

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.

G TOUGAS
R H HUNT
Division of Gastroenterology,
McMaster Medical Centre,
1200 Main Street West, Room 4W8,
Hamilton, Ontario L8N 3Z5, Canada

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Reply

EDITOR,—We should like to thank Dr Tougas and Professor Hunt for their interest in our study. The aim of our investigation was to compare different techniques of acupuncture in terms of their effect on gastric acid secretion stimulated by sham feeding. The results showed that, under the conditions of our study, only certain forms of acupuncture, namely electroacupuncture and transcutaneous electrical nerve stimulation, but not laser acupuncture or the classic needle acupuncture led to a significant inhibition of gastric acid secretion. Regrettably, the very interesting investigations into the mechanism of action of acupuncture on gastric acid secretion carried out by Tougas *et al*¹ were unfamiliar to us at the time of completion of our own study because of publication date, and for this reason were not cited. Had we known of the study, we would, of course, certainly have made reference to it. Instead, we cited the earlier results of this group, which were available in the form of an abstract.² In our introduction, the work of Li² was cited not only (correctly) with respect to earlier results on human gastric acid secretion, but also – erroneously – as one of two studies concerned with ulcer healing. In the discussion, however, the results reported by Li *et al*² – as warranted by the importance of their study – were again cited in detail. Our views about ulcer healing are in complete accord with those of Tougas *et al*, as we too

pointed out that the effect of acupuncture in this area has yet to be proved.

We are of the opinion that the results of the study performed by Tougas *et al*¹ together with our own results complement each other, and we hope that they will stimulate further studies on the mechanism of action of acupuncture on gastrointestinal functions, in particular on its effect on gastrointestinal diseases.

G LUX
J HAGEL
P BAECKER
B BAECKER
R VOGL
H RUPPIN
S DOMSCHKE
W DOMSCHKE
Department of Gastroenterology and
Internal Medicine,
Stadt Krantenhaus,
Gotenstraße 1,
42653 Solingen,
Germany

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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.

Paragraphs 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%)'.

I apologise for the inaccuracies detailed above.

A T RAXON
Centre for Digestive Diseases,
The General Infirmary at Leeds,
Great George Street,
Leeds LS1 3EX

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*.¹ We also detected CD44 v6 protein in all eight adenomatous polyps irrespective of the grade of dysplasia, and in 15 of 18 colorectal adenocarcinomas. In 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 homogeneous, 9 of 12 with heterogeneous CD44 v6 expression, and 3 of 3 in negative cases. In colorectal adenocarcinomas, Mulder *et al* saw a correlation with Dukes's stage and tumour progression. Our study shows no apparent correlation of CD44 v6 expression with tumour progression, there being no linear trend with Dukes's staging or differentiation. The decreased survival of patients with colorectal cancer who express CD44 v6 strongly and homogeneously, however, suggest that this expression may be an independent adverse prognostic marker rather than a determinant of tumour progression.

A M ABBASI
I C TALBOT
Departments of Pathology
and Gastroenterology
St Mark's Hospital, City Road,
London EC1V 2PS
I C TALBOT
ICRF Colorectal Cancer Unit,
St Mark's Hospital, City Road,
London EC1V 2PS

- Fox SB, Fawcett J, Jackson DG, Collins I, Gatter KC, Haris AL, *et al*. Normal human tissues, in addition to some tumours, express multiple different CD44 isoforms. *Cancer Res* 1994; 54: 4539-46.

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 have 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.

S T PALS
G J OFFERHAUS
Academic Medical Center,
University of Amsterdam,
Department of Pathology,
Meibergdreef 9,
1005 AZ Amsterdam,
The Netherlands

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