus to err on the side of caution, 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 prophylactic antibiotics in acute bleeding based on the meta-analysis by the authors quoted. A corrected and improved update of these guidelines is needed soon.

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Reply

Editor,—We thank Dr Burroughs and Dr Patch for their interest and helpful comments on the 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. For the preparation of the present “guidelines”, 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 line 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 organised by the British Society of Gastroenterology (BSG), Liver Section. This review process we believe was extensive and largely anonymous. The guidelines therefore reinforce the views of the BSG.

The guidelines were first commissioned in 1996 but first appeared for publication following several alterations in mid-1998. Some of the more important data were added 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 some significant differences between band ligation and sclerotherapy in their ability to control bleeding. Also, most patients who have had a variceal 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 shown 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 because 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 group.

(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 from trials that combination reduces 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 are needed before the combination treatment can be recommended in routine clinical practice.

(c) With respect to secondary prophylaxis of variceal haemorrhage, the literature suggests that various modalities such as sclerotherapy, β blockers, or a combination of these are similar in the long term (reviewed by D’Amico and colleagues). Most patients that we treat in the UK with variceal bleeding have underlying alcoholic liver disease and who have a questionable compliance. The recommendation is that if only a β blocker is used we should ensure that this is having some effect on the most important parameter predictive of rebleeding, 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. However, if these studies and also recent studies 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: “TIPS is more effective than endoscopic treatment in reducing variceal rebleeding but does not improve survival and is associated with more encephalopathy”. Three studies have shown that TIPS is used for publication six months after their initial variceal bleed.

Studies that have compared TIPS with band ligation have not shown any significant differences in encephalopathy between groups. This has, however, not been borne out in a meta-analysis. But it is clear from individual trials and also from the meta-analysis that TIPS significantly reduces the rate of rebleeding.

(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. The paper that Dr Burroughs and Dr Patch refer to as an analysis of data from a study that was first reported in 1993. A preliminary report of another study has not confirmed these findings and it is clear that more data are necessary before nitrate can be suggested 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 thought 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 variceal bleed was added during the proof stage following the availability of the meta-analysis by Bernard et al in 1999.

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.

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Long term follow up of interferon responder children with chronic hepatitis B

EDITOR—We read Bortolotti et al's article (Gut 2000;46:715–18) reporting the long term effect of interferon (IFN) alpha in children with chronic hepatitis B (CHB). Briefly, a total of 107 children with chronic HBV who received IFN alpha for three or six months in two clinical trials were followed for a mean period of 69 months. In the first trial 19 and 50 children received IFN alpha at a dose of 5 MU/m² three times a week for six months and for 12 months respectively. In the second trial 34 cases received IFN alpha at a dose of 3 MU/m² three times a week for six months, and 20 cases received IFN alpha at a dose of 10 MU/m² three times a week for six months. The cumulative IFN clearance rates at five years were similar between the patients (60%) and controls (65%). Loss of hepatitis B surface antigen (HBsAg) occurred in only four patients who responded during treatment.

We also wish to report on the long term follow up of 59 IFN responder children with chronic HBV (table 1). At the beginning of IFN therapy, all children (44 males: 76.4%) had abnormal or fluctuating transaminases for at least six months and were positive for HBV DNA. Mean age of the patients at diagnosis of chronic HBV was 6.8 (3.4) years (range 1–15). They were followed for a mean period of 19.9 (21.5) months (range 6–100) before treatment. Liver biopsy was performed on 29 patients; 15 (51.7%) had mild, 12 (41.4%) moderate, and two (6.9%) severe hepatitis. Forty three (84.3%) of 51 patients had at least one family member with positive HBV serology. Eight patients (group 1) had received IFN alpha at a dose of 10 MU/m² three times a week for six months, 34 (group 2) at a dose of 5 MU/m² three times a week for six months, five patients (group 3). Twelve patients (group 4) who were unresponsive to IFN therapy at a dose of 5 MU/m² received IFN alpha again at a dose of 10 MU/m² three times a week for six months. Response to treatment was defined as loss of HBsAg during the treatment period or within 12 months after stopping treatment. All patients were followed for a mean period of 35.3 (10.8) months (range 18–62) after treatment. Liver biopsy was performed on 29 patients; 19 patients cleared HBsAg at the end of IFN therapy and 17 (28.8%) within 12 months after stopping treatment. Four (6.7%) patients were late responders (HBsAg clearance was observed between 21 and 30 months after stopping therapy). The mean period of HBsAg clearance was 8.2 (7.0) months. Within the follow up period, antibody to hepatitis B surface antigen (anti-HBs) occurred in seven (18.4%) patients who responded during therapy and all but one lost HBsAg. After HBsAg clearance, anti-HBe seroconversion and loss of HBV DNA was observed in all patients. Alanine aminotransferase values normalised in 98.3% of patients. None had biochemical or serological relapse within the follow up period.

