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POSTSCRIPT

LETTERS

Recognised.

The key issue in determining the value of these different approaches to monitoring in IBD patients receiving this drug. The importance of identifying the risk of significant bone marrow toxicity in long term users of azathioprine. This conclusion is in agreement with other published work and emphasises the importance of ongoing haematological monitoring in IBD patients receiving this drug. The key issue in determining the value of these different approaches to monitoring is the time of onset of potentially life-threatening bone marrow suppression following initiation of azathioprine therapy. These data are not available for patients with IBD.

In a retrospective study, we analysed the time of onset of all drug related toxicity in IBD patients post initiation of azathioprine therapy. A total of 110 consecutive IBD patients with a history of azathioprine use were identified (table 1). Patients were identified from the hospital inpatient enquiry system, IBD clinic, and pharmacy records. Mean azathioprine dose was 2 mg/kg/day (range 1–3). Mean age of the patients on azathioprine was 38.11 years (18–76). Seventeen of 110 patients (15%) suffered from azathioprine related early toxicity (table 1). Mean azathioprine dose in those showing drug toxicity was 100 mg/day (50–150). Most (77%) drug related toxicity occurred in the first 12 weeks of therapy (fig 1). However, the mean time of onset of drug related toxicity depended on the side effect observed. For example, most drug related nausea was observed within two weeks of commencing treatment while all cases of deranged liver function tests were detected within eight weeks of treatment onset. Significantly, this was not true for bone marrow suppression. The mean duration of treatment in the two patients who experienced this side effect was 11 weeks (range 10–12). Both cases occurred outside the “stringent” eight week monitoring period recommended by the drug’s manufacturer. Hence identification of bone marrow suppression would have been delayed using the current British and manufacturer’s guidelines. The early detection of neutropenia was identified during long term (>3 months) treatment in three patients who continued on maintenance azathioprine (mean duration 101 weeks/patient, range 2 weeks to 5 years). In our practice, we feel that significant toxicity during the early (<3 months) period of therapy could have been missed by strictly following existing guidelines.

Early detection of abnormalities in asymptomatic patients helped in dose adjustment with resolution of side effects. In addition, early detection of azathioprine related bone marrow suppression is likely to save lives. We recommend that gastroenterologists employ an extended (three month) period of intensive haematological monitoring after initiation of azathioprine therapy in IBD. Although neutropenia is occasionally observed beyond this point, intensive monitoring for the duration of treatment, which may continue for years, is clearly not practical from a patient or service perspective. However, this serves to emphasise the importance of continuous patient education concerning “alarm symptoms” throughout the duration of azathioprine therapy.

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Table 1 Patient characteristics and side effects encountered during the initial period of therapy (three months) with azathioprine in patients with inflammatory bowel disease

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Ulcerative colitis</th>
<th>Crohn’s disease</th>
<th>Indeterminate</th>
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</thead>
<tbody>
<tr>
<td>Male</td>
<td>17</td>
<td>25</td>
<td>3</td>
<td>45</td>
</tr>
<tr>
<td>Female</td>
<td>15</td>
<td>48</td>
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<td>65</td>
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<td>Nausea/vomiting</td>
<td>1</td>
<td>5</td>
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<td>6</td>
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<tr>
<td>Abnormal LFTs</td>
<td>3</td>
<td>1</td>
<td>0</td>
<td>4</td>
</tr>
<tr>
<td>Neutropenia</td>
<td>1</td>
<td>1</td>
<td>0</td>
<td>2</td>
</tr>
<tr>
<td>Thrombocytopenia</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Infections</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Herpes rash (1)</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>1</td>
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<tr>
<td>Pancreatitis</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Miscellaneous associated symptoms</td>
<td>Tiredness (1)</td>
<td>Headache (1),</td>
<td>allergic skin rash (1)</td>
<td></td>
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<td></td>
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</table>

NLT, liver function test.

Figure 1 Time of occurrence for various toxicities during azathioprine therapy.

*Onset of toxicity relates to time (in months) from the start of azathioprine treatment to the appearance/detection of toxicity. SE, side effects.

Author’s reply

Measurement of thiopurine methyltransferase (TPMT) status, prior to the start of azathioprine therapy, has a role in identifying the TPMT deficient patient at risk of severe myelosuppression and TPMT heterozygous individuals who are prone to early myelosuppression. The risk of azathioprine toxicity is well recognised but, as the authors state, the matter for debate is the time of onset of potentially life threatening myelosuppression.

