Food elimination in IBS: the case for IgG testing remains doubtful

I read with interest the study of a diet for irritable bowel syndrome (IBS) based on serum IgG levels to foods (Gut 2004;53:1436-44).

In rigorous elimination diet studies, about one third of IBS patients are found not to have food intolerance.1-3 Yet it appears that everyone tested for specific IgG in this study had some positive reactions and was therefore subjected to dietary recommendations. This does not in itself suggest that serum IgG is a particularly useful test.

One notable finding of this study appears to be that 87% of patients gave a high level of IgG to yeast. In two large scale studies of IBS using diagnostic elimination diets, the percentages who had a symptomatic reaction to yeast when challenged were 5.5% (out of 73 unselected IBS patients) and 12% (out of 122 unselected IBS patients).4 It seems unlikely that yeast causes IBS symptoms in 87% of patients in Manchester but in only 5-12% of patients in Oxfordshire and Cambridgeshire. A logical implication is that high levels of IgG against yeast do not, in themselves, reveal anything significant in relation to IBS symptoms.

The same, in my view, would follow for several other foods. The numbers of patients with positive responses to eggs, cow’s milk, and cashew nuts, as judged by IgG levels, are much higher than one would expect from empirical dietary studies,1,2 while the numbers testing positive to chocolate and oranges appear far too low. Again, it seems doubtful that IgG can reveal sensitivities accurately in IBS.

The percentage of patients showing substantial benefit from this diet is disappointing. In studies using a well conducted and rigorous elimination diet, the “number needed to treat” is between 1.5 and 2.2.1-4 The “number needed to treat” in this study was 9. (The value of 2.5, calculated on the basis of those who fully complied with the diet, abrogates the intention to treat principle.) This seemingly poor response to an IgG based diet confirms the widely held view to date that IgG testing for food intolerance is not of value.1-3 These results suggest that if IgG testing identifies food intolerances at all, it does so fortuitously and with an apparent low degree of accuracy.

I conclude that the difference in outcome between the “true diet” and the “sham diet” groups can largely be explained, not by specific identification of food reactions, but by the gross differences between the two diets. The “true diet” excluded milk products for 84% of patients and wheat for 49% (both foods are known to be common offenders in IBS) while the total number of foods avoided by the group was 498 (value calculated from table 2). For the “sham diet” group, 1.3% avoided milk, 8% avoided wheat, and the total number of foods avoided was only 493. These overall differences between the diets could easily explain the modest difference in outcome between the two diet groups. The same diet sheets, distributed randomly to the patients in each group, regardless of IgG levels, would probably have produced the same overall result.

Similarly, I consider that the effectiveness of the blinding in this trial is questionable. The “nutritional advisor” giving support by telephone may have become aware of which patients were receiving the “sham diet” as this regularly excluded potatoes and rice while the “true diet” rarely did so—the reverse being true for wheat, milk, and yeast. The views of the nutritional advisor on the likely effectiveness of the diets could inadvertantly have been communicated to the patients, and unintentionally influenced their assessment of the outcome.

Before this trial was begun, in my view it would have made sense to try to answer the more basic research question: do high levels of IgG against a food predict an adverse reaction to that food? Only one very small trial has so far done this.5 It measured food specific serum IgG in individual IBS patients and compared the results with those from food challenges (following a period of avoidance); there was no correspondence between the foods identified. Such work needs to be repeated with larger sample sizes.

Despite these inconclusive results of this study, it has regrettably already been the subject of a press release and other publicity by the company that provided the IgG testing for this study, in order to promote IgG tests to the general public. On the company’s website, IgG testing is now described as “clinically proven” by the British Allergy Foundation on the basis of this study (The UK YorkTest website: www.yorktest.com). This blurring of the boundaries between what should be a disinterested scientific enquiry and the promotion of a commercial venture is regrettable.

