

From the authors:

We would like to thank J.E. Hansen for his detailed response to our recently published analysis regarding prevalence of bronchoreversibility in subjects enrolled in the National Emphysema Treatment Trial [1]. J.E. Hansen raises a concern that the American Thoracic Society (ATS)/European Respiratory Society (ERS) definition of bronchoreversibility is too restrictive in that it requires both an absolute forced expiratory volume in 1 s (FEV₁) \geq 200 mL and a 12% increase in FEV₁ to qualify as a positive test, which may underestimate bronchoreversibility in a patient population with very low lung function. J.E. Hansen has asked us for a reanalysis of the data with this in mind. If we use the ATS criteria for bronchoreversibility, 121 (22.2%) subjects met these criteria at least once during the period of evaluation. If we choose a 12% absolute increase in FEV₁ alone as our definition for bronchoreversibility, 452 (83%) subjects met this criterion at least once during the period of evaluation. As J.E. Hansen suspected, the number of subjects meeting this less restrictive criterion is significantly higher, although possibly too high, making the clinical utility less clear. As J.E. Hansen pointed out, identifying the “perfect” measure for bronchoreversibility is not an easy task and perhaps should depend on the subject

population being studied. The data we present in our paper helps us to better understand the prevalence of bronchoreversibility in the severe emphysema patient population, but the choice of definition for bronchoreversibility should ultimately be determined by the definition that best discriminates patients as they relate to clinically meaningful outcomes.

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- 1 Han MK, Wise R, Mumford J, *et al.* Prevalence and clinical correlates of bronchoreversibility in severe emphysema. *Eur Respir J* 2010; 35: 1048–1056.

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Inhaled corticosteroids: a controversial add-on treatment in COPD

To the Editors:

LOKE *et al.* [1] provide an excellent meta-analysis of the risk of cardiovascular events (CVEs) associated with inhaled corticosteroids (ICSs) in patients with chronic obstructive pulmonary disease (COPD) [1]. In their editorial, SIN and MAN [2] point out that the incidence of CVEs was very low, and I am also concerned that the available trials do not provide a definitive answer because of the way in which CVEs were ascertained. The only trial that explicitly ascertained CVEs as a cause of death and verified them using an adjudication committee was the TOwards a Revolution in COPD Health (TORCH) trial [3]. Indeed, a much higher rate of fatal CVEs was found than in all of the other trials, which cannot alone be explained by the long observation period of 3 yrs. In consequence, the meta-analyses are largely driven by the TORCH trial, whereas most other trials contribute little to the analyses. This may well be because CVEs were not rigorously captured, as pointed out by SIN and MAN [2], which is a common problem for secondary outcomes [4]. The authors may want to discuss whether or not the low incidence of CVEs in the other trials is a consequence of different study populations, insufficient CVE ascertainment or incomplete reporting of CVEs, and whether the latter two may be differential for treatment groups. In addition, I wonder whether they see any opportunity for assessing whether selective and/or differential reporting of CVEs could be present in some of the trials.

Although I agree with the editorial that current data do not provide a definitive answer regarding the role of ICSs, I largely disagree with some of the statements supporting their role. First, history shows that blockbuster or widely prescribed drugs are not necessarily effective or safe drugs (*e.g.* hormone replacement therapy, celecoxib or antibiotics for sinusitis or acute otitis media). Secondly, there is ample evidence that various forms of pharmaceutical marketing are effective in influencing physicians through opinion leaders, seeding trials and continuing medical education [5, 6]. Thirdly, the cited “unequivocal evidence” for the effectiveness of ICSs is selectively chosen and based on single observational studies or randomised trials. Additionally, the question as to whether ICSs are superior over placebo is not really relevant because ICSs are used as an add-on treatment to long-acting bronchodilators, and rarely without them. Current meta-analyses and network meta-analyses do not suggest a great additional value of ICSs in reducing exacerbations or improving health-related quality of life to a clinically meaningful extent unless the COPD is very advanced [7, 8]. Research should further explore ICSs but focus on the comparisons relevant for practice. Important clinical end-points should be carefully ascertained and fully reported so that future (network) meta-analyses provide more definitive answers regarding the role of ICSs in COPD patients.