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CORRESPONDENCE

Long-term oxygen therapy: do current guidelines need revision?

To the Editor:

VEALE *et al.* [1], in their recently published paper, have shown that 18.5% of a very large series (n=7,700) of chronic obstructive pulmonary disease (COPD) patients, who were prescribed long-term oxygen therapy (LTOT) *via* the French Association Nationale pour le Traitement A Domicile de l'Insuffisance Respiratoire chronique (ANTADIR) French network, had in fact, at the onset, a stable arterial oxygen tension (P_{a,O_2}) ≥8 kPa (60 mm-Hg) and, accordingly, were not fulfilling the conventional criteria of LTOT. The authors have observed that the survival of these patients was comparable to that of patients also given LTOT, whose initial P_{a,O_2} ranged between 6.7 and 8 kPa (50–60 mmHg). These results are undoubtedly of interest and, similar to previous studies of the ANTADIR network [2], they are based on a con-siderable series of patients. Nevertheless, the conclusions of this study are not quite clear to us and we wonder whether they are all justified by the results presented by the authors.

VEALE *et al.* [1] underline the fact that the survival of patients with a P_{a,O_2} >8 kPa was similar to that of patients who were more hypoxaemic: does this justify, *a posteriori*, the prescription of oxygen therapy in patients whose initial hypoxaemia was not severe? The fact that the prognosis was rather poor in these patients under LTOT does not mean that it would have been even worse without LTOT. As the authors state themselves, prognosis in COPD depends not on hypoxaemia alone, but first on the severity of bronchial obstruction, as demonstrated by ANTHONISEN *et al.* [3] and COOPER and HOWARD [4]. This applies particularly to patients of the emphysematous type who seem to have been dominant in the subgroup of patients with an initial P_{a,O_2} ≥8 kPa.

More importantly, the authors conclude that "the current guidelines for LTOT may need revision and (that) prospective studies of the effect of LTOT in these patients are needed". The beneficial effects of LTOT on survival have only been shown in patients with moderate-to-severe hypoxaemia [5, 6] and it must be noted that in the Nocturnal Oxygen Therapy Trial (NOTT) and Medical Research Council (MRC) studies, the average P_{a,O_2} at the onset was as low as 6.6 kPa (50 mmHg), and that most of the patients had polycythaemia and pulmonary hypertension which are less likely to occur in patients with a diurnal P_{a,O_2} >8 kPa. Indeed, until very recently we had no study comparing the life expectancy of COPD patients, given LTOT or not, with less severe hypoxaemia (P_{a,O_2} >7.3–8.0 kPa (>55–60 mmHg)), but we now have at our disposal such a study, that of GÓRECKA *et al.* [7], published in *Thorax* in 1997. This Polish study has been quoted by VEALE *et al.* [1], but more emphasis could have been placed on the results of GÓRECKA *et al.* [7], who observed in 135 COPD patients, whose stable P_{a,O_2} at the onset ranged between 7.4 and 8.7 kPa (56–65 mmHg) with an average P_{a,O_2} for the whole group of 8 kPa (60–61 mmHg), that there was no difference in survival rates between patients receiving LTOT and controls. Accordingly, there is no justification to the prescription of LTOT in patients with moderate-to-slight hypoxaemia, as emphasized by ZIELINSKI [8] in his Editorial related to the paper of VEALE *et al.* [1]. In our opinion, this is an adequate answer to the question raised by the authors and we are not sure that we need additional studies in this field, except if we want to investigate aspects other than survival, namely the quality of life, as suggested by ZIELINSKI [8], or the effect of nocturnal oxygen therapy in patients exhibiting only sleep-related hypoxaemia [9, 10].

E. Weitzenblum, A. Chaouat, R. Kessler
Service de Pneumologie, Hôpital de Hautepierre, 1, avenue Molière, 67098 Strasbourg Cedex, France. Fax: 33 388127827.

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REPLY

From the authors:

We thank E. Weitzenblum and his colleagues for their interest in our paper. Like them we do not wish to overinterpret our results and in our penultimate paragraph stated that the conclusions to draw from the similarities of survival was a "vexed" question and we concluded that only prospective studies could answer the question clearly. We acknowledge the primacy of forced expiratory volume in one second (FEV_1) as predictor of survival in chronic obstructive pulmonary disease (COPD) [1].

Our statement regarding the revision of the guidelines is addressed to the question of the use of nocturnal desaturation, exercise desaturation and pulmonary hypertension in the context of stable daytime gases at rest. If more and more patients are prescribed long-term oxygen therapy (LTOT) outside the guidelines, then the authorities will have to clarify the situation. We feel that survival is not the only issue in the treatment of any long-term illness and we agree with the comments that studies need to be performed to examine whether there is any clinically significant improvement in health-related quality of life that offsets the intrusion of LTOT into the person's life.

We agree that the study by GÓRECKA *et al.* [2] clearly answers a very important question on the survival issue and we agree with ZIELINSKI [3] that quality of life and the effects of nocturnal oxygenation need specific study. We note that preliminary data from the European multicentre study did not show prolonged survival in 27 patients compared to 23 controls with moderate hypoxaemia [4, 5].

D. Veale, E. Chailleux, A. Taytard, J.P. Cardinaud
CMTS, ANTADIR, 66 Bld St Michel, 75006 Paris, France.

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