J O Hunter

Correspondence to: Dr J O Hunter, Addenbrookes Hospital, Box 262, Cambridge CB2 0QQ, UK; john.hunter@addenbrookes.nhs.uk

Conflict of interest: none declared

References


Author’s reply

John Hunter states that the generally held view is that IgG testing for food intolerance is not of value and gives references in support of this contention.1 However, the consensus of the papers and others is that the research is of poor quality and better designed studies are needed to resolve this question. Designing trials in this field, which meet all of the criticisms that can be levelled at them, is always going to be difficult. However, we believe that we have conducted a pretty robust trial, which is the first in the field.

In his letter, Hunter also implies that irritable bowel syndrome (IBS) and food intolerance have the same basis. However, it is entirely possible that IgG antibodies may be important in IBS, where we now know there is an inflammatory component in some cases, whereas they may not be relevant in food intolerance in general. Furthermore, it is likely that only a subset of patients are likely to have an immunoinflammatory basis to their condition and these might be the very individuals who respond to dietary exclusion based on IgG antibodies. This would fit with our results only where a proportion of patients responded despite all having antibodies. This, of course, limits the specificity and usefulness of the test unless such subgroups can be identified beforehand. We should also bear in mind that an immunological reaction in the gut, as opposed to other forms of food intolerance, may make the gut more susceptible to other perturbing stimuli, such as stress, rather than necessarily causing symptoms directly.

It is of interest that Hunter singles out the level of IgG to cashew nuts, among other foods, as an anomaly. Since undertaking this study, we have been asking patients about cashew nut consumption and found an extraordinary high intake of this item. Of course, we do not know what the level of consumption is in the general population.

This study was undertaken independently, the data are the data, they are not overstated, and just because they challenge current dogma is not enough reason to reject them without further research. Progress in unravelling the pathophysiology of IBS will only be made if we continue to explore new avenues of research as well as re-examining issues that may have been regarded as unimportant in the past.

P J Whorwell, W Atkinson, T A Sheldon
University Hospital of South Manchester, Manchester, UK

Correspondence to: Professor P J Whorwell, Department of Medicine, Education and Research Centre, Salford Moor Road, Manchester M23 9LT, UK; peter.whorwell@mhui.mht.northwest.nhs.uk

Conflict of interest: declared (the declaration can be viewed on the Gut website at http://www.gutjnl.com/ supplemental)

www.gutjnl.com

Gut 2005;54:1203-1208

10.1136/gut.2005.069740

on September 23, 2023 by guest. Protected by copyright.
IgG antibodies to foods in IBS

Mawdsley et al raise the important question as to whether patients with irritable bowel syndrome (IBS) would gain as much symptomatic improvement if recommended to exclude the top four foods (yeast, milk, whole egg, and wheat) compared with an IgG antibody test based diet. In other words, does the test add specificity? This requires a trial which compares patients receiving an IgG antibody test based diet to those advised to eliminate some or all of the top four foods. We are currently seeking funding for such a trial.

There is some evidence however from our trial that the IgG antibody test based diet may provide a better response than simply eliminating a standard set of foods. When the change in IBS symptom severity score was compared for fully adherent true and sham diet patients who were advised to eliminate one or more of the top four foods, it was found that the true diet patients experienced a significantly greater reduction than the sham diet patients (difference = 94, 95% confidence interval 18, 170; p = 0.0017).

We agree with Sewell’s comment that the food elimination diets in the true and sham diet groups is a real one. This congruency in the results of two independently constructed models only serves to strengthen and validate the findings of both models.

C Hur, N S Nishioka, G S Gazelle

Massachusetts General Hospital, Gastrointestinal Unit and Institute for Technology Assessment, Boston, Massachusetts, USA

Author’s reply

I thank Hur et al for their interest in our article. I agree that his article, which appeared after the initial iterations of our manuscript, is highly pertinent to our work as it models the same clinical scenario.

There are clearly some differences in the models, which are likely due in part to the estimates used to construct it. For instance, average quality adjusted life expectancy when going from surgery to photodynamic therapy (PDT) in our model was increased by approximately 0.5 years whereas in the model by Hur et al the increase was 2.2 years, or four times our estimate. Also, some of our estimated lifetime costs for various therapies varied by as much as 25% from those estimated by Hur et al.

