Reviewer's report

Title: Comorbidity in patients with Chronic Obstructive Pulmonary Disease in Family Practice: a cross sectional study.

Version: 1 Date: 9 October 2012

Reviewer: Johan Hansen

Reviewer's report:

Review of Comorbidity in patients with Chronic Obstructive Pulmonary Disease in Family Practice: a cross sectional study. By Luis García-Olmos et al. BMC Family Practice

Overall: The article contains relevant information for those involved in studying similar topics, both in Spain and in other countries. As the main focus is on describing prevalence and comorbidity rather than trying to determine either causes or consequences, it runs the risk of falling into the ‘me too’ category of articles. As such the article can be a useful contribution to the field, but it would be good if the authors better clarify what their study adds, also related to other studies using somewhat different ways of measurement. Preferably, I’d also like to encourage the authors to add additional analyses that provide some more in-depth information, e.g. related to relevant patient, or practice / FP characteristics. By doing the latter it would become more interesting for a wider audience, as well as providing a solution for the possible differences in prevalence measurement with other studies.

Major Compulsory Revisions

1. The article has made a number of choices which are either hardly explained to the audience or where no or only very few references of earlier studies were added. As an example, on page 3 it is stated that few studies have addressed COPD and those that have focused on patients 40 years and older. While this might suggest that younger groups should have been added, the authors then also use the same age selection without a clear argument why (e.g. comparability with such earlier studies). After hardly any search I already found a number of other similar studies, some of which used another age selection (e.g. Feary et al; Thorax. 2010 Nov;65(11):956-62; see also the systematic review by Halbert et al 2006).

2. It is also unclear to me what the authors exactly consider to be COPD, other than that ‘patients were deemed to have COPD if this diagnosis appeared on their clinical histories’. Other studies sometimes use different descriptions of COPD. E.g., another recent Spanish (hospital based) study distinguishes between stages of COPD (and finds a very similar prevalence for Spain as an earlier Spanish study that the authors refer to), but only for stage II and higher (Miravitilises et al; Thorax 2009;64:863-868. Other studies include chronic
bronchitis and emphysema in their selection of COPD cases. More importantly, the systematic review and meta-analysis that the authors refer to themselves (by Halbert et al. 2006) makes a distinction between symptomatically defined COPD (as chronic bronchitis), anatomically defined (as emphysema) or, most commonly, physiologically defined (as airway obstruction). Without any clarification and discussion on these differences in definitions, it becomes extra difficult to critically assess the findings of this study compared to those of others.

3. A second reason why comparison is difficult, lies in the approach used, namely through FPs: on page 8 it is mentioned that COPD tends to be underdiagnosed by FPs (although no reference is added). But on the other hand, at page 4 it was stated that most studies are hospital based, using more complex patients. Wouldn’t that lead to an overestimation of the prevalence of COPD and if so, which estimates are most reliable? It relates to the plea by Halbert et al to search for more standardised measurements of COPD, thus leading to more meaningful comparisons of studies. Could the authors reflect on that a bit more?

4. Moreover, if indeed comparison of prevalences are so severely hampered by differences in measurement and methodology, isn’t it then a more fruitful approach to look for other ways of looking at the data? E.g., how can you describe the differences in COPD prevalence between practices: is it due to composition factors at the patient / population side (e.g. neighborhood income levels, see for example http://www.ices.on.ca/file/Chronic_Conditions_lhin10_May08.ppt), or is it caused by more practice or FP based indicators such as their level of experience or perhaps even the presence of specialized practice nurses?

5. On page 5 the authors describe their inclusion criteria based on EMR quality, but it would help if they could argue or add references why this is a smart choice and what its consequences are. E.g. the second criterion is that each FP should have a mean of more than 4 care episodes per patient. Does this indeed say something about data quality and not of something else (e.g. how long the FP is working / how long ago the EMR was introduced or how infrequent patients in that local setting visit their FP). And how many cases were removed due to these selections?

6. On the same page the ACG system is described. I’d recommend adding a few other references, indicating that this is indeed commonly used for this type of study, including the regrouping into so-called RUBs (e.g. see Prosser et al 2005; referring to two Spanish validation studies, showing the system to be effective in characterizing the morbidity burden of populations).

7. I’d also like to see some clarification on their selection of 40 and then 26 high prevalence / high impact conditions. I could not find if this is a strategy commonly used by others, not what the consequences are of using this strategy: doesn’t it hamper comparability with other studies and if so, what are the main illnesses that were picked up by other studies but not included here (or vice versa)? E.g., is the relatively uncommon association with chronic liver disease related to this selection?
8. A last point concerns representativeness. Only until the discussion section this
topic is addressed, claiming that the sample is representative of the Spanish
population. In the text I could not find on which factors this is based and how
similar the sample exactly is? Plus, if it is merely age and sex for example, then
how does the study take into account the fact that it is conducted in an urban
area, with possible differences in composition (e.g. income levels, education, risk
behaviors etc)? If there is indeed some form of bias due to the level of
urbanization, it would then be an extra argument to look more into associations
with other factors rather than focusing mostly on prevalence and comorbidity
(see point 4).

Minor Essential Revisions

Apart from a few sentence constructions, the overall writing is very good.

Discretionary Revisions

**Level of interest:** An article whose findings are important to those with closely
related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a
statistician.

**Declaration of competing interests:**

I declare that I have no competing interests