Diagnosis of Wilson’s disease: an experience over three decades

The paper by Gow et al. (Gut 2000;46:415–19) discussed the diagnosis of Wilson’s disease in 30 patients presenting to two different clinical facilities over 28 years (1971–1998). Because a paper of this type is likely to be viewed as an authoritative guide, it is important that the information be valid. For that reason, I call attention to the following significant errors in the paper.

The authors report urine copper values of 5, 4, 7, 4, 5, 2, and 2 µg per 24 hours in seven patients in their table 1. These data cannot possibly be valid. The normal range for urine copper is 20–50 µg per 24 hours, and these patients are far below the lower limit of normal. In performing several thousand 24 hour urine copper tests on patients and normal subjects in our own laboratory, I have never seen one below 10 µg per 24 hours, except in copper deficiency. The patients in table 1 have Wilson’s disease, the opposite of copper deficiency, making the data even more unbelievable. I also do not believe the data for two additional symptomatic patients in their table 1 who are reported to have urine copper values of 55 and 44 µg per 24 hours. Urine copper values in untreated symptomatic Wilson’s disease patients are invariably over 100 µg. That is our experience in all of 88 newly diagnosed neurologically presenting patients (urine copper range 106–1880 µg/24 hours) and in all of 18 newly diagnosed patients with hepatic presentation (urine copper range 106–1880 µg/24 hours). Not all liver disease patients with urine copper values over 100 µg will have Wilson’s disease but all untreated patients with Wilson’s disease with clinically presenting liver disease will have a value over 100 µg. If a patient with liver disease does not have a value that high, then look to another diagnosis, or to a laboratory error. No doubt the latter is the case here because the values in seven of the patients are not biologically reasonable. One caveat: if the patient has been treated with a chelating agent, even briefly, and then the drug stopped, there is often a rebound period when urine copper will drop below 100 µg.

I also question the diagnosis of Wilson’s disease in two of the patients in this series. Patient Nos 20 and 22 had liver disease but were negative for Kayser-Fleisher rings, had a non-elevated urine copper, and normal ceruloplasmin levels. The diagnosis was supposed made in patient No 20 by elevated liver copper. Non-Wilsonian chronic liver disease can also elevate hepatic copper.2 The diagnosis in this patient is uncertain, and disproved if urine copper is truly low. Patient No 22 also had elevated liver copper and a “positive radiolabelled copper plasma clearance test.” Again, hepatic copper is not proof of the diagnosis. The abnormal radiocopper test is supportive but Wilson’s heterozygotes, comprising about 1% of the population, are often falsely positive.3 Urine copper of 44 µg, if valid, would rule out the diagnosis.

In summary, if the data in table 1 of the current paper were to be believed (nine of 22 patients with low or normal copper values), it would appropriately denigrate the great usefulness of urine copper in the diagnosis of Wilson’s disease. Secondly, the liver copper value is not an absolute in the presence of chronic liver disease. Sometimes a therapeutic trial is the only way to come to a decision in these patients.

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References

Author’s reply
Dr Brewer in his letter raises two points regarding our recent paper (Gut 2000;46:415–9). Most importantly, he questions the diagnosis of Wilson’s disease in two patients. Both of these patients were very unusual in that they were both older than 40 years at diagnosis, had caeruloplasmin levels within the normal range, no Kayser-Fleischer rings, and low urinary copper excretion. The diagnosis in these two interesting patients is discussed in the paper. To summarise the discussion: in both patients the diagnosis was based on the findings of markedly elevated liver copper levels (634 and 1037 µg/d dry weight liver, normal liver copper 15–60 µg/d). In addition, the first patient had a family history of cirrhosis and the second had a positive radiolabelled copper plasma clearance test. In this situation the primary differential diagnosis relates to elevated liver copper from chronic cholestasis and liver disease. In both of these patients, chronic cholestatic liver disease was excluded by the absence of cholestatic biochemistry plus a lack of cholestatic findings on histological examination of the explanted liver. The important point to emphasise with these cases is the lack of sensitivity of caeruloplasmin, urinary copper excretion, and Kayser-Fleisher rings in the diagnosis of Wilson’s disease.

