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.

Importance of ongoing haematological monitoring in IBD patients receiving this drug. 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 the early detection and prevention of azathioprine related toxicity is well recognised. However, the duration of early monitoring is a matter of controversy. The current British National Formulary guidelines recommend that patients undergo blood tests on at least a weekly basis for the initial four weeks of therapy. The drug’s manufacturers recommend a more stringent monitoring policy and in their view, initial monitoring should continue for the first eight weeks of treatment. 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 early toxicity (table 1). Mean azathioprine dose in those showing drug toxicity was 100 mg/day (50–150). Most (77%) drug related toxic events manifested within 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. Three further episodes 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 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.

Table 1

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Ulcerative colitis</th>
<th>Crohn’s disease</th>
<th>Indeterminate</th>
<th>Total</th>
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<tr>
<td>Male</td>
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<td>3</td>
<td>45</td>
</tr>
<tr>
<td>Female</td>
<td>15</td>
<td>48</td>
<td>2</td>
<td>65</td>
</tr>
<tr>
<td>Nausea/vomiting</td>
<td>1</td>
<td>5</td>
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<td>6</td>
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<td>Abnormal LFTs</td>
<td>3</td>
<td>1</td>
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<tr>
<td>Neutropenia</td>
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<td>Thrombocytopenia</td>
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<td>Miscellaneous associated symptoms</td>
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<td>0</td>
<td>1</td>
<td>3</td>
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</tbody>
</table>

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

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


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 duration of early monitoring is a matter of controversy. The matter for debate is the time of onset of potentially life threatening myelosuppression.
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 marrow toxicity in the TPMT deficient patient indicate that bone marrow toxicity is recorded after 3–10 weeks (median 4) of azathioprine therapy.1,2 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) 1.6×10^9/l, platelets 25×10^9/l) at 10 weeks while another dosed at 1 mg/kg developed myelosuppression (WBC 3.8×10^9/l, platelets 31×10^9/l) within the first 8 weeks of therapy. Those individuals with wild-type alleles (“normal” TPMT activity) can experience myelosuppression in whom TPMT status is unknown, close adherence to the ABPI guidelines is stated in the Association of the British Pharmaceutical Industry (ABPI) medicines compendium,3 and that TPMT heterozygotes with one variant and one wild-type allele, Black and colleagues4 reported leucopenia (leucocyte count 0.9–2.9×10^9/l) within one month of initiating azathioprine (2–3 mg/kg) as second line therapy for chronic rheumatic disease.

Specifically for the patient with Crohn’s disease, Colombel and colleagues5 have reported that TPMT deficient individuals experience leucopenia within 15 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 colleagues6 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. The additional observation that those individuals with wild-type alleles (“normal” TPMT activity) can experience myelosuppression after weeks or years of azathioprine therapy illustrates the multifactorial nature of myelosuppression in this patient group and supports 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,7 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.

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 the only curative or palliative therapy available” for patients with ulcerative colitis. It has not been shown that performing appendectomy in healthy persons at increased risk of developing ulcerative colitis 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, aspirin 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 appendectomy in the absence of an inflamed appendix was not associated with a decreased risk of ulcerative colitis, suggesting that appendicitis rather than appendectomy protects against ulcerative colitis.1 Cosnes et al state that these results may not be correct as they included all patients with previous appendectomy 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 appendectomy reported in the present study, which could have been diluted by 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 appendectomy will protect these persons is lacking.

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Reference


Authors’ reply

Many French surgeons in 1900 did recommend removing preventively all appendices 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 appendectomy patients genetically at high risk of developing ulcerative colitis.

Ter Borg and van Buuren speculate that appendectomy 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 study1 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 overrepresented.

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, 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 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 appendectomy 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 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 appendectomy will protect these persons is lacking.
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 2002;51(suppl V):v12–23).

(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 coloscopic 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 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 longer than 8 years 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 lower colitis based on 26 reported studies in the meta-analyses 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 coloscopical 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 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 difficulty now faced with practice to the highest standards. However, the courts (Early v Newham Health Authority) will consider local guidelines. This will be of particular relevance 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 as preventive medicine in centres is uncertain, such an approach is widespread. The purpose of guidelines is to identify good practice and to provide a uniform approach throughout the country. The alternatives are to abandon surveillance or to offer haphazard and unstructured (and ineffectual) service. The anticipated cost of surveillance for both colitics and Crohn’s patients is estimated to be £9600 per annum.

We believe the evidence will be able to meet 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 it would be firm evidence of a scheme’s efficacy. There are no randomised studies comparing different surveillance protocols or for that matter even a measure of success of a guideline. Nevertheless Forbes 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 colonoscopy undertaken at least 12 years after the onset of symptoms. We feel that finding dysplasia is the very aim of surveillance. Certainly because it is found on the first colonoscopy in a surveillance programme it should not be deemed a failure of surveillance.