However, considering the number of assumptions and estimates inherent in modelling a complex clinical decision such as Barrett’s with high grade dysplasia (HGD), the model of Hur et al reports remarkably similar results to ours. An ablative approach with PDT yielded an increased quality adjusted life expectancy at a reasonable cost. I agree with Hur et al that the similar findings of the models strengthens and validates the findings. More generally speaking, I feel that any model that features an intervention with some efficacy in the setting of HGD is likely to demonstrate that this intervention will be cost effective. The frequent progression of HGD to cancer, the high cost associated with caring for subjects with cancer, and the poor prognosis associated with cancer all suggest that any intervention keeping even a small fraction of patients with HGD from developing cancer is likely to be cost effective. This is true even if the intervention itself is costly (such as PDT).

Of course, there is a possibility that both models share the same flaws, leading them to come to similar, but erroneous, conclusions. After all, these models are only as good as the data used to create them, and good data on the natural history of various subsets of Barrett’s patients are hard to obtain at present.

Conflict of interest: none declared

References


Author's reply

I thank Hur et al for their interest in our article. I agree that his article, which appeared after the initial iterations of our manuscript, is highly pertinent to our work as it models the same clinical scenario.

There are clearly some differences in the models, which are likely due in part to the estimates used to construct it. For instance, average quality adjusted life expectancy when going from surgery to photodynamic therapy (PDT) in our model was increased by approximately 0.5 years whereas in the model by Hur et al the increase was 2.2 years, or four times our estimate. Also, some of our estimated lifetime costs for various therapies varied by as much as 25% from those estimated by Hur et al.

However, considering the number of assumptions and estimates inherent in modelling a complex clinical decision such as Barrett’s with high grade dysplasia (HGD), the model of Hur et al reports remarkably similar results to ours. An ablative approach with PDT yielded an increased quality adjusted life expectancy at a reasonable cost. I agree with Hur et al that the similar findings of the models strengthens and validates the findings. More generally speaking, I feel that any model that features an intervention with some efficacy in the setting of HGD is likely to demonstrate that this intervention will be cost effective. The frequent progression of HGD to cancer, the high cost associated with caring for subjects with cancer, and the poor prognosis associated with cancer all suggest that any intervention keeping even a small fraction of patients with HGD from developing cancer is likely to be cost effective. This is true even if the intervention itself is costly (such as PDT).

Of course, there is a possibility that both models share the same flaws, leading them to come to similar, but erroneous, conclusions. After all, these models are only as good as the data used to create them, and good data on the natural history of various subsets of Barrett’s patients are hard to obtain at present.

Conflict of interest: none declared

References


P J Whorwell, K J Bentley, W Atkinson, T A Sheldon

University Hospital of South Manchester, Manchester, UK

Correspondence to: Professor P J Whorwell, Department of Medicine, Education and Research Centre, Southmoor Road, Manchester M23 9LT, UK; peter.whorwell@smuht.nwest.nhs.uk

Conflict of interest: declared (the declaration can be viewed on the Gut website at http://www.gutjnl.com/supplemental/)

Two models better than one

The study by Shaheen and colleagues (Gut 2004;53:1736–44) is the result of a decision analysis model which determined the cost-effectiveness of various management strategies for high grade dysplasia in Barrett’s oesophagus. We were surprised to note that the authors of this article did not reference our analysis which was published in July 2003. Our model and analysis had conclusions that were identical to those published by Shaheen et al. Similarities included the finding that endoscopic ablation (photodynamic therapy in our model) results in the greatest number of quality adjusted life years with similar incremental cost effectiveness ratios (ICER) compared with endoscopic surveillance. Also, both of our analyses found that endoscopic surveillance was less expensive than endoscopic ablation but associated with shorter survival.

The authors state in their discussion that their model has several strengths that distinguish it from previously published decision models of Barrett’s oesophagus, including the possibility of histological misdiagnosis of specimens as well as a non-linear progression to cancer, including the possibility of pathological regression. Our model also incorporated these strengths.

This congruency in the results of two independently constructed models only serves to strengthen and validate the findings of both models.

