TPMT in the treatment of inflammatory bowel disease with azathioprine

We read with interest the recent article by Lennard on the role of thiopurine methyltransferase (TPMT) enzyme in predicting azathioprine related toxicity in patients with inflammatory bowel disease (IBD) (Gut 2002;51:143–6). He concludes that measurement of TPMT activity has no specific role in 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 importance of haematological monitoring in IBD patients receiving this drug. The American College of Gastroenterology guidelines recommend a slightly different approach, with fortnightly blood counts for the initial three months of therapy. 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 treatment. 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 toxicity (table 1). Mean azathioprine dose in those showing drug toxicity was 100 mg/day (50–150). Most (77%) drug related toxicity was observed within the first 12 weeks of therapy (fig 1). However, the mean time of onset of drug related toxicity was 101 weeks/patient, range 2 months to 5 years. In our practice, we feel that continued on maintenance azathioprine 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. Further analysis of neutropenia were 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 find that significant toxicity during the early (<3 months) period of therapy could have been avoided by strictly following established 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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References

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.1,2 The risk of azathioprine toxicity is well recognised but, as the authors state, the duration of early monitoring is a matter of controversy. The matter for debate is the time of onset of potentially life threatening myelosuppression.

Table 1

<table>
<thead>
<tr>
<th>Category</th>
<th>Ulcerative colitis</th>
<th>Crohn’s disease</th>
<th>Indeterminate</th>
<th>Total</th>
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<tr>
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<td>25</td>
<td>3</td>
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<tr>
<td>Female</td>
<td>15</td>
<td>48</td>
<td>2</td>
<td>65</td>
</tr>
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<td>5</td>
<td>0</td>
<td>6</td>
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<tr>
<td>Abnormal LFTs</td>
<td>3</td>
<td>1</td>
<td>0</td>
<td>4</td>
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<tr>
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<td>1</td>
<td>0</td>
<td>2</td>
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<td>Headache (1)</td>
<td>allergic skin rash (1)</td>
<td>3</td>
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<tr>
<td>symptoms</td>
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<tr>
<td>LFT, liver function test.</td>
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</table>
Quasi 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.1–4 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) 1.6x10^9/l, platelets 25x10^9/l) at 10 weeks while another dosed at 1 mg/kg developed myelosuppression (WBC 5.8x10^9/l, platelets 22x10^9/l) within 1.5 months of azathioprine therapy.1 It may be that severe bone marrow toxicity can be precipitated by the drug dosage or its rate of administration. Because severe bone marrow toxicity has been observed in patients with a severe myelosuppression during azathioprine therapy, this observation is relevant to the discussion of drug dosage and its rate of administration.1,4

Specifically for the patient with Crohn’s disease, Colombel and colleagues1 have reported that TPMT deficient individuals experience leucopenia within 1.5 months of azathioprine therapy (100–150 mg/day) and that TPMT heterozygotes developed toxicity after 1–18 months (median 4) of therapy. Similar observations have been made by Schwab and colleagues2 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 heterozygotes patients after 2.5 and 3 months of therapy at dosages of 1.0 and 1.5 mg/kg, respectively. Additional observation is that those individuals with wild-type alleles (“normal” TPMT activity) can experience myelosuppression after weeks or years of azathioprine therapy. This suggests that the likelihood of myelosuppression in this patient group and support the need for continued vigilance with respect to blood count monitoring during azathioprine therapy. The drug manufacturer’s guidelines, as stated in the Association of the British Pharmaceutical Industry (ABPI) medicines compendium,3 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 doses of azathioprine, that severe bone marrow toxicity can be precipitated by the addition of aminosalicylate derivatives4 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.

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 can fully agree with this result, we disagree with the recommendation that appendectomy is beneficial. The association may as well have been caused by an unknown confounding factor, both leading to an increased risk of appendectomy 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 not 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 appendectomy in the absence of an inflamed appendix was not associated with a decreased risk of ulcerative colitis, suggesting that appendicitis rather than appendicectomy protects against ulcerative colitis.1 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 assuming this, as another possible explanation is that the effect of appendicitis is actually higher than the effect of appendicectomy reported in the present study, which could have been diluted 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 appendicectomy outside clinical trials as evidence showing that appendicectomy will protect these persons is lacking.

