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Letter to the Editor

The use of magnetic resonance imaging to assess tracheal stenosis following percutaneous dilatational tracheostomy

Dear Sir

We were interested in the paper by Callanan *et al.* (*JLO* **111:** 953–958) but would like to make some comments. Long term follow up studies after percutaneous tracheostomy have indeed been performed previously using nasendoscopy, tomography and MRI.

At Frenchay Hospital we followed up the long term survivors who had undergone percutaneous tracheostomy on the intensive care unit. From a series of 215 patients at the time, we were able to study 41 survivors who were living locally and who had been decannulated for at least six months, allowing any abnormalities to become apparent (Law *et al.*, 1997). Similar to the experience of Callanan and colleagues, none of our patients were symptomatic, but airway abnormalities were detected in 36 out of the 41 patients using nasendoscopy. In 32 patients the abnormalities were minor shelves or scars at the tracheostomy site, but in four individuals stenosis of 10-40 per cent of the tracheal lumen were observed. In three of these patients the stenosis involved the lateral tracheal wall, and only in one patient was the antero-posterior diameter of the trachea affected. While we accept the attractions and potential advantages of MRI we wonder if some lateral wall abnormalities could have been missed in Callanan's study if only the A-P diameter of the trachea was measured using MRI. Potentially this would mean that only one of the 36 abnormalities we observed in our study would have been identified by Callanan's methodology. The clinical importance of our findings is that airway abnormalities, although minor, are common after percutaneous tracheostomy and may mean that these patients will require a smaller size endotracheal tube for subsequent intubation.

Indeed we are not the only group to have identified long-term abnormalities after percutaneous tracheostomy. Waldmann and colleagues (1996) identified one case of tracheal stenosis when following up 15 patients using MRI at least six months following ICU discharge. Patients with stenosis severe enough to warrant surgical resection (Toursarkissian *et al.*, 1994), and others with 'string-like' webs across the tracheal lumen (Fischler *et al.*, 1995) have also been reported.

The work of Callanan adds to the general impression that percutaneous tracheostomy is at least as safe as surgical tracheostomy in terms of long term outcome. Indeed in our experience of over 450 procedures, no survivor has yet required surgery for tracheal stenosis. However, the high mortality rate in this group of critically ill patients means that many are lost to follow-up, as noted in our study. We calculated that a prospective study comparing the incidence of tracheal stenosis after percutaneous tracheostomy with that after surgical tracheostomy would need to recruit over 500 patients to achieve 80 per cent power (Law *et al.*, 1997). It is unlikely that such a study will be undertaken, and the question of which method is safer in terms of long-term airway morbidity is unlikely to be answered conclusively. The equally difficult question of early percutaneous tracheostomy versus prolonged endotracheal intubation has not to our knowledge been studied by anyone. It is also likely to require large numbers of patients, following up asymptomatic patients who had undergone prolonged intubation as well as those following percutaneous tracheostomy. It should aim to demonstrate advantages of one strategy over the other, not only in terms of airway abnormalities but also in terms of ICU stay, incidence of nosocomial pneumonia and hospital outcome. Certainly the study of nine patients by Callanan and colleagues, or indeed our own study could not be used to support a conclusion that 'the indications for earlier tracheostomy in intensive care with the low incidence of long-term complications may now be favoured'.

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