

From the authors:

We wish to thank A.E. Mirrakhimov for his interest in our article [1] and his comments on it.

With respect to these comments, we would like to raise a few points. It is important to note that we realise that this is an observational study and, as such, subject to the methodological limitations intrinsic to this type of study. It would obviously be desirable to adjust the results for many other variables that were not included in this study: the use of statins, hypotensive treatment and changes in all the cardiovascular risk factors analysed over the course of the study; and not just these variables, but also many others related to the evolution of patients and changes in their treatments. However, it must be borne in mind that the methodology used in this study only allows us to generate hypotheses and establish associations; our results and conclusions need to be corroborated by studies with a methodology that would offer a higher level of scientific evidence (randomised clinical trials). This is a message that features clearly in the last sentence of our article, as can be seen, although it is possible that there could be ethical problems involved in undertaking a long-term clinical trial in symptomatic patients [1].

In any case, even if we had evaluated all the variables mentioned above, the number of patients included in this study and the limited number of new cardiovascular events occurring over the course of the study would not have allowed us, from a statistical viewpoint, to adjust the results for a high number of variables. We thus decided to choose those variables that, based on the literature, we considered to be the most important or decisive cardiovascular risk factors, such as age, sex, stroke severity, previous stroke or transient ischaemic attack, previous ischaemic heart disease, previous atrial fibrillation, arterial hypertension, diabetes mellitus, hypercholesterolaemia, fibrinogen levels, body mass index, current smoking and the presence of significant carotid stenosis.

Furthermore, A.E. Mirrakhimov points out the possibility of a selection bias being introduced when comparing those patients who tolerate CPAP therapy with those with no tolerance, since intolerant patients could have a distinct profile for adherence to other treatments and, as a consequence, an increase in their cardiovascular risk. Although this could be the case, a recent publication concluded that treatment with continuous positive airway

pressure (CPAP) made no difference to adherence to cardiovascular treatment in patients with obstructive sleep apnoea [2].

We have carefully read the preliminary study recently published by HABA-RUBIO *et al.* [3] on the effect of transnasal insufflation in acute stroke and, although the changes in sleep parameters are modest and only 10 patients were included, we found it very interesting. As we also pointed out in our study, and reiterate here, it is of great interest to find formulas to increase adherence to CPAP treatment in stroke patients, as this is very low in the majority of studies and, at least according to the literature currently available, CPAP could have a beneficial effect on the functional recovery and vital prognosis of these patients.

Finally, we wish to state that each and every one of the comments made about our manuscript by A.E. Mirrakhimov was highlighted and explained in the part of the discussion dealing with the limitations of our study [1], so readers of our manuscript can clearly appreciate the study's limitations and, therefore, draw whatever conclusions they think most appropriate.

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