

LETTERS TO THE EDITOR

Endoscopic palliation of tracheo-bronchial malignancies

I read with interest the review by Drs M R Hetzel and S G T Smith (May 1991;46:325-33). I think that cryotherapy is almost unknown in England, and used in only one centre.¹

There are some errors regarding cryotherapy. The bronchial probes now use nitrous oxide and not liquid nitrogen. The temperature obtained on the tip of the probe reaches -70° (or -80°C) but the tumour or tissues are frozen at -40°C. No cases of perforation have been reported with this technique. Flexible probes were not described by Sanderson;² he used rigid cryosurgical probes (I was the first to use flexible probes in 1985). The authors say that no comparative studies of laser treatment and cryotherapy have been published but there is one study in a French journal,³ and others.⁴⁻¹⁰

This technique is well known in France and used more widely than laser therapy. There are at the moment 75 cryotherapists and 1500-2000 patients have been treated.

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AUTHOR'S REPLY I thank Dr Homasson for his helpful comments. We tried to give a balanced view of endoscopic palliation techniques and I believe that this has been the most comprehensive review to date. Inevitably, the space available for individual techniques was limited. Our review was primarily intended for British readers, among whom

cryotherapy has been less popular than laser photoresection.

The papers cited by Dr Homasson make the additional point, which we should have included, that the response to cryotherapy is relatively slow. This is clearly a disadvantage in severe airways obstruction, where an immediate response may be achieved with the laser. The greater safety claimed by cryotherapists might therefore reflect the fact that, presumably, they cannot treat the most severe and life threatening obstructions, which are also likely to carry the greatest operative risk.

The comparison of laser and cryotherapy quoted¹ is interesting, but only eight patients were included in each treatment group. They were apparently selected by the authors as most suitable for one or other technique. Although the cryotherapy group apparently did better, only two of the five laser failures subsequently benefited from cryotherapy. Studies like this are clearly desirable, but we need much larger numbers and no bias in patient selection if useful comparisons are to be made.

I would like to have heard Dr Homasson's views on our point that, although cryotherapy is claimed to be safer, several initial treatments are given by most cryotherapists as opposed to the single treatment used by most laser bronchoscopists. This still seems to me to be an important disadvantage for patients with very limited survival, who might perhaps prefer a possibly higher operative risk with laser resection if fewer admissions for treatment were required than is apparently the case with cryotherapy.

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Pulmonary function in chronic renal failure: effect of dialysis and transplantation

We read with interest the pulmonary function data compiled by Drs A Bush and R Gabriel for patients with renal transplants and patients with chronic renal failure (June 1991;46:424-8). The authors' proposal that lung function change in the groups with chronic renal failure resulted from pulmonary oedema would be consistent with the published data on patients with chronic congestive cardiac failure. For example, Wright *et al* have described diffusion impairment in 31% of patients awaiting heart transplantation and as many as 67% of their patients have diffusion abnormalities if those who also have restrictive and obstructive change are included.¹

The suggestion of a reduction in carbon monoxide transfer factor (TLCO) in patients with renal transplants is not new. Reduced TLCO has been described in 12 recipients of cadaveric renal allografts during active cytomegalovirus infection, who were compared with 12 control patients with renal transplants.² It has been suggested that there is a causal relation between complement activation and the TLCO change in these patients with cytomegalovirus infection.

Do Drs Bush and Gabriel have information

on whether their patients with renal transplants were infected with cytomegalovirus?

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AUTHOR'S REPLY As we acknowledged, a reduction in carbon monoxide transfer after successful cadaveric renal transplantation has been described before¹ and has been related to opportunist infection.^{2,3} Both these groups studied patients less than 15 months after transplantation. The new findings in our study were the reduction in carbon monoxide transfer factor and residual volume in well patients many years after transplantation. Of the seven patients studied more than five years after the procedure, four had a low residual volume, all had a low TLCO, and four had a low TLCO per litre of accessible lung volume (defined as below 85% predicted in all cases). Unfortunately, we do not have the detailed information on cytomegalovirus status recorded by van Son *et al*,³ nor could we relate the changes to number or type of overt infective episodes after transplantation. Ours was a cross sectional study; a longitudinal study, with detailed microbiological assessment, is needed to address the relation of opportunist infection to changes in lung function.

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Silicosis in a Himalayan village population: role of environmental dust

We read with interest the paper by Dr T Norboo and others (May 1991;46:341-3), which reports for the first time progressive massive fibrosis of the lung, in three patients, as a result of environmental dust exposure.

One patient was a woman farmer; the sex and occupation of the other two patients were not given. Environmental silica, unlike occupational silica, was thought until recently to be free of fibrinogenic activity.

We have seen a Saudi bedouin woman who developed progressive massive fibrosis (conglomerate fibrosis over 1 cm seen on computed tomogram) as a complication of desert lung.¹ As this occurred in a woman and has not been reported in men (who have the same exposure to environmental dust) we believe that occupation could have played a part. Our patient was regularly engaged in household chores, such as grain grinding and floor

sweeping, in the enclosed atmosphere of a hut and tent. These activities have been considered to be responsible for the fact that desert lung is predominantly a disease of women.^{2,3} It would be interesting to know whether the woman farmer with massive fibrosis in the paper by Dr Norboo and others had performed any of these traditional chores. Furthermore, being a farmer, had she engaged in ploughing, or earth or well digging, occupations which generate considerable dust exposure during the dry season? Are we truly dealing with progressive massive fibrosis of purely environmental origin?

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AUTHORS' REPLY Dr Al-Kassimi and his colleagues are, of course, right to believe that a woman farmer in Ladakh is exposed to dust while she works in the fields or sweeps the dusty floors in her house, and to that extent her condition could certainly be described as

occupational, though it is clearly not industrial. Because women do most of the work in the fields, including carrying baskets of earth, sowing, and weeding, they are more exposed to dust than are the men, which probably explains their higher prevalence of silicosis. As the spring dust storms affect the whole village all the inhabitants are exposed to this environmental hazard and not only those whose main work is in the fields.

The three individuals whose radiographs were shown in our paper have since died, but no details are available.

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BOOK NOTICE

Exercise, Pulmonary Physiology and Pathophysiology. Edited by B J Whipp and K Wasserman. (Pp 605; \$150 US and Canada, \$172 elsewhere.) New York: Dekker, 1991. ISBN 0-8247-8230-5.

Physical exercise in patients with lung disease

is important for two reasons. It provides a cardiopulmonary stress that can be used to identify abnormality and define disability. It can also be used through a training programme to improve physical performance. The inclusion of a volume on exercise physiology in this famous series is a welcome but tardy addition. The format is familiar, with editors of international standing and individual chapters written by acknowledged experts on the subject. The text is not, by the editors' admission, comprehensive and does exclude detailed discussion of some subjects, particularly methodology, training, and nutrition. Lung mechanics, gas exchange, and the control of breathing among others are, however, thoroughly explored. All the chapters are written with authority and some with great clarity, but a few chapters suffer from mathematical inaccessibility. There is excellent mutuality of subjects and little repetition, which is a tribute to the editors' skill. Although there is some discussion of pathophysiology, the emphasis of the book lies in the exploration of the exercise phenomenon. Consequently, only those respiratory physicians with a serious interest in exercise physiology will benefit from the book. Nevertheless, it is an outstanding compilation of reviews, which the enthusiast will strive to afford.—MDL

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