Dr Brewer’s second point relates to reported low levels of urinary copper excretion in seven of our patients. In these patients the levels were reported as µmol/24 hours when they should have been converted to µg/24 hours. The true levels for patient Nos 6, 7, 8, 11, 12, 17, and 20 are 312, 117, 437, 1250, 762, 125, and 100 µg/24 hours, respectively.

Dr Brewer also questioned the urinary copper excretion values in patient Nos 16 and 22. In both of these cases the data were rechecked and the reported values were found to be correct.

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Small intestinal bacterial overgrowth, intestinal permeability, and non-alcoholic steatohepatitis

In a recent issue, Wigg and colleagues (Gut 2001;48:206–11) reported that small intestinal bacterial overgrowth (SIBO), as diagnosed by a combined C-o-xylene/lactulose breath test, is significantly more common in patients with non-alcoholic steatohepatitis (NASH) than in control subjects without liver disease. The authors investigated the possible pathogenic significance of this observation by examining whether intestinal permeability and circulating levels of endotoxin and tumour necrosis factor α are increased in NASH patients with SIBO compared with those without. No significant differences in any of these parameters could be demonstrated in the two groups.

An important factor influencing the validity or otherwise of these findings is the diagnostic accuracy of the “C-o-xylene and lactulose breath tests for SIBO. Our experience, using a sterile endoscopic technique to sample small intestinal secretions under direct vision, is that these breath tests lack sensitivity and specificity for culture proven SIBO. Endogenous CO2 production and colonic metabolism of o-xylene are important factors inherently limiting the accuracy of the “C-o-xylene breath test for SIBO. Furthermore, reliance on the finding of “double peaks” in serial breath hydrogen or methane levels after ingestion of lactulose to improve the accuracy of the “C-o-xylene breath test, or as a diagnostic marker in its own right, is problematic. In a study in which a scintigraphic tracer was administered concurrently...
with lactulose, we found that each of the dou-
ble peaks in breath hydrogen values may occur af-
after the arrival of the test meal at the caecum, par-
allelly delivering patterns of fermentable sub-
strate to caecal bacteria. A caecal source of each peak was suggested on 50% of occasions, ra-
ting that the test peak necessarily resulted in small intestinal metabolism of lactulose by over-
growth flora as purported. Conversely, a single rise in breath hydrogen levels commenc-
ing before the test meal reached the caecum was evident in 22% of subjects with culture pro-
ven SIBO. Thus both false positive and false
negative diagnostics of SIBO may result.1 2
Indeed, as pointed out in the accompanying com-
mentary (Gut 2001;48:168–9), the prevalence of SIBO as measured by breath testing in con-
trol subjects in Wigg et al’s study seems remarkably high.

Rather than seeking to establish the preva-
ience of SIBO in patients with NASH, as in the
study of Wigg et al, we have investigated the pre-
valece of liver damage, as reflected by ele-
vated liver enzyme levels in serum, in patients with culture proven SIBO.3 Bioc hemi-
evance of liver injury was found in 0/11 patients with SIBO with salivary type bacteria,
only 0.21 patients with SIBO with facultative anaerobic (Enterobacteriaceae) but not obli-
gate anaerobic (Bacteroides spp) colonic type bac-
teria, and 1/8 patients with SIBO including Bacteroides spp. Alkaline phosphatase and 
gamma glutamyl transferase levels were ele-
vate in this patient, although liver ultra-
sonography and cholangiography revealed no abnormality. Small intestinal permeability was in-
creased and, together with liver enzyme
abnormalities, normalised following eradica-
tion of SIBO with a metronidazole based anti-
biotic regimen. We concluded that liver injury, reversible with antibiotic treatment, occurs
uncommonly in patients with SIBO, and only when the overgrowth flora includes obligate anaerobes such as Bacteroides spp, in keeping with earlier findings implicating such flora in the pathogenesis of liver injury associated with experimental SIBO in rodents.4 Liver injury associated with SIBO with Bacteroides spp was not a necessary consequence of increased small intestinal permeability, which was also evident in 30% of patients with SIBO with Bacteroides spp who had no evidence of liver damage.

