LETTERS TO THE EDITOR

Differential expression of cyclooxygenase 2 in human colorectal cancer

EDITOR,—We were puzzled by the recent paper by Dimberg and colleagues (Gut 1999;48:730–732) which reported that upregulation of cyclooxygenase 2 (COX-2) protein expression was prominent in rectal adenocarcinomas compared with that in adenocarcinomas arising from the colon. “Low or undetectable levels of COX-2 protein expression” were demonstrated in 15 of 19 colonic adenocarcinomas located proximal to the rectum. Overall, upregulation of COX-2 protein expression was reported in only 56% of colorectal cancers.

Previous reports,1 which include one by the current authors on a not dissimilar case series,1 and two in the joint authorship of the accompanying commentary writer,2 have shown consistent upregulation of COX-2 expression in colonic and rectal adenocarcinomas (in 85–90% of cases) compared with matched normal colon tissues using different techniques, including northern blot analysis, RT-PCR, western blot analysis, and immunohistochemistry. Furthermore, four of these studies refer to the distribution of adenocarcinomas throughout the colon without showing evidence of differential COX-2 expression between rectal and more proximal tumours.3–5 In the one previous study which analysed COX-2 protein expression in human colorectal cancers by western blot analysis,6 immunoreactive COX-2 was detected in 76% of cases with a 10-fold increase in median tissue COX-2 concentration compared with normal colonic mucosa.

In our view, the authors should attempt to explain the discrepancy between their results and previously published data. It is interesting to note that, in the study of Kargman et al., five of six patients taking NSAIDs had low or undetectable COX-2 protein expression.7 Moreover, COX-2 expression has recently been shown to suppress induction of COX-2 mRNA and protein in interleukin-1β and phorbol ester stimulated human endothelial cells and fibroblasts.8 Do the authors have data on NSAID use in their cohort of patients prior to surgery?

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Reply

EDITOR,—We agree with Drs Hull and Langman that we found upregulation of COX-2 protein expression in a lower fraction of colorectal cancers (CRC) than previously reported. In part, this may simply be explained by the composition of different tumour types within CRC—that is, the number of colonic versus rectal tumours in our cohort compared with others. In the papers referred to it is difficult to assess the fraction of the different tumour types studied. The differences may also be dependent on the genetic basis for the CRCs studied, which we also have indicated but perhaps not emphasised sufficiently. CRCs with a defective mismatch repair capability, recognised by microsatellite instability (MSI), are accompanied by reduced COX-2 levels.1 At present, we do not know the fraction of MSI type tumours in our series and therefore cannot assess this possibility. An indirect estimate may be achieved since the Min mouse model and human studies provide direct evidence that COX-2 expression may be related to loss of APC function.2 APC and β-catenin mutational analysis of our tumour series shows a good, although not perfect, correlation with COX-2 protein upregulation. Among 18/20 rectal tumours with COX-2 protein upregulation, 12 contained mutations in the APC/β-catenin genes. In contrast, only one of three APC/β-catenin mutated colon tumours revealed COX-2 protein induction and among the remaining 15 non-mutated tumours, two displayed COX-2 protein upregulation. Thus the fraction of APC/β-catenin mutated tumours was also slightly lower (21/38—55%) than previously reported.3 In accordance with the differential COX-2 induction observed, this may indicate that a larger fraction of CRCs in our cohort are of the MSI type.

Other possibilities for the differences in the fraction of COX-2 upregulation in our tumour series may be the definition of “induction”. In our case, a tumour/normal ratio from densitometric scanning of western analysis, RT-PCR, western blot analysis, and immunohistochemistry. Furthermore, four of these studies refer to the distribution of adenocarcinomas throughout the colon without showing evidence of differential COX-2 expression between rectal and more proximal tumours.3–5

References


Proton pump inhibitors for Barrett’s oesophagus

EDITOR,—Recently, the authors of two leading articles, Triadafilopoulos (Gut 2000;46:144–46) and Shepherd (Gut 2000;46:147–49) referred to our paper in Gut.1 We would like to draw attention to the fact that the legend in tables 4 and 5 in our paper should be read as (cm/month), (squares.month), and (%.month) since the variable is the area under the curve (AUC), which is the product of length and surface area.
MCP-3 in inflammatory bowel disease

Editor,—We read with interest the article by Wedemeyer and colleagues (Gut 1999; 44:629–35) on chemokines in inflammatory bowel disease.

