

Interactive comment on “Results from the validation campaign of the ozone radiometer GROMOS-C at the NDACC station of La Réunion Island” by Susana Fernandez et al.

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General remarks.

The manuscript is scientifically very interesting and discusses stratospheric ozone and wind measurements carried out at La Reunion Island by means of a radiometer, GROMOS-C, recently designed and built at the Institute of Applied Physics of the University of Bern. This manuscript is a valuable contribution to the ACP special issue dedicated to NDACC and, in my opinion, should be published.

There are, however, a few issues concerning the manuscript that I think the authors should clarify/address before publication.

C1

1) This manuscript greatly relies on the information provided to AMT readers in a previous published work by Fernandez et al., AMT, 2015 (hereafter F15), where the O₃ measurements of GROMOS-C were first validated. I think the authors should underline in the manuscript the differences and the additional information that this manuscript shows/provides with respect to F15. Furthermore, whenever GROMOS-C technical details or retrieval aspects are mentioned without being explained (which I find appropriate, given F15), the authors should cite F15, possibly explicitly indicating the section or page of F15 where a specific information can be found.

2) The comparison of wind measurements is qualitative, not quantitative. The contour maps shown in Figure 9 are definitely helpful in showing the potentials of GROMOS-C in measuring stratospheric winds, but do not sufficiently characterize the data set and cannot really be considered a validation. For one thing, authors do not state what the uncertainty on the wind vertical profiles is.

3) The case study discussed in section 6, although potentially interesting, is not covered in depth. What is the point of showing this stratospheric updraft? From a scientific standpoint, why do we care about it? What originated it? Where did the air originate from?

4) The comparison of ozone profiles relies partly on Figure 8, and some conclusions are drawn from it. Figure 8, however, appears to compare a daily mean on one specific day. If this is the case, I argue that the comparison on that specific day does not necessarily apply to the whole data set.

5) I feel the overall mixing ratio uncertainty on the GROMOS-C ozone vertical profiles is missing. Why did the authors leave that out?

6) The English could be improved a bit. There are very few grammatical errors, but mostly there are places where adjacent sentences are not well tied to one another and a few changes could make the reading flow better.

C2

7) A few additional citations could be added in the introduction. The older they are, the better.

8) I recommend the authors to use only one altitude variable throughout the manuscript, either pressure or height, their choice, but I would stick to only one of them.

Additional and more specific comments are inserted as notes on the pdf of the manuscript, here uploaded as supplement.

Kind regards, Giovanni Muscari

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/acp-2016-117/acp-2016-117-RC1-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-117, 2016.