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Quasim et al state that this information is not available for patients with inflammatory bowel disease (IBD). However, data can be derived from observations in other patient groups which may serve as useful guidelines for this time interval.

Reports of azathioprine induced severe myelosuppression in the TPMT deficient patient indicate that bone marrow toxicity is recorded after 3–10 weeks (median 4) of azathioprine therapy. In these reports the drug dosage varied from 1 to 2.9 mg/kg (median 1.7). One patient taking azathioprine at a dosage of 1 mg/kg developed myelosuppression (white blood cell count (WBC) 1.6×10⁹/L, platelets 25×10⁹/L) at 10 weeks' while another dosed at 1 mg/kg developed myelosuppression (WBC 3.8×10⁹/L, platelets 100×10⁹/L) in seven weeks. For the patient with an intermediate TPMT activity (heterozygotes with one variant and one wild-type allele), Black and colleagues reported leucopenia (WBC 0.9–2.9×10⁹/L) within one month of starting azathioprine (2–3 mg/kg) as second line therapy for rheumatic disease. Specifically for the patient with Crohn's disease, Colombel and colleagues have reported that TPMT deficient individuals experience leucopenia or thrombocytopenia within 15 months of azathioprine therapy (100–150 mg/day) and that TPMT heterozygotes developed toxicity after 1–18 months (median 6) of therapy. Similar observations have been made by Schwab and colleagues who reported myelosuppression in a TPMT deficient Crohn's patient after 1.75 months of azathioprine at 1.5 mg/kg and in two TPMT heterozygous patients after 2.5 and 3 months of therapy at dosages of 1.0 and 1.5 mg/kg, respectively. Additional observation that those individuals with wild-type alleles (“normal” TPMT activity) can experience myelosuppression after weeks or years of azathioprine therapy illustrates the multi-factorial nature of myelosuppression in this patient group and supports the need for continued vigilance with respect to blood count monitoring.

The drug manufacturer's guidelines, as stated in the Association of the British Pharmaceutical Industry (ABPI) medicines compendium, advise that at a minimum, complete blood counts should be monitored weekly during the first eight weeks of therapy. The guidelines then continue, “this frequency may be reduced during later therapy to monthly intervals, or at least at intervals no longer than three months”. The data presented in the reports above indicate, particularly for the patient on low dose azathioprine in whom TPMT status is unknown, close adherence to the ABPI guidelines and continuation of, at a minimum, weekly full blood counts during the first three months of treatment. Because severe bone marrow toxicity can be precipitated by the addition of aminosalicylate derivatives to the azathioprine regimen, the drug manufacturer's more stringent blood count monitoring scheme should be considered following such adjustments in the combination therapy of refractory IBD.

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References

Appendectomy and ulcerative colitis
Cosnes et al demonstrated that previous appendectomy is not only associated with a lower incidence of ulcerative colitis, but also with a less severe course of the disease. Although we fully agree with this result, we disagree with the recommendation that “Patients genetically at high risk of developing ulcerative colitis may be considered as candidates for appendicectomy for the objective of preventing the development of ulcerative colitis and also decreasing its severity” (Gut 2002; 51: 803–7). All previous studies, as well as the present study, have demonstrated an association only between previous appendectomy and ulcerative colitis. It has not been shown that performing appendicectomy in healthy persons at increased risk of developing ulcerative colitis is beneficial and the association may as well have been caused by an unknown confounding factor, both leading to an increased risk of appendicectomy and a decreased risk of developing (severe) ulcerative colitis. To illustrate this point is the following theoretical example: an epidemiological study on cardiovascular morbidity finds that the risk of cardiovascular events is inversely related to the risk of developing upper gastrointestinal haemorrhage. This could be due to the factor “treatment with aspirin” that could well explain the increased bleeding risk and the lower incidence of cardiovascular events. The confounding factor is the use of aspirin. It would be wrong to conclude that in patients without a history of bleeding, attempts should be undertaken to induce upper gastrointestinal bleeding in order to prevent cardiovascular morbidity.

Another argument against performing this surgical procedure in healthy persons is the finding that appendicectomy in the absence of an inflamed appendix was not associated with a decreased risk of ulcerative colitis, suggesting that appendicectomy rather than appendicectomy protects against ulcerative colitis. Cosnes et al state that these results may not be correct as they included all patients with previous appendicectomy and still found a less severe course. We believe they are incorrectly arguing this, as another possible explanation is that the effect of appendicectomy is actually higher than the effect of appendicectomy reported in the present study, which could have been influenced by the inclusion of patients without appendicitis.

