LETTERS TO THE EDITOR

Cancer in an ileoanal reservoir

Sir,—Dr Appelman (Gut 1990; 31: 1161) has asked how we determined that the carcinoma within the rectal cuff surrounding an ileoanal reservoir that we described in our article was a primary one and not a metastatic tumour. If one were only to assess the histology presented then the question would be extremely difficult to answer, as he has pointed out. In standard clinical practice, however, difficult diagnoses such as this are generally made by the clinician and pathologist in concert, considering both clinical and pathological features. Thus, if we do so in this case we believe we can defend the argument that we put forward.

To establish a diagnosis of metastatic cancer we would have to accept that the original cancer which was resected in 1977 metastasised 10 years later in 1987. Moreover, the presence of metastases would have been a selective transcœlomic spread to predominate within the cuff of the new pouch with no evidence of other metastases to the true pelvis or to the remainder of the abdominal cavity. Although this is technically possible, the combination of events would be extraordinarily unlikely.

In favour of a new primary cancer the following evidence can be considered. The tumour predominated grossly within the cuff. Secondly, the original indication for the pouch procedure was for severe dysplasia in the remaining rectal stump.

Although it is not possible to clearly distinguish the two diagnoses on microscopy alone, the weight of evidence is sufficiently compelling in favour of a primary tumour developing in the mucosal fragments of the rectal cuff that we feel justified in submission of the title of this article and its substance.

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Interleukin 1 in ulcerative colitis

Sir,—We read with interest the recent article by Liguonsky et al in which the authors reported a significantly higher interleukin 1 (IL 1) content and release from colonic mucosa of patients with untreated active inflammatory bowel disease, compared with that of control subjects.

We have recently conducted a similar study determining the IL 1 production from fresh and cultured biopsy specimens and its serum concentrations in 15 patients with active ulcerative colitis, 16 with ulcerative colitis in remission, and 13 normal control subjects. The disease activity was assessed clinically, sigmoidoscopically, and histologically, by using the criteria of Trueove and Richards.

IL 1B was measured using ELISA (Cytoxon Biotechnologie). Mucosal biopsy specimens, obtained at colonoscopy, were weighed (average weight 10 mg), washed vigorously in 1 ml of 9% sodium chloride solution, and then cultured for 24 hours in 10% fetal calf serum/RPMI.

IL 1B activity was determined in the 1 ml of washing solution and in the medium after the culture.

Only slight IL 1 activity was detected in three plasma samples, all from patients with active disease, confirming IL 1 production is only rarely found in plasma, even in active disease.

Fresh and cultured colonic mucosa obtained from patients with ulcerative colitis in remission produced significantly higher values of IL 1 compared with control mucosa (p<0.01). Furthermore, a random sample of patients with active disease produced significantly more IL 1 than those from patients with disease in remission (p<0.01).

In conclusion, our findings are very much in agreement with those of Liguonsky et al. We also found a significantly higher IL 1 production in active patients than in those with ulcerative colitis in remission. The determination of IL 1 production from fresh colonic mucosa in the washing solution seems to represent a reliable method.

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Non-steroidal anti-inflammatory drug induced enteropathy

Sir,—We read with interest the article by Bjarnason et al on treatment of non-steroidal anti-inflammatory drug (NSAID) induced enteropathy. We wish to comment on some aspects of their study.

Although the authors set out to study the effect of sulphasalazine on the intestine in arthritic patients on NSAIDs, they have failed to design adequately the study to test their hypothesis. Such a drug study should have been done as a randomised double blind study. Particularly since the main outcome measure was estimation of faecal excretion of 111bium labelled leucocytes. Inaccurate collection represents a potential major source of bias.

The evidence of small intestinal inflammation and subsequent improvement was provided by the above test. However, this test is unable to distinguish between leakage of leucocytes from large or small bowel. There is increasing evidence that NSAIDs produce inflammation in large as well as small intestine.

The authors' finding that patients on gold therapy did not show deterioration in leucocyte excretion is interesting. This seems to contradict current evidence which suggests that gold can be acutely toxic to buccal, gastric, and colonic mucosa. Do these findings in fact suggest that (as with NSAID treatment) initial mucosal membrane damage heals in spite of continuing therapy with development of tolerance?

Further well designed studies should be done to confirm these important findings by Bjarnason et al before sulphasalazine can be considered as the preferred second line therapy in arthritic patients on indefinite NSAID therapy.

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Reply

Sir,—It is not possible to construct a double blind trial without preliminary studies to show rates and magnitude of change.

Your correspondents fail to grasp the advantage of the 111bium leucocyte technique. The four day faecal excretion of 111bium is an objective and not a subjective measure of intestinal inflammation. Inaccurate faecal collections do not apply to our studies that are carried out in a purpose built metabolic research ward, where there is access to an open lavatory. All that the patient passes is collected (or spit and therefore recovered).

Saverymuttu et al are misquoted by your correspondents when they suggest that the 111bium leucocyte technique cannot distinguish between small and large bowel inflammation. The article specifically comments on the accuracy of the technique for localising colonic disease but it may slightly underestimate the extent of small bowel disease.