C Hur, N S Nishioka, G S Gazelle

Massachusetts General Hospital, Gastrointestinal Unit and Institute for Technology Assessment, Boston, Massachusetts, USA

Correspondence to: Dr C Hur, Massachusetts General Hospital, Gastrointestinal Unit and Institute for Technology Assessment, 101 Merrimac Street, 10th Floor, Boston, MA 02114; chur@mgh-ita.org

Competing interest: none declared

References


When acquired thrombophilia mattered

A 52 year old previously healthy Afro-Caribbean woman was admitted as an emergency with a 12 hour history of epigastric pain. She was a non-smoker, denied alcohol use, and had no significant comorbidity. Heart rate, respiratory rate, and temperature were normal at presentation. Abdominal examination revealed mild epigastric tenderness with guarding. Baseline investigations (full blood count, clotting, urea and electrolytes, and liver function tests) were within normal limits, except for a raised white cell count (12.1) (normal range 4–11) x10^9/l (neutrophilia)) and a raised amylase level (2409 (normal <220) U/l). Abdominal and chest x rays were also normal. She was diagnosed with acute pancreatitis and treated supportively with intravenous fluids, analgesia, and thromboprophylaxis.

Twelve hours after admission the patient deteriorated significantly, with signs of abdominal peritonitis and a marked metabolic acidosis. She underwent an emergency laparotomy where she was found to have a...
perforated necrotic gall bladder with biliary peritonitis. The common bile duct was dilated but no gall stones were identified. In addition, there was no evidence of atheroma or vasculitis.

Following surgery she ran a prolonged septic course requiring ventilatory and renal support, and on day 13 had a large upper gastrointestinal bleed secondary to intestinal ischaemia. Serial computed tomography scans to identify the source of sepsis were normal until day 21 when a large right subphrenic collection was identified. In addition, an area of low attenuation at the site of the spleen and a cystic mass in the pancreatic tail, consistent with a pseudocyst, were noted. Radiological drainage of the abscess was performed and over the next week the patient was successfully weaned and withdrawn from circulatory and renal support. At this stage her blood film demonstrated the presence of Howell-Jolly bodies, which were consistent with the splenic changes identified on computed tomography.

Recent intrabdominal sepsis at day 42, noted on computed tomography drainage, necessitated a further laparotomy. The collection was drained and the remnants of her autolysed spleen and pancreatic tail removed. At this point the possibility of a thrombotic disorder was raised. Histology showed no evidence of vasculitis and she was antineutrophil cytoplasmic antibody and antoantibody negative. Her thrombophilia screen revealed low levels of protein C (functional: 45 (65–250) u/dl; antigen: 52 (65–130) u/dl) and antithrombin III (functional: 99 (80–120) IU/l, antigen: 70 (80–120) u/dl). Free protein S levels were normal (73 (55–250) u/dl; antigen: 52 (65–130) u/dl). APC resistance ratio was normal 2.05 (1.8–2.5). There were no gall stones identified. In addition, neither factor V Leiden nor prothrombin defects were detected. Protein C levels were normal (73 (55–250) u/dl; antigen: 52 (65–130) u/dl), and neither factor V Leiden nor prothrombin deficiencies.

Two months after discharge her anti-thrombin levels had returned to normal and her warfarin was stopped. She had developed no further problems on follow up at 12 months.

No association of the NFKB1 promoter polymorphism with ulcerative colitis in a British case control cohort

Recently, Karban and colleagues reported an association of a common NFKB1 gene polymorphism, -94ins/delATTG, with ulcerative colitis (UC) in a non-Hispanic, non-Jewish North American population. The deletion was significantly associated with disease in both family based and case control studies: in the combined case control cohort, the allele frequency of -94delATTG (D) was significantly increased in 350 non-Jewish UC cases (45.3%) compared with 802 non-Jewish controls (38.8%, p = 0.002). In a recessive model of inheritance, the homozygous (DD) genotype was significantly increased in 350 UC cases (21.4%) compared with controls (14.8%) (p = 0.0043), giving an odds ratio of 1.57 for the DD genotype (95% confidence interval 1.14–2.16).

