Distinction between “high grade MALT” and diffuse large B cell lymphoma

In order to communicate data about various lymphomas, it is necessary that we all speak the same language. To this end, many lymphomas, it is necessary that we all speak the same language. To this end, many classification schemes have been devised over the years. The most recent and accepted, they risk misinterpretation or that their data may be overlooked. Moreover, the REAL classification explicitly states that marginal zone lymphomas should not be graded. Large cell lesions such as those depicted by Nakamura et al in their online response should be referred to as “diffuse large B cell lymphoma.” In that online response, the authors state that “our cases with high grade MALT lymphoma were categorised into diffuse large B cell lymphoma plus areas of marginal zone/MALT lymphoma”. Again, this is confusing because the REAL/WHO classification terms “marginal zone lymphoma” and “diffuse large B cell lymphoma” were not used in the paper. In closing, one important point should be reiterated: in order for readers to derive meaningful information from lymphoma studies, those studies must use widely accepted lymphoma classification terminology. In studies where deviation from such terminology is necessary, the materials and methods must explain the classification scheme precisely and explicitly.

Are Nakamura et al saying that gastric diffuse large B cell lymphoma can be cured by anti-Helicobacter pylori therapy?

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References

Authors’ reply

In our paper (Gut 2001;48:454–60), the recently proposed WHO classification of lymphoid neoplasms1 was not applied as our study was conducted during 1994–1998, and our manuscript was submitted in 1999. As we have previously responded to Dr Ely on the Gut website, the five cases with high grade MALT (MALT) lymphoma in our study were categorised as diffuse large B cell lymphoma plus areas of marginal zone/MALT-type lymphoma, according to the WHO classification. The percentage of neoplastic large cells in these five cases was as follows: 30% and 40% each in two cases which regressed after eradication of Helicobacter pylori and 30%, 70%, and 90% each in three cases which did not respond to eradication therapy. It took us a long time to realize that the reactive germinal centres were not overestimated.

To date, more than 20 cases of gastric diffuse large B cell lymphoma with or without areas of low grade MALT lymphoma have been reported to have regressed after H pylori eradication.2,3 Based on these observations, we consider that high grade MALT lymphoma/diffuse large B cell lymphoma with areas of marginal zone lymphoma in the early stage possibly responds to H pylori eradication. To determine whether or not patients with a response to H pylori eradication relapse in the future, a longer follow up study in a large number of patients would be necessary.

In addition, recent publications have shown that gastric diffuse large B cell lymphoma with areas of marginal zone lymphoma (high grade MALT lymphoma in our classification) had a better survival compared with that without evidence of MALT lymphoma.4,5 Many investigators still use the term “high grade MALT lymphoma” and2,3,6,11,12,16,19,20 whichever term is accepted widely in the future, we believe that gastric diffuse large B cell lymphoma with areas of marginal zone lymphoma should be distinguished from that without MALT lymphoma.

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References
Mortality with oesophageal varices: different things to different people

LeBrec (Gut 1997;42:127-4) claimed that mortality with oesophageal varices has significantly decreased in the past few decades. Because of the limited follow up in some of these publications, we have examined the published mortality from bleeding varices where follow up data were available for at least one year (group BV). For comparison, similar data were collected from studies in patients with known varices who had not bled (group NBV).

For inclusion, patients had to be reported in a randomised controlled trial, published in full between 1984 and 2001. Management after the protocols had to be initiated promptly and survival data had to be recorded for at least one year. The few patients reported as transplanted were counted as survivors. In most instances there was no statistical difference in survival between treatment arms and the data were combined. Where this was not the case (three in the BV group and four in the NBV group) only data from the treatment arm showing better survival were included in the analysis.

Thirteen publications in the BV group representing 1321 patients of mean age 53.4 (SEM 1.13) years met the inclusion criteria. The NBV group comprised 2472 patients of mean age 54.9 (0.90) years. Mortalities are shown in table 1.

There was no statistically significant difference in mortality between the BV and NBV groups for the two years for which comparisons were possible. There was no correlation between mortality and the number of patients included in each report or the date of publication.

These data show that patients with oesophageal varices, although in their mid-eighties, have a life expectancy of normal individuals in their mid-eighties. The presence of varices, whether they have bled or not, is an omenous prognostic sign regardless of how they are treated.

There are limitations to this analysis. The prevalence of varices is not known. The fate of patients who die before reaching a treating facility or where the outcome is unreported is also unknown. Yet, as pointed out by Graham and Smith,15 the timing of randomisation for any treatment programme has a major impact on outcome. Again, the relatively large number of patients included in this analysis and the wide range of mortalities reported may obscure real differences in as yet unidentified subgroups. None of these data are sobering. As pointed out by Smith and Graham more than 20 years ago, we have learnt much and accomplished little16,17.

