Susceptibility to primary sclerosing cholangitis in Brazil is associated with HLA-DRB1*13 but not with tumour necrosis factor \( \alpha \) −308 promoter polymorphism

Susceptibility to primary sclerosing cholangitis (PSC) is linked to HLA-A1-B8-DRB1*0301-DQB1*0602 and HLA-DRB1*1301-DQB1*0603 haplotypes in different populations of Northern European origin and also to HLA-DRB1*1501-DQB1*0602 in the UK. Mitchell et al have reported an association between tumour necrosis factor alpha promoter gene (TNFA) polymorphism at position −308 and PSC (Gut 2001;49:288–94). In this respect, increased distribution of the TNF*2 allele, in strong linkage disequilibrium with the HLA-A1/B8/DRB1*0301 haplotype, was observed in PSC patients from Norway but not from the UK. However, analysis of the combined data confirmed a significant association of TNFA*2 with PSC. This overrepresentation of TNF*2 was seen only in subjects with HLA-A1-B8-DRB1*0301, indicating that the observed association of PSC with TNFA*2 might in fact be secondary to linkage disequilibrium within this haplotype.

Bernal and colleagues have previously reported an increased frequency of TNF*2 in another cohort of British patients with PSC. This association was dependent on the presence of HLA-B8 and DRB3*0101 but not of HLA-DRB1*0301. Based on these results, the authors proposed that the associations with TNFA*2 and HLA-B8 were stronger than those observed with HLA-DRB1 and DRB3.

We have investigated the frequencies of HLA-B, DRB1, DQB1, and TNFA alleles in 63 Brazilian patients with PSC and 83 healthy controls from the metropolitan area of São Paulo, Brazil, using polymerase chain reaction based techniques, as previously described.11 This population is of highly admixed origin with different percentages of Caucasian, African, and Amerindian ancestries. The diagnosis of PSC was based on the findings of typical clinical, laboratory, cholangiographic, and histological features. None of the patients had evidence of concurrent hepatitis B or C or hepatic schistosomiasis. Twenty seven patients (18 males; mean age 15 (±7) years) were less than 16 years at disease onset and were considered children, and 36 subjects were adults (25 males, mean age 34 (±11) years). Forty one patients had inflammatory bowel disease (IBD). None of the subjects, including all children, had any evidence of laboratory or histological features of overlapping syndromes of PSC and autoimmune hepatitis (AIH).

No increase in the frequency of HLA-B, DRB3, DQB1, or DRB5 alleles was observed in PSC patients compared with healthy controls. Likewise, the distribution of TNFA alleles was similar in patients and controls. The frequency of HLA-DRB1*1301 (52% v 20% of controls; \( p=0.0009, \ RR=4.3 \)) and HLA-DQB1*06 (59% v 41% of controls; \( p=0.04, \ RR=2.1 \)) was significantly increased in PSC patients (table 1). However, one third of HLA-DRB1*13 positive carriers carried other HLA-DQB1 alleles (data not shown). This overrepresentation of HLA-DRB1*13 was seen in both paediatric (44% v 20% of controls; \( p=0.02, \ RR=5.1 \)) and adult patients (58% v 20% of controls; \( p=0.0009, \ RR=5.4 \)). However, this association was seen only in patients with IBD (61% of patients with IBD v 20% of controls (\( p=0.0001, \ RR=6.1 \)) and 36% of patients without IBD v 20% of controls (NS)).

In summary, our data indicate that predisposition to PSC in Brazil is primarily linked to HLA-DRB1*13 and suggest that the association with TNFA*2 previously observed in Norwegian and British patients with PSC could be due to linkage with HLA-DRB1*0301. The association of HLA-DRB1*13 with PSC was observed in both children and adults with the disease but was restricted to patients with concurrent IBD, as previously described by Donaldson and colleagues.13 Interestingly, AIIH type 1 was also associated with HLA-DRB1*13 but not with the TNFα*2 allele in Brazil.14 Of note, shared HLA antigens have also been associated with AIIH type 1 and PSC in other populations.15 These findings suggest that the same HLA-DQB1 alleles confer susceptibility to distinct autoimmune diseases of the liver such as AIIH type 1 and PSC and point to the presence of similar immune mechanisms leading to different clinical outcomes.