While we agree that there is increasing evidence that non-steroidal anti-inflammatory drugs (NSAIDs) produce inflammation in the large bowel, the reference to this in a letter describing two patients on a number of drugs capable of damaging the large bowel is hardly appropriate and the association is refuted in a simultaneously published letter. There are two recent comprehensive reviews on the effect of NSAIDs on the large bowel. We scanned 60 patients in our study and found no evidence of colonic inflammation. Neither did De Vos et al during colonoscopy in more than 200 patients on NSAIDs.

The mucocutaneous reactions to sodium aurothiomalate cited are very rare and tend to be idiosyncratic. No such patients were included.

We never suggested that sulphasalazine should be the preferred second line therapy in rheumatoid arthritis, nor would we contemplate its use in arthritic patients on indefinite NSAID therapy.

There is no evidence whatsoever that the small intestinal mucosa develops tolerance to NSAIDs.

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Duodenal ulcer and carbohydrate

Sir,—We have read with interest the results of the study from Nottingham by Katschinski et al. (Gut 1990; 31: 993-6) concerning the association between duodenal ulceration and fibre and refined carbohydrate intake. The findings suggested that relative risks were reduced by a high vegetable fibre and low refined sugar intake but not a high intake of cereal fibre.

We have continued to gather information about the geographical distribution of duodenal ulceration and the staple diets of high and low incidence areas, and we find no correlation between duodenal ulcer incidence and fibre intake alone. There are high incidences in Ethiopia, Rwanda, and Burundi and in sorghum eating areas of India where the fibre intake is high.

The overall picture suggests that areas where polished rice, yams, or cassava are the staple foods the duodenal ulcer incidence is high. Where unrefined wheat, soya, sultas or millets, or certain green vegetables form a large part of the staple diet the incidence is low.

Experimental work on several animal models of peptic ulceration shows that the foodstuffs mentioned above low incidence areas contain a protective fraction which is liposoluble. The fraction is present in wheat bran but to a less degree in wheat germ. We think that it is a protective factor present in certain high fibre foods and not the fibre itself that protects against duodenal ulceration.

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3 Tovey FI, Jayaraj AP, Clark CG, Fibre and duodenal ulcer. Lancet 1982; ii: 878.

Lipid pattern and plasma insulin in diabetics with gall stones

Sir,—We read with great interest the paper by Laasko and colleagues (Gut 1990; 31: 344-7) regarding the relation between serum lipids, plasma insulin, and gall stones in non-insulin dependent diabetic women. The authors suggest that diabetics with gall stone disease have higher fasting insulin concentrations and lower total and low density lipoprotein cholesterol than diabetics without gall stones.

In the introduction they state that no studies have been published comparing lipids and lipoproteins in diabetics with or without gall stones. Some years ago we reported different results on the relations between gall stones and serum lipids in non-insulin dependent diabetic patients. We studied total cholesterol, serum triglycerides, and apolipoproteins A I and B in 81 subjects with non-insulin dependent diabetes mellitus affected by gall stones and 305 diabetics without gall stone disease. We documented increased concentrations of triglycerides and decreased levels of apolipoproteins A I in diabetic women with gall stones compared with those without, while no difference was shown in men. Total cholesterol and apolipoprotein B concentrations did not differ between groups. The observation of high concentrations of triglycerides in gall stones has been reported by most authors. This experience is in agreement with published papers and conflicts with the data reported by Laasko. Our finding of such an association only in women agrees with the observation of more severe lipid alteration in women than in men, an association of gall stones, low concentrations of high density lipoprotein cholesterol, and coronary disease found only in women.

The serum lipid pattern in our patients might be related to insulin concentrations, as suggested by Laasko et al in another paper. In this regard, in a case-control study (34 patients with gall stones and non-insulin dependent diabetes mellitus v 30 controls without gall stones, comparable for sex, age, body mass index, and duration and metabolic control of diabetes) we also documented increased values of C peptide in subjects with gall stones compared with controls.

Concerning the possible mechanism by which hyperinsulinaemia could enhance gall stone formation, Laasko et al report that high insulin concentrations could activate low density lipoprotein receptors and increase plasma-bile clearance of low density lipoprotein cholesterol. It, however, has also been reported that insulin is able to enhance the activity of f-hydroxy B-methylergosterol reductase and enzyme A reductase and to suppress s alpha-hydroxylase with consequent increased cholesterol and decreased bile acid secretion in bile. According to this finding Bennon and Grundy showed that insulin administration in non-insulin dependent diabetics could increase cholesterol saturation of bile. In a preliminary retrospective evaluation of 386 subjects with non-insulin dependent diabetes mellitus we showed a significantly higher frequency of gall stones in patients treated with insulin compared with those being managed by diet or oral hypoglycaemic agents. This finding seems to support the hypothesis of an increased risk of gall stones in diabetics treated with insulin, but prospective investigations on this topic are necessary.

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