Monocyte chemotactic protein 3 (MCP-3) expression in inflammatory bowel diseases is a very interesting observation and we agree with the authors that MCP-3 might play an important role in the pathophysiology of these diseases.

We have recently published an article on the C-X-C chemokines interleukin (IL)-8 and IP-10, and the C-C chemokines MCP-1 and MCP-3 in the mucosa of active ulcerative colitis. It concerns an immunohistochemical study in which we showed increased expression of these chemokines in the lamina propria of patients with ulcerative colitis compared with normal controls. Furthermore, we observed a significant difference in expression between inactive and moderate/severe ulcerative colitis compared with the histochemical grading in MCP-1, MCP-3, and IL-8.

Wedemeyer and colleagues state in their discussion that MCP-1 is expressed in the epithelial cells and lamina propria whereas MCP-3 is almost exclusively produced by epithelial cells. However, in the results section and further in the discussion the authors mentioned sporadic MCP-3 expression in the lamina propria of inflamed tissue. The photographs show only epithelial cells and it is not possible to see the staining pattern of the lamina propria.

We found MCP-3 expressing cells in the lamina propria which was significantly increased in active ulcerative colitis compared with both inactive ulcerative colitis and normal controls. Furthermore, MCP-3 expression in lamina propria was also enhanced in patients suffering from pouchitis compared with patients with a normal pouch (unpublished data).

In the study of Wedemeyer et al., unfortunately the data on MCP-3 expression in Crohn’s disease were not significant which might be because of the small number of patients examined. It would be interesting to further evaluate the role of chemokines in Crohn’s disease.

In conclusion, albeit with some minor differences, both studies have shown that MCP-3 plays an important role in ulcerative colitis.

One minute unbuffered urease test: should it be read at 10 minutes?

Editor,—The one minute unbuffered rapid urease test, previously described in your journal, 1 was adopted for use at the Royal Melbourne Hospital endoscopy day ward because of its affordability, ease of use, and rapidity. Over time, we had noticed a number of cases where the test had been negative at the one minute mark but later became positive. As we were unsure of whether these “late” positive results represented true or false positives, we decided to run a study to assess the accuracy of the urease test compared with the “gold standard” of histology.

To this end we read and recorded the urease test at one and 10 minutes and compared the results with histological demonstration of Helicobacter pylori on a single antral biopsy. This was carried out on 90 unselected patients undergoing upper gastrointestinal endoscopy for varied indications. Forty one patients were found to have H pylori on histology. The urease test was positive in 20 of these 41 when read at one minute compared with 34 at 10 minutes. There were two false positive results at the one minute mark and four at the 10 minute mark. The performance of the urease test at one and 10 minutes is compared in table 1.

We have demonstrated a significant disparity from published data2 in the sensitivity of the ultra rapid urease test in our hands. Previous reports have shown a difference between the test results at one minute compared with 15 minutes but this was attributed to the lower initial temperature of the test solution, as it was kept refrigerated until just prior to use.3 In our ward the test solution is made up in batches and stored at 4°C in the refrigerator but the test tubes are put out at the beginning of the day and thus start off at room temperature. There is evidence to suggest that storage at 4°C for a number of days has no deleterious effects on the performance of the rapid urease test4 but this factor may explain the poor performance of the one minute test in our hands.

These factors aside, it is important to point out that we have concluded that the rapid urease test is quite accurate, with sensitivity and specificity comparable with published values5 for other urease tests, if the reading time is modified to 10 minutes. There are other instances6 of variability of urease test performance depending on the time interval at which it is read. It may be that, prior to use, these tests need to be validated as conditions may vary from the prescribed ones under which the test was designed.

At 10 minutes the unbuffered urease test still provides results quicker than most rapid urease tests and in fact allows us to inform patients and organise further management for them prior to discharge from the endoscopy suite. Given the overall performance of the test, we are quite happy to plan the treatment of H pylori on the basis of its results. Histology can be reserved for those cases where urease testing is equivocal or other signs such as mucosal abnormalities, are being sought.