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References

Cytokeratin immunoreactivity of intestinal metaplasia

We read with great interest the well designed study of Couvelard et al (Gut 2001;49:761-6). In agreement with other studies,11 the authors reported that cytokeratin (CK) 7 and 20 immunoreactivity in the specialised intestinal metaplasia found in Barrett's oesophagus differs from the intestinal metaplasia found in the stomach. The specific pattern of CK7/CK20 expression, so-called Barrett's type, is characterised by strong CK7 staining of both superficial and deep glands together with a strong superficial CK20 stain. The authors report that both clinical and endoscopic findings support this differentiation.

The origin and development of intestinal metaplasia at the gastro-oesophageal junction have been a matter for debate. There are findings suggesting that intestinal metaplasia of the cardia has an immunophenotype similar to Barrett's oesophagus11 while others suggest that it is similar to the rest of the gastric mucosa.12 We evaluated the CK7/CK20 pattern of gastric cardia with intestinal metaplasia and compared it with Barrett's oesophagus, corpus, and antrum metaplasia in 68 endoscopic biopsies and selected surgical specimens.13 Immunostaining was performed using the same monoclonal antibodies for CK7 and CK20 as in the study of Couvelard et al for all specimens of Barrett's (n=17), cardia metaplasia (n=15), corpus metaplasia (n=14), and antrum metaplasia (n=22).

We found three patterns of CK7/CK20 immunostaining and identified them as IM-1, IM-2, and IM-3. IM-1 is characterised by strong diffuse CK7 staining in both superficial and deep glands and strong superficial CK20 immunostaining, corresponding to the so-called Barrett CK7/CK20 pattern. IM-2 is characterised by either negative or weak patchy CK7 staining of the surface and crypt epithelium and a strong diffuse surface epithelium and patchy crypt CK20 staining (corresponding to the so-called gastric CK7/CK20 pattern). IM-3 pattern is characterised by strong and patchy CK7 staining of the surface and crypt epithelium and strong diffuse surface and patchy crypt CK20 immunostaining. Sixteen of the 17 cases with long segment Barrett's oesophagus (94%) and one of the 15 cases with cardia metaplasia expressed the IM-1 CK7/CK20 pattern. The IM-1 or Barrett's CK7/CK20 pattern had a high specificity as none of the 36 cases representing intestinal metaplasia of the corpus and antrum had this immunostaining pattern. The IM-2 pattern was present in most of the specimens with intestinal metaplasia in the stomach (34 of 36) (table 1).

We hypothesise that the differences in the immunophenotypes observed in intestinal metaplasia of the cardia are mainly associated with different practices in collecting biopsy samples. As in the study of Couvelard et al, we paid particular attention to try to have the mucosal biopsies directly across from the Z line by adequately positioning the biopsy forceps. None less, biopsy samples taken less than 1 cm proximal to the gastric folds could actually represent “short segment Barrett's oesophagus” in some cases.14 As stated by the authors, no absolute histological criteria for diagnosing Barrett's mucosa have yet been established. Therefore, it would be interesting to know the types of epithelium that were

Table 1 Mortality (%) in patients with bleeding varices (group BV) compared with those in patients with known varices who did not bleed (group NBV)

<table>
<thead>
<tr>
<th></th>
<th>Group BV</th>
<th>Group NBV</th>
</tr>
</thead>
<tbody>
<tr>
<td>Year 1</td>
<td>24.9 (4.40)</td>
<td>19.5 (1.80)</td>
</tr>
<tr>
<td>Range</td>
<td>7.4-64.2</td>
<td>5.6-49.7</td>
</tr>
<tr>
<td>Year 2</td>
<td>32.2 (6.50)</td>
<td>31.4 (3.45)</td>
</tr>
<tr>
<td>Range</td>
<td>14.7-71.1</td>
<td>9.6-62.6</td>
</tr>
<tr>
<td>Year 3</td>
<td>46.0 (7.31)</td>
<td>46.0 (7.31)</td>
</tr>
<tr>
<td>Year 4</td>
<td>33.6-82.7</td>
<td>33.6-82.7</td>
</tr>
</tbody>
</table>

Downloaded from http://gut.bmj.com/ on June 1, 2017 - Published by group.bmj.com
Table 1: Distribution of CK7/CK20 immunostaining patterns in long segment (LS) Barrett’s oesophagus, cardia, corpus, and antrum intestinal metaplasia (IM)

<table>
<thead>
<tr>
<th>CK pattern</th>
<th>LS Barrett</th>
<th>Cardia IM</th>
<th>Corpus IM</th>
<th>Antrum IM</th>
</tr>
</thead>
<tbody>
<tr>
<td>IM-1</td>
<td>94%</td>
<td>7%</td>
<td>0%</td>
<td>0%</td>
</tr>
<tr>
<td>IM-2</td>
<td>6%</td>
<td>80%</td>
<td>100%</td>
<td>91%</td>
</tr>
<tr>
<td>IM-3</td>
<td>0%</td>
<td>13%</td>
<td>0%</td>
<td>9%</td>
</tr>
</tbody>
</table>

revealed on the gastric side of the Z line (cardiac, fundic, or perhaps specialised columnar epithelium).