As far as the possible cost was concerned, it is possible was obtained before the guidelines were published. A national audit of the surveillance practices of gastroenterologists in the UK 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 and then by the IBD section of the BSG. After this they were posted on the BSG website for six weeks to attract comments from other members of the society. The guidelines then went back to the Clinical Services Committee after amendments were made on the basis of comments from the wide range of consultants. Finally, the guidelines were put to the Executive Committee of the BSG for a further review and signing off. Thus the guidelines have been very carefully evaluated and 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
years 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 additional preventive measure on the ground of
routine practice of some gastroenterologists.

The meta-analysis does show an increasing
cancer risk in the second and third decades of
disease which is not controversial in the least.
The whole point of stratified data was to see if the
cancer incidence did increase by decade 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 inci-
dence stratified by decade and duration of patient follow
up (19 studies). The decade specific incidence rates correspond to a cumu-
lative 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 patients 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 unstratified data were examined the cancer incidence still increased by decade of disease! The unstratified cumulative proba-
bilities 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
supporting 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 col-
tis overcomes the inadequacies of any reliance
from 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 sur-
veillance 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-
ance at all if it is not conducted to the best
standard? The data underlying the assumption
from the unpublished St Mark’s data how
many biopsies were taken per colonoscopy. 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,
they would be doing them a disservice to exclude from
them a surveillance programme.

Left side colitis also carries an intermediate risk
for colorectal cancer and as such our guide-
lines reflect this. Indeed, one of the signato-
ries to the Forbes et al letter has himself ad-
voated a similar approach after discussion with
that patient.

The guidelines were formulated on the best
evidence available at present. Surveillance
was being conducted in an extremely disor-
dered way in the UK; which is not ac-
ceptable in the current climate of clinical
practice. The BSG has properly encouraged
a national approach to cancer surveillance in a
rare disease. However, the principles, which underlie such an approach, are that of
best practice throughout the country. The
law no longer relies on the Bolam principle; rather
we are now expected to practice to the best
standards.1 If we are to offer long term care to
patients with inflammatory bowel disease we
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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Crohn’s disease: ethnic variation in CARD15 genotypes

Crohn’s disease shows significant variability in incidence between different world popula-
tions. For example, the endangered population of the
Inuit of Greenland was shown to have an incidence of
Crohn’s disease of 48 per 100,000 population, as
compared to an incidence of 4.1 per 100,000
population in the Netherlands. Several articles have
recently been written on the possible role of genetic
factors in the development of Crohn’s disease. For
example, two articles have recently been written on
the genetic contribution of CARD15 to the risk of
Crohn’s disease. However, the ethnic differences in
the frequency of CARD15 genotypes in different
populations have not been reported.

The guidelines were formulated on the best
evidence available at present. Surveillance
was being conducted in an extremely disor-
dered way in the UK; which is not ac-
ceptable in the current climate of clinical
practice. The BSG has properly encouraged
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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 a
series of complex but personally satisfying
interventional gastrointestinal endoscopy but
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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 oesophageo-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 allowed to go 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 melena. After the weekend he was admitted to hospital. His blood count showed Hb 9.7, MCV 105, WCC 10.3, and platelets 240. As in case No 1, he was listed for OGD and colonoscopy. At OGD he was found to have a marked antral gastritis and multiple duodenal erosions in a deformed duodenal cap. At colonoscopy, multiple diverticulae were found in the sigmoid. The patient was observed to have a quite normal and well-functioning colonic peristalsis. The patient was referred promptly for surgery. There was little faecal contamination and the patient recovered well after resection of a short length of colon.

In both of these cases the physicians/gastroenterologists appeared to be working to the dictum: bleeding from the gut requires oesophago-gastro-duodenoscopy (OGD) and colonoscopy. Yet in neither case was the need for colonoscopy clearly indicated. So was this Chris Booth right to be concerned about a simplistic approach to the management of possible gastrointestinal pathology, it has its dangers. Analysis of two cases in the past month reminded me of this.

Which 5-ASA?

I read Dr Travis’ therapy update (Gut 2002;51:548–9) with interest; the topic is timely in a year that is about to be challenged by new generic mesalazine brands. I note the choice of time dependent mesalazine (Pentasa) but, if mesalazine is to be relied upon exclusively, some reservations suggest Pentasa may not be the best choice. The recent study by Krusis and colleagues1 in the maintenance of remission in ulcerative colitis (UC) found that with Pentasa 1.5 g/day, the six month remission rate was 56.8% compared with 77.5% with balsalazide 3 g twice daily (p=0.045).