Table 1 Comparison of the unbuffered rapid urease test performance at one and 10 minutes

<table>
<thead>
<tr>
<th>Time (min)</th>
<th>Sensitivity</th>
<th>Specificity</th>
<th>Positive predictive value</th>
<th>Negative predictive value</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>49%</td>
<td>83%</td>
<td>&lt;0.001</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>10</td>
<td>96%</td>
<td>92%</td>
<td>0.20</td>
<td>0.43</td>
</tr>
<tr>
<td>P value</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>


2. Ying S, Meng Q, Zeibecoglou K, et al. Eotaxin-2, RANTES, monocyte chemoattractant protein-3 (MCP-3), and MCP-4, and C-C chemokine receptor 3 expression in bronchial biopsies located in these two cell types and in epithelial cells.

3. Wedemeyer A, Lorenz T, Manns M, Bischoff S. Department of Gastroenterology and Hepatology, Medical School of Hannover, Hannover, Germany. Email: bischoff.stephan@mh-hannover.de

4. Sengupta S, Crosthwaite G. University Department of Surgery, Royal Melbourne Hospital, Parkville 3050, Australia.
EDITOR,—I read with interest the case report of thalidomide treatment of oesophageal ulceration by Boissonay et al. (Am J Gastroenterol 1997;92:2230–1). We find the statement “yielding conflicting results” on page 654. In our opinion the only flow-volume measurements can be used as a reliable indicator. The fact that Maconi et al did not find a correlation between SMA volume flow and disease activity is probably caused by their choice of reference standard, as pointed out by Kjeldsen and colleagues, Hodgson and Bhatti, and van Oostayen and colleagues.

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Percutaneous drainage of echinococcal cysts (PAIR—puncture, aspiration, injection, reaspiration): results of a worldwide survey for assessment of its safety and efficacy

EDITOR,—In 1996 a letter (Gut 1996;38:936) about the use of PAIR (puncture, aspiration, injection of a scolicidal agent, reaspiration) raised a criticism of Dr Morris, a leading expert on the treatment of echinococcosis. At the same time the WHO Informal Working Group on Echinococcosis launched a survey to evaluate the status of this procedure. A number of centres around the world known to be active in this field were requested to complete forms for patients treated with PAIR: 765 abdominal cysts, mostly hepatic, treated with this technique were reported from various countries. We report the results of this survey (table 1).

<table>
<thead>
<tr>
<th>Table 1 Results of the survey on PAIR by the WHO Informal Working Group on Echinococcosis</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total cases (cysts)</td>
</tr>
<tr>
<td>Follow up &gt;5 y</td>
</tr>
<tr>
<td>Major complications</td>
</tr>
<tr>
<td>Spillage</td>
</tr>
<tr>
<td>Minor complications</td>
</tr>
<tr>
<td>Fever (33), rash (14), pain (30), infection of cavity (11), nausea and vomiting (10), intracystic haemorrhage (3), hypotension (2)</td>
</tr>
<tr>
<td>Failures</td>
</tr>
<tr>
<td>Recurrences</td>
</tr>
</tbody>
</table>

The use of PAIR is widespread and increasing, especially in countries where echinococcosis is endemic. This is also because of its low cost and high efficacy. These data are in accordance with the literature: as of today more than 2400 cysts have been punctured and reported in indexed journals, and success and complication rates are even lower than those of our survey. PAIR is a safe and effective therapeutic tool; the risk of anaphylaxis during PAIR has been greatly overrated. Complication rates, recurrences, and mortality rates are lower than those of surgery. Accuracy of follow up may be a problem where the population is nomadic, but so far no case of peritoneal dissemination after PAIR has been reported.
There is a need for further studies on PAIR. One of the main issues is to standardise at least some of the points of the various PAIR protocols, under the supervision of the WHO, to compare their efficacy, set up prospective studies, and distribute guidelines to optimise the use of the treatment. Whereas before we felt that the technique was limited to a narrow group of patients, today we believe that PAIR is not only an alternative but an effective first choice diagnostic and therapeutic tool in the management of human cystic echinococcosis.

C FILICE E BRUNETTI R BRUNO F G GRIGGA

WHO-INFORMAL WORKING GROUP ON ECHINOCOCCOSIS-PAIR NETWORK

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I remain unconvinced of two basic points. Firstly, that PAIR is effective if used in a narrow group of patients, and secondly, that it is the best method, or the only method, for dealing with multilocular, recurrent liver cysts. What is the recurrence rate of PAIR is now less than surgical therapy? I doubt it! Only one PAIR study reported reappraisal at three days post-PAIR and 2/14 patients had live protoscoleces.