Table 1

<table>
<thead>
<tr>
<th>HLA-DRB1 Allele</th>
<th>PSC patients (n=63)</th>
<th>Healthy controls (n=83)</th>
<th>p Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>DRB1*03</td>
<td>12 (19)</td>
<td>23 (28)</td>
<td></td>
</tr>
<tr>
<td>DRB1*13</td>
<td>33 (52)</td>
<td>17 (20)</td>
<td>0.00009</td>
</tr>
<tr>
<td>DRB3</td>
<td>53 (84)</td>
<td>62 (75)</td>
<td></td>
</tr>
<tr>
<td>DQB1<em>02</em></td>
<td>20 (3%6)</td>
<td>41 (49)</td>
<td></td>
</tr>
<tr>
<td>DQB1<em>06</em></td>
<td>33 (5%9)</td>
<td>34 (41)</td>
<td>0.04</td>
</tr>
<tr>
<td>TNFA1/1/TNFα1</td>
<td>41 (65)</td>
<td>63 (76)</td>
<td></td>
</tr>
<tr>
<td>TNFA1/TNFα2</td>
<td>21 (33)</td>
<td>19 (23)</td>
<td></td>
</tr>
<tr>
<td>TNFA2/TNFα2</td>
<td>1 (2)</td>
<td>1 (1)</td>
<td></td>
</tr>
<tr>
<td>TNFA2 allele carriage</td>
<td>22 (27)</td>
<td>20 (25)</td>
<td></td>
</tr>
</tbody>
</table>

*Only 56 patients with PSC were typed for HLA-DQB1.

Numbers in parentheses are percentages.

References


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Slow transit constipation: more than one disease?

Emmanuel and Kamm reported on the response of behavioural treatment, biofeedback, in constipated patients (Gut 2001;49:214–19). Biofeedback is an established therapy for outlet obstruction due to paradoxical anal sphincter contraction. Beyond that, Emmanuel and Kamm demonstrated that slow transit constipation (STC) can also be improved by biofeedback with normalisation of the slow transit in most symptomatic responders. These results contrast with the common belief of STC as a manifestation of a panenteric disease, predominantly of autonomic nervous system dysfunction.

Disturbances of oesophageal motility, gastric emptying, small bowel transit, and gall bladder motility have been described. Dysmotility of the small intestine has been thoroughly investigated by manometry in STC patients. Disturbed motility—for example, abnormal configuration or disturbed aboral migration of phase III of the migrating motor complex, bursts, and sustained uncoordinated activity—occur in up to 60% of these patients. In our recent study using long term small bowel manometry in 30 clinical STC patients, disturbed aboral migration of phase III was present in 47%, and bursts/sustained uncoordinated activity occurred in 33% of patients, respectively.

It is well established that these manometric findings are markers of a neuropathy of the myenteric plexus and occur in an identical way in patients with chronic intestinal pseudo-obstruction of neuropathic origin. Furthermore, treatment by colectomy has been reported to result in excellent long term outcome in 90% of patients with dysmotility limited to the colon whereas patients with generalised intestinal dysmotility experience a surgical outcome in only 13%. It is hard to understand how these manifestations of neuropathy, especially in the myenteric plexus of the small intestine, can be successfully treated by biofeedback therapy. An alternative explanation is that STC is an inhomogeneous group of different aetiologies. In transit studies, slow transit can be the result of a right sided or global delay (the “classical” finding in idiopathic STC), a left sided delay, or a rectal marker accumulation delay. In physiological evaluation, an overlap of slow transit and outlet obstruction can be seen in some patients. At least in healthy volunteers, voluntary suppression of defecation resulted in a marked prolongation of colonic transit.
Surveillance for hepatocellular carcinoma in liver cirrhosis: have programmes improved because patients have?

In their commentary (Gut 2001;48:149–50), Bruix and Llovet discuss the paper by Bolondi et al (Gut 2001;48:251–9) and emphasise the fact that survival in patients with hepatocellular carcinoma (HCC) is mainly related to tumour stage and degree of liver function impairment at diagnosis. This is most likely true because if the peculiar features of HCC, which almost inevitably arises in the “minefield” of a cirrhotic liver whose residual function is one of the main factors influencing therapeutic options and prognosis.

Nevertheless, a trend towards increased survival after diagnosis of HCC has recently been observed, although the surveillance programme has not changed over the years (liver ultrasonography and α-fetoprotein determination every six months). As Bruix and Llovet affirm, this increase in survival may be due to advances in diagnosis even in the absence of effective treatment, to the availability of multidisciplinary treatment, or both.

However, it must be emphasised that HCC stage (parameter of the tumour) and residual liver function (parameter of the affected patient) are clearly related and influence each other, and that both can influence the choice of treatment and prognosis. Therefore, what should improve survival over the years be attributed to since surveillance programmes are only able to detect a minority of “early” HCCs?

Bruix and Llovet do not specify what the outcome of a cohort of 27 heavy drinkers (23 males, four females) and 12 control patients (five males and seven females) in the early paragraphs of the materials and methods section. Later, the authors discuss “rectal biopsies of 17 alcohol abusers (10 males, seven females) and 14 age matched controls (six males, eight females)” making the reader wondering if this sample is sufficient in size and power to adequately support the conclusions drawn from their regression analysis. Moreover, by not reporting a r or an adjusted r value, the accuracy of the model is not also addressed. Possible correlations between independent variables should be investigated and discussed when reporting multiple regression results to further support the validity of the analysis.

