letters

TO THE EDITOR

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Management of intestinal obstruction in malignant disease

Editor – We were interested to read the article on management of intestinal obstruction in Malignant disease (*Clin Med*, July/August 2003, pp 311–4) but were surprised no mention was made of endoscopic endoluminal stent placement as a means of palliating large bowel obstruction.[1]

We assessed the feasibility, efficacy and safety of colonic stenting in a district general hospital in a prospective study at Hinchingbrooke Hospital, between September 1998 and January 2003.[2]

This consecutive series examined a total of 23 patients, median age of 71 years (range 50–90), who presented with symptoms of large bowel obstruction, ie abdominal colic, distension or both, was confirmed radiologically. In each case the decision to stent was taken either to prepare the patient for elective surgery, as a definitive treatment in incurable cancer or because of severe comorbidity.

The cause of the stenosis was malignant in 22 cases with the sites being rectal (35%), rectosigmoid (9%), sigmoid colon (39%), splenic flexure (9%), transverse colon (9%).

The strictures were stented by conventional methods. In proximal lesions, stents were usually inserted via the colonoscope channel but in the more distal lesions the stents were placed under colonoscopic vision directly over a wire. In the case of long stenoses two stents were sometimes placed over the same wire (Fig 1). If a first attempt failed either further attempts were scheduled where possible or the patient was referred for surgical treatment.

Stent insertion was successful at the first attempt in 16 patients. Two patients were stented on subsequent procedures. The success of the procedure was assessed by technical success rate (successful first stent placement and deployment) and clinical success rate (decompression of the bowel within 96 hours without endoscopic or surgical reintervention after successful stent placement and deployment),[1] 70% and 78% respectively. All patients successfully stented had excellent symptomatic relief.

In five patients the procedure could not be performed either due to failure to identify the stenosis or failure to pass a guide wire. These patients were referred for surgery.

Only one patient suffered a complication of the procedure which was proximal stent migration leading to reobstruction.

After a median survival of nine weeks (range three days to 20 months) sixteen patients died in follow up, due to causes unrelated to stent insertion. Seven patients were alive after a median follow up of one month (range seven days to 12 months).

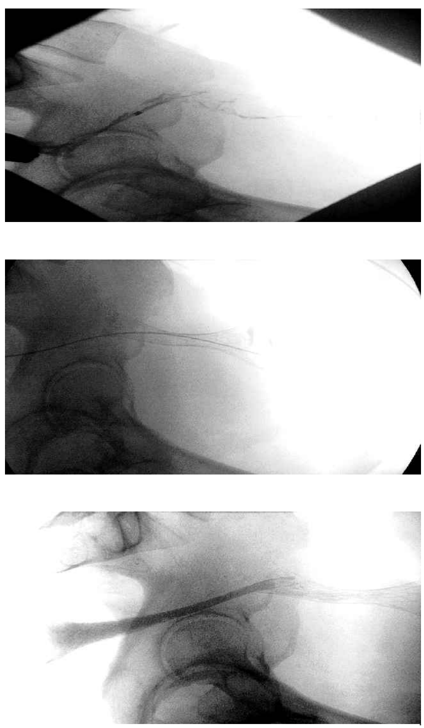

Fig 1. Radiographs showing placement of two enteral stents in series in a patient with ovarian cancer.

On the basis of this series, we believe that colonic stents have an important place in the management of large bowel obstruction and palliation of colonic carcinoma and that we have demonstrated that this technique is feasible, efficacious and safe in district general hospitals in the hands of experienced endoscopists.

References

1 Khot UP, Lang AW, Murali K, Parker MC. Systematic review of the efficacy and safety of colorectal stents. *Br J Surg* 2002:**89**:1096–102.

2 McNamara I *et al*. Clinical outcome following insertion of stents for the palliation of large bowel obstruction. A DGH experience. *GUT* 2003;**52** Supplement 1, A74.

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In addition to McNamara *et al*'s letter, correspondence was also received from Galletly NP, Bansi DS, and Thillainayagam AV, and from Tham TCK, making a similar point regarding the use of stents.

Complementary medicine: evidence base, competence to practise and regulation

Editor – The interesting article by Lewith *et al* alludes to the 'ill-informed debate that often surrounds the issues raised by CAM practice' (*Clin Med* May/June 2003, pp 235–40). Unfortunately, parts of this article could themselves be seen as misleading. In the section on 'manipulative therapies', the authors state that 'today, only extreme traditionalists are confined by these [historical] theories' on which chiropractic was founded some 100 years ago. Recent data, however, suggest that 'nearly

80% of US chiropractors teach a relationship between subluxations and internal health'.[1]

The authors also review the evidence for spinal manipulation as a treatment of acute back pain and state that a recent review shows 'significantly better outcomes than traction, corsets, bed rest, home care, topical gel, no treatment or massage and modestly better outcomes than physical therapy with exercise'. The published version of this review,[2] however, states that 'for patients with acute low back pain, spinal manipulative therapy was superior only to sham therapy or therapies judged to be ineffective or even harmful. Spinal manipulative therapy had no statistically or clinically significant advantage over general practitioner care, analgesics, physical therapy, exercises, or back school'.

