Postoperative infection in shunts for hydrocephalus

SIR,—I read the report by Mr B P Gardner and Mr O S Gordon (26 June, p 1914) with some concern as the authors do not make it clear what they regard as "infection in shunts." They do not make it clear whether, on the one hand, they are discussing colonisation of ventriculoatrial, peritoneal, or pleural shunts or, on the other hand, infection of the wounds after their insertion. Secondly, they make no attempt to differentiate between infection in ventriculoatrial, ventriculoperitoneal, and pleural shunts. Thirdly, if they are discussing colonisation of shunts they do not tell us how they establish that this infection is present or what evidence they have that their infection rate is not higher. It would be of considerable importance to know that the diagnosis was established on blood culture, culture of cerebrospinal fluid after ventricular valve taps, and culture of valves or catheters removed at subsequent operations. There is no mention of staphylococcal antibody titres.

Even taking all this into account, reinfections in 200 consecutive cases provided an incidence of 1.5%. Without the above information it would seem likely that the incidence was much higher.

J O N LAWSON

St Thomas's Hospital,
London SE1 7EH

*We sent this letter to Mr Gardner, who replies below.—ED, *BMJ*.

SIR,—I would like to make the following points in reply to Mr Lawson's comments. Firstly, both shunt colonisation and wound infection are included in our definition of shunt infection. Secondly, our article points out that two of the infections occurred in ventriculoperitoneal shunts out of a total of 132 ventriculoperitoneal shunt operations and one occurred in a ventriculoatrial shunt out of a total of 55 ventriculoatrial shunt operations performed. There were no ventriculopleural shunts in our series. Thirdly, we do not routinely carry out blood cultures, cultures of cerebrospinal fluid, or valve taps on patients unless there is a clinical indication to do so. We consider that if a patient is clinically well and has good shunt function valve tapping is contraindicated as infection may be introduced. When shunts are inserted or revised cerebrospinal fluid is sent for bacteriological analysis. When a valve or catheter is removed at operation and shunt colonisation is suspected then the valve is also sent for bacteriological analysis. We do not routinely carry out staphylococcal antibody titres. Positive titres do not confirm, nor do negative titres exclude, shunt infection.

It is not clear to me what Mr Lawson means by his final paragraph. None of our three reported cases became reinfected during the review period—that is, until January 1982, and no other shunt inserted or revised during this period became infected within six weeks of operation. Mr Lawson states that "It would seem likely that the incidence is much higher." There is no basis for this statement unless he is supposing that there are several patients in the series who harboured shunt infection despite being clinically well with functioning shunts. Since we do not interfere with shunts or investigate patients unless there is clinical indication to do so, such possible infections

would be missed. Such cases would be rare and of no importance if causing no harm to the patient. The main point we made is that a very low shunt infection rate can be achieved by careful surgical technique alone and without recourse to prophylactic antibiotics.

B P GARDNER

Regional Spinal Injuries Centre,
Promenade Hospital,
Southport PR9 0HY

After-exercise thermography

SIR,—One of the common hazards of clinical research is that other workers claim to repeat a clinical trial but in fact radically alter the study in either the method or the patients. They arrive at a different result and then suggest that the original work was fallacious.

Much as I respect the quality of the work of Dr A Lindhagen and others (19 June, p 1825) I believe that this is what they have done in their study of the prognostic value of after-exercise thermography. The type of patient, the age of the patients, the incidence of positive after-exercise thermography, and the type of surgery performed were not obviously significantly different in their study and our own. They, however, gave all their patients prophylaxis against deep vein thrombosis. This reduced the incidence of deep vein thrombosis overall to approximately 12%. The incidence of deep vein thrombosis in our patients (who received no prophylaxis) was 39%. Thus their study can in no way be considered comparable.

They claim in their penultimate paragraph that, "It seems improbable that the thromboses so prevented were those that might be predicted by after-exercise thermography." I would suggest that it is highly probable that after-exercise thermography does detect the legs in which prophylaxis is effective. The fact that the incidence of patients with normal thermograms who developed deep vein thrombosis was approximately the same (10-15%) in both Lindhagen's and our study supports this hypothesis. These patients may suffer deep vein thrombosis for pathophysiological reasons unrelated to the patterns detected by thermography. The purpose of our trial was to test whether after-exercise thermography can detect patients who would benefit from prophylaxis. To test the technique on patients who are necessarily going to receive prophylaxis defeats this object and the conclusion that the after-exercise thrombosis is of no value is groundless.

HUGH HENDERSON

Leicester Royal Infirmary,
Leicester LE1 5WW

Laser photoradiation for lung cancer

SIR,—Dr Ian W B Grant's leading article (31 July, p 323) on laser photoradiation for lung cancer may create the impression that no progress is being made in this field in Britain. Like Dr Hugh-Jones (1 May, p 1324), we have experienced difficulties in setting up and maintaining our project, but readers may be reassured to hear that after a pilot study in 1980-1¹ the DHSS has been supporting us in the development of the use of a British Neodymium YAG laser system in the palliative treatment of bronchial carcinoma.

Many of the foreign reports quoted lack

objective assessment to determine which patients really benefit from laser treatment. Our current project assesses response by respiratory function tests, exercise tests, chest radiographs, and symptom questionnaires and shows that careful patient selection is crucial. Laser irradiation can destroy only small amounts of tumour tissue. Best results are therefore achieved in tracheal or main bronchial obstruction, in which small changes in airway calibre produce impressive improvement in airflow.

While we are also interested in photosensitisation techniques with haematoporphyrin derivatives, we submit that the potential of the thermal effects of the YAG laser has not yet been fully realised. The optical fibres used to transmit the laser beam need further development to withstand the very high energies needed for tumour destruction—in contrast to the treatment of gastrointestinal haemorrhage, for which these machines were originally designed. Regrettably, manufacturers are slow to improve them, so that several treatments are needed and the palliative value of laser therapy is therefore reduced. This is, however, a very safe technique. The beam penetrates only a few millimetres and the very small risk of perforation of a major blood vessel is largely irrelevant to these desperate patients. There are no side effects after treatment under local anaesthesia. Photosensitisation with haematoporphyrin, however, can increase airways obstruction from subsequent exudation from the tumour, and skin photosensitivity persists for several weeks.²

We believe laser therapy has a future place in the treatment of lung cancer. It is also valuable in some rare non-malignant conditions.³ While it may help only a minority of patients, its capital cost compares favourably with radiotherapy, and treatment sessions should prove cheaper than chemotherapy.

M R HETZEL

Whittington and
University College Hospitals,
London

E M NANSON
F J C MILLARD

St James's Hospital,
London

¹ Hetzel MR, Millard FJC, Williams IP, Bridges C. *Thorax* 1981;36:235.

² Kato H, et al. *Proceedings of the 4th Congress of the International Society for Laser Therapy*. Japan: Inter Group Corporation, 1981:14-5, 14-8.

³ Williams IP, Radcliffe G, Hetzel M, Millard J. *Thorax* (in press).

Psychological impairment and low-dose benzodiazepine treatment

SIR,—We were interested to read the short paper by Dr A N Nicholson and Mr M B Spencer (10 July, p 99) showing a strong practice effect in normal subjects for the digit symbol substitution test and thus throwing doubt on our suggestion¹ that long-term benzodiazepine users show psychometric impairment. We have also found a pronounced practice effect with this test. However, in comparing 22 long-term users of benzodiazepines in normal doses with controls, we found both digit symbol substitution and symbol copying to be impaired in the patients; reaction time, tapping rate, and a cancellation task were normal. This pattern of impairment resembles that following the acute administration of benzodiazepines^{2,3} and supports