There is sufficient evidence to suggest that “Barrett’s CK7/CK20 pattern” is a useful tool in distinguishing between Barrett’s oesophagus and intestinal metaplasia of the cardia. However, more research is needed for a better understanding of the development and meaning of intestinal metaplasia of the cardia.

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References

Authors’ reply
We thank Mouzas et al for their comments on our study. We read with interest their results that are partly in keeping with our data and that confirm that Barrett’s mucosa has a specific pattern of cytokeratin (CK) expression. Two comments can be made with respect to this work. Firstly, we have noticed that the authors identified three CK7/20 patterns of intestinal metaplasia (IM-1, IM-2, and IM-3). Although IM-1 is identical to that designated as Barrett’s pattern by Ormsby et al, it is important to note that IM-2 and IM-3 do not strictly parallel the two other CK7/20 patterns defined by Ormsby et al as corresponding to the gastric type of intestinal metaplasia. Thus the significance of IM-2 and IM-3, as proposed by Mouzas et al, have still to be clarified. In the small areas of intestinal metaplasia that are found in biopsy specimen from the gastro-esophageal junction, we have made the distinction between the typical “Barrett’s phenotype” (as described by Ormsby et al and corresponding to IM-1 type) and other types that we have considered as “gastric phenotypes”. Secondly, Mouzas et al identified only one case of Barrett’s type intestinal metaplasia among 15 cases of cardiac intestinal metaplasia, suggesting that intestinal metaplasia of the gastro-esophageal junction is only rarely related to short segments of Barrett’s oesophagus. This finding is not consistent with our results in that we found 16 patients presenting with Barrett’s CK7/20 pattern among 54 patients that were evaluated for immunohistochemistry. However, this discrepancy may be related to the limited collection of biopsy samples and may be influenced by Helicobacter pylori status, age, sex, and ethnic origin of the patients, data that were not reported in the present work of Mouzas et al.

At the end of their letter, Mouzas et al question the type of epithelium that we found on the gastric side of the gastro-esophageal junction. As the site of biopsy sampling may be critical, we reviewed all gastro-esophageal junction biopsy specimens for histological evidence of the squamous-columnar junction. Among the 988 biopsy specimens corresponding to the 234 patients with an endoscopically normal gastro-esophageal junction included in our study, 382 (39%) containing both squamous and columnar epithelium were considered as directly taken across the Z line. Cardiac mucosa was present in 373 of the 382 biopsy specimens (97.6%), in association with fundic mucosa. Moreover, we found the same ratio when patients were divided into two groups, one group corresponding to 60 patients with intestinal metaplasia at the gastro-esophageal junction and four biopsy specimens across the Z line, containing cardiac mucosa in 113 (97.4%) in association with fundic mucosa in 18/113 (15.9%) and containing only fundic mucosa in 2/113 (1.7%) and the other group corresponding to 194 patients without intestinal metaplasia at the gastro-esophageal junction (266 biopsy specimens across the Z line, containing cardiac mucosa in 260 (97.7%) in association with fundic mucosa in 56/260 (21.5%) and containing only fundic mucosa in 6 (2.26%). These data, in line with those obtained in an autopsy series, support the concept that the gastric cardia is present as a constant structure. However, it must be remembered that some workers recently proposed the hypothesis that cardiac-type mucosa arises as a metaplastic phenomenon.

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References

Serrated adenomas in FAP
We read with interest the article by Matsumoto et al reporting their observations on the presence of serrated adenomas in familial adenomatous polyposis (FAP) patients in relation to germline APC mutations (Gut 2002; 50:402–4). Their small colonoscopic study identified three FAP patients with serrated adenomas; all had less than 100 polyps and they concluded that serrated adenomas may be characteristic of attenuated FAP.

It is our practice to perform prophylactic colectomy with ileorectal anastomosis or ileoanal pouch formation in patients with FAP in the second or third decade or as soon as possible after a new diagnosis is established. An expert histopathologist performs a meticulous examination of the colectomy specimen, including a formal polyp count. Thereafter any rectal remnant is surveyed six monthly by flexible sigmoidoscopy with endoscopic snare polypectomy and argon plasma coagulation of suspicious lesions.

A simple search of the St Mark’s polyposis registry has revealed eight patients in whom serrated adenomas have been identified. In five patients the lesion was present in the colectomy specimen; in two the diagnosis was...
made on flexible endoscopic surveillance, and in one case a serrated adenoma was present in a polyp surgically excised from the rectum (table 1).

As in Matsumoto’s study, in the majority of the St Mark’s cases the serrated adenoma was located distally either in the sigmoid colon or rectum. However, in our patients serrated adenomas were not restricted to those with the attenuated phenotype. Seven of the St Mark’s patients with serrated adenomas have classical FAP with more than 100 colonic polyps in the resection specimen. (In one of these patients preoperative colonscopy reported a low polyp count.) The genetic mutations have been identified in three of our patients and all were in exon 15, rather than more proximally.