Near factor KB (NFkB) is an important transcription factor implicated in the inflammatory response. The NFKB1 gene, which encodes the p105/p50 subunit of the NFkB family of proteins, maps to chromosome 4q24, in a region showing linkage to inflammatory bowel disease1; a mouse locus for colitis, cdsl, maps near the mouse homologue of human NFKB1. The -94ins/delATTG polymorphism in the promoter region of NFKB1 near transcription factor binding motifs may regulate expression of the gene. As NFkB is a plausible inflammatory bowel disease candidate gene, we sought to replicate the findings of Karban and colleagues.

We genotyped the -94ins/delATTG polymorphism in 472 independent British UC cases (for ascertainment and diagnosis see Cuthbert and colleagues2 and 657 ethnically matched healthy controls. This compares with 350 cases and 802 controls in the Karban study. Case control studies have increased power to detect association compared with family based tests (for example, the transmission disequilibrium test)3. The χ² test was used to analyse differences in allele and genotype frequencies between cases and controls, and to test for Hardy-Weinberg equilibrium. Our study was well powered to replicate this association, with 86% power to detect a significant difference in D allele frequency (significance level 5%) based on the allele frequencies of allele D observed by Karban et al, and 79% power to detect a significant difference in DD genotype frequency (significance level 5%) in a recessive model of inheritance.

The NFKB1 promoter region was amplified by polymerase chain reaction (PCR) using the primers promoter f and reverse (labelled with FAM fluorescent dye) and sequenced as described by Karban and colleagues, and PCR products sized by electrophoresis on an ABI 3100 Prism Genetic Analyser. The size of the product determined the presence or absence of the deletion. The χ² test was performed with Yates' correction. There was no significant difference in allele D frequency (40.1% v 39.7%, χ² = 0.04, p>0.5, df = 1) or in the frequency of the DD genotype (16.3% v 14.6%, χ² = 0.62, p=0.5, df = 1) (see table 1) between UC cases and controls. The odds ratio (OR) for the DD genotype in our sample was 1.14 (95% confidence interval 0.822–1.579) compared with an OR of 1.57 (95% confidence interval 1.14–2.16) in the Karban study. The confidence intervals for the two studies overlap, with the OR estimate of Karban et al lying at the upper end of the range for our study.

There are several possible reasons for non-replication of association studies. There could be phenotypic differences in the UC population from the two studies, such as different proportions of patients with limited or extensive disease. Data on site of disease were available from 251 patients in our study; the frequency of allele D was very similar in patients with distal (n = 92, f = 40.8%) or extensive (n = 159, f = 39.9%) disease. There...
may also be population specific differences in the contribution of this variant to UC susceptibility although other loci such as CARD15 and IBD5 have been widely replicated in North American and British populations. Alternatively, the original report may be a false positive: it involved multiple testing against various phenotypes and Jewish versus non-Jewish populations that has not been corrected for. However, the UC association was detected in both family based and case control study designs. Lastly, the size of association of the 94delATTG polymorphism that increases risk for ulcerative colitis may also be population specific differences in susceptibility.

In summary, we found no evidence for association of the 94delATTG polymorphism with ulcerative colitis in the British population. A more detailed survey of the NFκB activation pathway is in progress to assess its contribution to susceptibility to inflammatory bowel disease.

M M Mirza, S A Fisher, C Oinnie, C M Lewis, C G Mathew
Department of Medical and Molecular Genetics, Guy's King's and St Thomas' School of Medicine, King's College London, Guy's Hospital, London, UK

J Sanderson
Department of Gastroenterology, St Thomas' Hospital, London, UK

A Forbes
St Mark's Hospital, Northwick Park, Watford Rd, Harrow, Middlesex, UK

Correspondence to: Professor C G Mathew, Department of Medical and Molecular Genetics, GKT School of Medicine, 8th Floor Guy's Tower, Guy's Hospital, London SE1 9RT, UK; christopher.mathew@genetics.kcl.ac.uk
doi: 10.1136/gut.2005.069740

