of cytokines such as transforming growth factor \( \beta \) is likely to be an epiphenomenon of the inflammatory process. Finally, we support the view of Cavallini et al that while calcification might contribute to the pathogenesis of chronic pancreatitis in its later stages, there is no well documented evidence fulfilling Koch's postulates that it is probably a significant aetiologic factor in the disease. Hence, we also cannot support the hypothesis of the Marseilles school.3

We consider that the disease known as 'chronic pancreatitis' is not a single pathological entity but rather a group of different aetiologies and pathogenetic processes sharing a few common morphological end points.3 Within this overall group, we anticipate that a precise definition of the disease and an idealisation of the genetic abnormality is the likely primary aetiologic factor responsible for at least a proportion of cases of chronic pancreatitis. Whether this defect occurs within the pathway of alcohol metabolism or is responsible for promoting an appropriate cell mediated cytotoxic response to some pancreatic cellular antigen is presently unknown. Nevertheless, responses to antigens are vital if biologically appropriate treatment regimens for different aetiologically and pathogenetically distinct types of 'chronic pancreatitis' are to be developed and affected patients treated more rationally than at present.

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Diagnosis of invasive amoebiasis: renaissance of the morphology era

EDITOR—In his leading article entitled Diagnosis of invasive amoebiasis time to end the morphology era (Gut 1994; 35: 1018–21), Professor Ravdin rightly emphasizes that microscopy cannot differentiate between pathogenic and non-pathogenic strains of Entamoeba histolytica. This applies equally to the cysts, and cultured trophozoites. Professor Ravdin goes on to suggest that microscopy should be abandoned in the diagnosis of invasive amoebiasis. He has underestimated the diagnostic value of one important point described by Losch in 1875.1 In clinical specimens from patients with amoebic dysentery trophozoites of E histolytica may be seen with ingested red cells. These erythrophagocytic trophozoites are a specific feature of infection with invasive strains of E histolytica and may be seen on microscopy of fresh faecal specimens or in fixed smears stained with Field's stain. The current recommended test for the diagnosis of amoebiasis are microscopical examination of fresh stools and stained fixed faecal smears, and amebic cultures of stool specimens.2 3 A study4 of patients with dysentery, diarrhoea, and asymptomatic carriage of pathogenic and non-pathogenic E histolytica confirms that microscopy is a highly efficient diagnostic method for amoebic dysentery. The sensitivity and specificity of erythrohagocytic trophozoites were 96% and 100% respectively, when compared with amebic culture and subsequent zymode type of the isolated strains to confirm pathogenicity. We agree with Professor Ravdin that there is a need for diagnostic methods that do not depend on the detection of intact parasites. Tests that detect faecal or circulating amebic antigen, or both, are appropriate to this purpose. To validate such assays they must be compared with a gold standard. With the current state of knowledge the standard should include the finding of erythrohagocytic trophozoites. We do not agree that recently available antigen detection tests have supplanted microscopy in the diagnosis of amoebiasis. Many things have changed since 1875 but the findings of erythrohagocytic amoebic trophozoites in stool specimens remains the simplest, cheapest, and most reliable test in the diagnosis of amoebic dysentery. Welcome to the morphology era!

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Relation of acupuncture and vagal gastric acid secretion

EDITOR—We read with interest the paper of Lux et al (Gut 1994; 35: 1026–9), which examines the effects of various forms of acupuncture on stimulated gastric acid output in healthy volunteers. These results are entirely in accord with our own findings, previously published both in abstract form (the first of which was in this journal six years ago) and in a peer reviewed journal, that acupuncture produces a significant decrease in sham feeding stimulated acid output under randomised, placebo controlled conditions in humans.

In those studies we also showed that the effects of acupuncture decreased sham feeding stimulated acid output was through naloxone sensitive opioid mechanisms, involving vagal efferent pathways. Furthermore, acupuncture produced neither a decrease in gastrin release nor a diminished parietal cell sensitivity to gastrin. While we agree with Lux et al that the mechanism through which acupuncture exerts its effect is not fully elucidated, it seems to be at least in part mediated via opioid pathways, which may be similar to the mechanisms participating in the analgesic properties of acupuncture.