Based on these observations, we suggest that future studies examining the prevalence of SIBO in patients with NASH and its possi-
ble pathogenic significance should use culture of small intestinal aspirate rather than breath testing as the diagnostic modality and focus on the presence or absence of overgrowth with obligate anaerobic flora such as Bacter-
oides spp. Such an approach would be prefer-
able to simply assessing for any improvement in NASH following a therapeutic trial of met-
ronidazole and tetracycline.

Authors’ reply

We thank Rordion et al for their own observa-
ions concerning the diagnosis of small intesti-
nal bacterial overgrowth (SIBO) and liver injury associated with SIBO. We agree with the suggestion that Bacter-
oides spp may be just as important as other bacterial species in causing liver injury. This may be an explanation for our failure to detect endotoxin elevations in those patients diag-
nosed with both NASH and SIBO (endotoxin is derived only from Escherichia coli bacteria and not Bacteroides spp) (Gut 2001;48:206–11). We must however correct their statement that no significant differences were found in our study between NASH patients and control subjects for tumour necrosis factor α. A statistically significant difference was found between these groups (p<0.001) (Gut 2001;48:206–11).

Their comments highlight the longstanding difficulty in gastroenterology of diagnosing SIBO. Although the traditional 14C-xylose breath test has been associated with a high sensitivity in some studies,1 the specificity of this test is unacceptable in our experience. The high false positive rate associated with this breath test probably relates to catabolism of unabsorbed 14C-xylose by the colon, resulting in CO2 expiration. In an attempt to retain sensitivity and improve specificity, we have developed a combined 14C-xylose-lactulose breath test. Lactulose, which is not absorbed and requires large bacterial concen-
trations for its catabolism to H2 and CH4 acts as an internal traffic marker of colonic metabolism. Small bacterial concentrations only are required for the catabolism of 14C-xylose to CO2. Thus in SIBO, catabolism of 14C-xylose by small numbers of bacteria results in an early CO2 peak prior to the colonic H2 and CH4 peaks. Specificity is improved because CO2 peaks due to colonic metabolism of unabsorbed 14C-xylose can be identified when they rise simultaneously with H2 and CH4 colonic peaks.

When both the 14C-xylose breath test and the combined breath test were done, in a group of 11 patients, only four had positive combined tests compared with nine positive 14C-xylose breath tests (Gut 2001;48:206–11). This suggests that the combined test has achieved a greater specificity. We feel that the combined 14C-xylose-lactulose breath test is a sensitive and specific non-invasive alterna-
tive to culture of small intestinal aspirates. We note the concern of Rordion et al in the use of double H2 peaks for the diagnosis of SIBO, based on their observations with scinti-
graphic studies. In very severe SIBO, a double peak of H2 and CH4 may be produced due to catabolism by bacteria in both the small intestine and colon. As suggested by Rordion et al, double peaks may reflect catabo-
lism of lactulose by colonic bacteria rather than by bacteria in the small intestine and then the colon. Diagnosis of SIBO in our study was based on early CO2 expiration before the appearance of a H2 or CH4 peak in all cases. Double H2 or CH4 peaks were observed in only one of the 16 breath tests recorded as positive in our study. In this patient, significant CO2 was expired prior to the first peak.

The studies quoted by Rordion et al have used cultures of small intestinal aspirates as the gold standard for the diagnosis of SIBO. Isolation of intestinal bacteria or jejunal con-
tent conditions a satisfactory gold standard for the diagnosis of SIBO? This diagnostic method is not universally accepted. It is likely that the small volume of proximal intestinal contents aspirated does not accurately repre-
sent the bacterial flora of the entire small intestine. This may explain the problems with sensitivity and reproducibility described by some investigators.1 Lack of standardisation of specimen collection and the invasive nature of the test are further problems, particularly in the setting of studies involving a healthy control population. The use of culture of small intestinal aspirated material aspirated to assess the performance of breath tests may therefore not be valid.