Secondly, clarification of their patient populations is also required. They originally reported a cohort of 27 heavy drinkers (23 males, four females) and 12 control patients (five males and seven females) in the early paragraphs of the materials and methods section. Later, the authors discuss “rectal biopsies of 17 alcohol abusers (10 males, seven females) and 14 age matched controls (six males, eight females)” making the reader wondering if this sample is sufficient in size and power to adequately support the conclusions drawn from their regression analysis. Furthermore, by not reporting a r or an adjusted r value, the accuracy of the model is not also addressed. Possible correlations between independent variables should be investigated and discussed when reporting multiple regression results to further support the validity of the analysis.

In summary, clarification regarding the above mentioned omissions would greatly solidify the conclusions of their research.

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Author’s reply

We appreciate the interest of Dr Filion which gives us the opportunity for additional clarification.

As the effect of alcohol on colonic cell proliferation was found to be significant (p<0.05), no type 2 error with respect to the effect of alcohol has to be considered. In this context it should be noted that in case of statistically significant findings, only type 1 errors may occur. The effect of alcohol on colonic cell regeneration was the primary question which was investigated in the study. As stated in the methods section of the paper, a multiple regression analysis was performed to assess possible confounders due to sex and smoking. Thus the p values reported for sex and smoking should only be interpreted in a descriptive manner.

On the basis of numerous epidemiological studies it is generally accepted that the independent variables alcohol, smoking, and sex do correlate. This is in fact the reason for performing an adjusted analysis on the impact of alcohol on cell regeneration.

In 27 heavy drinkers and 12 controls, statistics on proliferative cell nuclear antigen (PCNA) expression were performed. In a second group of 17 alcoholics and 14 age matched controls, various staining procedures were performed, including Ki67, RB1, p53, and cytokeratins, without statistical analysis.

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References


Rectal proliferation and alcohol abuse

The study by Simanowski et al described some important features of rectal proliferation and alcohol abuse (Gut 2001;49:418–22). However, there are some methodological issues pertaining to the study which need clarification. Firstly, when performing multiple linear regression, it is essential to perform and report sample size and power estimate calculations. This omission, especially with a sample size of only 39 patients, leaves the reader wondering if this sample is sufficient in size and power to adequately support the conclusions drawn from their regression analysis. Furthermore, by not reporting a r or an adjusted r value, the accuracy of the model is not also addressed. Possible correlations between independent variables should be investigated and discussed when reporting multiple regression results to further support the validity of the analysis.

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Molitin agonists and dyspepsia: throwing out the baby with the bath water

I read with great interest the paper by Talley and colleagues (Gut 2001;49:395–401) and the accompanying editorial by Tack and Peeters (Gut 2001;49:317–8). There are many important issues that are raised in the paper and editorial. I believe the paper provides an opportunity to identify areas where study design might be enhanced in future studies.

Firstly, the fact that gastric emptying was not measured at the end of the study leaves wide open the question of whether the prokinetic approach should be abandoned in the treatment of dyspeptic symptoms in diabetics. Thus it would be inappropriate to conclude from this study that prokinetics are not indicated. This point is also emphasised in the editorial by Tack and Peeters.

Secondly, the authors conclude that baseline gastric emptying does not influence the response to ABT-229. This conclusion is based on weak foundations as the method used to measure gastric emptying appears to provide data that are scarcely believable. Thus the 13C-octanoic acid breath test for gastric emptying (130±45 (SD?) minutes) is remarkably outside the normal range reported using the gold standard (mean 110±4 (SEM) minutes, 10th percentile 70 minutes, 90th percentile 150 minutes in our laboratory). The methods section does not unequivocally state what mathematical analysis was used with the stable isotope breath test at the central laboratory used in the study. Improved mathematical analyses of gastric emptying using breath tests in the more recent literature provide a higher level of accuracy relative to scintigraphy. It is claimed that the method was validated in 19 diabetics in whom a significant correlation (r=0.73) was observed between scintigraphy and breath test data. Correlation does not equal to accuracy and, in a Bland-Altman or similar analysis, the gastric emptying data are suspect and cannot be used to classify patients to assess the relationship between symptoms and emptying, or to address the role of baseline gastric emptying as a covariate in the response to treatment. It is also unclear if the study was sufficiently powered to appraise an effect of delayed gastric emptying on response to therapy, given the fact that only 28% of the study cohort were classified as having delayed gastric emptying. A type II error cannot be excluded.