In their report, Lux et al cite another of our publications as concluding that acupuncture accelerates peptic ulcer healing.1 This is incorrect; the cited study was conducted in healthy volunteers to examine the effects of acupuncture on sham feeding stimulated acid output. In our comprehensive review of all the


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published literature on the gastrointestinal effects of acupuncture, we were unable to find any controlled study showing the effect of acupuncture on ulcer healing.4 There have been three reports, however, of uncontrolled studies5-7 suggesting that acupuncture may be of therapeutic benefit in peptic ulcer disease and Lux et al cited one of these. It is unfortunate that Lux et al failed to recognise our own work, but more so that they did not extend our initial studies into the mechanisms participating in the inhibition of acid secretion by acupuncture.

Clearly, we agree with them that further studies are needed to examine therapeutic efficacy.

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Cancer surveillance in ulcerative colitis

EDITOR,—In re-reading the editorial I wrote (Gut 1994; 35: 587-9) I have identified an error that I wish to correct.

Paragaphs six and seven state that the analysis of 11 prospective colonoscopic surveillance studies compared groups of patients with and without low grade dysplasia. This is incorrect, the two groups compared were all patients submitted for surveillance on the one hand and those found initially to have low grade dysplasia on the other.

Sentence two in paragraph six should have read ‘In all, 73 cancers were found in 1656 patients (4.4%) whereas 26 cancers were found in the subgroup of 313 patients with low grade dysplasia (8.3%). If dysplasia associated lesions or masses are excluded this falls to 6.2%’. A similar mistake occurs in paragraph seven.

The second sentence of which should read ‘In all, cancer was present in 93 of 2044 patients (4.5%) whereas 35 cancers were found in 101 patients with high grade dysplasia (35.3%)’. I apologise for the inaccuracies detailed above.

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Colorectal tumorigenesis

EDITOR,—We noted with interest the paper by Mulder et al (Gut 1995; 36: 76-80) on expression of mutant p53 protein and CD44 variant proteins in colorectal tumorigenesis. The authors in their report have shown that CD44 v6 expression is restricted to moderately and severely dysplastic adenomatous polyps and colorectal cancers, but that it is not expressed in normal colon and mildly dysplastic adenomas. They also suggested that CD44 v6 expression is associated with tumour progression. We have studied CD44 v6 in frozen and paraffin wax embedded tissue sections from 11 normal colons, eight adenomatous polyps, and in 18 colorectal adenocarcinomas, with immunohistochemistry using anti-CD44 v6 antibody.1 In contrast with Mulder et al we found expression of this variant in normal colonic crypt epithelium, and similar expression was also seen by Fox et al.2 We also detected CD44 v6 protein in all eight adenomatous polyps irrespective of the grade of dysplasia, and in 15 of 18 colorectal adenocarcinomas. The positive colorectal cancers CD44 v6 expression was strong and homogeneous in three, and heterogeneous and weak in 12. Survival at five years was: 0 of 3 in patients with homogenous, 9 of 12 with heterogeneous and 2 of 3 in negative cases. In colorectal adenocarcinomas, Mulder et al saw a correlation with Duke’s stage and tumour progression. Our study shows no apparent correlation of CD44 v6 expression and tumour progression, there being no linear trend with Duke’s stage or differentiation. The decreased survival of patients with colorectal cancer who express CD44 v6 strongly and homogeneously, however, suggests that this expression may be an independent adverse prognostic marker rather than a determinant of tumour progression.

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Reply

EDITOR,—We appreciate the comments on our article concerning p53 and CD44 expression in the adenoma-carcinoma sequence. The authors point to some discrepancies with their own results and these differences are not easily explainable. We assume that they used different antibodies, similar to the ones used by Fox et al. It is noteworthy that Fox et al found only weak positivity in the bottom of the crypts. The authors also mention the use of both paraffin wax embedded and fresh frozen tissue, but it is not clear from their writing from which of these two the presented numbers are derived. In our hands antibodies against CD44 v6 give only reliable results on fresh frozen tissue. Finally, their findings of prognostication are comparable with ours,1 which we consider reassuring as far as the value as prognostic marker of CD44 is concerned.

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