The use of albendazole for four hours to seven days prior to and for 1–4 weeks after PAIR is clearly an attempt to reduce the risk of recurrence. In my original laboratory work it took of the order of 30 days to be effective and, in humans, two patients who received albendazole for one and three weeks, respectively, prior to operation had viable protoscoleces. The use of post-spillage therapy to reduce the risk of implantation has been variably effective in animal models of spillage.

We have made at least some attempt to define the minimum length of such therapy. The over representation of a poor presentation of data, which I suspect is of even poorer quality, does not improve my view of PAIR, or of the WHO working group. I am quite prepared to accept that PAIR may be the best available option in some areas of the world where surgery and perioperative care are compromised by economic factors or lack of experience, but its comparison with surgery should await careful long term follow up.

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Reply

EDITOR,—I remain unconvinced of two basic things: is it (PAIR) safe and is it effective? With regard to safety these are three issues. (a) Anaphylaxis. This occurred in four patients after PAIR in the current series and one after surgery. (b) Dissemination. The peritoneal dissemination of hydatid disease due to needleling a liver cyst will take some time to present—how long? In a series of patients with peritoneal hydatid disease, presentation did not occur until nine years (5–14 years) after surgery. Current follow up of PAIR does not address this issue. Spillage of hydatid material only causes recurrence in approximately 30% of patients. Peritoneal hydatid can be a serious or fatal problem.

(c) Sclerosing cholangitis. A significant proportion of hydatid cysts communicate with the biliary tree; use of sclodial agents even at open surgery has caused sclerosing cholangitis. The surgeon has the opportunity of identifying and protecting such a communication prior to the use of sclodics.

Is it effective? The comment that the recurrence rate of PAIR is now less than surgery is either simplistic or deliberately misleading. Filice et al state that 75 patients (or is it cysts?) have been followed up for five years—the type and frequency of follow up is not stated and this is really critical. Careful ultrasonic follow up can demonstrate recurrence following surgery in up to 22% of patients but one can equally well quote surgical series with poor follow up with low recurrence rates; to claim that recurrence rates are lower following PAIR when the type and completeness of follow up is not even stated in scientifically quite invalid.

That cysts shrink (variably) following PAIR is reported, but what does this mean—is this synonymous with parasite death? I doubt it! Only one PAIR study reported reappraisal at three days post-PAIR and 2/14 patients had live protoscoleces.

The use of albendazole for four hours to seven days prior to and for 1–4 weeks after PAIR is clearly an attempt to reduce the risk of recurrence. In my original laboratory work it took of the order of 30 days to be effective and, in humans, two patients who received albendazole for one and three weeks, respectively, prior to operation had viable protoscoleces. The use of post-spillage therapy to reduce the risk of implantation has been variably effective in animal models of spillage.

We have made at least some attempt to define the minimum length of such therapy. The over representation of a poor presentation of data, which I suspect is of even poorer quality, does not improve my view of PAIR, or of the WHO working group. I am quite prepared to accept that PAIR may be the best available option in some areas of the world where surgery and perioperative care are compromised by economic factors or lack of experience, but its comparison with surgery should await careful long term follow up.
and radiologists. The list of authors includes leading figures in the field of digestive endosonography, namely those who took part in the development of the first pieces of equipment and who described the basic principles of endoscopic ultrasound, and the new generation of practitioners responsible for the most recent developments in this area, particularly the introduction of the endoscopic ultrasound guided puncture. This collective work is complete and exhaustive, and is divided into seven sections, supplemented by a very detailed and helpful index.

The book is a popular work and the teaching material it contains is very practical, detailed, and useful for beginners. However, the book relies on the experiences of the expert authors, which I find to be of much less interest. Much of their experience is now outdated and there is little scope for discussion of other practices. It contains few illustrations of variable quality. I find it strange that Doppler and endoscopic images are grouped at the beginning of the book and reproduced in black and white in appropriate chapters.