Thirdly, the theoretical point is made by Tack and Peeters regarding tachyphylaxis of this particular molitin agonist, previously demonstrated in the study of Verhagen and colleagues.3 However, other prokinetics, including other molitin agonists, may not be as effective in the treatment of dyspepsia in diabetics with impaired gastric emptying.4

Fourthly, the observation that over time some of the symptoms continued to be aggra- vated suggests that the last arm of the study suggests that the drug was still effective and worsened symptoms, rather than simply being ineffective in the patients evaluated.

Fifthly, the study illustrates the importance of thoroughly characterising the pharmacology of a novel agent before embarking on expensive potentially harmful therapeutic trials. Inhibition of accommodation by motilin agonists may indeed be responsible for aggravation of bloating and other symptoms over time. Fortunately, these effects are likely to be reversible and no permanent harm was reported.

However, it is still worth emphasising the general point—clinical pharmacology and pharmacoepidemiological studies have an important role to play in the drug development process. This is especially relevant in the context of “gastroparis” or dyspepsia as there are non-invasive approaches to study gastric emptying, accommodation, and fundic sympto- ms. These methods permit proper dose- response studies prior to exposing patients to potentially harmful agents or inappropriately selecting subgroups of patients for such large and expensive studies. Among patients with diabetes, neuropathy may alter both gastric emptying and gastric accommodation via dif- ferent mechanisms (for example, extrinsic vagal or intrinsic nitrergic neuropathy). Thus selection of those with only impaired emptying (based on a reliable test) and normal accommodation might have provided a fairer opportunity to assess the efficacy of the drug.

Finally, as acknowledged by Talley et al, assessment of autonomic neuropathy requires a more formal assessment than the “opinion of the attending endocrinologist”. In fact, disturbances of the autonomic nervous system, evaluated with detailed tests, have been shown to significantly influence the symptom response to a prokinetic.5 Approaches that carefully characterise the drug before exposure of patients and selecting subgroups of patients after thorough understanding of the effects of the drug may save potentially effective medications from being abandoned. These patients need effective therapies. As one of many physicians who struggle to help relieve these patients’ symptoms, we cannot afford to “throw out the baby with the bath water”.1 I trust that this appeal may encourage pharmaceutical companies to reconsider whether the medication or a derivative with improved pharmacokinetics should be given a “second chance.”

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References

Authors’ reply
A number of the issues raised by Dr Camilleri are important and relevant although some of the points require clarification. We stand by our position that drugs which act solely as gastric prokinetics are unlikely to be beneficial in either diabetic gastropathy or functional dyspepsia. Our data (both here and elsewhere)’ suggested that the molitin agonist tested actually worsened symptoms in both diabetics and non-diabetics with unexplained dyspepsia, regardless of baseline gastric emptying status. Other recent data suggest that molitin agonists impair fundic accommodation and this physiological disturbance may induce symptoms in a subset with dyspepsia.3 Cisapride also relaxes the fundus and enhances its therapeutic benefits in dyspepsia.4 Our observations have important implications for future drug development; we agree with Dr Camilleri that ideally the mechanisms of drug action need to be understood prior to planning clinical trials, although this is often completely impractical and could impart progress at times. It is also fair to point out that data on fundic accommodation have only become available relatively recently and preceded the planning of the trials.

While we agree that there are limitations with C13-octanoic acid breath testing, we believe that the data are sufficiently robust to support the conclusion that there was no significant effect of ABT-229. Indeed, we applied a number of cut offs for delayed gastric emptying versus normal but were unable to identify any influence of baseline gastric emptying on the response of the molitin agonist tested.1

Dr Camilleri has emphasised the fact that gastric emptying was not measured at the end of the study. There has been a reluctance on the part of the pharmaceutical industry to re-measure gastric emptying in clinical trials because of the recognised lack of correlation of changes in gastric emptying with symptom improvement.5 Furthermore, there is an absence of reliable standardised reference methods for gastric emptying that can be applied in multicentre trials. However, we agree that it is optimal in prokinetic trials to test gastric emptying at baseline and on drug, and this should be the “gold standard.”

The issue of tachyphylaxis is important. We conclude, based on the available evidence, that tachyphylaxis was unlikely but agree that the issue needs to be carefully considered in all studies evaluating prokinetics. Indeed, in our studies, as Dr Camilleri points out, the drug was actually deleterious (this study and Talley and colleagues).5 This strongly suggests that tachyphylaxis did not occur and did not explain the negative results with ABT-229.