Authors’ reply

Many French surgeons in 1900 did recommend removing preventionally all appendixes of young people (see Marcel Proust, “A l’ombre des jeunes filles en fleurs”). 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.1 Note also that the Swedish study which found that only appendectomy for inflammatory conditions protects against ulcerative colitis did not take into account cases of mild ulcerative colitis, a subgroup in which appendicectomised patients may be over represented.

Finally, we do not believe that the effect of appendectomy 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 therapeutical 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 appendicectomy 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 2002;51 (suppl V):v10–12). (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 within 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, appear 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 consultation.

(3) The recommendations for patients with extensive colitis of duration three years during the second decade of disease, every second year during the third decade, and annual colonoscopy 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 stratified figures for patients with moderate 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 of 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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Reference

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 2002;51 (suppl V):v10–12). 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 stone and may need to be amended at future dates to continue to reflect best practice. The case of Wilsher v Essex Area Health Authority demonstrates the way in which the courts, now not only to practice to the highest standards. However, the courts (Early v Newham Health Authority) will consider local guidelines. This will be of particular importance to units that are unable to deliver standards that have been identified nationally, provided the local practice has been formulated into a local guideline. There is a wealth of data supporting the increased cancer risk in patients with ulcerative colitis and although evidence of the effectiveness of surveillance at present, centres is uncertain, such an approach is widespread. The purpose of guidelines is to identify good practice and to achieve a uniform approach throughout the country. The alternatives are to abandon surveillance or to offer haphazard and unstructured (and ineffective) practice. The alternative approach of surveillance for both colitics and Crohn’s patients is estimated to be £9600 per annum. Hopefully, after a careful weigh up of this cost but we realise funds may not be available initially which is one of the reasons for suggesting the guidelines are audited in five years. It is clearly stated in the guidelines that a discussion should take place between the doctor and patient informing them of their individual risk so that the patient can make an informed decision before embarking on a surveillance programme. Gone are the days of 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 imposed there should be firm evidence of a scheme’s efficacy. There are no randomised studies comparing different surveillance protocols or for that matter even within the guidelines. Although all of us 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 improve 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 colonoscopy undertaken at least 12 years after the onset of symptoms. We feel that finding dysplasia is the very aim of surveillance and just because it is found on the first colonoscopy in a surveillance programme it should not be deemed a failure of surveillance.

As much consultation as possible was obtained before the guidelines were published. A national audit of the surveillance practices 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 Clinical Services Committee. The guidelines were then referred to the BSG Executive Committee of the BSG for a further review and signing off. Thus the guidelines were evaluated, accepted and published. They are not simply the opinions of two consultant gastroenterologists.
(3) We appreciate that increasing the colonoscopy frequency with increasing dura-
tion 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 an 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 and is not controversial in the least. However, specific incidence rates correspond to a cumulative 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 13.5–21.5%) by 30 years.

The 26 studies Forbes et al refer to also included studies which reported cancer inci-
dence where only duration of patient follow up was reported—that is, the incidence rates were not broken down for each decade. Even when the duration 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 overcomes the inadequacies of any reliance 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 overcomes the inadequacies of any reliance 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 overcomes the inadequacies of any reliance 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 overcomes the inadequacies of any reliance on firm data. 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Case No 1
An elderly man was admitted to hospital with severe anaemia. The houseman obtained a history of aspirin ingestion and, over the preceding few weeks, recurrent melena. 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 discharged to the gold home 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 multorgan 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. In the USA, many health care organizations cite trials of SASP versus newer agents were treated after the trial with balsalazide. The recent study of the oral preparations and that balsalazide is more effective; these trials are in patients with newly published views that mesalazine is ineffective, but it does require a reliable delivery system that the mesalazine release mechanism is important for the efficacy, reliability, delivery, and safety of the oral preparations and that balsalazide is more effective and better tolerated than mesalazine in the treatment of active ulcerative colitis. Results of a controlled trial of tissue drug concentrations and clinical pharmacokinetic parameters. Br J Clin Pharmacol 2000;49:323–30.


