Cytokine production in inflammatory bowel disease

EDITOR.—We read with considerable interest both the paper by Dr Mazlam and Professor Hodgson concerning cytokine production in inflammatory bowel disease (Gut 1994; 35: 77-83), and the accompanying leading article. A number of points emerge that are worthy of further discussion.

There are many practical difficulties in attempting to compare, quantitatively, the acute phase response in Crohn’s disease with the response in ulcerative colitis. In particular, it is critically important to match patient groups precisely for disease activity, extent, and drug treatment before drawing any conclusions regarding differences in monocyte cytokine production in Crohn’s disease and ulcerative colitis.

Mazlam and Hodgson have used clinical indices of disease activity only, and provide no details of histology or endoscopic appearances. These clinical indices are subject to considerable criticism, and reappraisal. Moreover, in the patient group described as having ‘active’ ulcerative colitis, seven had mildly and four moderately active disease. No patients with symptoms of severe active colitis—the most likely to have an acute phase response, and systemic illness—were included. Therefore, we would suggest that there is not sufficient information to draw any valid conclusions regarding cytokine production in acute inflammatory bowel disease.

In their study, most patients with ulcerative colitis, had limited distal disease. Only one of 22 patients had total colonic involvement. It is not only our clinical experience but also well reported1,2 by other authors that patients with proctitis or distal ulcerative colitis may fail to display an acute phase response, as assessed by C-reactive protein and monocyte or erythrocyte sedimentation rate. Normal values may occur, even in patients with symptoms of severe acute colitis.

Mazlam and Hodgson have themselves recently shown the effects of antibiotic interventions (and 5ASA drugs) on cytokine production by peripheral blood monocytes.3 Other workers have described4 an inhibitory effect of sulphasalazine and SASA on the actions of tumour necrosis factor. This important aspect, however, is not discussed in their paper. We would like to know whether corticosteroid treated patients had active disease at the time of venesection, and the relation between drug therapy and monocyte cytokine production, for individual patients.

Although previous studies (including our own early findings5) have suggested differences in cytokine production by peripheral blood mononuclear cells in ulcerative colitis and Crohn’s disease, we continue to have reservations. There remains a need for a further study, using well matched groups of patients with active disease. Studies of mucosal cytokine production in such patients are also needed.


Reply

EDITOR.—Satsangi and Jewell raise a considerable number of points relevant to our immunological studies in inflammatory bowel disease, which though well recognised by workers in the field perhaps merit a general airing.

Any investigation of immunological aspects of inflammatory bowel disease must indeed attempt to describe as far as possible the extent, distribution, and activity of the inflammatory process at the time. We have considered this topic in its own right in a recent review.2 We feel guilty to not having provided simultaneous histological and endoscopic assessment of all our patients at the time peripheral blood was taken for assessment of cytokine activity—even in the 14 Crohn’s patients who had ileal involvement or ileocolitis this would have presented formidable difficulties; more seriously, we worry that Satsangi and Jewell are taking us to task for not accurately matching patients with Crohn’s disease with patients with ulcerative colitis. That is clearly impossible, given the differences in distribution of inflammation in patients in whom a firm clinical distinction can be made. If we match two patients with similar degrees of mucosal inflammation who have been limited to the mucosa extending for a similar distance proximally from the rectum, we may have difficulty persuading the reviewers we are comparing ulcerative colitis and Crohn’s disease!

With respect to drug treatment: in preparing the paper we assessed whether or not the use of anti-inflammatory drugs could explain the differences in cytokine production by either group. We did not find evidence of inflammatory bowel disease when active and, or, between ulcerative colitis and Crohn’s patients with similar disease activity. Clearly, as in most published studies in inflammatory bowel disease, many patients become small (that is, subgroup — ulcerative colitis, inflammation active, distribution left sided, corticosteroid treatment—local yes, systemic no, salazopyrine treated), as identified by the analysis is therefore impressive. On that level neither the use of corticosteroids or aminosalicylates as treatment abolished cytokine generation, or explained the differences noted. Incidentally, the correspondents’ own work has shown the ability of mononuclear cells from inflamed inflammatory bowel disease tissue to continue to produce abnormally high amounts of cytokines despite corticosteroid treatment.2

We look forward to the correspondents’ next contribution in this field, and hope it may resolve both their and our reservations.

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Oesophageal acid clearance

EDITOR.—The report of reduced oesophageal acid clearance in patients with progressive systemic sclerosis by Basilio et al (Gut 1993; 34: 1487-91), reflecting the disordered oesophageal motility in this condition, would prompt clinicians to use more potent gastric acid inhibitory treatment for oesophagitis in such patients. Such a policy, however, might not be without hazard, particularly with respect to the frequency of oesophageal candidal infection.

Hendel et al found Candida albicans in oesophageal mucosal brushings from 44% of systemic sclerosis patients, but in a subgroup of patients treated with either high dose ranitidine (more than 300 mg/day) or omeprazole (40 mg/day) for oesophagitis, the frequency rose to 89%1. Hence, oesophageal dysmotility predisposes to candidosis but inhibition of gastric acid secretion significantly enhances the risk. On the basis of such results and other reports of candidiasis complicating therapeutic interventions producing hypopacidity, it has been suggested2 that physiological gastro-oesophageal acid reflux is a major protective action against oesophageal candidiasis, and that diminution or abolition of acid reflux by agents such as H2 receptor antagonists or omeprazole may exacerbate the risk of developing candidiasis, particularly in patients with impaired oesophageal motility. These considerations suggest that, notwithstanding their impaired oesophageal acid clearance, potent inhibitors of gastric acid secretion such as proton pump inhibitors should be prescribed with caution in patients with systemic sclerosis,1 or in conjunction with prophylactic anti-candidal treatment such as nystatin or, preferably, fluconazole.

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Reply

EDITOR.—Larner’s letter raises the issue that gastric acid inhibition may increase the frequency of oesophageal candidal infection in patients with systemic sclerosis, and suggests