Author's response to reviews

Title: Thoracic gas compression during forced expiration in patients with emphysema, interstitial lung disease and obesity

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Author's response to reviews:

The BMC Pulmonary Medicine
Editorial Board

Helsinki, February 4th, 2014

Dear Sir,

The manuscript Piirilä et al. entitled “Thoracic gas compression during forced expiration in patients with emphysema, interstitial lung disease and obesity” has now been revised according to the comments of the referee of the BMC Pulmonary Medicine. The corrections point by point are indicated on the next pages. The corrections made in the manuscript are also presented in the next pages point by point.

We hope that the manuscript now is suitable for publication in the BMC Pulmonary medicine.

Sincerely yours,

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Reviewer's report
Title: Thoracic gas compression during forced expiration in patients with
emphysema, interstitial lung disease and obesity

Version:2Date:7 January 2014
Reviewer:Tomasz Golczewski
Reviewer's report:

As I have written previously, the manuscript presents findings on so called ‘thoracic gas compression’ that can be found interesting by scientists in the field. Unfortunately, the main doubts (imperfections) are still present. Additionally, some new imperfections have been introduced.

Major Compulsory Revisions

1. As I have stressed previously, the term ‘compression’ can mean (see dictionaries, please) both the process of compressing and its effect (compressed gas, i.e. alveolar pressure higher than the atmospheric one). Additionally, the term ‘thoracic gas compression’ may mean the general phenomenon. The authors use this term without explanation or another sign, which of those meanings are used in particular places. For example, the examined difference between thoracic and mouth flows corresponds entirely to the compressing process while ‘compression’ as the phenomenon or compressed gas seems to be mainly used in the Introduction and Discussion parts. For that reason, evaluation of the poor Discussion is now difficult.

I would suggest to use ‘compressing’ instead of compression=process and ‘compressed air/gas’ instead of compression=effect to avoid confusions.

ANSWER: The authors have tried to be careful to use the right terms in the whole manuscript.

2. Since the examined difference between flows is related to compression=process, the negative difference in majority of ILD patients means a temporary decrease of the alveolar pressure from the initial value that causes expiratory airflow beginning and thus is higher than the atmospheric one. This should be discussed taking into account mechanics of airflow (esp. why is ‘negative compressing’ observed mainly in ILD patients and one healthy?); problems with measurement may be noted only as a possible additional cause.

ANSWER: Thank for the referee to open our eyes to these results. We really first thought that the inverse relation must be only caused by methodological problems. However, now when we are looking the figures again, the inverse relation in the ILD group as well as one control subject is prominent and repeated
at several flow levels. The explanation for this phenomenon, however, is difficult and complicated. The discussion on the results in the ILD group have been moved to page 12, last paragraph. Discussion on negative difference is now on page 13. Reference 2 (Goldman et al.) is attached to this letter (if this is possible to do).

3.
The authors treat the ‘thoracic flow’ versus volume of expired air curve as ‘true’. Why? In particular, why we, the others, should treat this curve as the ‘true curve’ when spirometry is considered?

ANSWER: The thoracic curve has been called the true curve in the literature, and this term was adopted from there. For example in Coates AL et al. Eur Respir J 1997;10:1415-1427, see page 1419. However, the word true has been deleted in the manuscript.

Additionally:
- on the one hand, the parameter called as the thoracic flow is the change of body volume, in fact, i.e. it is the change of the gas volumes both in thorax (exhaled and compressed) and abdomen (compressed); (is it possible that obese subjects have wider intestines, and thus more gas inside the abdomen?)
  ANSWER: The obese subjects usually have increased fat also in the abdomen. Increased intestines and more gas in them is possible, but this cannot be measured here.

- on the other hand, the mouth flow per se is treated as ‘bad’ while its integration (i.e. the volume) is ‘good’.
  ANSWER: In literature the lower flow at mouth compared to thoracic flow has been considered to be a compression artefact, for example in Fairshter RD et al. J Appl Physiol 1989;67:780-5 or Coates et al. Chest 1988;94:976-82.

4.
Despite I have noted previously that elastic properties of parenchyma have not any direct influence on the gas compression, the authors seem to discuss again this influence. In particular, there are unexplained statements related to association between the gas compression and these properties (the paragraph next to the last one). Certainly, indirect association exists (through two-ways influence of the properties on airways resistances), which should be discussed (both ways).

ANSWER: This has now been corrected in several paragraphs throughout the manuscript.

Minor Essential Revisions
5. (Abstract) It is not true that the authors compared the magnitude of thoracic compression. They compared the difference between the thoracic and mouth flows.

ANSWER: the sentence in abstract has now been changed into: We compared the magnitude of the difference between the thoracic and mouth flows during forced expiration measured with a flow plethysmograph as an indication of thoracic gas compression in subjects with different types of pulmonary diseases characterized by limitations in pulmonary mechanics.

6. Captions for Figs.2-5 and Table 3 - those items do not present gas compression but the difference between flows.

ANSWER: This has been changed to be: “Difference between thoracic and mouth flows…”

7. Fig.1 is again incorrect. This time the horizontal axis is wrongly described (I suppose that this axis is related to % of FVC).

ANSWER: Figure 1 has been corrected.

8. FRCpleth is defined differently three times (including the list of abbreviation), and only the first definition is related to the commonly used abbreviation of the functional residual capacity.

ANSWER: FRCpleth has been corrected to be the functional residual capacity measured with plethysmograph.

9. The term ‘significant’ (e.g. ‘significant gas compression’ in the first sentence of the Discussion) is related only to the statistical significance but may be understood as equivalent to ‘big’, ‘great’, etc. compression. Therefore, the term ‘statistical significance’ should be used. Moreover, the authors did not show ‘significant gas compression’ but only statistically significant difference between the thoracic and mouth flows.

ANSWER: This has been corrected, great instead of significant in the first paragraph of the discussion chapter.

10. Despite that the text has been checked by a native English, there are many errors such as typographical errors (e.g. ‘p=0.01-<0.001’ in the Abstract, ‘froups’ instead of ‘groups’ in the Results, or ‘recidual’ instead of ‘residual’, etc.) and meaning of words (e.g. neither the difference between the thoracic and mouth flows nor gas compression was measured; they are calculated, estimated, assessed, etc.). Check, please, the text really carefully.

ANSWER:
Discretionary Revisions

(Abstract) The statistical significance is usually presented as $p < x$ rather than $p = x$ (from the definition of this statistical measure).

ANSWER:
- the p-values have been changed to be $< 0.01$ etc. in abstract and the figures.

Level of interest: An article of importance in its field

Quality of written English: Not suitable for publication unless extensively edited

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'