We stand by the study design used although further improvements are feasible. Phase I data were available indicating that there were unlikely to be any significant serious effects of ABT-229 and therefore we dismiss the concern raised about potential harm; this was borne out in the phase II trials (present study and Talley and colleagues). However, we agree that this may not apply to other novel pharmacological agents in development for diabetic gastropathy and functional dyspepsia. We conclude that the molitin agonist class is likely to be disappointing in unexplained

Reducing dyspepsia costs in the community

Valori and colleagues (Gut 2001;49:495–501) assessed the effectiveness of an educational programme to reduce dyspepsia costs in the community.

Given one of the hypotheses was that quality of care would be improved because of “a more active stepdown approach for reflux symptoms and a switch from ranitidine to generic cimetidine” an analysis of changes in the type and volume of specific drugs would appear warranted to support the authors conclusions. It would also provide much needed data on the effectiveness of the “stepdown” approach recommended for the management of gastro-oesophageal reflux disease. The authors also report a subsequent fall in admissions to the gastrointestinal bleed unit in West Gloucestershire. Data are needed to assess whether this is due to their intervention or to natural variation. Of particular interest is the proportion of admissions for Helicobacter pylori related peptic ulcer bleeds in west compared with east Gloucestershire. The high prevalence of non-definitively treated H pylori associated peptic ulcer disease in primary care has been demonstrated in a number of studies and remains a difficult management issue. In Australia in 1999 only 1.3% of all antiulcerant prescriptions were for H pylori eradication therapy. Analysis of the volume of prescriptions for eradication therapies in each region during the study period would allow assessment of the impact of their strategy on the prevalence of H pylori associated peptic ulcer disease.

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References


Author's reply

Details of individual drug usage were not available for the entire study period and therefore it was not possible to analyse changes for particular drugs.

The purpose of this study was to provide the gastrointestinal bleed unit and other data was to give an indication of whether the intervention might have adverse effects on other health outcomes related to dyspepsia. We were particularly concerned that any intervention might increase demand for endoscopy or increase morbidity from peptic ulcer complications. We acknowledge that during the study period it is possible that there may have been a natural decline in referral for endoscopy and gastrointestinal bleeding. Thus without a control group for these outcomes it is possible that the stable levels demonstrated in the study represent a real increase. However we believe that this is exceedingly unlikely given the continued strong demand for endoscopy elsewhere and the steady rise in emergency medical admissions in the UK. We do not have sufficiently accurate data to make comment on whether the intervention reduced Helicobacter pylori related peptic ulcer bleeds.

It was not possible in this study to identify individual H pylori prescriptions. A more relevant outcome might have been the number of patients who, following eradication therapy (for whatever reason), no longer needed long term acid suppressing medication. Feedback from general practitioners suggests that there were many patients who responded in this way. Unfortunately, we have no hard data to support the anecdotal reports of the impact of H pylori eradication on drug costs.

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Causes of obvious jaundice in South West Wales

We read with interest the article describing the causes of obvious jaundice (serum bilirubin >120 µmol/l) in South West Wales (Gut 2001;49:499–13). The authors make the point that contrary to the perception of many doctors, viral hepatitis is an unusual cause of jaundice (two of 121 cases) while sepsis/shock is a relatively common cause (27 of 121 cases). We have performed a retrospective assessment of 100 cases of jaundice identified on biochemical testing who had presented to the Accident and Emergency Department or had been admitted to the acute medical or surgical admission wards at Stobhill Hospital, Glasgow. Our survey therefore looked at acute admissions with jaundice while that of Whitehead et al also included established inpatients who developed jaundice (22 of 117 inpatient cases). We drew a lower cut off level of serum bilirubin (>60 µmol/l) as above this level jaundice should be clinically detectable.

The causes of jaundice we identified differed significantly from those of Whitehead et al (fig 1). The predominant cause in our series was alcoholic liver disease (ALD) which may reflect the catchment area of our hospital. Only two patients presenting with jaundice had a diagnosis of “shock/sepsis”. It should be noted that 20 of the 27 patients with “shock/ sepsis” in the South West Wales series developed jaundice as inpatients. Rather than suggest “shock/sepsis” as a common reason for jaundice which is often overlooked, it might have been more accurate to note that jaundice due to shock/sepsis occurs in a particular clinical setting such as an intensive care unit, postoperatively, or in patients with multiple medical problems. In this context we doubt the aetiology of the jaundice is “overlooked”. Our own study clearly indicates that shock/sepsis is indeed an unusual reason for patients to present to medical care with jaundice.

The authors also noted that 16 of 61 patients with common bile duct (CBD) stones had a bilirubin level greater than 120 µmol/l, and comment that such high levels of bilirubin are more likely to be related to malignant obstruction. In contrast with this, our series demonstrates that patients with CBD stones had bilirubin values greater than 120 µmol/l. There was no difference in mean bilirubin values between patients with CBD stones and those with malignant disease (120 (±15) v 168 (±28) µmol/l), nor indeed with those with ALD (142 (±18)).

