LETTERS TO THE EDITOR

Prediction of severity of acute pancreatitis

Sirs,—We read with great interest the article by Fan et al (Gut 1989; 30: 1591–5) in which they have shown that two factors (serum urea and plasma glucose) were as good as the conventionally used multifactor scoring system of Imrie et al1 and Ranson et al2 using nine and 11 factors respectively for clinical and biochemical assessment of severity of acute pancreatitis. The major drawbacks of multifactor include (a) use of too many factors, (b) need for a longer duration (48 hours) before assessment of severity can be made, and (c) effect of treatment on various assessment parameters during 48 hours of observation.

Although fascinating, it seems unlikely that the authors’ new approach — the use of a discriminant value of the two factors (serum urea >7.4 mmol/l and plasma glucose >11.0 mmol/l) in assessment of severity of pancreatitis — would stand the test of time because of the following reasons. Firstly, the raised serum urea has a very non-specific value as it can be altered because of dehydration, repeated vomiting, poor intake, and other non-pancreatic factors like gastrointestinal bleeding and renal dysfunction. Secondly, the occurrence of upper gastrointestinal bleeding (occurring in 10–20% of patients with acute pancreatitis)3 may significantly affect the serum urea concentration even though it may have no relation to severity of pancreatitis.

Thirdly, the authors’ explanation that high serum urea concentration could be a reflection of poor physiologic reserve of major organ system does not seem to have convincing scientific appeal.

Moreover as the plasma glucose intolerance and incidence of diabetes mellitus increase with age and the authors fail to mention whether or not underlying diabetes mellitus was ruled out in their patients with acute pancreatitis, it is possible that a proportion of their patients may have had raised plasma glucose secondary to pre-existing glucose intolerance or diabetes mellitus rather than because of underlying severe pancreatitis.

Finally, we believe that from the standpoint of the clinical management there is no harm in waiting for a day or two to observe the course of acute pancreatitis on conservative treatment even though the course may alter (maybe for the good) the score of the multifactor scoring system, rather than rush to predict the severity of acute pancreatitis at admission.

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Reply

Sirs,—We agree that raised serum urea and blood glucose at the time of admission may be influenced by many factors. This inadequacy was reflected by the relatively low predictive value of positive and negatively discussed in the report. However, all the possibilities leading to raised serum urea mentioned by Dr Arora and Acharya were definitely related to a severe attack of acute pancreatitis and I cannot agree that gastrointestinal bleeding is unrelated to severity.1 Patients with underlying diabetes mellitus were not specifically defined in our report. However, diabetic patients with underlying major organ dysfunction are certainly at higher risk of developing complications of acute pancreatitis and deserve to be carefully monitored and aggressively treated at admission.

The policy of waiting for 48 hours to monitor the course of the disease to collect complete data for grading of severity is not justifiable in modern day medicine. In our previous report, 13–8% of patients deteriorated within 48 hours. With adequate treatment, fewer patients did so.

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Why do patients with ulcerative colitis relapse?

Sirs,—In reply to my letter (Gut 1990; 31: 599) Riley et al state that of the many studies I quoted ‘all are uncontrolled’. This is incorrect and if unchallenged may lead further research workers to discount those criticisms regarding inappropriate methodology which remain valid and thus perpetuate the likelihood of further needless waste of research effort. My 1959 article stated that 98 radiotherapy patients were used as control subjects and interviewed according to the same protocol as the 173 ulcerative colitis patients, while McMahon et al used healthy siblings as controls in their investigation of 23 patients by means of psychometric tests including the Minnesota Multiphasic Personality Inventory and psychiatric interviews. In another investigation of 35 patients entitled ‘Psychopathology of ulcerative colitis’ Roubbiec and Martonova used 20 healthy subjects as controls and confirmed the limited value of standard psychological tests in these emotionally guarded colitis patients by means of sensitive interviewing and the Thematic Apperception Test designed to penetrate emotional defences. Riley et al are right to emphasise the continuing need for ‘controlled clinical trials’ but if the questions asked are irrelevant to pathogenesis, or the instruments of investigation are too blunt for the purpose asked of them, no amount of control data will help. They may even deceive people into thinking that proper scientific rigour has been applied.