In summary, this is a book of high quality work with some good illustrations. The division between the technical sections and those on anatomy is well balanced, which is original to this type of work and is very informative. A number of chapters are extremely useful, particularly those on the linear array echoendoscope and portal hypertension. Some areas covered have less impact, particularly those concerned with the authors’ different experiences of gastrointestinal and retroperitoneal pathology. Others, such as retroperitoneal endosonography, are poorly covered; biliary echoendoscopy is not discussed at all. This significant gap is an invitation to other authors to publish a work dedicated to bilipancreatic echoendoscopy, a useful supplement to the work of doctors van Dam and Sivak.

L. PALAZZO


It seems almost unimaginable to me that, somewhere out there, exists a clinical gastroenterologist who would not want to own this book. Maybe I was destined to be the curator of the book review section of Gut just so that a review copy of this majestic atlas might come across my desk. What little effort it is to find words of praise for this tour de force of gastrointestinal radiology.

In one of the most delightfully understated introductions of the century, Reddy MacSween writes that “…this volume brings credit to radiology as a discipline”. Oh yes indeed, and so very much more! Dr Vallance and selected colleagues have produced a book in which every single illustration (and there are many hundreds) is crystal clear. There are many radiological texts that are comprehensive, and there is a lesser number in which the pictures are clear. There are few books indeed in which every picture credibly reveals the pathology in a totally convincing manner. I do not believe there is single illustration in this book that is not of a high order, and this applies equally to plain radiographs, barium studies, ultrasound, CT, MRI, angiography, or EUS.

Despite its visual excellence, there are idiosyncrasies. Quite what CT and MRI scans of parotid tumours are doing in a book of GI radiology quite escapes this reviewer. Less satisfactory still are some of the mini essays introducing each system. I suspect most readers will not be particularly enlightened by the two page essays that introduce each organ—too brief to say any more than most clinicians must surely know already. For example, who would learn much from: “Ileostomy enema. The distal small bowel may be examined satisfactorily in patients with an ileostomy by retrograde infusion of barium with or without air, introduced by Foley catheter.”

The essays are weak, but the legends and the figures are of exceptional quality. A well constructed legend obviates the need for arrows, or other marks, on the radiograph. In this atlas, arrows do appear from time to time, but they are not intrusive. I suggest that this atlas might very well be added to the extremely short list of books that every gastroenterologist should own.

IAN FORGACS

NOTES

Sir Frances Avery Jones British Society of Gastroenterology Research Award 2001

Applications are invited by the Education Committee of the British Society of Gastroenterology who will recommend to Council the recipient of the 2001 Award. Applications (TEN COPIES) should include:

• A manuscript (2 A4 pages ONLY) describing the work conducted
• A bibliography of relevant personal publications
• An outline of the proposed content of the lecture, including title
• A written statement confirming that all or a substantial part of the work has been personally conducted in the UK or Eire.

An applicant need not be a member of the Society. The recipient will be required to deliver a 20 minute lecture at the Annual meeting of the Society in Glasgow in March 2001. Applications (TEN COPIES) should be made to the Endoscopy Section Secretary, British Society of Gastroenterology, 3 St Andrews Place, London NW1 4LB by 1 December 2000.

CORRECTIONS

An error occurred in figure 1 in the paper by Fisher et al (Gut 2000;46:534–539). Levels of protein C, protein S, antithrombin and factor VII were confused too high throughout the manuscript. In the Methods section, normal ranges for protein C, protein S, antithrombin and factor VII should have read 66–122 U/dl, 68–146 U/dl, 75–140 U/dl, and 50–150 U/dl. Similar corrections should apply throughout the Results section and in the legend to figure 1. This was an editorial error for which Gut apologises.

An error occurred in figure 1 of the paper by Jeppesen and Mortensen (Gut 2000;46:701–706). The correct figure is published below. The correct figure appears on the Gut website (www.gut.jnl.com) and thus diverges from the print version of the May issue. We apologise for any confusion this error may have caused.

Figure 1 48 hour balance studies defining intestinal failure. Absorption of net weight and energy in relation to the basal metabolic rate (BMR) calculated by the Harris-Benedict equations in 44 patients managing without parenteral support (non-HPN patients, open circles) and in 45 patients depending on home parenteral nutrition (black triangles). The 5% confidence limits of the non-HPN patients, defining intestinal failure, are given by the lines. Energy absorption/BMR was 84% and net weight absorption 1.41 kg/day.
Proton pump inhibitors for Barrett's oesophagus

F T M PETERS

Gut 2000 47: 154-155
doi: 10.1136/gut.47.1.154a

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