Conflict of interest: none declared

References


We report a case of an adult onset of CIPO secondary to an autoimmune process affecting exclusively the small intestine without any other systemic organ involvement. A 53 year old Black man with an unremarkable past medical history experienced symptoms of “mechanical obstruction” (nausea/vomiting). After three abdominal explorations, including small bowel resections, he failed enteral feeding rendering him fully TPN dependent. Antroduodenal manometry demonstrated low amplitude contractions in the distal duodenum, and gastrointestinal scintigraphy revealed normal stomach emptying and colonic transit, but delayed small bowel transit. Trypanosoma cruzi antibodies and an extensive serological work up for collagen-vascular disease were negative, except for antinuclear antibody (ANA 1/1280). During five years on TPN, the patient developed multiple episodes of line sepsis and progressive liver disease. He then successfully underwent intestinal transplantation.

Intraoperatively, the small bowel was dilated only in the proximal 270 cm (18 cm circumference). Microscopic examination showed marked degeneration of the muscularis propia with pronounced atrophy of muscle fibres (fig 1). Eosinophilic hyaline globular inclusions were detected within smooth muscle cells, predominantly in the perinuclear regions. Masson-trichrome stain revealed fibrous tissue deposition around atrophic muscle bundles. The neuronal plexus was entirely preserved. Histological findings were compatible with an idiopathic visceral myopathy. Positive immunofluorescence staining for anti-IgA and anti-IgG was found in degenerated muscle fibres but not in areas of intact musculature (fig 1). Nine months post transplant, a full thickness biopsy of the intestine showed no evidence of recurrent disease in the graft. The patient’s ANA became negative one month after transplant and remained undetectable after 15 months of follow up.

Only one similar case of a two year old boy who developed intestinal pseudo-obstruction following an episode of gastroenteritis has been reported. In that case, ANA, anti-neutrophil cytoplasmic, and antimicrofold muscle antibodies became negative on

Table 1 – 94delATTG allele and genotype frequencies in British ulcerative colitis (UC) cases and controls

<table>
<thead>
<tr>
<th></th>
<th>n</th>
<th>WW (%)</th>
<th>WD (%)</th>
<th>DD (%)</th>
<th>Frequency of D allele (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Controls</td>
<td>675</td>
<td>231 (34.7)</td>
<td>330 (50.1)</td>
<td>94 (15.2)</td>
<td>39.7</td>
</tr>
<tr>
<td>UC</td>
<td>472</td>
<td>170 (36.0)</td>
<td>225 (47.7)</td>
<td>77 (16.3)</td>
<td>40.1</td>
</tr>
</tbody>
</table>

WW, wild insertion homogyzote; WD, heterozygote; DD, deletion homogyzote; D, 94 del ATTG allele
Correspondence to: G Gondelesi, Surgical Director of

our report is the first to describe an adult

and may only represent a late stage of a

humoral immune system. The findings how-

In summary, some patients with idiopathic CIP0 may suffer from a primary intestinal autoimmune disease, an autoimmune pro-

cess exclusively directed towards the intest-

e. An early full thickness intestinal biopsy

may indicate the need for immunosuppres-

sion. At late stages, timely intestinal trans-

plantation is an acceptable option before

patients develop irreversible liver disease.**

HCV genotype 2 as a risk factor for reactivation of chronic HCV infection

Little information is available in the literature on the exacerbation of chronic hepatitis C (r-CHC).} **Taiwan, Sheen et al estimated an annual incidence rate of 11.9% in this study. 40.2% of 78 patients experienced at least one episode of reactivation during a mean observation period of six years and a total of 151 episodes of reactivation were observed, 45% of them symptomatic. The paper by Rumi et al from Milan (Gut 2005;54:402–6) on r-CHC in relation to hepatitis C virus (HCV) genotyping described it as frequent in patients with genotype 2c (39% of 100 patients) and infrequent in those with genotype 1b (7.5% of 106 patients), with a rate ×1000 persons/year of 5.6 and 15.0, respectively. From January 2002 to the present, we have enrolled 49 consecutive patients with acute hepatitis C (AHC group) and 97 consecutive patients with r-CHC (r-CHC group) in a prospective follow up study. All patients were hospitalised at our ward because the illness was symptomatic.