In conclusion, we believe that at present healthy persons at risk of developing ulcerative colitis should not be considered candidates for appendectomy outside clinical trials as evidence showing that appendicectomy will protect these persons is lacking.

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Authors’ reply
Many French surgeons in 1900 did recommend removing preventively all appendixes of young people (see Marcel Proust, “A l’ombre des jeunes filles en fleur”). That was not our purpose. Indeed, the sentence pointed out by ter Borg and van Buuren in our paper did not give a recommendation but only made a suggestion to consider for appendicectomy patients genetically at high risk of developing ulcerative colitis. Ter Borg and van Buuren speculate that appendicectomy and a benign course of ulcerative colitis may be linked through a confounding factor but they do not document their hypothesis. In fact, there is a large body of evidence supporting a causal relationship between appendicectomy and no (or benign) ulcerative colitis, and the strongest demonstration of this relationship is the protective effect of early appendicectomy in the T cell receptor α (TCR-α) knockout mouse model. Note also that the “Swedish study” which found that only appendixectomy for inflammatory conditions protects against ulcerative colitis did not take into account cases of mild ulcerative colitis, a subgroup in which appendicetomised patients may be over represented.

Finally, we do not believe that the effect of appendicectomy on the course of ulcerative colitis is so high that it would remain after excluding all patients without appendicitis, thus probably two thirds of our patients’ 1 a key point however, like in the TCR-α knockout mouse model, is the date of appendicectomy. Appendicectomy protects against severe ulcerative colitis only when performed at a young age, and therefore disease onset. 1 This latter observation argues against any therapeutic effect of appendicectomy after onset of ulcerative colitis. The problem is different when considering patients at risk for the disease.

We do believe that in a few years it will be possible to screen out young patients with a
predisposing genotype for ulcerative colitis, and a clinical trial assessing the benefits of prophylactic appendectomy will be warranted.

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References

Screening and surveillance for asymptomatic colorectal cancer in IBD

We would like to voice our concerns about some of the recommendations in the guidelines recently published by the British Society of Gastroenterology and Association of Coloproctology for screening and surveillance for asymptomatic colorectal cancer in patients with inflammatory bowel disease (Gut 2005;54(suppl V):v10–v12).

(1) In the present medicolegal environment, failure to comply with guidelines which carry the imprimatur of respected national bodies will require vigorous defence should mishap occur. We do not believe the evidence is strong enough to justify the recommendation that every patient with extensive colitis of duration greater than 8–10 years should undergo regular colono­scopy. Firstly, it must be determined at each hospital whether it is possible and considered sufficiently cost effective to offer such a service with the constraint of local resources available. Secondly, if regular colono­scopy can be offered, then each patient should decide whether or not to accept regular colono­scopy after full discussion of its possible advantages and limitations.

(2) The success of colono­scopic surveillance programmes is disputed. Although some centres (including our own) have been protagonists for this approach, others have argued that it is not only labour intensive but also ineffective. Before imposing global national guidelines we should have firm evidence of a scheme’s efficacy or, failing this, we should have multicentre consensus. The guidelines, as published, are to be the sincerely held opinions of a single consultant team based on their own research and assessment of the literature, followed by approval of a committee, but no indication is given of widespread con­sensus.

(3) The recommendations for patients with extensive colitis of duration every third year during the second decade of disease, every second year during the third decade, and annual colono­scopies thereafter are complex. The evidence for an increasing risk of cancer in the second, third, and succeeding decades of disease duration is controversial, and is not borne out by the unstratified figures for patients with ulcerative colitis based on 26 reported studies in the meta-analysis reported by the authors of the guidelines, although an increase was observed in stratified data. The strategy suggested therefore is not based on firm data, and in particular pub­lished data after the third decade are few due to the small numbers involved.

(4) We have concerns about the benefits during routine colono­scopic examinations of taking two to four random biopsy specimens every 10 cm. This approach is time consuming for both the colono­scopist and the pathology department, and adds a considerable financial burden to the programme (which is not included in Eaden and Mayberry’s cost analy­sis). In theory, the risk of a false negative examination is reduced as more biopsies are taken but in practice the additional yield is very low indeed. During a recent study at St Mark’s Hospital, almost 3000 random surveil­lance biopsies from such patients yielded no dysplasia (unpublished data).

