intrauterine *exchange* transfusion. Now that would have been a triumph for television!

Perhaps I should stay silent and not pillory the poor doctor, who, after all, was only playing the same part that we all play to our patients—that of appearing to know everything. But a more serious point arises. When I find errors in programmes about which I know something I worry about the errors in programmes about which I know nothing. Television is such a suggestive medium. How many people, I wonder, would swear that they saw on the television monitor the cannula entering the baby's vein and later saw the doctor syringing blood backwards and forwards to and from the fetus? Yet it did not happen. Are we all suffering from Galen's delusion? He "saw" invisible pores in the septum of the heart in order to satisfy his view of the world. How many invisible pores in our view? Perhaps that was the point that Dr Miller was trying to make.

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A plea to all vasectomists

SIR,—Requests for reversing a vasectomy are relatively uncommon, but (as expected just from the greater number of vasectomies being performed) these requests are progressively increasing. Reconstruction after a properly performed vasectomy is an easy procedure with at least a 70% chance of restoring a good sperm count, though the motility and pregnancy rate may be reduced by sperm antibodies in some cases.

It is insufficiently realised that the most important factor which determines the success or otherwise of a vasovasostomy is how the original vasectomy was performed. Excision of long lengths of the vasa to prevent spontaneous reunion is an entirely unnecessary mutilation and may make reconstruction impossible. A less well known fault in technique is to perform the vasectomy too low down so that it involves the convoluted and thinner part of the vas; this makes the reconstruction much more difficult and obviously greatly reduces the chance of success.

Whenever possible a vasectomy should be performed about the level of the head of the epididymis where the vas is a thick, straight tube. No more than 1 cm should be excised for histological purposes; prevention of spontaneous reunion requires only the placing of the two ends in different tissue planes.

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Physical therapy in chronic bronchitis

SIR,—Drs D A G Newton and H G Bevans (2 December, p 1525) are to be congratulated on their thorough and extensive clinical study. It is all the more unfortunate that they have chosen to misuse the term "intermittent positive pressure ventilation (IPPV)," which is widely accepted as the term referring to artificial mechanical ventilation of the lungs via an endotracheal or tracheostomy tube. The term "intermittent positive pressure *breathing* (IPPB)" is surely preferable. This is used to describe a short period of treatment during which air (or air plus oxygen), often together with water and/or drugs, is delivered to a patient via a mouthpiece from a patient-triggered nebuliser. An even more appropriate term to describe this treatment, in view of the findings of Drs Newton and Bevans, is "giving them the Bird?"

Their use of the term "IPPV" in this study is thus careless, and those who only read the summaries of papers could be seriously misled.

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***We sent a copy of this letter to Dr Newton, whose reply is printed below.—ED, *BMJ*.

SIR,—I am grateful to Dr Leigh for pointing out the ambiguity in our summary. Unfortunately our original text mentioned "intermittent positive pressure-ventilation" once and we had thereafter abbreviated this to "IPPB"; this was subedited to "IPPV" and we allowed this to pass rather than changing "ventilation" to "breathing."

Dr Leigh's suggested title would no doubt unleash a spate of correspondence from frustrated readers no longer able to write to *The Times*. I suspect the Editor would prefer the occasional V for a B and hope that readers of the *VMJ* will read their articles right through.

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***We must admit to having compounded the original error by introducing "intermittent positive-pressure ventilation" into the summary and into a revised version of the title of the paper, which originally read "Physical therapy in exacerbations of chronic bronchitis." We apologise for our part in this confusion of terms.—ED, *BMJ*.

SIR,—It was with some concern that we read the paper by Drs D A G Newton and H G Bevans (2 December, p 1525). We would like to make the following comments:

(1) This study has not separated the effects of breathing exercises and postural drainage from those of "IPPV" (presumably intermittent positive pressure *breathing* was intended) and minimal details are given about the administration of either treatment.

(2) Physiotherapy given in their "standard fashion"[1] refers to postural drainage for three minutes in each of four different positions. Some patients with severe obstructive chronic bronchitis would not tolerate the prone and supine positions without signs of respiratory distress. With frequent changes in position and no relaxed controlled diaphragmatic breathing patients participating in such a treatment regimen would be exhausted; bronchial secretions would be mobilised but incompletely cleared in the period of time allowed.

(3) In this study the bronchodilator was not co-ordinated with the time of physiotherapy and the bronchodilator was given by pressurised aerosol. It has been shown that a bronchodilator is more efficient given by intermittent positive pressure breathing (IPPB) than by pressurised aerosol.[2] Indeed, one of the indications for using IPPB in this type of patient is to give bronchodilator by a very effective means before physiotherapy.

(4) Other workers[3] have shown that well-planned physiotherapy can remove secretions and reduce airflow obstruction. This study was done in patients producing over 30 ml of sputum a day. Most patients in the study of Drs Newton and Bevans produced relatively small volumes of sputum even during exacerbations of infection. One would expect patients with small amounts of sputum to benefit less from postural drainage and IPPB than those with copious sputum.

We consider that in patients with acute exacerbations of chronic bronchitis, especially those who have large volumes of secretions,[4] carefully planned physiotherapy and the administration of a bronchodilator before physiotherapy by IPPB or nebuliser is an essential part of treatment.

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[1] Newton, D A G, and Stephenson, A, *Lancet*, 1978, **2**, 530.
[2] Shenfield, G M, *et al*, *American Review of Respiratory Diseases*, 1973, **108**, 501.
[3] Cochrane, G M, Webber, B A, and Clarke, S W, *British Medical Journal*, 1977, **2**, 1181.
[4] *Lancet*, 1978, **2**, 1241.

Dexamethasone in acute stroke

SIR,—The trial reported by Dr Graham Mulley and his colleagues (7 October, p 994) and the subsequent correspondence (28 October, p 1230; 25 November, p 1500; 9 December, p 1639) raise a number of important points. We are grateful to Dr R G Wilcox for supplying additional details of the patients in the trial.

(1) *Trial design*—(i) The authors attached no importance to separating the stroke patients into defined diagnostic subgroups before treatment was allocated. Yet to consider them all as a single patient group is illogical because the natural histories differ for infarction and haemorrhage and for cerebral and brainstem lesions. As the literature already indicates that dexamethasone is not effective in unselected acute strokes this further trial would have been better aimed at determining whether a particular patient subgroup will benefit or whether the effect of dexamethasone is related to the timing of administration. (ii) Importance was similarly not attached to stratifying patients according to level of consciousness; dependence was placed instead on random allocation. As a result twice as many patients in the dexamethasone group were fully alert and twice as many in the placebo group were responding only to pain; this prejudiced the placebo group from the outset of the study. (iii) Dr Mulley and his colleagues are to be congratulated on obtaining speedy admission (mean 5½ h), but the delay in initiating treatment was different in the two groups—namely, 9 h in the placebo group and 12 h in the dexamethasone group. This difference would appear to prejudice the dexamethasone group and is difficult to explain in a double-blind trial.

(2) *Neurological scoring systems*—Many stroke trials use different systems to "score" neurological deficit which, although claimed to be easy to use by the authors, never appear to be reproducible by other centres. As in this trial, the total score is usually obtained by adding separate scores for a number of variables; thus even when two total scores are the same the contributing subscores may be quite dissimilar. It is therefore both statistically unsound and misleading to say that the two groups were well matched on neurological scores at admission. The only way to obviate this difficulty is to define and analyse each parameter separately.[1]

In conclusion, the trial reported by Dr

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