Serrated adenomas may be a feature in FAP but they are not characteristic of the attenuated phenotype. Colonoscopy alone may underestimate the number of colorectal polyps, especially in difficult cases. We believe that dye spray colonoscopy by an experienced endoscopist and careful examination of colono-
tomy specimens are necessary to completely characterise the FAP phenotype. The clinical significance of the presence of serrated adenomas in FAP patients has yet to be determined. Further studies in this interesting area are required.

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Authors’ reply

We would like to thank Drs Gallagher and Phillips for their comments on our article. They raise the point that experienced colono-
scopists should assess colorectal lesions in familial adenomatous polyposis (FAP) using a dye spraying technique. It has been shown previously that conventional colonoscopy would underestimate the number of adenomas in patients with attenuated FAP. In two of the three subjects with polyps less than 100 in number, chromo-scopy identified numer-ous and diminutive areas of flat configuration in the colorectum where tubular adenomas were confirmed histologically. While chromoscopy identified numerous non-polypoid areas of tubular adenomas in two of the three subjects with serrated adenomas, their sparse colono-
tomy polyps and the APC gene mutation were compatible with FAP of the attenuated form.

Another important issue raised by Drs Gallagher and Phillips is the fact that in their histological survey of resected specimens three patients with serrated adenomas had an APC mutation at the proximal part of exon 15. This discrepancy may have arisen from differences in the procedure of assessment for colo-
rectal adenomatosis. In our 15 colonoscopists have been identified, however, we have not yet found any serrated adenomas. Based on the comments of Drs Gallagher and Phillips, other colonoscopists are under investigation at our institute. Many more patients with FAP or attenuated FAP are identified, the correlation between serrated adenomas and the genotype of FAP remains controversial.

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References

Table 1 Patient characteristics of eight patients in whom serrated adenomas were identified in St Mark’s polyposis registry

<table>
<thead>
<tr>
<th>Patient No</th>
<th>Age at colectomy (y)</th>
<th>Preoperative endoscopy</th>
<th>Colectomy specimen polyp count</th>
<th>Site and size of serrated adenoma</th>
<th>APC mutation analysis</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 Male</td>
<td>29</td>
<td>Not available</td>
<td>3550</td>
<td>Sigmoid colon 1.2 cm (colectomy)</td>
<td>Ex 15 4175 c &gt;g</td>
</tr>
<tr>
<td>2 Female</td>
<td>36</td>
<td>Classical</td>
<td>1230</td>
<td>Descending colon 0.5 cm (colectomy)</td>
<td>Under investigation</td>
</tr>
<tr>
<td>3 Male</td>
<td>19</td>
<td>Low polyp count</td>
<td>Not available</td>
<td>Rectum 0.5 cm (colectomy)</td>
<td>Unsuccessful</td>
</tr>
<tr>
<td>4 Female</td>
<td>39</td>
<td>Classical</td>
<td>900</td>
<td>Rectal polyp (surgical excision)</td>
<td>Unsuccessful</td>
</tr>
<tr>
<td>5 Female</td>
<td>17</td>
<td>Low polyp count</td>
<td>648</td>
<td>Rectal biopsy</td>
<td>Unsuccessful</td>
</tr>
<tr>
<td>6 Male</td>
<td>19</td>
<td>Low polyp count</td>
<td>868</td>
<td>Rectal biopsy</td>
<td>Ex 15 2367/2368 deletion</td>
</tr>
<tr>
<td>7 Male</td>
<td>19</td>
<td>Classical</td>
<td>Hundreds</td>
<td>Rectal biopsy</td>
<td>Unsuccessful</td>
</tr>
<tr>
<td>8 Female</td>
<td>14</td>
<td>Not available</td>
<td>1425</td>
<td>Colectomy specimen</td>
<td>Unsuccessful</td>
</tr>
</tbody>
</table>

FAP, familial adenomatous polyposis.
Coeliac disease and the risk of autoimmune disorders

We recently suggested in a study on 909 adolescent and young adults aged 15+ years (mean 16.1) that the prevalence of autoimmune diseases in coeliac adults is related to the length of gluten exposure, independently of the expected age effect.1 Recently, Sategna Guidetti et al (2001;49:502–5) presented a hypothesis.

However, we feel quite happy with the contribution of Sategna Guidetti et al as we found strong confirmation of our findings in their paper. As mentioned by the authors, their paper stimulates some interesting observations.

(1) The people they studied was affected by a very strong “age” selection as the vast majority were aged over 40 years and hence all had maximum exposure to the risk factors (100% had been exposed to gluten for >20 years, including “actual gluten exposure”) and there was no modulation of effect, just the end point, which surprisingly was identical to our own results. We have not studied a paediatric population, but young adults with a mean age of 16.7 years and the risk factor was evaluated over the whole range of ages before the outcome (autoimmune disease) was expected.