(5) Considering the disputed efficacy of current colono­scopic surveillance pro­grammes for patients with extensive ulcerative colitis, it is inappropriate at present to extend this by default to patients with left sided colitis or (by implication) those with Crohn’s disease.

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Authors’ reply

We would like to thank Dr Forbes et al for their response to the guidelines published by the British Society of Gastroenterology (BSG) and the Association of Coloproctology of Great Britain and Ireland on screening and surveil­lance for asymptomatic colorectal cancer in patients with inflammatory bowel disease (Gut 2005;54(suppl V):v10–v12). They raise a number of points which will be answered in turn.

(1) There are medicolegal implications of failing to comply with recommendations from a respected body but a guideline is precisely that—a guideline. They are not etched in a paternalistic attitude, as patients should now accept some responsibility for their illness.

(2) Forbes et al raise the point that before any guidelines are implemented there must be firm evidence of a scheme’s efficacy. There are no randomised studies comparing different surveillance protocols or for that matter even parallel surveillance studies. The guidelines are largely based on published evidence from journals and the conclusions of reviews (the authors of the guidelines, et al are well aware, it will never be possible to provide grade A recommendations on this issue and the best we can do is to assess sur­veillance programmes retrospectively. Data are accumulating that surveillance partic­i­pants have reduced morbidity and mortality, as outlined in the guidelines, and one of the signatories to your letter has stated that surveillance improves survival.

One notable review of surveillance programmes from the Leeds group did not show any benefit from surveillance but the group had very stringent criteria. They felt inclusion of dysplasia alone as a measure of success was of debatable value. Therefore, they limited their audit to those eventually found to have cancer. They also decided to exclude patients in whom cancer was found at an initial examination undertaken at least 12 years after the onset of symptoms. We feel that finding dysplasia alone is the very aim of surveillance. Because it is found on the first colonoscopy in a surveillance programme it should not be deemed a failure of surveillance.

As with any consultation, it was possible to be informed of the guidelines before they were published. A national audit of the surveillance programmes of gastroenterologists was conducted which revealed that although 94% of gastroenterologists performed surveillance, there was wide variation in practice. This alone suggests there would be little chance of a consensus opinion across the UK. Once the guidelines had been formulated, the Clinical Services and Standards Committee comprising approximately 40 individuals reviewed them. They then went through the usual guidelines process after being seen by the Executive Committee of the BSG for a further review and signing off. Thus the guidelines evaluated by the Committee had been accepted and published. They are not simply the opinions of two consultant gastroenterol­ogists.
(3) We appreciate that increasing the colonoscopy frequency with increasing duration of disease is more complicated than 1–2 yearly surveillance. However, we are sure that it is not too difficult to calculate and it actually reduces the number of colonoscopies being performed initially, so this must be regarded as a potential improvement on the present routine practice of some gastroenterologists.

The meta-analysis does show an increasing cancer risk in the second and third decades of disease. It is only stratified data that can be used in this way. Such data will give the most accurate estimate as it is only these data that included studies which reported cancer incidence stratified by decade and duration of patient follow up (19 studies). The decade specific incidence rates correspond to a cumulative risk of 1.6% (95% confidence interval (CI) 1.2–2%) by 10 years, 8.3% (95% CI 4.8–11.7%) by 20 years, and 18.4% (95% CI 15.3–21.5%) by 30 years.

The 26 studies Forbes et al refer to also included studies which reported cancer incidence where only duration of patient follow up was reported—that is, the incidence rates were not broken down for each decade. Even with the stratified data were examined the cancer incidence still increased by decade of disease! The unstratified cumulative probabilities give a risk of 4.4% (95% CI 2.0–6.8%) at 10 years, 8.6% (95% CI 4.0–13.3%) at 20 years, and 12.7% (95% CI 6.0–19.3%) at 30 years.

Therefore, the strategy suggested is based on firm data. Of course the numbers of patients by the third decade are few but this is the nature of the beast. The use of a meta-analysis of cancer risk in ulcerative colitis.tosis overcomes the inadequacies of any reliance on smaller studies from single specialist centres.

