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COPD in exclusive narghile smokers: Some points to verify

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Dear Editor,

I read with great interest the paper of Bahtouee et al. "The prevalence of chronic obstructive pulmonary disease in hookah smokers."[1] The topic of the paper is very interesting since the social phenomenon of narghile use has developed to a worrying extent.[2] However, five serious methodological points were noted in the above paper and should be highlighted.

The *first point* concerns the lack of precision about which spirometric norms were applied. In fact, only the following sentence was cited "the pulmonary function test results were expressed as percentages of the expected values adjusted for age, sex, height, weight, body mass index, and race."[1] It is central to reminder that numerous respiratory functional laboratories allow the default settings for spirometric norms offered by the manufacturer (especially, European Respiratory Society (ERS)/ European Community for Steel and Coal[3]). The use of the above norms resulted in misinterpretation of spirometry data in a significant proportion of subjects and this could result in inappropriate diagnosis and/or management.[4] Moreover, a recent North African study does not recommend the use of the recent multiethnic norms derived by the ERS global lung initiative to interpret spirometry in local adults' population.[5]

The *second point* concerns the use of a fixed threshold of 80% to classify spirometric values as normal or abnormal. The use of a fixed threshold as a lower limit of normal (LLN) has been widely criticized and more importantly, clinicians may have to review and revise previous diagnoses.[6] Actually, the use of the Z-score is encouraged by scholarly societies.[7]

The *third point* concerns the applied spirometric criteria (only FEV_1) to define reversibility. Since 2005, the use of the forced vital capacity (FVC) was recommended by scholarly societies,[8] and in case of COPD patients, several papers promoted its inclusion in the bronchodilator response.[9–11] On the one hand, an improvement in FVC provides helpful information about the function of small airways, the most important sites of inflammatory and remodeling processes that are difficult to measure.[9] On the other hand, the appraisal of FVC was proposed as a mean to get supplementary information regarding hyperinflation.[9]

The *fourth point* concerns the "unusual" applied definition to retain the diagnosis of a restrictive ventilator defect (RVD; $FEV_1 < 80\%$ and $FEV_1/FVC > 0.8$[1]). Moreover, authors haven't neither argued their choice by a solid reference nor discussed the above definition as a serious study limitation. In a similar Tunisian comparative study including 36 exclusive narghile smokers (ENSs) of more than 10 narghile years and 106 exclusive cigarette smokers for more than 10 pack-years,[12] the recommended international

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definition for an RVD (total lung capacity < LLN[8]) was applied. In that study,[12] the percentages of ENS having restrictive or obstructive or mixed ventilator defects were different from those reported by Bahtouee et al.[1] (respectively, 36% vs. 7.4%, 8% vs. 10.2%, and 3% vs. 0.8%). In addition, in a study named "spirometric profile of narghile smokers,"[13] where expiratory flows and static lung volumes were determined in 110 ENS, all forced expiratory flows were reduced (compared to 81.6% of subjects with normal lung function in Bahtouee et al.'s study[1]). In addition, a different spirometric profile of ENS was advanced[13]: 36% had lung hyperinflation, 14% had small airway obstruction, 14% had RVD, and 6% had large airway obstruction. Surprisingly, the above two studies[12,13] were omitted by Bahtouee et al.,[1] which can be considered as a striking form of bibliographical bias.

The present Letter to Editor is a call for researchers and physicians to keep a watchful eye on any manuscript on the effects of narghile use on lung function.

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