(2) “Age at diagnosis” is a robust variable and is unlikely to be biased. Sategna Guidetti et al showed, very consistently, that age at diagnosis was related to outcome. The actual prevalence of autoimmune diseases was even higher than that observed by us (possibly due to age range?).

(3) The variable “actual gluten exposure”, artificially built by the authors, was largely based on age at diagnosis (hard data) together with minor components related to self reported compliance and follow up.

(4) In summary, if they included in a multivariate model the strong variable “age diagnosis” which explains a significant part of the variance in the outcome variable, it is very unlikely that a second variable (supposed “actual gluten exposure”) containing the first strong variable adds any further contribution to the outcome variance.

(5) One important prerequisite for a multivariate model is to include variables independent of each other, which was definitely not the case here. In the logistic regression model the variables included were the “strong” ones, as expected in this type of analysis. The outcome (prevalence of autoimmune diseases) was significantly related to present age and age at diagnosis of coeliac disease. What else could contribute to the derived variable “actual gluten exposure”?

To add strength to this finding, we have new prospective data from a cohort of 74 coeliac patients (46 females) diagnosed before the age of five years and followed up for an average period of 18.4 years (range 10–30); their actual mean age is now 20.34 years. Of these, 5/74 developed an autoimmune disease during this follow-up period (two dermatitis herpetiformis, one thyroiditis, one MMC, one psoriasis): all of these cases had been exposed to gluten challenge for 11–48 months after a variable length of time on a gluten free diet. These indeed had “gluten exposure”, unfortunately added on a relatively precarious diagnosis. None of the other 69 patients has developed an autoimmune disease to date.

We thank our colleagues for their significant confirmations and hope that they will share our will to explore the biological reasons which may explain why age at diagnosis is so strongly correlated with the prevalence of autoimmune diseases in adults.

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References


Authors’ reply

We thank Ventura et al for their comments on our paper, and we are delighted to have made them happy, but we would like to clarify a few points.

As underlined in our article, we believe that the conclusions of the two papers are not to be considered antithetical for the following reasons.

(a) They were conducted in different coeliac disease (CD) age groups and, as underlined by Londei in his commentary on our article, ‘’to date there is no consensus that child and adult CD are the same condition, nor that subjects in whom CD has been diagnosed in adulthood have had CD all their life’. Unless there was a recent revision of which we are unaware, paediatric age comprises adolescence and up to 18 years of age, raises critical questions on the relationship between AI disorders.

(c) When considering a paediatric population, we should be aware of a possible screening bias due to different clinical suspicions and presentations.

(d) Although the conclusions at first glance seem to be similar in the two papers concerning age at diagnosis as a risk factor, this cannot be viewed as a “surprising” confirmation of Ventura et al’s study hypothesis. Rather this should suggest the applicability of the “actual gluten exposure” concept to their population, as also proposed by Londei.

The “strong” age at diagnosis variable can be biased by screening procedures and medical awareness directed both to coeliac and autoimmune (AI) diseases, often diagnosed in silent or oligosymptomatic patients.

The “artificially built variable” actual gluten exposure may be perceived with difficulty by a cursory and hasty reading. It is not a “self reported compliance and follow up” but provides a better indication of the effective gluten exposure as it takes into account not only age at CD diagnosis but also age at diagnosis of AI disorders. The beginning of gluten free diet in patients in whom recovery of AI disorders was ascertained was considered as the end of gluten exposure.

In other words, the period of gluten exposure matched the time of AI disease onset in patients in whom AI disease preceded CD diagnosis, and the beginning of gluten withdrawal (with ascertained compliance by means of the above mentioned criteria) in patients in whom a CD diagnosis was made before AI disease onset, respectively.

Thus when adult CD patients with and without AI associated diseases were compared, age at CD diagnosis, considered as an indirect mirror of duration of gluten exposure, was significantly higher in patients with associated AI disorders, while actual gluten exposure was similar in both groups; moreover, in 35% of patients an AI disease appeared after a diagnosis of CD, even in subjects in whom recovery of intestinal mucosa was ascertained.

This fact and the finding of a 30% prevalence of AI disorders in our patients aged 41 (±19) years compared with 23% in Ventura et al’s study in patients aged 15+ years (unless there was a recent revision of which we are unaware, paediatric age comprises adolescence and up to 18 years of age), raises critical questions on the relationship between AI disorders.
The variable “actual gluten exposure” not only reflects more accurately the duration of gluten exposure but eliminates weighty confounding factors that contribute strongly to the apparently significant relationship between age at diagnosis and outcome in the logistic regression model.

In CD, there is a generalised increase in permeability to macromolecules, making it likely that Peyer’s patches are not the only site where gliadins are in contact with the immune system. Gluten peptides may encounter the gut immune system in a fashion that bypasses the normal controlled sampling, leading to sensitisation or loss of tolerance to the antigen.