We also have a concern about the accuracy of diagnosis on a retrospective review of the causes of liver disease, particularly since our enterologists managed only one third of patients in the Welsh study. In our series we noted that few patients had a “complete” serological screen for liver disease. It is therefore possible that patients might have been inadequately investigated and so were placed in an inappropriate diagnostic group.

The authors also highlight the value of the aspartate aminotransferase/bilirubin ratio in the assessment of jaundice. A further analysis of our own data does not substantiate the use of this value in diagnosis. Mean values for ALD, gall stone related jaundice, and malignancy were 3.5, 3.8, and 2.7 respectively (NS).

In conclusion, we believe that the perception of most clinicians that shock/sepsis is an unusual cause for patients to present with jaundice to medical care is an accurate one. Shock/sepsis related jaundice is much more likely to develop among inpatients with complex disease. We do agree that viral hepatitis is an unusual cause for jaundice, although investigation of viral disease is still an important aspect of the assessment of such patients. We also agree that jaundice is associated with alcoholism.
a significant inpatient death rate (32% in Whitehead's series and 19% in our own).

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Authors' reply

We thank Drs E and J Forrest for the interest they have shown in our article on jaundice and we were pleased to learn of their retrospective assessment of 100 cases of jaundice presenting to acute services in a large Glasgow hospital. Although they emphasised the differences between their experience and ours, this is the nature of medical correspondence and we were more struck by the similarities which we found gratifying. The series cannot be compared too closely because the differences between their experience and ours—this is the nature of medical correspondence and we were more struck by the similarities which we found gratifying.

The commonest cause of jaundice, and use a cut off bilirubin value of >60 µmol/l. We will respond to their comments seriatim.

(1) The commonest cause of presentation with jaundice to Stollhill Hospital was alcoholic liver disease. In Swansea, if analysis is restricted to those 95 patients presenting to hospital with jaundice, then alcoholic cirrhosis ran a very close second to malignancy as the commonest cause.

(2) As Forrest and Forrest point out, sepsis is a common cause of jaundice requiring admission to hospital either in Glasgow or Swansea, but in our experience was the predominant cause of jaundice developing while in hospital for other reasons. As to whether it is overlooked, our results speak for themselves—indeed, we would consider these to be similar rather than dissimilar proportions. The absolute values of bilirubin from the two centres cannot be compared without knowledge of the timing of samples. Clearly, samples taken on admission might show lower bilirubin levels than samples taken later on, particularly with malignant biliary obstruction awaiting mechanical relief. Our experience is that gall stone biliary obstruction was often transient and not profound whereas malignant obstruction led to ever increasing levels of bilirubin unless there was mechanical intervention.

(4) We share Forrest and Forrest’s concern about the accuracy of diagnosis on retrospective case note review but respectfully point out that our study was prospective while theirs was retrospective. We accept that not every patient in the Swansea series had every investigation but we cannot consider it good practice to perform tests unless clinically indicated. Thus most patients with proven obstructive jaundice did not have serological tests—most patients with intrinsic hepatocyte dysfunction did.

(5) Our observations on separate amino-transferase (AST) bilirubin ratios were of interest alone. We did not propose that this should be used as a test but simply commented that the ratio had some diagnostic value. Our only comment on the Glasgow figures relates to their patients with alcoholic liver disease where the ratio was reported to be 3.5. Mean bilirubin level for this group was 142 µmol/l which translates to a mean AST value of approximately 500 IU/l. This is an exceptionally high figure for AST in alcoholic liver disease where AST is characteristically much lower, usually <200 IU/l.

(6) Causes of jaundice and causes of jaundice requiring hospital admission are not the same and clinicians should guard against using the experience of one clinical setting when assessing another.

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Behaviour of Crohn’s disease according to the Vienna classification

I hasten to congratulate Louis et al on their meticulous and insightful study on the stability of Crohn’s disease phenotypes according to the Vienna classification (J Crohns Colitis 2001;49:777-82). It was particularly gratifying to learn from them (in a separate communication) of the remarkably high degree of interobserver agreement in classifying patients by this system.

The principal message that the authors draw from their study is that the initial “behavioural” classification of B1 (non-stricturing non-penetrating) at the onset of Crohn’s disease remains stable over the lifetimes of the patient but almost invariably progresses in time to either B2 (stricturing) or B3 (penetrating) disease. Naturally, this finding hardly comes as a surprise either to the authors of the Vienna classification or in fact to any clinician caring for patients with Crohn’s disease. More important and revealing, in my opinion, is the observation by Louis et al that “the proportion of initially B2 patients changing from B2 to B3 was [only] 15.4% (only 2/13 patients)”. Therefore, once “inflammatory” (B1) disease has made its almost invariable progresses to either B2 or B3, why should we not be able to incorporate this relatively stable “choice” of pathway into a phenotyping system suitable for genotypic correlations?