(4) We accept that the cost of biopsies was not included in the cost analysis. There are numerous articles debating the number of biopsies which should be taken during a surveillance colonoscopy. ‘Yes it is time consum- ing but we all know that to stand any chance of detecting dysplasia, the more biopsies taken the better. What is the point in surveil- lance at all if it is not conducted to the best standards? If, for example, only 10 biopsies were being taken at each examination, we would expect the chance of detecting dysplasia to be low.

(5) As patients with Crohn's colitis have been shown to have the same cancer risk as patients with extensive ulcerative colitis, it would be doing them a disservice to exclude them from a surveillance programme.6 Left sided colitis also carries an intermediate risk for colorectal cancer and as such our guide- lines reflect this. Indeed, one of the signatories to the Forbes et al letter has himself advocated a similar approach after discussion with that patient.7

The guidelines were formulated on the best evidence available at present. Surveillance was being conducted in an extremely disor- ganised way in the UK which is not acceptable in the current climate of clinical governance. The BSG has properly encouraged a national approach to cancer surveillance in a rigorous and reliable fashion. The principles, which underlie such an approach, are that of best practice throughout the country. The law must discuss with them the nature of surveil- lance and its inadequacies. If patients then choose to have surveillance we are obligated to provide a service which reaches the highest standards—similar to those in other screening services.

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References
1 Wilsher v Essex Area Health Authority. (1986) 3 ALL ER 80
2 Early v Newham Health Authority. (1984) 5 Med LR 214

Crohn's disease: ethnic variation in CARD15 genotypes
Crohn's disease shows significant variability in incidence between different world popula- tions. For example, Kurata and colleagues studied the annual incidence of Crohn's disease in Caucasian, African, Asian, and His- panic individuals, with an observed range from 43.6 per 100,000 population for Caucasian, 29.8 for African, 5.6 for Asian, and 4.1 for Hispanic panic individuals, with an observed range from 43.6 per 100,000 population for Caucasian, 29.8 for African, 5.6 for Asian, and 4.1 for Hispanics. Recently, a genetic basis for Crohn's disease has been described. The CARD15 gene (NO23, MIM 605956) acts as a sensor for bacterial products. When functioning cor- rectly, this would lead to activation of nuclear factor kappa B. Sixty seven variations in the CARD15 gene's genomic sequence have been reported. Of these 67, three variations (2104 C>T [R702W]; 2722 G>C [G908R]; 3020insA [1007fs]) have been consistently correlated with increased susceptibility to Crohn's disease.9

Currently, these three variants have only been extensively assessed in patients of Euro- pean, French Canadian, or American Cana- dian descent. Using Pyrosequencing, we ana- lysed all three variants in genomic DNA from 95 European (American Caucasian), 95 Afri- can (Ghanaians), and 53 Asian (Chinese) unrelated healthy volunteers. Frequencies for the R702W variant allele were 2% and 0% in European and African samples, respectively. 3% and 1% for G908R, and 3% and 0% for 1007fs (p<0.05 in all cases). None of the vari- ants was observed in the Asian population, consistent with a recent study of Japanese patients with Crohn's disease.10 The ethnic variation seen here could, in part, contribute to the variations in the frequency of Crohn's disease in different world populations. Attention should be paid to the discovery of novel geographically selective variants before evaluat- ing association with Crohn's disease in non- European populations.

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Acknowledgements
This work was supported in part by U01 GM63340.

References

Doctor or technician
In 1984, Sir Christopher Booth (President British Society of Gastroenterology (BSG) (1979) gave a lecture in Berlin on the effect of technology on clinical practice. He lauded the rapidly expanding benefits of diagnostic and interventional gastrointestinal endoscopy but was led to ask “Will the gastroenterologist simply become a technician who carries out these procedures?”

Most gastroenterologists remain general physicians but in talking with specialist
registries I have been surprised by their overwhelming interest in honing endoscopic skills. If this leads to a simplistic approach to the recognition of possible gastrointestinal pathology, it has its dangers. Analysis of two cases in the past month reminded me of this.

Case No 1
An elderly man was admitted to hospital with severe anemia. The houseman obtained a history of aspirin ingestion and, over the preceding few weeks, recurrent melanoma. He described feeling a hard liver edge. A blood count showed haemoglobin (Hb) 4.3, mean corpuscular volume (MCV) 55.7, white blood cell count (WCC) 6.6, and platelets 63. The patient was transfused and without further investigation the physician/gastroenterologist arranged for oesophago-gastro-duodenoscopy (OGD) and colonoscopy.