Similar contradictions between this review and the Lewith article are evident in what is being said about chronic back pain.[2] Lewith *et al* also imply that the evidence for spinal manipulation as a treatment for neck pain and headache is modest but generally positive. This opinion is contradicted by the most up-to-date systematic reviews of these subjects.[3,4] Our headache article stated that 'the data available to date do not support such definitive conclusions'.[4] And my neck pain article concluded that 'the notion that chiropractic spinal manipulation is more effective than conventional exercise therapy in the treatment of neck pain is not supported by rigorous trial data'.[4]

Finally, Lewith *et al* reiterate the often-quoted incidence figures on serious adverse effects of spinal manipulation, which they believe are 'very rare'. Such statements ignore evidence from our survey[5] (which Lewith *et al* also cite), pointing out that under-reporting of such events can be as high as 100% which, in turn, renders these incidence figures simply nonsensical.

References

1. *Chiropractic News Digest* 2003, June 11, http://www.chirobase.org/18CND/03/03–02.html
2. Assendelft WJJ, Morton SC, Yu EI, Suttorp MJ, Shekelle PG. Spinal manipulative therapy for low back pain. *Ann Intern Med* 2003;**138**:871–81.
3. Astin JA, Ernst E. The effectiveness of spinal manipulation for the treatment of headache disorders: a systematic review of randomised clinical trials. *Cephalalgia* 2002;**22**:617–23.
4. Ernst E. Chiropractic spinal manipulation for neck pain – a systematic review. *J Pain* (in press)
5. Stevinson C, Honan W, Cooke B, Ernst E. Letter to the Editor: Neurological complications of cervical spine manipulation. *J Roy Soc Med* 2001;**94**:314–15.

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In response

The Lewith *et al* article to which Ernst refers was written in small sections by each author. Ernst's letter raises issues in relation to the section on manipulation and this therefore has triggered a joint reply from the main author of the paper (Lewith) and Breen who was largely responsible for the section on manipulation.

Ernst's letter somewhat misrepresents what we have said about manipulation therapy. We are accused of presenting contradictory information by referring to a recent review of its effectiveness,[1] because a later review based on the same data[2] reported less positively. It should be obvious from the citations below that the second review had not been published when our paper went to press. It is also obvious that different analysis methods were used in these reviews. Our article merely reported results[1] so that readers could see the evidence about where manipulation might be more, and less, helpful in low back pain. Ernst mentions only negative statements from the second review.

We are also accused of being 'misleading' for suggesting that only extreme traditionalists are nowadays confined by the original theories of chiropractic and osteopathy. As evidence, a US survey of chiropractors[3] is quoted which says that '77% of them teach a relationship between subluxations and internal health'. Ernst fails to include the rest of the sentence, which states that '98% of chiropractors recommend exercise to their patients; 94% offer periodic maintenance or wellness care; 93% make a differential diagnosis; 93% offer ergonomic recommendations; 88% provide general nutrition advice and 86% give stress-reduction recommendations'. What could be plainer evidence that these practitioners are not confined by original theories? Ernst could not have failed to see these statistics, because they immediately precede the single figure that he chose to quote.

Our suggestion that the evidence on manipulation for neck pain and headaches is currently insufficient to support a full systematic review is misrepresented as being some kind of 'definitive conclusion' about positive effectiveness, when it is clearly the opposite. Nor, as suggested, have we said that manipulation is more effective than exercise therapy for neck pain, or ignored the evidence from his own survey,[4] which like his letter, sounds alarms, but presents no systematic evidence.

We do not wish to make unsubstantiated claims and believe that the evidence for both the efficacy of manipulation and its safety requires further investigation. However, we do not think that it is scientifically helpful to quote data in the selective manner, chosen by Ernst, in his response to our article.

References

1. Assendelft WJJ, Morton S, Yu E, Shekelle PG. The relative effectiveness of therapy that includes spinal manipulation compared to other therapies for patients with low back pain: a meta-regression analysis of randomised clinical trials. Proceedings of the 5th International Forum for Primary Care Research on Low-Back Pain. Montreal, Canada. May 2002.
2. Assendelft WJJ, Morton S, Yu E, Suttorp MS, Shekelle PG. Spinal manipulative therapy for low back pain: a meta-analysis of effectiveness relative to other therapies. *Ann Intern Med* 2003; **138**:871–900.
3. *Chiropractic News Digest* 2003, June 11, http://chirobase.org/18CND/03/03–2.html.
4. Stevinson C, Honan W, Cooke B, Ernst E. Letter to the Editor: Neurological complication of cervical spine manipulation. *J Roy Soc Med* 2001:**94**: 314–315.

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