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References


Authors’ reply

We thank Professor Sachar for his kind comments on our work. As it has become obvious that Crohn’s disease is a multifactorial polygenic heterogeneous entity, apart from molecular genetic studies a major task is now to identify stable phenotypes of Crohn’s disease that may correspond to particular genetic backgrounds. The propensity of Crohn’s disease to develop as a penetrating disease (Crohn’s disease behaviour) has been considered for some time as a potential suitable phenotype for genetic correlations. However, results have been inconclusive. Several explanations are plausible: (a) there is no major genetic influence on Crohn’s disease behaviour and the significant concordance within multiply affected families is explained by environmental factors; (b) the genes involved have not yet been tested and it is true that only a small number of candidate genes have been tested in this setting; and (c) patients with Crohn’s disease have not been classified adequately into subphenotypes, and it is true that several classifications have been proposed and that the application of these various classifications does not result in homogeneous categories.

In relation to the first two hypotheses, progress in the understanding of the biology and behaviour of stricturing and fistulising disease, as well as of the influence of environmental factors, including smoking and medical treatment of the disease, is needed. Regarding the third point, the classification used necessarily must result in stable categories of patients if we have shown, even the most recent and reproducible classification is not suitable as patients change categories over time. As emphasised by Sachar, it seems from our data that patients who are classified as a stricturing (B1) tend to remain B2 over time. This is mainly true for patients who are already B2 at diagnosis as 88% remained B2 over a median follow-up of seven years whereas, it seems as if patients who develop penetrating lesions (B3) associated with stricturing lesions tend to develop these simultaneously and thus are directly classified as B3 while patients who develop clinically significant stricture disease without concurrent penetrating lesions do not tend to develop such lesions afterwards. Furthermore, in our population, only a few pure stricturing lesions (B2) developed after 10 years of evolution. Therefore, in our experience, patients who develop a pure stricture disease over 10 years of evolution seem to represent a homogeneous phenotype that may be suitable for studies of genetic factors potentially involved in stricture development. However, this does not seem to be the case for penetrating disease (B1). In our patients, penetrating phenotypes continued to develop at a constant rate (approximately 25% of patients/five years), even after 20 years of evolution, mainly directly from the non-penetrating non-stricturing phenotype (B1). Therefore, the subgroup of patients with non-penetrating non-stricturing disease can never be considered as homogeneous as even after 25 years some may evolve to the penetrating phenotype (B3). Furthermore, a patient who develops penetrating lesions with non-stricturing disease of evolution may be biologically and genetically very different from a patient who develops such lesions after 25 years. To some extent this point can also be applied to the stricturing phenotype (B2).

An alternative would be to take into account the speed of development of the B2 or B3 phenotype. Indeed, the inclination to develop such a phenotype is most probably multifactorial. We would be surprised if a
unique gene were responsible for stricture development for example. Therefore, if a gene is involved it may be rather by facilitating or by speeding up the development of these phenotypes, together with other genes and environmental factors. In this hypothesis we may have more chance to disclose predisposing genes when comparing patients who have rapidly developed stricture or penetrating phenotypes (within five years for example) with other patients. We believe that when performing genotype-phenotype correlations for Crohn’s disease behaviour, several classification options have to be tested according to these various hypotheses of gene implication. Furthermore, we should aim towards disclosing environmental factors and stratifying patients according to these factors or to consider these factors in multivariate analyses.

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References

Pediatric Gastroenterology and Nutrition in Clinical Practice


“Of the making of many books there is no end and much study is a weariness of the flesh.” We spend too much time reading—or rather we are expected to take in vast volumes of information from text. Not just the written word in books but from journals and more and more directly from the screen. Few of us have time to sit down to read systematically, and most of us scan contents pages, chapter titles, and abstracts. We take in “new knowledge” more by accident than design, and all forms of the written word compete with each other.

Books have a historical advantage over what we still regard as more ephemeral sources of information—journals and the Internet. Books are portable and we like to turn to it for help. I was pleasantly surprised when the answer to my conundrum was available within minutes. A little while later I was approached by one of my SHOs with a question on the genetics of juvenile polyposis. After a short consultation of the book, I was able to give the answer confidently; no need for Internet searches this time.

This book is the first to my knowledge that deals solely with gastrointestinal polyps. It covers all regions of the gastrointestinal tract and is abundantly illustrated with endoscopic photographs and colour photomicrographs. For each type of polyp, descriptions of prevalence, endoscopic appearances, and pathological features are given, followed by discussion of biological behaviour and associated conditions. For some types of polyp, details of management strategies are also provided.

