Correspondence

GI disorders — the name’s the thing

Sir,—In his perceptive and provocative editorial (Gut 1987; 28: 1–4), Dr Read has made a number of points that should not pass without comment. No one could quarrel with his central thesis, which is that progress in understanding the physiology and pathophysiology of small bowel motility is obstructed by our collective inability to agree on precise definitions of ‘functional disorder’ and ‘irritable bowel syndrome’ and, more recently, of ‘chronic idiopathic intestinal pseudo-obstruction’ (or CIIP).

It is the next step of his argument that is contentious. He asserts that ‘knowledge of the range of small bowel motor patterns in normal human subjects is still fragmentary’, thereby implying that there may have really been nothing abnormal about the Mayo patients reported by Stanghellini et al and ignoring the fact that under standardised conditions of study, human small bowel motor activity in health has been well defined. Dr Read may have seen aberrant patterns in his own small bowel while suffering from severe headache, and in other bowels subjected to rapid fluid distension, but these are not the protocols under which data have been acquired from volunteers and patients. In common with the Gastroenterology Unit at the Mayo Clinic, our experience of studying human small bowel motility over seven years amounts to several thousand hours, and confirms that the abnormalities described by Stanghellini et al do not occur in health. And while Dr Read is right to question whether some of the observed effects may have been sequelae of previous surgery, his citation of the study on the effects of vagotomy is misleading; this showed that in some — but not all — patients, the effect of food on motility was attenuated, but in contrast with the Mayo series, never absent.

As he points out, it is difficult to decide whether the Mayo patients were indeed suffering from CIIP, as biopsy material was largely absent, and even when available, was inconclusive. His central questions remain. Were these patients suffering from CIIP? Indeed, is CIIP a useful diagnostic term? The answer to the second question must surely be negative; if so, the first question is meaningless. ‘Enteric neuropathy’ and ‘enteric myopathy’ are to be preferred; the former characterised by abnormal patterns of contraction, as in the Mayo series, and the latter by abnormal, usually diminished, amplitude of contractions similar to those reported in scleroderma. Both neuropathy and myopathy, if sufficiently severe, may produce the clinical picture of intestinal obstruction.

Perhaps it is unwise to aggregate CIIP and the irritable bowel into a common category of ill defined functional disorders. Neuropathy and myopathy are defined pathological conditions, even though diagnosis is not always easy, whereas the pathology of the irritable bowel remains unknown. According to Dr Read, ‘The name’s the thing . . .’. But is it? Undue emphasis on taxonomy obscures the important clinical message of Stanghellini’s paper, which is that intestinal manometry may reveal evidence of organic motor disorders that contraindicate surgery. It is likely that many of Stanghellini’s patients arrived at the Mayo Clinic because of the failure of previous surgery to relieve symptoms. If the original diagnostic investigation had included manometry, this might not only have saved them the journey but also the surgery.

Finally, is he not being unduly pessimistic in his prediction of progress in this field when he comments that ‘the next 20 or 30 years could witness a proliferation of terms describing subsets of the irritable bowel syndrome . . .’? 1987 is only the 10th anniversary of the publication of the first paper describing normal human small bowel motility; since then interest in this field of study has increased exponentially, as has the accumulation of knowledge. By his clear identification of the major issue, Dr Read has encouraged investigators to address pertinent questions, and it does not seem unreasonable to predict that these controversies will be largely resolved within the next quinquennium, if not sooner.

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References
Intestinal permeability and statistics

Sir,—We read with interest the paper by Bjarnason et al (Gut 1986; 27: 1292–7) about the effect of non-steroidal anti-inflammatory drugs (NSAIDs) on the permeability of the human small intestine by using 51Cr-EDTA method. Their conclusions may be summarised as follows: (1) there is a stepwise increase in excretion values of 51Cr-EDTA after ingestion of NSAID according to their potential for inhibiting cicloxygenase. (2) The effect of NSAID in intestinal permeability is systemically mediated. (3) An increased urinary excretion of 51Cr-EDTA is not related to increased glomerular filtration rate due to indomethacin. (4) Prostaglandin E₂ decreases the absorption of 51Cr-EDTA but does not prevent the indomethacin-induced increased intestinal permeability. The paired Student’s t test was used to assess all variables in this study.

In our opinion the statistical analysis in this paper merits some criticism. The Student’s t test for paired data is an appropriate test when used as it was in the above stated third conclusion. There, the authors compare two means obtained in the same individuals, and the t test for paired data is a good choice. When more than two means are to be compared, as in conclusions 1, 2, and 4, analysis of variance is the most useful technique, because it determines whether there are differences between the means of several groups. When significant differences do exist, a multiple comparison test — that is, Scheffé, Tukey, Newman-Keuls and Duncan tests — should be used to assess which are the populations which differ from the others. If multiple t test are used for this latter purpose, the results obtained are difficult to interpret. Each time the test is applied the level of significance increases and therefore there is an increase in the probability of labelling a result as ‘statistically significant’, even when all populations have identical means. Unfortunately this lack of accuracy in selecting the statistical methodology may mislead the reader. This is especially regrettable, as the measurement of intestinal permeability, as shown by Bjarnason et al, promises to be important in the understanding of the physiopathology of IBD, some systemic illnesses and of the noxious effects of certain agents such as alcohol and NSAID’s on the integrity of the gut. This technique is becoming important in clinical research and the results obtained with it have to be carefully handled in order to take best advantage of its possibilities.

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Reply*

Endoscopic sclerotherapy using absolute alcohol

Sir,—Thank you for giving us the opportunity to reply to the letter of Bhargava et al. Gut 1986; 27: 1518. In their experience, sclerotherapy with absolute alcohol resulted in a high complication rate. Whilst respecting their technical skill, a number of minor details need attention to better assess their results. The use of Olympus NM 1 K or 3 K injector is not ideal for intravariceal sclerotherapy. The frequency of inadvertent paravariceal or intramural injection of the sclerosant could be quite high. We therefore, recommend the use of a transparent Teflon injector, through which blood can be seen to flow up into the tube on puncturing the varix ensuring an intravariceal injection. We feel also that the optimal amount of the sclerosant to be used per puncture must be determined. In our experience, ‘blanching’ is a useful indicator when to stop while injecting alcohol or ethanolamine olate. If larger amounts of sclerosant are injected, complications would