Author's response to reviews

Title: Validity of a questionnaire-based diagnosis of chronic obstructive pulmonary disease in a general population-based study

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Version: 2  Date: 1 January 2014

Author's response to reviews: see over
To:
Prof. Mark Dransfield
Prof. Catherine Greene
Section Editors, BMC Pulmonary Medicine
and
Prof. Yoshinosuke Fukuchi
Prof. Geertjan J Wesseling

Reviewers of the research article

Validity of a questionnaire-based diagnosis of chronic obstructive pulmonary disease in a general population-based study

Dear Professors Dransfield, dear Prof. Greene, dear Prof. Fukuchi, dear Prof. Wesseling,

Thank you very much for considering this article for eventual publication in your prestigious journal; following the instruction of the editorial office of BMC Pulmonary Medicine, this cover letter would address a point by point response to your concerns.

Prof. Yoshinosuke Fukuchi’s comments:

“1. COPD should be diagnosed with post-bronchodilator FEV1 % < 70% and spirometry in this study is done before SABA inhalation. As discussed in the manuscript this is a major and difficult to clear limitation, so the author should discuss how much over diagnosis might arise in this setting. Previous investigation indicated that the difference was up to plus/minus 20%. This should influence the figure for sensitivities which needs to be described in addition.”

This is really a relevant comment and we have added in the revised manuscript some further explanation in discussion section (validity issues).

“2. LNN as employed in this study is criticized by others that Lambda/My method might better be applied to more precise estimate of LNN taking non linear character of age decline of FEV1. It would be nice to have some discussion on this point.”

We agree on this point, LNN by Quanjer et al. (1993) could have some drawbacks in estimating the real LNN, however, the aim of this study was to assess the diagnostic accuracy of a questionnaire and, to make our results comparable and applicable in clinical and epidemiological practice, we had to rely on those methods still most widely used, despite some relevant limitations. In any case we add some discussion on this point also.

“3. Chronic bronchitis complaints are poor predictor of Air Flow Limitation, yet it may convey an important health problems in lay public. It will be nice to for the authors to give some message in this regard.”
We agree that chronic bronchitis is a very important public health issue, but the primary aim of this study was to assess the diagnostic accuracy of a questionnaire question on physician diagnosed COPD, likely based on spirometry, against significant airflow limitation defined by spirometry. Moreover, in this study those complaining for chronic bronchitis symptoms were not overlapped to those with physician diagnosed COPD (data not shown), making this first group a completely different cluster of patients, so we preferred to not discuss their features in this manuscript.

"4. Air Flow Limitation and Air Flow Obstruction are both interchangeably used in the text. It should be fixed to either one."

This is a relevant point, we have changed throughout the manuscript

Prof. Geertjan J Wesseling comments:

“These data have been collected a number of years ago. Even taking into account that comparisons of outcomes of questionnaires and result of spirometry in large populations have remained scarce ever since, the fact that spirometry is now considered mandatory to establish a diagnosis of COPD reduces the scientific value of this analysis.

We agree with this comment, spirometry is mandatory to diagnose COPD. Anyway questionnaires are still widely used, especially in primary care and in developing countries, where spirometry is still not always widely spread. The results of this study, given the low sensitivity, would make aware and cautious researchers on using solely questionnaire data when dealing with COPD studies, encouraging the use of spirometry data. However in large epidemiological studies concerning risk factors for a disease, when other more accurate disease definitions (spirometry for example) are not available, a “proxy” definition with an high specificity (characterized by a low number false negative), is very important to not bias the risk estimate.

“Also, having only prebronchodilator lung function measures is a further limitation. As these data have been collected many years ago, this shortcoming obviously cannot be taken care of.”

We have already pointed out that this is the main limitation of this study and we agree on this comment. The significance of newly designed studies, such as BOLD study for example, would have been greater. However we strongly believe that large population based studies such this one or the ECRHS II, the results of which continue to be publish despite the absence of post-bronchodilator data (de Marco R, et al. Risk factors for chronic obstructive pulmonary disease in a European cohort of young adults. Am J Respir Crit Care Med. 2011 Apr 1;183(7):891-7.) can give still important information on frequent phenomenon, such as COPD.

“The questionnaire is not provided with the manuscript and will not easily be found by many readers. Whether it is frequently used cannot be confirmed.”

The questions in the questionnaire used in this study are presented in the method section, under the sub-section “population and questionnaire”.

It may be true that a bronchodilator response suffers from a lack or reproducibility may be correct but that is not relevant to establish a diagnosis of COPD, as it is not so much the improvement in FEV1 resulting from the administration of a bronchodilator that is being tested, but the FEV1/FVC ratio after (maximal) bronchodilation.
We agree that the FEV1/FVC ratio after bronchodilation is the main issue to be considered and we add some further comment on that, following also the suggestion of the other reviewer. However looking to the data from one of the new added references (Albert P et al Bronchodilator responsiveness as a phenotypic characteristic of established chronic obstructive pulmonary disease. Thorax. 2012 Aug;67(8):701-8), it seems that variability of FEV1/FVC ratio is not lower than FEV1 variability and the reproducibility during follow up visits seems to be even worse than FEV1, especially in those with COPD.

We hope that we have cleared your concerns and you will still consider our manuscript of value and suitable for publication in your prestigious journal.

Best Regards,

On behalf of all authors

Nicola Murgia, MD

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