Despite their diverse aetiology, certain pathogenetic mechanisms are common to all AI diseases: as a rule, they require the presence of self reactive CD4 positive T lymphocytes which are believed to be deleted in the thymus and to be present only when they arise following somatic mutation, producing “forbidden clones”.

The question is why only a minority of CD patients manifest an AI disease?

Most AI diseases show a particular bias for certain gliadins, usually belonging to class II, which encodes important immune response regulating genes: thus some rational connection may exist between the genetic constitution and susceptibility to AI disorders: as a rule, they require the presence of a particular HLA allele (usually HLA-DQ2 or -DQ8) and present gliadins to the T cell receptor.

Gliadin recognition is a prominent cofactor in modifying the immunological response earlier in life in susceptible individuals is a prominent cofactor in modifying the immunological response earlier in life in susceptible individuals. Prolonged exposure to gliadins may influence the expression of HLA-DQ2 and -DQ8 and the number of CD4+ T cells specific for gliadins.

In CD, there is a generalised increase in transaminase levels with gliadin exposure, but eliminates weighty confounding factors that contribute strongly to the apparently significant relationship between age at diagnosis and outcome in the logistic regression model.

CD seems to be a “normal” condition of a true AI disease triggered by an environmental agent (gluten) in genetically predisposed individuals. It has been estimated that the HLA contribution to the development of CD among siblings is 36% and recent data suggest that a gene or genes other than the HLA unlinked locus must also participate and are likely to be strong determinants of disease susceptibility.

This may suggest that exposure of the immature thymus to gliadin in susceptible individuals is a prominent cofactor in modifying the immunological response earlier in life and thus predisposing susceptible individuals, normally capable of DQ2/DQ8, but also to AI diseases. In other words, “les jeux sont fait” early in life.

Thus the search for genetic characteristics of CD patients with associated AI diseases could be more stringently focused on meaningful controversy.

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Non-alcoholic steatohepatitis (NASH): why biopsy?

The leading article by Day (Gut 2002;50:585–8) provides a valuable summary of the current understanding of NASH, and clinical relevance of non-alcoholic steatohepatitis (NASH). The article also makes two points clear: (a) we have little ability to provide accurate prognostic information in an individual patient, and (b) there is the promise of new treatments, the only known effective therapy at present, for the obese patient, is weight loss. Should these patients be subjected to liver biopsy?

Day proposes that a subgroup of patients with suspected fatty liver should undergo biopsy, including those with alamine aminotransferase >ALT values, “moderate” central obesity, non-insulin dependent diabetes, hypertension, and hypertriglyceridaemia. Gastroenterologists are commonly referred patients fulfilling these criteria but is liver biopsy likely to affect their management? The only therapeutic option at present is weight loss and all obese patients are naturally advised to lose weight, whether they have simple steatosis, NASH, or even normal liver biochemistry.

A number of arguments may be used to justify liver biopsy in these patients; histopathological analyses are not only unexpected findings and the results may allow more accurate prognostic information to be given to the patient.

Sherwood and colleagues identified 342 patients found on screening by their general practitioner to have liver enzymes raised above twice the upper normal limit who had not been referred to a specialist for further assessment. Of these, half had raised liver enzymes for more than 6 months and half were thought to require further investigation, approximately one third of whom had normal results on repeat testing. Following investigation of the remainder in a gastroenterology clinic, alcoholic liver disease and non-alcoholic fatty liver disease (NAFLD) accounted for 43% and in nearly all of the others a diagnosis could have been reached with the aid of careful history taking (alcohol and drugs), serological testing (renal hepatitis, markers of iron overload, primary biliary cirrhosis, α, antitrypsin deficiency), and ultrasound examination (common bile duct stones). These data would suggest that biopsy for only a small number of patients rarely yields an unexpected diagnosis and can be reserved for a selected subgroup of patients following non-invasive testing. None of the patients with NAFLD were cirrhotic on biopsy, although 11 (42%) had fibrosis. The results of liver biopsy would not have affected the clinical management of the small number of patients with unexpected histological findings; six patients raised anti-“autoimmune” or “cryptogenic” hepatitis, but assuming transaminase levels were only approximately twice the normal upper limit there would be no justification for immunosuppressive therapy.

It is difficult to justify liver biopsy simply to provide the patient with better prognostic information. Patients with marked fibrosis are more likely to develop cirrhosis and die from liver disease but it is not yet clear that patients with simple steatosis and mild fibrosis can be reassured they will not, in time, develop more severe liver disease.

Most gastroenterologists, particularly those who work in district general hospitals, adopt a pragmatic approach to the management of patients referred with abnormal liver biochemistry. This is especially important for investigations that are costly and/or are associated with significant morbidity, as with liver biopsy. The presence of cirrhosis will affect patient management but, in his published article, cirrhosis rates in NAFLD are likely to be overestimated because most studies have not been done on unselected patient groups. Improved criteria for selecting patients likely to have marked fibrosis or cirrhosis are required to target biopsies more appropriately.