The criteria for a diagnosis of AHC were: (a) negative serum anti-HCV and normal serum alanine aminotransferase (ALT) levels in the four months preceding the onset of symptoms; and (b) positive anti-HCV/HCV-RNA and increased ALT (>5 times the highest value of normal) during the acute stage of the illness. The diagnosis of r-CHC was made for patients with: (a) positive serum anti-HCV and plasma HCV-RNA during the six months before the onset of symptoms and on admission; and (b) ALT increase >5 times the mean of the ALT values observed during the previous six months. As a control group for patients in the r-CHC group, 57 hepatitis B virus surface antigen (HBsAg) negative, symptomatic free, untrained patients with chronic hepatitis C (CHC group) hospitalised in the same period for their first liver biopsy, were pair matched by age (±5 years), sex, and risk factors for acquisition of parenteral infection.

All patients in the r-CHC and CHC groups lacked serum HBsAg, antibodies to hepatitis B core antigen (anti-HBc) IgM, anti-hepatitis D virus (HDV) and anti-hepatitis A virus IgM, and IgM to the herpes viruses. Excluded were patients treated with interferon and ribavirin in the last 24 months, anti-human immunodeficiency virus (HIV) positive subjects, those with a history of alcohol abuse, and those treated with potentially hepatotoxic drugs. Plasma HCV-RNA was determined by qualitative reverse transcriptase-polymerase chain reaction (HEPA-Check; C Nuclear Laser Medicine) and HCV genotyping by Line-Probe Assay (INNO-LIPA HCV II; Innogenetics). Anti-HCV, anti-HIV, HBV, and HDV serum markers were determined using a commercial immunoenzymatic assay.

Management of acute pancreatitis

No account of the complications of acute pancreatitis (Gut 2005;54:426–36) would be complete without mention of diabetic keto-acidosis as an association, which is either fortuitous or one which exists as a complication in its own right. Recognition of this association has been inhibited by the complicated relationship of diabetic keto-acidosis, acute abdominal pain, and hyperamylasaemia, notwithstanding the

**Ghirardo and B Sauter contributed equally to this letter.

do: 10.1136/gut.2005.069005

Conflict of interest: none declared

References

11 PostScript 1207

www.gutnl.com on September 23, 2023 by guest. Protected by copyright.
fact that, as long ago as 1961, a patient with subsequent post mortem validation of acute pancreatitis did present with sudden deterioration of diabetic status, the latter being characterised by unequivocal diabetic ketoacidosis.

Subsequently, it was also recognised that diabetic ketoacidosis could present with acute abdominal pain and elevation in serum amylase (even beyond four times the upper limit) without necessarily signifying acute pancreatitis. The relationship between the two disorders was clarified by a recent study comprising 100 consecutive episodes of diabetic ketoacidosis in which all patients with either abdominal pain or elevation in serum amylase to “more than three times normal” had an abdominal computerised tomography (CT) scan. Eleven per cent of patients had CT evidence of acute pancreatitis, and this was associated with abdominal pain in eight. Among the three without abdominal pain was one who was comatose on admission. Accordingly, although in the context of diabetic ketoacidosis and abdominal pain the presence of “pancreatitis levels” of serum amylase does not necessarily signify acute pancreatitis, it is nevertheless also true that unequivocal acute pancreatitis can be associated with diabetic ketoacidosis, the latter being either a complication or a coincidence. Either way, this is an association which has to be acknowledged rather than ignored, given the prevalence of the association (11% of 100 consecutive cases), the potential lethality of either of the two disorders, and the fact that, at least one of the complications of diabetic ketoacidosis, namely, acute respiratory distress syndrome, can be identical in its presentation with its counterpart in acute pancreatitis.