At OGD the stomach was described as showing a moderate erythematous/exudative gastritis. At colonoscopy no abnormality was seen apart from a little fresh bleeding, the cause of which was not apparent. The patient was then sent home only to be admitted 36 hours later with faecal peritonitis. The colon had been perforated at the rectosigmoid junction. The patient recovered well from reparative surgery but died six weeks later of multiorgan failure. At necropsy he was found to have cirrhosis of the liver. In retrospect, photographs of the gastric mucosa were consistent with portal gastropathy.

Case No 2
An elderly man taking diclofenac for osteoarthritis of the hip began drinking heavily after the death of his wife. One Saturday he felt faint, vomited black fluid, and passed urine in his room. He was admitted the next day with acute hepatitis. In retrospect, the dictum: bleeding from the gut requires a simplistic approach to the management of patients with occult gastrointestinal blood loss. If this leads to a simplistic approach to the recognition of possible gastrointestinal pathology, it has its dangers. Analysis of two cases in the past month reminded me of this. The physician/gastroenterologist arranged for oesophago-gastro-duodenoscopy (OGD) and colonoscopy.

At OGD the stomach was described as showing a moderate erythematous/exudative gastritis. At colonoscopy no abnormality was seen apart from a little fresh bleeding, the cause of which was not apparent. The patient was then sent home only to be admitted 36 hours later with faecal peritonitis. The colon had been perforated at the rectosigmoid junction. The patient recovered well from reparative surgery but died six weeks later of multiorgan failure. At necropsy he was found to have cirrhosis of the liver. In retrospect, photographs of the gastric mucosa were consistent with portal gastropathy.
TIPS for gastric varices

We recently read with interest the study by Tripathi and colleagues investigating the outcome of TIPS in patients with gastric (GV) compared to oesophageal varices (OV). This study confirmed the previous finding of lower mean portosystemic pressure gradient (PPG) in patients with GV bleeding relative to those with a history of OV bleeding. Indeed in this study 35% (14/40) of GV patients compared to only 8% (20/202) of OV patients had a PPG <12 mm Hg.

The group of patients who bled at PPG <12 mm Hg (group 1) is particularly intriguing. As mentioned by the authors, low PPG in GV patients is only 8% (20/232) of OV patients had a PPG <12 mm Hg (group 1) is particularly intriguing. As mentioned by the authors, low PPG in GV patients is only 8% (20/232) of OV patients. Previously, Sanyal et al. found that 50% (6/12) of patients who underwent TIPS for prevention of GV re-bleeding failed to decompress the varices as documented by endoscopy, 46% of these patients had a large GRS and a PPG <12 mm Hg. Thus, based on our previous study in 1 patient in the current study (both GV and OV) are likely to have had a spontaneous GRS already decompressing the portal system. It would be valuable to know if the authors have any data on the presence of GRS in their patient population, perhaps documented by portogram taken at the time of TIPS? Also, did they document decompression of varices post-TIPS as an indicator of the clinical efficacy of the procedure? For example, it would be interesting to know if patients in group 1 failed to decompress varices post-TIPS more often than patients in group 2. Anecdotally, we have experience of a number of patients with large GV who had a baseline PPG <12 mm Hg and a large GRS. Following TIPS in these patients, there was a minimal or no reduction in PPG and filling of the GV was not shown to be reduced on post-TIPS portogram.

Finally, the authors noted in group 2 (baseline PPG >12 mm Hg) that lower post-TIPS PPG was associated with a lower risk of bleeding, as would be hoped. However, in group 1, there was no difference in post-TIPS PPG between patients who did and did not re-bleed, suggesting that PPG may not be a critical determinant of variceal bleeding in patients who have a low PPG to start with. The role of PPG in dictating the natural history of GV is not known. Conceptually, insertion of an artificial portosystemic shunt into a patient who already has a large spontaneous shunt effectively offloading the portal pressure would not seem to confer much benefit. Do these GV (and possibly OV) patients with low PPG pre-TIPS and with a possible GRS really benefit from TIPS?

MR angiography can accurately assess for presence of a spontaneous GRS. There is a compelling argument that this should be an essential part of the assessment algorithm of patients with GV if a large spontaneous shunt is present, and PPG (as measured by hepatic vein wedge pressure gradient (HVPG)) is <12 mm Hg, then perhaps other therapeutic options such as B-RTO (balloon occluded retrograde transvenous obliteration) should be considered. Hopefully more prospective data, examining the role of PPG, TIPS, and B-RTO in the management and outcome of GV will help clarify these issues.