The majority of our patients cleared HBV DNA and E2Ag during therapy whereas only 15% of patients in the study of Bortolotti et al responded during therapy. They followed patients for an average of 69 months and observed that 113 responders remained HBsAg and HBV DNA negative. Although our observation period was shorter than theirs, we also observed that all responders had sustained results at the end of follow up. Similar to Bortolotti et al's results, all HBsAg cleared patients were early responders to IFN therapy in our group. In conclusion, response to IFN alpha in children with chronic hepatitis B is predictable. It is necessary to follow these patients for longer periods to see the long term effects of IFN alpha therapy, such as prevention of cirrhosis and/or hepatocellular carcinoma.

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Renal sodium handling in preascitic cirrhosis

EDITOR,—We read with interest the commentary by Claria and Rodés (Gut 1999;45:639) on our paper published in Gut which re-examined the mechanisms of renal sodium retention in patients with preasctic cirrhosis.1 In our previous study, 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 delivery of fluid and sodium to the distal segments (measured as creatinine clearance) and, mainly, to increased distal tubular retention of sodium when expressed as a fraction of the filtered sodium load that is reabsorbed by the distal natriuretic (26.9 (6.7%) compared with 12.5 (3.4%) respectively, p<0.05).2

Claria and Rodés advanced two criticisms and confirmed that our results, obtained by means of the lithium clearance and fractional excretion technique, may be influenced by two fundamental flaws. Firstly, the reliability of lithium clearance as a marker of distal fluid delivery in clinical conditions characterised by low fractional sodium excretion (50% of the 0.40%) has not been proved due to possible lithium reabsorption in the distal natriuretic.3 Secondly, in Claria and Rodés’s opinion, our observation of more avid fractional sodium reabsorption by the distal natriuretic in compromised cirrhosis merely reflects diminished delivery of fluid and sodium to the distal segments (due to reduced glomerular filtration) rather than increased distal tubular sodium reabsorption.

Long term follow up of interferon responder children with chronic hepatitis B

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 (n=8)</th>
<th>Group 2 (n=34)</th>
<th>Group 3 (n=5)</th>
<th>Group 4 (n=12)</th>
<th>Total (n=59)</th>
</tr>
</thead>
<tbody>
<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>32 (94.1%)</td>
<td>5 (100%)</td>
<td>12 (100%)</td>
<td>17 (28.8%)</td>
</tr>
<tr>
<td>End of follow up</td>
<td>8 (100%)</td>
<td>34 (100%)</td>
<td>10 (100%)</td>
<td>12 (100%)</td>
<td>59 (100%)</td>
</tr>
</tbody>
</table>

Interferon alpha dosage and duration: group 1, 10 MU/m² three times a week for six months; group 2, 5 MU/m² three times a week for six months; group 3, 5 MU/m² three times a week for 12 months; group 4 (non-responders to previous interferon alpha treatment), 10 MU/m² three times a week for six months.
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 0.02% and not 0.4%. Obviously, our non-ozotemic prasacitic 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 (30.9 (9.3) vs 27.5 (6.7) ml/min and 4.25 (1.30) vs 3.9 (1.0) mEq/min, respectively; all p>0.05). In effect, not surprisingly, we observed no correlation between values of CCr and distal delivery of fluid or sodium. Furthermore, because of the inverse correlation in the cirrhotic group between levels of plasma active renin and sodium 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 Rodés 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 prasacitic cirrhosis.1

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Reply

Editor—In their letter, Sanseo 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, Sanseo 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 prasacitic 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 Sanseo 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. 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 It has been shown that the estimated limit of fractional sodium excretion below which this problem has arisen has been established as 0.02%. 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 prasacitic cirrhotic patients included in Sanseo et al’s study (Gut 1999;45:750–5) had significantly lower values than controls for glomerular filtration rate, as determined by creatinine clearance. Therefore, the 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 Sanseo and Ferrari have cast doubt on 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 sodium for avidity in prasacitic cirrhotic patients.