O M Jolabe
Correspondence to: Dr O M Jolabe, Manchester Medical Association, Manchester, UK; oscajolobe@yahoo.co.uk
Conflict of interest: none declared

References

 BOOK REVIEW
Kirsner’s Inflammatory Bowel Disease, 6th edn

This single volume comprehensive reference tome on inflammatory bowel disease (IBD) is now in its sixth edition, having been re-issued five yearly for the past 30 years. Balfour Sartor and William Sandborn have extensively revised it, with a greater focus on basic science and translational areas. Indeed, the first third of the book covering basic science issues is exceptionally good, and would make a superb background primer for investigators setting out in the IBD research field. The clinical sections thoroughly cover the expected areas: diagnosis, including endoscopy, imaging and laboratory investigations; medical and surgical therapy; and complications associated diseases. The medical therapy section is particularly strong, as one would hope given the authors are some of the leading study investigators, with first rate sections on somewhat neglected areas such as clinical trial design, clinical pharmacology, and pharmacoeconomics. There are numerous diagnostic and therapeutic algorithms throughout.

The entire book has a nice feel—very clear layout, compact text (even more compact references), clear figures, and comprehensive tables. The latter often provide a rapid guide to the key studies—for example trials of nutritional therapy and strictureplasty in Crohn’s disease. Unfortunately, a few of the tables have been poorly edited, with unreferenced citations or poor layout, but these are the minority. There are also a few areas of overlap between chapters (50 in all)—for example, two chapters covering different aspects of the genetic advances in IBD pathogenesis. Use of colour is a little sparse; in a book of this cost I was disappointed to find some histology slides reproduced in black and white. Although the editors are proud of the short seven month final submission to publication timeline, this nevertheless means today’s purchaser of the book (perhaps having read this review) is getting a text written in mid-2003. I still like the book format however and find it quick and easy to use. To research a topic I would happily look first in Kirsner’s Inflammatory Bowel Disease and obtain more recent papers with a PubMed search. A personal copy is a luxury but the book would be a good buy for a department or institutional library.

How does it compare to the competition? To my surprise, an Amazon search generated a list of over a hundred books on inflammatory bowel disease. While most of these were monographs, or covering highly specific topics, there were several other comprehensive general IBD textbooks. Those with a recent edition (last three years) included hardbacks edited by Satgangi and Sutherland (Churchill Livingstone) and Cohen (Humana Press). The Satgangi and Sutherland text was described by a recent Gut reviewer as the “Ferrari” of IBD books (Gut 2004;53:1880) and has a predominantly European outlook. Sartor and Sandborn differs in its mainly North American viewpoint (three quarters of the 87 contributors) but the books have more similarities than differences, are both good, and which to buy comes down to a matter of personal preference. If pushed to choose, I would probably go for Sartor and Sandborn, based on the more attractive cover, easier to read text and tables, and lighter weight.

D A van Heel

NOTICE
First Beijing International GI Summit: call for papers

Researchers, academics, and technology companies are all encouraged to submit their posters for consideration by this unique international collaborative conference organised by the Digestive Disease Research Center of the University of Peking, China Medical Tribune, and Jounal Watch Gastroenterology, with support from the New England Journal of Medicine.

Poster space is limited and gastroenterologists interested in submitting a poster should send a scientific abstract of not more than 250 words (English) or 500 characters (Chinese), full contact information, and a US$50 non-refundable application fee to The Goodwin Group, 79 Broadway, Suite I, Arlington, MA 02474, USA. Electronic submissions can be sent via email to goodwingroup@comcast.net. Submissions are due no later than 15 August 2005.

The summit is scheduled to take place on 5–6 November 2005 at the Golden Resources Hotel, Beijing. For more information about the first International GI Summit in Beijing, please visit the conference web site at http://www.gisummit.com.

Abstract 420 of supplement II, BSG Annual Meeting Abstracts, April 2005, is incorrect (A prospective audit to establish if infliximab is safe to be administered by a nurse specialist in a district general hospital. Thomson et al, p A111). The two letter abbreviation of CD was mistakenly changed to Coeliac disease rather than Crohn’s disease throughout the abstract.

www.gutjnl.com