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References


Authors’ reply

We agree that the presence of gastroesophageal variceal haemorrhage is likely to explain the low portal pressure gradient (PPG) post-transjugular intrahepatic portosystemic stent shunt (TIPS). Portography at the time of index TIPS insertion was primarily performed to identify varices and not specifically to look for the presence of GRS, although the splenic vein was visualised if not always in its entirety. Given these limitations, we have looked at the portograms of over 400 patients who have had a TIPS for any cause and identified shunts in 18.3%. A wider portographic review and a prospective magnetic resonance angiography study will be required to answer the questions raised. For the present study, we used a post-TIPS PPG <12 mm Hg as an indicator of the efficacy of the TIPS procedure for patients with both gastric and oesophageal variceal bleeding. In light of our findings we have revised our target PPG post-TIPS to <7 mm Hg.

Our finding of a lack of a statistical difference in the post-TIPS PPG of those patients who did or did not rebleed may be due to the small numbers in group 1. It may be that factors other than portal pressure such as variceal size and variceal wall tension play an important part in the risk of variceal bleeding in patients with a PPG of <12 mm Hg. It is also true that portal pressure directly affects the variceal wall tension, and attempts to reduce the portal pressure by a TIPS will be beneficial. We strongly believe that TIPS has a significant role in patients who have refractory gastric variceal bleeding, as mirrored by studies from others. At the present time it is the most effective non-surgical method of treating gastric variceal haemorrhage and preventing rebleeding. Of course, there are some therapeutic options that are promising, and we have previously reported on the effective use of human thrombin in the treatment of acute gastric variceal haemorrhage. However, controlled studies are required before universal recommendation of endoscopic therapies for gastric variceal haemorrhage.

References

available evidence about different management approaches. The book also has a feel of having been composed from an upstream viewpoint. Apart from a hepatologist, the other five contributors are all surgeons—it might have been useful to have had the perspectives of a general physician and a general practitioner even if only to raise the pragmatic queries that arise at the earlier stage of management of hepatobiliary problems. An example is the potential prophylactic management of patients with varices. As it stands, variceal management in this publication commences essentially after the bleed with only a few lines on prophylactic management, and even those largely dismissive of possible measures apart from the global use of beta blockers. So what should a primary care practitioner dealing from the global use of beta blockers. So what should a primary care practitioner dealing with patients with varices actually do, and at what stage of abnormal liver tests or clinical findings is referral likely to be rewarding?

Increasingly, on a worldwide basis, a primary care clinician or a general practitioner with generic interests provides initial health care. Many of them will rely on such publications as a ready source of information. Ease of access to the information is important—this is assisted in this ABC by the use of summary points and clear illustrations. The aim of the present book is to provide a general overview and to enable the clinician to keep abreast of advances in the common and the rarer conditions. It succeeds in this.

**Therapeutic Roles of Selective COX-2 Inhibitors**


The review editor sent me a book to review for Gut with holidays looming. He left a week before Christmas and is currently observing whales off the east coast of the USA where I hope he will encounter the albino whale, 1, on the other hand, am in a more peaceful location surrounded by the truly magnificent lochs, standing stones, stone circles, chambered cairns, duns, and crannogs on the Outer Hebrides. At last (Ballou et al) a detailed and intelligible account of the lessons that we should have learned from the study of COX-1 and -2 selective drugs is assisted in this. It succeeds in this. It is abreast of advances in the common and the rarer conditions. It succeeds in this. It is assisted in this.

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**Drug-Induced Liver Disease**


On the face of it, this hardback does not sound like a very good bedtime read and splashing out £195 might seem a little extravagant for what might appear to be some dry tome that would readily gather dust. However, I am pleased to say that these were ever the odd consultant gastroenterologist! The book would be very useful for hospital pharmacists, nurse practitioners, and also for medical students who need to refer to the subject in more depth. However, on considered reflection, I think that most gastroenterology units should think carefully about buying a copy—with increasing drug usage by patients, illicit or otherwise, it is an exploding problem. The book provides an aide-memoire for those that need it and it won’t gather dust for long!