In view of the difficulties associated with diagnosing SIBO, the association of SIBO with NASH found in our study requires confirma-
tion by other investigators. Studies using cul-
ture of small intestinal aspirate to diagnose SIBO, which can also provide qualitative bac-
terial information, will be complementary to our study using a combined 14C-xylose-
lactulose breath test.
CORRECTION

Abstract 6/12 in Gut 2001;49(suppl II):A33 contained an error. Q Song should be affiliated with institution 1 (University of Ulm).

In abstract 8/09 (Gut 2001;49(suppl II):A47), the author list should read AT Dubois1, C Seminomora1, H Woreta1, S Doi1, I Carlstedt1.

1USUHS: Bethesda, MD, USA; 2University of Lund: Lund, Sweden.

NOTICES

Broad Medical Research Program—Inflammatory Bowel Disease Grants

Funds for inflammatory bowel disease (IBD) research are available immediately from the Broad Medical Research Program of The Eli and Edythe L Broad Foundation for innovative projects regarding etiology, therapy, or prevention. Grants totalling approximately US$100,000 per year are available for basic or clinical projects. Larger requests may be considered. Initial letter of interest (no submission deadline), simple application, rapid (60 day) peer review, and funding. Criteria for funding includes new ideas or directions, scientific excellence, and originality. Early exploratory projects, scientists not currently working in IBD, and/or interdisciplinary efforts are encouraged. Further information: Marciana Poland, Research Administrator, Broad Medical Research Program, 10900 Wilshire Blvd., 12th Floor, Los Angeles, CA 90024-6532, USA. Tel: +1 310 954 5091; email: info@broadmedical.org; website: www.broadmedical.org

GI Malignancies Can be Prevented and Treated: from the Bench to the Bedside

This international meeting will be held on 15–20 January 2002 at the Dead Sea, Israel. Further information: Secretariat, GI Malignancies, PO Box 29041, Tel Aviv 61290, Israel. Tel: +972 3 5175150; fax: +972 3 5175155; email: gi@targetconf.com

Malignant Liver Tumours: Basic Concepts and Clinical Management

This Falk Workshop will be held on 24–25 January 2002 in Leipzig, Germany. Further information: Falk Foundation e.V. Congress Division, Leinenweberstr. 5, PO Box 6529, D-79041 Freiburg, Germany. Tel: +49 761 15 14 0; fax: +49 761 15 14 359; email: symposia@falkfoundation.de

European Association for the Study of the Liver: 37th Annual Meeting

The EASL Annual Meeting will be held on 18–21 April 2002 in Madrid, Spain. Further information: EASL Liaison Bureau, c/o Kees International, 17, rue du Cendrier, PO Box 1726, CH-1211 Geneva, Switzerland. Tel: +41 22 908 04 88; fax: +41 22 732 28 50; email: info@easl.ch; website: www.easl.ch

Falk Symposium No 128: Exogenous Factors in Colonic Carcinogenesis

This will be held on 2–3 May 2002 in Würzburg, Germany. Further information: see Falk Workshop details above.

Endoscopic Oncology: Gastrointestinal Endoscopy and Cancer Management

This ASGE Annual Postgraduate Course will be held on 22–23 May 2002 in San Francisco, USA. Further information: American Society for Gastrointestinal Endoscopy. Tel: +1 978 526 8330; fax: +1 978 526 7521; email: asge@shore.net

11th International Symposium on Hepatic Encephalopathy and Nitrogen Metabolism

This meeting will be held on 30 May to 1 June 2002 in Amsterdam, The Netherlands. Further information: Secretariat, Nicolaes Tulp Institute, Academic Medical Center, PO Box 21323, 1100 DS Amsterdam, The Netherlands. Tel: +31 20 566 8585; fax: +31 20 696 3228; email: tulpins@amc.uva.nl. Deadline for receipt of abstracts: 1 February 2002.

Gastroenterology and Endotherapy European Workshop: XXth Anniversary

This course will be held on 17–19 June 2002 in Brussels, Belgium. Further information: Nancy Beauprez, Gastroenterology Department, Erasme Hospital, Route de Lennik 808, B-1070 Brussels, Belgium. Tel: +32 (0)20 555 49 00; fax: +32 (0)20 555 49 01; email: beauprez@ulb.ac.be