Riley et al appear to have listened to commonly recited, but uncorroborated views of others, rather than checked the original sources. Pelser and I have given examples of how this has often delayed scientific progress for years.

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Effects of albumin infusion in cirrhotic patients

Sirs,—Intravenous albumin infusion has been reported as an effective treatment of hypoa- nutraemia in cirrhotic patients with ascites (McCormick et al, Gut 1990; 31: 204–7). The management in renal sodium handling in cirrhosis is well known; however, the mechanisms mediating this abnormality remain incompletely defined.4 Changes in effective circulatory volume trigger hormonal alterations inducing sodium and water retention. A large proportion of cirrhotic patients with ascites formation show decreased effective plasma volume, activated vasoconstrictor hormone systems, hypoalbuminemia, and hypotension.5

We investigated 15 patients with liver cirrhosis and ascites (5 women, 10 men, aged 52–65 years). Patients were on longterm diuretic treatment and a low sodium diet containing 30 mmol/day of sodium. An intravenous infusion of 20% albumin was given in a dose of 1 g/kg. The diuretic and natriuretic responses as well as the albumin induced changes in vasoactive hormone profile were measured.

Albumin infusion induced nearly a fourfold increase in diuresis and sodium excretion in nine of 15 patients (group A), with the normalisation of serum sodium (Table). Albumin also increased the plasma level of arterial natriuretic factor (ANF) to normal, while decreasing the high plasma renin activity

<table>
<thead>
<tr>
<th>Group</th>
<th>U1 (mmol/l)</th>
<th>U1 (mmol/l)</th>
<th>Na (mmol/l)</th>
<th>alb (g/l)</th>
<th>ANF (fmol/ml)</th>
<th>PRA (nmol/1)</th>
<th>AVP (pg/ml)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control</td>
<td>0.7 (0.2)</td>
<td>40 (6.1)</td>
<td>130 (1.4)</td>
<td>29.1 (5.5)</td>
<td>9.7 (1.0)</td>
<td>8.5 (1.5)</td>
<td></td>
</tr>
<tr>
<td>Albinum</td>
<td>2.4 (2.0*)</td>
<td>175 (20.2)</td>
<td>135 (1.2*)</td>
<td>34.0 (1.2)</td>
<td>49.5 (6.0*)</td>
<td>6.5 (0.8)</td>
<td></td>
</tr>
<tr>
<td>Control</td>
<td>1.2 (0.3)</td>
<td>60 (8.0)</td>
<td>136 (2.0)</td>
<td>34.0 (0.7)</td>
<td>37.6 (3.9)</td>
<td>6.0 (0.9)</td>
<td></td>
</tr>
<tr>
<td>Group</td>
<td>0.9 (0.2)</td>
<td>73 (11.1)</td>
<td>136 (1.5)</td>
<td>52.0 (1.2)</td>
<td>31.0 (2.9)</td>
<td>23.0 (0.5)</td>
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</tr>
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</table>

*p<0.001 v control; tP<0.01 v control; tsP<0.001 v group A-control; sP<0.01 v group A-control; sP<0.05 v group A-control.


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Reducibility of oesophageal pH monitoring

SIR,—We were interested to see the data which Vandenplas et al presented on the reducibility of oesophageal pH monitoring used for the diagnosis of gastro-oesophageal reflux in children.1 There are, however, two ways in which our own, similar, study differs from theirs which we think merit discussion. The first is that Vandenplas et al document the child’s meal times, position, and behaviour on the first day and exactly mimic this on the second day. In our study we did not impose such limitations with a view to gaining a better understanding of the likelihood variation in the amount of gastro-oesophageal reflux occurring from day to day. In the normal situation behaviour varies in a way which may influence the pattern of gastro-oesophageal reflux.