**NOTICES**

New In Vivo Imaging Modalities for Molecular Biology, Cell Biology and Physiology

This Jacques Monod conference will be held on 31 May–4 June 2003 in Rocquencourt, France. Further information: Bertrand Tavitian, INSERM M10103, Service hospitalier Frédéric Joliot, CEA Direction des Sciences du Vivant, Direction de la Recherche Médicale, 4 place du Général Leclerc, 91401 Orsay Cedex, France. Tel: +33 169 867 779; fax: +33 169 867 739; email: tavitian@shlj.cea.fr
Prague Hepatology Meeting
To be held on 5–7 June 2003 in Prague, Czech Republic. Leading speakers from Europe and the USA will present new ideas and suggestions on pathophysiology, diagnostics, and therapy of liver diseases in ten programmes blocks. Further details: Ms Veronica Revicka. Tel: +420 241 445 759; fax: +420 241 445 806; email: veronika@congressprague.cz

Falk Symposia—New Findings on Pathogenesis and Progress in Management of IBD
Two symposia and a workshop will be held on 10–14 June 2003 in Berlin, Germany. Further details: Falk Foundation e.V., Congress Division, PO Box 6529, Leinenweberstr. 5, 79041 Freiburg/Bf, Germany. Tel: +49 761 15 14 359; email: symposia@falkfoundation.de; website: www.falkfoundation.de

Gastroenterology and Endotherapy: XXIst European Workshop
This will be held on 16–18 June 2003 in Brussels, Belgium. Further details: Nancy Beauprez, Administrative Secretariat of the Workshop, Gastroenterology Department, Erasme Hospital, Route de Lennik 808, B-1070 Brussels, Belgium. Tel: (+32 (0)2) 555 49 00; fax: (+32 (0)2) 555 49 01; email: beauprez@ulb.ac.be

European Helicobacter Study Group (EHSG)
This meeting, on Helicobacter infections and gastroduodenal pathology, will be held on 3–6 September 2003 in Stockholm, Sweden. Further details: Professor Torkel Wadstrom, President- EHSG, Lund University, Department of Infectious Diseases & Medical Microbiology, Division of Bacteriology, Solvegatan 23, SE-223 62 Lund, Sweden. Tel: +46 46 467 241; fax: +46 46 152 564; email: Torkel.Wadstrom@mmb.lu.se; website: www.helicobacter.org

Falk Symposium 135—Immunological Diseases of Liver and Gut
This symposium will be held on 12–13 September 2003 in Prague, Czech Republic. Further details - see Falk Symposia above

The Association of Coloproctology of Great Britain & Ireland
This annual meeting will be held on 7–10 July 2003 in Edinburgh, UK. Further details: Conference Secretariat, The ACGBI at the Royal College of Surgeons of England, 35–43 Lincoln’s Inn Fields, London WC2A 3PE. Tel: +44 (0)20 7973 0307; fax: +44 (0)20 7430 9235; email: acgbii@asgbi.org.uk; website: www.acgbii.org.uk

The European Society of Parenteral and Enteral Nutrition (ESPEN)
ESPEN will celebrate its silver anniversary at the time of the annual congress, which is to be held on 20–23 September 2003 in Cannes, France. Further details: www.espen.org

XII Falk Liver Week
The XII Falk Liver Week, in honour of Hans Popper’s 100th birthday, will be held on 15–22 October 2003 in Freiburg, Germany. Further details - see Falk Symposia above.

European Course on Laparoscopic Endoscopy
This course will be held on 18–21 November 2003 in Brussels, Belgium. Further details: Secretary to Professor Cadière, Service de Chirurgie Digestive, Rue Haute 322, Brussels 1000, Belgium. Tel: +32 (02) 648 07 60; fax: +32 (02) 647 86 94; email: straeb.asmb@proximedia.be; website: www.straeb-asmb.com

Hong Kong-Shanghai International Liver Congress 2004
This conference will be held on 14–17 February 2004 in Hong Kong. The topic of the conference is “Liver Diseases in the Post-Genomic Era”. Further details: Ms Kristie Leung, Room 102–105 School of General Nursing, Queen Mary Hospital, 102 Pokfulam Road, Hong Kong. Tel: +852 2818 4300/8101 2442; fax: +852 2818 4030; email: kristieleung@hepa2004.org; website: www.hepa2004.org

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TPMT in the treatment of inflammatory bowel disease with azathioprine

A Qasim, J Seery, M Buckley and C O Morain

Gut 2003 52: 767
doi: 10.1136/gut.52.5.767

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