The assertion that the advantages of the azo bond delivery to the distal colon can be matched by simply increasing the dose of pH dependent (Asacol) or time dependent release (Pentasa) has not been borne out by laboratory or clinical studies. Tissue level studies have indicated that double dose mesalazine is delivered to the kidney, not the colon. A large clinical trial of Pentasa in mild to moderate active UC found remission rates of 29% for both 2 g/day and 4 g/day.2 This latter study highlights the lack of efficacy of mesalazine in patients with mild to moderate ulcerative colitis; UC and specifically exclude patients with newly diagnosed or previously untreated UC were randomised to sulphalazine or balsalazide; 34% were intolerant of sulphalazine at the modest dose of 3 g 5 times daily compared with 5% of patients taking 6.75 g daily. The number needed to treat to avoid sulphalazine intolerance at this rate is only 3, and in new patients it seems particularly important to use well tolerated effective treatment first line and avoid the loss of confidence that drug intolerance produces.

It seems a sad reflection on the pharmaceutical industry sponsored research that the most recent trial on UC treatment with 5-ASA quoted in the therapy update was from 1998. Large clinical trials of one 5-ASA brand against another are expensive and the advent of generic mesalazine preparations is unlikely to improve this situation. My interpretation of recent published evidence is that the mesalazine release mechanism is important for the efficacy, reliability of delivery, and safety of the oral preparations and that balsalazide is now the standard for other agents to be judged against.

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References

1 Krusis S, Schreiber T, Theuer D, et al. Low dose balsalazide (1.5 g twice daily) and mesalazine (0.5 g three times daily) maintained remission of ulcerative colitis but high dose balsalazide (3 g twice daily) was superior in preventing relapses. Gut 2001;49:533–9.


5 Levine DS, Riff DS, Pratt R, et al. Randomized double blind dose response comparison of balsalazide 67.5 g, balsalazide 2.25 g and mesalazine 2.4 g in the treatment of active mild-to-moderate ulcerative colitis. Am J Gastroenterol 2002;97:1398–403.


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 portal systemic 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% (20/230) 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 linked to 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, 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. 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. Additionally, 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 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 rebleeding. This is consistent with previous studies from others. The section on gall stones, for example, was very useful. In some areas, such as gall stone management, there are still problems in interpreting some of the evidence for use in the pragmatic clinical setting. The topic is covered from investigational studies 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 still problems in interpreting some of the information for use in the pragmatic clinical setting. The section on gall stones, for example, while full with 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 outstripes evidence but it would have been useful to have had

References


Authors’ reply

We agree that the presence of gastrorenal shunts (GRS) 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 TIPS for any cause, and identified shunts in 18.3%. A wider portographic review and a uniform approach to treatment.

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References


ABC of Liver, Pancreas and Gall Bladder


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 investigational studies 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.
available evidence about different management approaches.

The book also has a feel of having been constructed 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 questions 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 should a primary care practitioner dealing 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 healthcare. 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 publishers is to make the desire to provide an overview and to enable the clinician to keep abreast of advances in the common and the rarer conditions. It succeeds in this.

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 due date and is 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 Botling, 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 Botling set the pace with an authoritative introduction, excellently outlining the history, biochemistry, and physiology of COX-2 and its 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 of rofecoxib and celecoxib along with discussions on the various test systems to assess selectivity. The excellent chapters on the renal effects of COX-2, the role of COX-2 in inflammation in experimental models kept me awake for the whole night. This is a true master at work!

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 deficient animals. Indeed, four, cardiovascular 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 other’s “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 irritation 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 researchers, and PhD 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.

I Bjarnason

**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 unwieldy 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 readable book. The authors, a 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 succeeds in this 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 important, when many patients and doctors are seen to be coming to the cockpit with H. pylori and anticoagulants (covering everything from adenomas, focal nodular hyperplasia, and frank malignancies to vascular abnormalities in the liver) and on environmental toxins very interesting and informative.

It would be easy to 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 gastroenterologist 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 memoir for those that need it and it won’t gather dust for long!

S D Taylor-Robinson

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 Roscoff, France. Further information: Bertrand Tavitian, IN- SERM 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 15 140; 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 Beuarez, 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, 33–43 Lincoln’s Inn Fields, London WC2A 3PE. Tel: +44 (0)20 7973 0307; fax: +44 (0)20 7430 9235; email: acgbi@acgbi.org.uk; website: www.acgbi.org.uk

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 46 152 364; 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 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 (0)2 648 07 60; fax: +32 (0)2 647 86 94; email: straeb.asnb@proximedia.be; website: www.straeb-asnb.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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