

# PostScript

## LETTERS TO THE EDITOR

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### Non-invasive ventilation

We read with interest the recent paper published in *Thorax* by Plant *et al*<sup>1</sup> which analysed predictors of the outcome of non-invasive ventilation (NIV) in patients with acute exacerbations of chronic obstructive pulmonary disease. Few data are available on this important question, and studies specifically designed to propose answers are either retrospective<sup>2</sup> or have been performed in a small sample of patients.<sup>3</sup> We recently published the results of a trial<sup>4</sup> using a design similar to that of Plant *et al*. Our findings indicated that the greatest predictive power lies in variables reflecting the evolution of arterial blood gases (particularly pH) after a brief (1 hour) initial trial period of NIV. Our multivariate predictive model had an adequate power of discrimination, correctly classifying more than 95% of the patients with a sensitivity of 97% and a specificity of 90% in a subsequently analysed sample of new patients. Our model therefore performed quite differently from that of Plant *et al* which had poor discriminant value even though they studied a greater number of patients.

The explanation for the differences between our findings and those of Plant *et al* may arise from the fact that they used data from a previous study that had not been designed to predict the result of NIV.<sup>5</sup> In fact, Plant *et al* did not analyse all possible variables. In particular, they did not look at those which we found to be of great predictive value—namely, the evolution of the arterial gases after a short session of ventilation. Instead, they chose to analyse the arterial blood gases after 4 hours of ventilation. From a clinical standpoint, 4 hours seems to be too long if the object is to detect a failure of NIV in order to choose another therapeutic option (invasive ventilation). It would have been interesting to calculate the power of prediction of our previously published model<sup>4</sup> had it been applied to the large sample of patients analysed by Plant *et al*.

Finally, as the model they proposed was not validated in a subsequent study with a new sample of patients, the clinical usefulness of the predictors is poor, as noted by the authors.

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### Non-invasive ventilation

Non-invasive ventilation (NIV) has been shown to be of benefit in the treatment of decompensated respiratory acidosis due to an exacerbation of chronic obstructive pulmonary disease (COPD).<sup>1,3</sup> The reduction in the need for intubation and in mortality offered by this treatment make it at least a useful initial therapy in this situation, and in some cases represents the only option available if the patient is deemed unsuitable for invasive ventilation. Plant *et al*<sup>1</sup> specify ward based NIV in their paper, yet there is no reason why NIV could not be initiated in the emergency department, provided that the equipment is available and the nursing and medical staff are given the necessary training.

NIV has previously been assessed in this role and found to be of use as an initial treatment, with the option of intubation should treatment fail.<sup>2</sup> Plant *et al*<sup>1</sup> concur with previous studies that the most important initial data predicting the need for intubation and mortality are the degree of acidosis and level of hypercapnia.<sup>6-8</sup> It seems reasonable to assume that the earlier NIV is commenced, provided it is used appropriately, the better the outcome will be. I therefore feel that NIV should be routinely available in the emergency department in order to offer the best initial treatment at the earliest opportunity. Since intubation may well be needed should an initial trial of NIV fail, ICU staff should always be involved at an early stage with frequent reassessment of progress being part of a strict protocol of care.

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### Decision analysis in NSCLC

The paper by Dowie and Wildman<sup>1</sup> exploring the decision analysis for surgically high risk patients with stage Ia non-small cell lung cancer (NSCLC) was interesting. However, I have reservations about the figures used to inform the decision tree.

There has never been a randomised trial comparing surgery with radical radiotherapy for these patients and the authors have taken 5 year survival figures for quite different groups of patients. The surgical survival figures come from a very large series of patients of all ages and levels of fitness,<sup>2</sup> while the radical radiotherapy figures come from one relatively small retrospective series of patients who were considered unfit for (or refused) surgery.<sup>3</sup> These patients have, for reasons of age and co-morbidity, a lower background probability of 5 year survival. The Cochrane Review on this topic<sup>4</sup> identified 26 studies, including over 2000 patients. For patients with T1 tumours from six studies, 5 year survival ranged from 29% to 37%, but there was a high rate of intercurrent deaths (median 25%). So, in the model, either the 5 year survival probability for surgery needs to be adjusted down or that for radical radiotherapy adjusted up to allow for the different case mix.

Also, we have no evidence on the natural history of untreated stage Ia disease. The probability of 5 year survival without treatment of 1% that is used in the model may be unduly low because it is the figure generally quoted for patients with advanced disease at presentation treated palliatively.

Why are the utilities for survival 10 points lower after radical radiotherapy than after surgery? A patient who is alive at 5 years with a given level of disability should express the same utility whatever the original treatment. The utility after surgery could, in fact, be lower because some patients are troubled by post-thoracotomy chest wall pain.

Like Treasure,<sup>5</sup> I have the feeling that this model was derived from a theoretical study of the literature without adequate discussion with clinicians. It is, however, a start. I would be happy to cooperate on a more clinically based further exploration of the topic.

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#### Authors' reply

We are grateful for the specialist input of Dr Macbeth who uses his expertise to contribute to the refinement of the probabilities and utilities in the decision tree. It was clearly not our intention to provide a definitive tree, simply an illustrative one which would demonstrate the potential of decision trees in integrating the best evidence with individual patient preference in a coherent and transparent way. But we were able to use this tree to

confirm that the surgical mortality threshold for an individual patient should be determined by their expected mortality (and health related quality of life) under the alternative options, rather than by considering the surgical consequences alone.

The study by Gauden *et al*<sup>3</sup> was selected to provide a point estimate of the 5 year survival possible following radiotherapy (range of 5 year survival in the Cochrane review<sup>2</sup> 29–37%, Gauden study 5 year survival 32%). The Cochrane review highlights the fact that survival following radiotherapy is influenced by patients' performance status, presence or absence of weight loss, and tumour size. The tree provides a structure to allow this complexity to be explicitly taken into account. We see decision trees allowing experts in the various disciplines involved in cancer care to operationalise textual guidelines and evidence for multidisciplinary decision making in a transparent and analytically coherent way. Consensus techniques would be required to identify the important patient archetypes, the best evidence applying to these archetypes, and the implications of the archetypes for each of the candidate treatment modalities. Macbeth illustrates the necessity of this process by suggesting the need to alter the 5 year survival probability in the light of case mix. A similar consensus process (taking patient characteristics into account) could be used to refine the probability of 5 year survival without treatment.

As far as utilities are concerned, these are best elicited directly from patients using techniques that also take account of attitudes to risk. In the tree in the paper the utility of 5 year surgical survivors is set at 100 in patients with only mild shortness of breath, but at 70 in 5 year surgical survivors with severe shortness of breath. In order to keep the illustrative tree

relatively simple, 5 year survivors of radiotherapy include all levels of dyspnoea and the utility chosen lies between the two groups of surgical survivors to take account of this.

Both Macbeth and Treasure<sup>3</sup> are absolutely correct to observe that the paper represents a largely theoretical demonstration of the potential of decision analysis, though that does not mean it has not already yielded some important insights. We agree that a substantive decision analysis requires extensive discussion and collaboration with both clinicians and patients, and we would welcome an opportunity to take this work forward in such a cooperative environment. In our experience clinicians—and funding bodies—vary enormously in their assessment of the value of analysis based decision technologies, but it is reassuring to see that there is increasing recognition of their future potential.

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