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% (20/232) 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 has been shown to increase the presence and size of a spontaneous gastrorenal shunt (GRS) which is present in up to 85% of GV patients but present in only about 20% of OV patients. Previously, Sanjal et al. found that 50% (6/12) of patients who underwent TIPS for prevention of GV rebleeding failed to decompress the varices as documented by endoscopy. 4/6 of these patients had a large GRS and a PPG <12 mm Hg. Thus, based on probability, the group 1 patients 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 of <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 GRS 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 rebleeding. 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 the 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 gastrorenal shunts (GRS) is likely to explain the low portal pressure gradient (PPG) post-transjugular intrathepatic 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 TIPS for any cause, and identified shunts in up to 85% of GV patients but present in only 20% of OV patients.

Although the list of topics is complete, there are still problems in interpreting some of the information for use in the pragmatic clinical setting. The section on gall stones to liver and biliary disease to the clinical conditions themselves, ranging from gall stones to liver and pancreatic transplantation. Because of the nature of the clinical area, the book will probably appeal more to the hospital clinician than the general practitioner although the latter will gain much in having a source of reference for unusual and awkward clinical situations.

The ABC Series are well established as handy reference guides but they sometimes struggle from not being clear about their target readership. Midway between a textbook and an update, this publication follows the usual format. The topic is covered from investigations used in liver and biliary disease to the clinical conditions themselves, ranging from gall stones to liver and pancreatic transplantation. Because of the nature of the clinical area, the book will probably appeal more to the hospital clinician than the general practitioner although the latter will gain much in having a source of reference for unusual and awkward clinical situations.

Although the list of topics is complete, there are some problems in interpreting some of the information for use in the pragmatic clinical setting. The section on gall stones, for example, while full of detail of anatomy and presentation does not make it any easier for the clinician trying to decide whether to refer the patient with stones or when cholecystectomy is indicated. It may well be that in some areas, such as gall stone management, clinical judgement still outstides evidence but it would have been useful to have had...
available evidence about different management approaches.

The book also has a feeling of having been compiled upstream vis-à-vis an 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 with a patient who might have 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 authors is assisted in this overview and to enable the clinician to keep abreast of advances in the common and the rarer conditions. It succeeds in this.

At last (Ballou et al) a detailed and intelligent account of the lessons that we should have learned from the study of COX-1 and -2 deficient animals. Indeed four, bowel, cardiovascular, muscular system, arthritis, and bone are a treat, and reflect the scope of the book. Whittle, Hawkey, and Rodriguez could have combined their three chapters on the gastro-intestinal toxicity of NSAIDs as their knowledge is complementary. They invade each others “intellectual” territory which gives the impression of conflict and confusion where none exists. The chapter by DuBois on COX-2 in colorectal cancer is only disappointing because of its brevity; the man has so much more knowledge to share! However, this is partially compensated by an excellent review of the role of COX-2 in other cancers. The book ends with rather biased accounts on the virtues of each of the COX-2 selective agents.

This book is currently the best available reference source on a subject that is growing in importance and complexity. The only irruptions are some unnecessary self-congratulatory comments and unashamed pleas for financial support that will raise the eyebrows of the purchaser of this book in Hay-on-Wye bookshops at the end of this century. I can thoroughly recommend it for all established prostaglandin and COX research- ers, and Ph.D students. Clinical gastroenterologists will find something new of interest. Those with time, concentration, and a biochemical background will surely enjoy this book as much as I did. Aah! Catherine my lifelong companion has just brought me a wee dram (Talisker single malt whisky with 10% v/v local water). Life just does not get much better than this. My euphoria is however marred by the knowledge that the COX-2 selective drugs are not available to the citizens of Cuba as a result of the USA’s embargo.

