From the authors:

We would like to thank T.L. Petty for his interesting comments regarding our trial of ambulatory oxygen (AO) in chronic obstructive pulmonary disease (COPD) [1]. The question raised by T.L. Petty was the following: when is the best time to initiate AO in oxygen-dependent COPD? He suggested AO should be provided as early as possible after the introduction of long-term oxygen therapy (LTOT) to avoid rapid adjustment to the limitations imposed by the stationary oxygen delivery system.

Unfortunately, inappropriate prescription of LTOT is not unusual [2]. Our trial strictly targeted oxygen-dependent patients. Thus, to be included, patients had to be on LTOT for $\geqslant$3 months. This was to avoid the inclusion of patients who were prescribed oxygen following an acute exacerbation of the disease and who may not fulfil LTOT criteria upon re-evaluation. Therefore, at study entry, our patients were not oxygen naive.

The problem is that oxygen dependence cannot be easily confirmed in oxygen-naive patients. In our experience, acute exacerbation precedes the prescription of LTOT in most patients, *i.e.* during a period of clinical instability. In addition, $\geqslant$30% of patients meeting criteria for domiciliary oxygen after 1 month of apparent stability no longer met the same criteria after an additional 3 months of observation [3]. Whether provision of AO to oxygen-dependent patients would have an effect on quality of life if it was introduced earlier would pose important, methodological problems to clinical trials.

In conclusion, we would rather ask the following question: what is the best way of initiating ambulatory oxygen in oxygen-dependent chronic obstructive pulmonary disease? In this regard, we agree with T.L. Petty that it would be of interest to evaluate whether pulmonary rehabilitation in conjunction with ambulatory oxygen could facilitate compliance and further improve quality of life.

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Randomised trial of ambulatory oxygen in oxygen-dependent COPD

To the Editors:

I read with interest the article by LACASSE *et al*. [1], which found no benefit in using ambulatory oxygen (AO) in chronic obstructive pulmonary disease. I think that there were not enough patients in the study not to recommend its use. The authors have not shown the variations of oxygen saturation during the 6-minute walking test (6MWT). Oxygen therapy may improve exercise capacity in patients with desaturation during the walking test [2].

Liquid oxygen is better than gas from cylinders for AO therapy. The ENRIGHT and SHERILL [3] reference values of a 6MWT are very different from those of TROOSTERS *et al*. [4] or GIBBONS *et al*. [5]. The latter references may be preferable because the equation by ENRIGHT and SHERILL [3] gives smaller values. In my opinion, the average partial pressure of oxygen at rest of the patients, 7.0 kPa (53 mmHg), was not very low. In patients with severe hypoxaemia, the results could possibly differ.

I believe that ambulatory oxygen therapy in chronic obstructive pulmonary disease should depend on each patient. A certain subgroup of patients with chronic obstructive pulmonary disease might benefit from ambulatory oxygen. The 6-minute walking test is a prognostic tool that gives a valuable insight into the normal activities of daily living.

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From the authors:

I. García-Talavera's reaction to the results of our trial of ambulatory oxygen (AO) in oxygen-dependent chronic obstructive pulmonary disease (COPD) is of no surprise to us. We realise that our findings, *i.e.* that as-needed AO provided for a period of 3 months had no effect on quality of life and walked distance [1], are against the stream of current guidelines (*i.e.* that active patients receiving long-term oxygen therapy should have both stationary and mobile systems of oxygen delivery) [2, 3].

Negative trials are often inconclusive because of imprecise estimates of treatment effects that leave uncertainty about their conclusions [4]. However, the width of the observed confidence intervals around the mean responses, and their relation to the respective minimal clinically important differences, are clear indications that the sample size of our trial does not explain its negative results. Should we repeat this experiment in 100 different samples of patients with oxygen-dependent COPD, we would find 95 times that the treatment effect of AO is smaller than what is usually considered clinically significant [5]. This is evidence-based medicine.

Other issues raised by I. García-Talavera relate to the variations of oxygen saturation during the 6-minute walk test and the mean arterial oxygen tension (P_{a,O_2}) of our patients. We studied patients with oxygen-dependent COPD. The mean P_{a,O_2} (7.0 kPa (53 mmHg); SD: 4) is a reflection of the trial inclusion criteria. All were desaturated during the baseline, and walk test was performed while breathing room air (mean nadir: 83%; SD: 5). AO was titrated to maintain a saturation $\geqslant$90% during ambulation. It is our experience that patients with more severe hypoxaemia have more advanced disease and are more often house-bound. Whether the results of a similar trial would be different in patients with more severe hypoxaemia is possible but, in our opinion, unlikely. This remains to be seen.

We do not negate that a small proportion of patients with oxygen-dependent chronic obstructive pulmonary disease could really benefit from ambulatory oxygen. Our challenge is to identify these patients. Unfortunately, acute response to ambulatory oxygen does not predict long-term improvement in quality of life in patients with chronic obstructive pulmonary disease not fulfilling criteria for long-term oxygen therapy, but with exertional desaturation [5, 6]. We came to the same conclusion in oxygen-dependent chronic obstructive pulmonary disease following secondary (and as yet unpublished) analyses of our data.

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Cystic lesions of the lung: a forgotten menace

To the Editors:

We read with interest the article by BATTISTINI *et al.* [1] concerning a young female with spontaneous pneumothorax as the presenting feature of pulmonary lymphangioleiomyoma, which appeared in a previous issue of the *European Respiratory Journal*. The differential diagnoses, which included lymphangioleiomyoma, tuberous sclerosis and Langerhans cell histiocytosis or eosinophilic granuloma, were based on bilateral cystic pulmonary lesions on high-resolution computed tomography, which were slow to progress, as well as the relatively normal pulmonary function tests [2].

We recall a 46-yr-old male who presented to us with left spontaneous pneumothorax, followed 2 days later by the development of contralateral pneumothorax. A computed