All of the authors are well known gastrointestinal pathologists with a wealth of experience in this field, so it is not surprising that they have managed to put together such a comprehensive text. I could not think of any entities they had omitted, and there were several that I had never heard of. Overall, the presentation of this book is of a high quality; the text is succinct but readable and, apart from a few exceptions, the illustrations are excellent.

This is primarily a diagnostic book and if it does have a defect it is in the descriptions of molecular biology and therapeutic approaches, which inevitably lack detail that some purists would desire. This aside, the book will undoubtedly appeal to histopathologists and endoscopists alike, not only for the diagnostic details it provides, but also for the associated clinical-pathological information. I have found it an ideal companion and am sure that others will think the same.

P Domizio

Gastrointestinal Polyps


I suspect that to the vast majority of gastrointestinal histopathologists, and probably to general histopathologists and endoscopists too, the idea of a book devoted solely to gastrointestinal polyps is appealing. After all, most endoscopists see such lesions every day and most pathologists will see at least one a week. Often a verdict of “hyperplastic” or “inflammatory” polyp is the best that can be offered but this diagnosis is not very satisfying for pathologist and clinician alike. Consequently, it was with eager anticipation and in the hope of transforming my approach to gastrointestinal polyposis that I started to read this book.

As luck would have it, the slides for the EOA in gastrointestinal pathology had landed on my desk the previous day. They included at least two difficult polyloid lesions for which a diagnosis was currently eluding me. I thought that this book would be of use as a reference and turned to it for help. I was pleasantly surprised when the answer to my conundrum was available within minutes. A little while later I was approached by one of my SHOs with a question on the genetics of juvenile polyps.
copious diagrams and tables. The use of a scalpel icon to highlight text and references associated with reasonable evidence based practice is a particularly good idea.

This is a welcome addition to what has become established, in a very short space of time, as an essential read. It will continue to appeal to surgical trainees and consultants alike, but will also be of interest to medical, radiological, and pathology colleagues who wish to have a broader understanding of their own area of expertise. I unreservedly recommend it.

M Winslet

Challenges in Inflammatory Bowel Disease


This latest entry into the inflammatory bowel disease textbook sweepstakes is intended neither as a comprehensive reference work nor as a guide to everyday management. This demurral is just as well. After all, the former category of texts is already well represented by such heavyweights as Kirsner (WB Saunders), Allain et al (Churchill Livingstone), or Targan and Shanahan (Williams and Wilkins); the latter arena is quite thoroughly covered in works by Gitnick (Igaku-Shoin) and even more notably by Bayless and Hanauer (BC Decker).

The current volume, rather, adopts a self described “new approach.” It focuses on specific questions ranging from basic science to clinical management, and it seeks to adduce best evidence in addressing controversies in these fields. In taking this particular tack, the editors and publishers have succeeded admirably in at least three respects.

Firstly, they have assembled an all-star cast of contributors. The editors, Derek Jewell, Bryan Fareen, and Neil Mortensen—the worlds world class troika of clinician researcher, pathologist, and surgeon—have recruited 38 renowned authors from top centres worldwide besides their own.

Secondly, they have constructed this monograph ingeniously. Each chapter title is graph ingeniously. Each chapter title is designed critically with scores of references that

The six chapters on epidemiology and research methods are particularly thorough. If the evidence”. The authors analyse and interpret the evidence in ways that allow each chapter to reach reasonably well founded conclusions.

The six chapters on epidemiology and genetics are pertinent and up to date (at least through 1999). Six chapters address epidemiology of low-grade dysplasia appears advis-

The current one chapter section on prognosis by Karel Gobe's nicely illustrated chapter on therapy implications. The chapter on genetics versus environment explores the potential mechanisms of functional interaction between genes and environment.

In the section on diagnosis and assessment, two pathologists take wonderful advantage of the book's format by posing and discussing 18 “controversies in histopathologic diagnosis”, while a second chapter on “new diagnostic tools” deals with advanced imaging techni-

8. Harter pointedly reminds us not to forget the often overlooked consideration of patient preferences!

The section on cancer surveillance opens with Karel Gobe's nicely illustrated chapter on therapy implications. The chapter on genetics versus environment explores the potential mechanisms of functional interaction between genes and environment.

This is a welcome addition to what has become established, in a very short space of time, as an essential read. It will continue to appeal to surgical trainees and consultants alike, but will also be of interest to medical, radiological, and pathology colleagues who wish to have a broader understanding of their own area of expertise. I unreservedly recommend it.

Due to an error in the production process, the Therapy Update in the August issue of the Journal (Gut 2002;51:182–3) was missing references 14 to 24. The references are printed below, and Gut apologises for the omission.

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