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index 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 hepatic 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 the teaching of 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 brief review of the clinical problem followed by a range of images used in establishing diagnosis, thus putting the image in context with the clinical findings at the appropriate point in the management pathway. There are also chapters on particular 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 gastroenterology 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 gastroenterology 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 is based on 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 cardiomotony 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 cardiomotony for achalasia, a success rate related to open cardiomotony 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 achalasia. It is a pity in laparoscopic antireflux surgery that more comment is not made on the significant increase in the incidence of achalasia. It is a pity in laparoscopic antireflux surgery that more comment is not made on the significant increase in the incidence of achalasia. It is a pity in laparoscopic antireflux surgery that more comment is not made on the significant increase in the incidence of achalasia.

The book is written and compiled by acknowledged experts in their field. Reference lists are suitably brief and up to date.

R C MASON

CORRECTION

An error occurred in the abstracts supplement Gut 2000;48(suppl I):A68. For abstract 254, PC Hayes was the senior author.

NOTES

Falk Workshop

The workshop entitled Update in Inflammatory Bowel Disease will be held in Liubiana, Slovenia, on 5 May 2001. Further information: Prof Dr S Markovic, University Medical Center Liubljana, Division of Internal Medicine, Jajfeva 2, 1525 Liubljana, Slovenia. Tel: +386 (1) 231 6925; fax: +386 (1) 433 4190; email: sasa.markovic@kicl.si

11th International Workshop of Digestive Endoscopy, Ultrasonography, and Radiology

This workshop will be held on 17–18 May 2001 in Marseille, France. Further information: Nathalie Fontant, Atelier Phenix, 41 rue Docteur Morucci, 13006 Marseille, France. Tel: +33 (0) 9 37 50 85 53 (P) +33 (0) 4 91 57 15 28; email: nfontant@aphenix.com

EPGS Endosonography Live in Amsterdam

This European Postgraduate Gastro-Surgical School congress will take place on 31 May and 1 June 2001 in Amsterdam, the Netherlands. Further information: Mrs Helma Stockmann/Mrs Joët Goedkoop, European Postgraduate Gastro-Surgical School, Meibergdreef 9, 1105 AZ Amsterdam, The Netherlands. Tel: +31 20 566 3926; fax: +31 20 566 6569; email: Wj.Stockmann@amc.uva.nl, website: www.epgs.nl.

33rd European Pancreatic Club

The meeting will take place on 13–16 June 2001 in Toulouse, France. A training course will be organised on 13 June on “Genomics and post genomics: developments in biochemical sciences”. Further information: Dr Ninon Vaissé, Inserm U531, CHU Rangueil, 31403 Toulouse, France. Tel: +33 (0) 5 61 32 24 02; fax: +33 (0) 5 61 32 24 03; email: nicole.vaiss@ranguel.inserm.fr; website: www.e-p-c.org.

Gastroenterology and Endotherapy: XIXth European Workshop

This course, to introduce the experienced gastroenterologist to the growing field of therapeutic endoscopy, will be held on 18–20 June 2001 in Brussels, Belgium. Further information: Mrs Nancy Beauprez, Gastroenterology Department, Erasme Hospital, Route de Lennik 808, B-1070 Brussels. Tel: +32 02 555 49 00; fax: +32 02 555 49 01; email: beauprez@ulb.ac.be

Falk Symposium

The symposium Inflammatory Bowel Disease: A Clinical Case Approach to Pathophysiology, Diagnosis, and Treatment will be held in Bologna, Italy on 22–23 June 2001. Further information: Prof Dr M Campieri, Policlinico S. Orsola - Malpighi, Dipartimento di Medicina Interna e Gastroenterologia, Via Massarenti 9, I-40138 Bologna, Italy. Tel: +39 (051) 6364 116 or 6364 122; fax: +39 (051) 392938; email: campieri@med.unibo.it or paolo@med.unibo.it

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