When effective medical treatments are available, clear criteria for liver biopsy should follow. Until then, liver biopsy in the majority of these patients outside a research setting cannot be justified.

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Author’s reply

In his letter, Dr McNair states that liver biopsy is not justified in patients with suspected non-alcoholic steatohepatitis (NASH) outside a research setting. His principal arguments are that liver biopsy is unlikely to: (a) improve diagnostic accuracy, (b) provide more accurate prognostic information, and (c) effect management.

To support his first argument, Dr McNair cites a study which examined whether abnormal liver function test results are investigated appropriately in primary care and reported on the eventual diagnosis after full investigation. I am sure that the authors of this study would be surprised at this use of their data by Dr McNair both in drawing conclusions present in the citation and the conclusions drawn. For example, contrary to McNair’s letter, just over 20% (36/157) of patients thought to require further investigation had normal results on repeat testing and in the remainder, 56% (68/121) rather than 43%, had alcoholic liver disease or non-alcoholic fatty liver disease. Contrary to McNair’s statement that: “These data would suggest that biopsy for those with raised liver enzymes rarely yields an unexpected diagnosis and can be reserved for a selected group of patients following non-invasive testing”, the cited report states that: “Liver biopsy provided a diagnosis in 81 (patients) when serological tests gave normal results”. Furthermore, contrary to McNair’s statement that liver biopsy did not affect clinical management, the cited report states that: “Of the 81 patients with AIH (62%) had an identifiable diagnosis requiring hospital intervention or follow-up or both”. Therefore, this cited report suggests strongly that liver biopsy is useful for diagnosis in patients with negative serology and that the eventual diagnosis has implications for management. Specifically referring to NASH, further evidence of the utility of liver biopsy for diagnostic reasons comes from a study in which a group of eminent hepatologists were
Risk of gastric carcinoma in patients with atrophic gastritis and intestinal metaplasia

We read with interest the study of Whitting et al (Gut 2002;50:378–81) in which they reported an 11% risk of development of malignancy among patients with atrophic gastritis and intestinal metaplasia over a 10 year period. We agree with their conclusion that their findings should be further evaluated in larger studies, as confirmation of a high risk of malignancy would have important implications for clinical practice. We would however like to raise two issues.

Firstly, in Whitting’s study, biopsies were taken only when there was macroscopic abnormality. These patients may therefore not be representative of the general population of dyspeptic patients with intestinal metaplasia. We analysed 100 consecutive patients endoscoped for uncomplicated gastro-oesophageal reflux disease or functional dyspepsia for whom two biopsies were taken routinely from the antrum, body, and cardia, irrespective of macroscopic findings. The proportions of our patients with intestinal metaplasia, atrophic gastritis, both, or either were 17%, 27%, 6%, and 38%, respectively. Twenty of 27 patients with atrophy had mild changes: seven had moderate changes and none had severe atrophic gastritis. In four of 17 patients, intestinal metaplasia was widespread, 13 had focal changes. Thirty seven patients had Helicobacter pylori gastritis: 23 of these had concomitant atrophic gastritis and/or intestinal metaplasia. It seems unlikely that 4% (38%±11%) of our patients will develop gastric cancer over the next 10 years.

Our patients with atrophic gastritis and intestinal metaplasia are more representative of the general dyspeptic population and a different group from those studied by Whiting et al. Perhaps the high risk of malignancy they describe is associated with a combination of macroscopic abnormalities, the severity of the changes, the type of intestinal metaplasia, concomitant atrophic gastritis and intestinal metaplasia, atrophic gastritis, and H pylori infection rather than the histological findings per se. Long term follow up of a representative population of UK patients with uncomplicated dyspepsia is warranted. Meanwhile, we are concerned that their findings should not be uncritically extrapolated as the basis for surveillance recommendations for patients with uncomplicated dyspepsia and atrophic gastritis and/or intestinal metaplasia.

Secondly, what was the H pylori status of their patients with intestinal metaplasia and atrophic gastritis? In Uemura et al’s follow up study of 1526 Japanese patients over a mean period of 7.8 years, gastric cancers developed in 2.9% of patients infected with H pylori but in none of the uninfected patients. Among patients with H pylori infection, those with severe gastric atrophy, intestinal metaplasia, and corpus predominant gastritis were at significantly higher risk. If the patients described by Whiting et al were H pylori positive, antibiotic treatment may be a more cost effective approach compared with endoscopic surveillance.

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References

Somatostatin Analogs in Cancer Management


It may be common practice to publish book versions of journal supplements but this is the first such example to reach the desk of this reviewer. Somatostatin analogues have been available for over a decade (Hepatology 2001;47:supplement 2). In this age of instant electronic information retrieval the idea of producing such a hard copy version seems rather retrogressive, but for frequent referral it is still easier and quicker to reach for a book from the shelf than to suffer the vagaries of Internet access. But is this a book that needs to be at one’s fingertips?