The second point which must be discussed relates to the analysis and interpretation of their results. As correctly discussed, the use of correlation coefficients is inappropriate for examining reducibility and difference analysis should be used (as shown diagrammatically and stated in the text), the differences between the two results increase as the mean result increases—that is, the difference is proportional to the mean. Thus the differences should be analysed after logarithmization of the results and expressed either as a ratio or a percentage difference and not as absolute values as used here. Using this method we showed a 95% chance that a second reflux index would be between 27 and 37% the first.

Vandenplas et al’s unlogged results for reflux index give a 95% chance of a second study having a result within 8% of the first (as shown in their Fig 3). This would mean that a reflux index of 10% one study might be followed by a second study with a result anywhere between 2% and 18%. Although none of the children studied had results which changed from normal to abnormal, this potential variation is considered to justify the support that the pH test is ‘highly reducible.’ If the logged results had been used the difference would have been a proportion rather than an absolute value and thus less with lower reflux indices and more with others.

The data presented show that considerable differences may occur in 24 hour oesophageal pH studies in children even when restrictions are placed on their behaviour. Our own study suggests that such restrictions these differences are even greater. This must be taken into account both clinically and in trials of treatment.

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Reply

SIR,—The comments of Dr Hampton and Dr MacFadyen are very interesting and confirm our opinions. The study by Hampton et al is a logical continuation of our study: once pH data had been shown to be reproducible, all factors of possible influence being standardised (our study),1 a second study was necessary to analyse how much previous related factors influence pH data (Hampton study).3 It is logical that data recorded under these study conditions are less reproducible.

The study on the reproducibility shows that if data are clearly within normal ranges there is a good possibility of the results being comparable when recorded a second day. The same is true if data are clearly ‘abnormal,’ although differences between two consecutive recording sessions might be higher. Hampton and MacFadyen confirm this in their letter: if the logged results had been used, there would be less difference with lower reflux indices and more with higher ones. So, the conclusion with unlogged or logged results is exactly the same.

We knew of Bland’s book.4 It is our opinion, however, that it is preferable to compare data in the same way they are presented. Taking logarithmics might be mathematically correct, but pH monitoring data for clinical purposes are presented as such. Reducibility also depends on the material that is used: comparability of simultaneous recordings with two identically located electrode systems for the same patient (r=0.90 for the % time pH <4 reflux index) is statistically inferior to the comparability obtained with two glass electrode systems (r=0.99 for the reflux index).4 Therefore, the results of the reproducibility study of Johnson and Joelsson are excellent and comparable to ours: they report a correlation coefficient of 0.87 for two consecutive 24 hour recordings with antimony electrodes.

We children were lucky to have no patient who changed from ‘normal,’ or the other way around. Normal ranges of physiological gastro-oesophageal reflux have been shown to vary ‘quite a lot’ in large groups of asymptomatic controls.5 The percentiles for the reflux index in a ‘normal’ population were shown to vary from 0 to 10% (P5 to P95).

As we stated in a recent review article,6 the answer to the simple question ‘are the normal or abnormal’ is clear cut in only a few cases. There is a considerable overlap between ‘normal’ and ‘pathological.’ The results of our study show that there are only a few problems with the interpretation of, for example, a reflux index of <5% or >15%, since the risk that the interpretation of the results of a second day’s recording would be different is minimal. There remains a problem for children in only a few cases. The reason for this is that the term ‘normal’ and ‘pathological’ is therefore the use of a cut off limit for a software program pH 3-99 is ‘abnormal’ and pH 4-01 is ‘normal.’ For the patient, however, there is no difference at all. Therefore, we think that the development of new parameters, such as the ‘ocular index’ is of great interest. This parameter calculates the percentage of time the pH oscillates around pH 0.90—that is, between 2 and 37%. This therefore quantifies the all or none consequences of the application of a cut off limit.

Our results show that 24 hour pH monitoring data are ‘highly’ reproducible. When data are checked line by line (and less related to the use of a cut off limit than to patient or technique related factors) they should be interpreted with care, and use of the percentiles as well as the ocular index might be very helpful.

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Effects of albumin infusion in cirrhotic patients.

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