P Hungin

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 the deadline, and was currently observing whales off the east coast of the USA where I hope he will encounter the albino whale. I, 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 Hebridean island of North Uist.

The book Therapeutic Roles of Selective COX-2 Inhibitors is the latest in a series of similar books edited by Vane and Bolting, and I have found it in many ways as interesting and extraordinary as my surroundings. It exceeded all expectations, bringing together some of the people that have driven the COX-2 story through from birth of a concept to a marketable drug.

Vane and Botting set the pace with an authoritative introduction, elegantly outlining the history, biochemistry, and physiology of COX-1 and COX-2 and their clinical potential. In this book they gracefully allow etodolac and nimesulide into the privileged COX-2 selective club or, as they call it, the “COX-1 sparing drug” club. Then follow high quality chapters on the discovery and studies on rofecoxib and celecoxib along with discussions on the various test systems to assess selectivity. The excursion from an up and coming Derek Willingham’s chapter on COX-2 in inflammation in experimental models kept me awake for the whole night. This is a true master at work!

At 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 on a shelf. It was therefore with some trepidation that I received this ostensibly unwrapping 575 page bound and green covered book, and turning the pages quickly, the casual reader might still be deceived into casting it into the nether regions of a desk drawer or the dinner corners of an office. However, I am pleased to say that these were all poorly grounded misapprehensions, because this is an expertly written and very detailed overview of the whole galaxy of experts, covering the mechanisms of drug induced liver injury in an easily readable format (who cannot stand up and say that paracetamol/acetaminophen poisoning is a mystery to most mere mortals, despite the fact we all know the antidote?). The book also could have been turned into a roll call of chapters on the hepatotoxicity of specific drugs for quick and easy reference, which is set firmly in clinical context, and for the generalist, a very useful clinical section on diagnosis and management of drug induced liver disease.

Another reason not to look at such a book might be that many of those emanating from the USA have an American perspective only, but this is a real exception as at least one third of the chapters are written by authors from countries as far apart as Switzerland and India and the book as a whole develops a truly international theme. It is well referenced in a very authoritative and up to date way, and sheds light on a whole host of topics that are bothersome to most gastroenterologists, such as the usage of methotrexate in psoriasis and rheumatoid arthritis (with helpful guidelines on when to offer baseline and follow up liver biopsy and to whom), antituberculous agents (and when to worry!), and in this day and age, sections on psychotropic agents, drugs of abuse, and importantly, when many patients will be keen to seek out the veritable pharmacopeia that can be found in the average health food shop or in Chinatown, an authoritative compendium on alternative medicines, vitamins, and natural hepatotoxins. With the increasing incidence of certain liver tumours, I found sections on the adverse effects of hormones (covering everything from adenosomas, focal nodular hyperplasia, and frank malignancies to vascular abnormalities in the liver) and on environmental toxins very interesting and informative.

I would say that at the price, most people would not want to rush out and buy a copy immediately, I would wholeheartedly recommend that every hospital library invests in a copy for use by gastroenterology trainees, trainees from other disciplines, and even the odd consultant gastroenterologist! The book would be very useful for hospital pharmacists, nurse practitioners, and also for medical students who need to reference 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!

S D Taylor-Robinson

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 on a shelf. It was therefore with some trepidation that I received this ostensibly unwrapping 575 page bound and green covered book, and turning the pages quickly, the casual reader might still be deceived into casting it into the nether regions of a desk drawer or the dinner corners of an office. However, I am pleased to say that these were all poorly grounded misapprehensions, because this is an expertly written and very detailed overview of the whole galaxy of experts, covering the mechanisms of drug induced liver injury in an easily readable format (who cannot stand up and say that
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 15 140; fax: +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

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, 39–41 Lincoln's Inn Fields, London WC2A 3PE. Tel: +44 (0)20 7973 0307; fax: +44 (0)20 7430 9235; email: acgbi@asgbi.org.uk; website: www.acgbi.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 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 46 173 241; 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

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