The book consists of a preface and introduction to the editor followed by a series of 10 invited reviews of different aspects of therapy with somatostatin analogues. These include overviews of the pharmacology and clinical applications of these compounds plus more specific chapters concerned with their use in the treatment of cancer of the breast, lung, prostate, gastrointestinal tract, pancreas, and liver, while the book ends with a quite comprehensive look into the future.

The book suffers from the shortcoming of nearly all textbooks in that it is not up to date. One would perhaps have expected better from a journal supplement but only a handful of the numerous references in this book date from later than 1998. That would not necessarily be a problem if the field had matured to the extent that recent developments would not significantly affect the take home message from the text, but that is not the case. The field of somatostatin analogues is very active, with the molecular biology of the SRIF receptors has developed considerably in the last few years. The book also suffers from a high degree of repetition. If you did not know there were five receptor subtypes before reading this book, then you would be left in no doubt by the end since all 12 chapters would have told you so! Less facetiously the take home message from nearly all of the chapters is also the same—octreotide therapy should...
work but it doesn’t. The rationale for a cytotoxic-taxic mechanism is clearly stated—indirectly through suppression of the GF/IGF-1 axis, inhibition of angiogenesis, reduction in T lymphocyte production, etc, and direct antiproliferative actions. However, with the exception of promising activity in the treatment of primary liver cancer, all of the proposed clinical indications fail to show any meaningful efficacy although many of the authors call for more controlled studies. It is disappointing that despite featuring the use of targeted radiation on the front cover of the book, none of the chapters is particularly well written. Its strength lies in the use of targeted radiation on the front cover of the book, none of the chapters is particularly well written. Its strength lies in the compendium of chapters, which are often too long. Although it merits several passing mentions, this book represents a snapshot of the state of clinical application of somatostatin analogues in cancer in the late 1990s. There is undoubtedly utility in the palliation of the effects of hypersecretion of peptide hormones but no real indication of any useful cytotoxic activity in most cancers. Only time will tell if this turns out to be the end of the story.

S J Mather

NOTICES

Sir Francis Avery Jones BSG Research Award 2003

Applications are invited by the Education Committee of the British Society of Gastroenterology who will recommend to Council the recipient of the 2003 Award. Applications (TWENTY COPIES) should include:

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

Entrants must be 40 years or less on 31 December 2002 in New York, USA. Further information: Michele Centonze Conseil, 6 bis Rue des Cendriers, 75020 Paris, France. Tel: +33 1 44 62 68 80; fax: +33 1 43 49 68 58.

15th European Intensive Course (SMIER) Digestive Endoscopy

This course will take place on 16–17 December 2002 in Strasbourg, France. Further information: Marie Jeanette Teygat will be held on 12 December 2002 in Amsterdam, the Netherlands. Deadline for registration is 1 November 2002 (no registration fee) and registration should be done via email to: jjgoedkopp@amc.uva.nl.

The Future of Gastro-entero-hepato-pancreatology is bright

This Academic Farewell Symposium of Guido NJ Tiygat will be held on 13–15 December 2002 in Amsterdam, The Netherlands. Further information: European Cancer Centre, PO Box 9236, NL 1006 AE Amsterdam, The Netherlands. Tel: +31 (0)20 346 2547; fax: +31 (0)20 346 2525; email: ccc@iacc.nl.

Cancer of Oesophagus and Gastric Cardia: from Gene to Cure

This conference will be held on 13–5 December 2002 in Amsterdam, The Netherlands. Further information: European Cancer Centre, PO Box 9236, NL 1006 AE Amsterdam, The Netherlands. Tel: +31 (0)20 346 2547; fax: +31 (0)20 346 2525; email: ccc@iacc.nl.

The Sheila Sherlock Memorial Symposium

Dame Sheila Sherlock, who died earlier this year, was responsible for creating hepatology at the Royal Free Hospital, London. This memorial symposium will take place on 26–28 January 2003 at the Royal Free Hospital, London, UK. Further information: Terri Dolan, Royal Free and University College Medical School, Royal Free Campus, Centre for Hepatology, Upper 3rd Floor, Rowland Hill Street, London NW3 3PF, UK. Tel: +44 (0)20 433 2851; email: t.dolan@rfc.ucl.ac.uk.

3rd Chester International Inflammatory Bowel Disease Meeting

This meeting will be held on 10–11 February 2003 in Chester, UK. An international programme includes speakers from the USA, France, Italy, and the UK, and will cover clinical problems, pathogenesis, medical and surgical treatment. Registration details and programme from: Professor Jonathan Rhodes, Department of Medicine, University of Liverpool, Dalby Street, Liverpool L69 3GA, UK. Tel: +44(0)151 706 3558; fax: +44(0)151 706 5832; email: rhodesj@liverpool.ac.uk.
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Updated information and services can be found at: [http://gut.bmj.com/content/51/6/898](http://gut.bmj.com/content/51/6/898)

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