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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone monitoring with the GOMOS-ENVISAT experiment version 5" *by* P. Keckhut et al.

Anonymous Referee #2

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GENERAL

This paper discusses a very important research topic. It addresses the capability of a very novel satellite instrument to contribute to the monitoring of the ozone layer. The GOMOS instrument on ENVISAT is in principle self-calibrating but some issues in the dark charge correction seem to disturb the ability to provide a proper trend monitoring. The authors propose a new method to correct for this. Although the topic is very relevant, the authors miss to consolidate their message in a structured paper. The final conclusion of the paper should be that the new ESA v6 might improve GOMOS ozone profiles in Northern Hemisphere mid-latitudes. Given the potential available resources for processing and access to validation data sources, this conclusion should be much stronger and more extended. This would also take away the mismatch between the

introduction and the final conclusion, as the intro addresses the Southern Hemisphere polar ozone loss while the conclusion can only be drawn on NH mid-latitudes. The objective is not clear. The authors should be more specific on which latitude band and altitude range they would like to contribute with GOMOS monitoring. It should also become clear why they limited themselves to just using a very limited set of comparison sites, as the mentioned NDACC, EQUAL and VALID projects provide a much wider global coverage. If the altitude range is limited to those of the reference monitoring satellites, then it should be justified why the authors choose to include comparisons with microwave instruments. The paper would also be much stronger if the prototype version could be used to provide more evidence that the new ESA v6 will provide ozone profiles suitable for ozone trend monitoring. Despite these substantial remarks, I believe this paper has lots of potential and with the required revision should result in a very interesting paper.

MAJOR:

1. The English in writing style in especially the abstract, introduction, section 2.1 and the conclusions is insufficient. The paper would benefit from a native speaker reviewing the text. In general sentences are too long, incoherent and incomplete. In the minor comment section a non-exhaustive list will be provided.

2. The order of the sections might need to be reconsidered if the objective is revised. I would expect the evidence and hence need for an improved algorithm to come before the solution (the new dark charge correction). Unless the authors present a thorough comparison of correlative data with GOMOS data from GOPR v7ab, then the comparison with ESA v5 should come before the proposed new correction.

MINOR:

1. P14714, line 2, remove derived.
2. P14714, line 6, differences between what?
3. P14714, line 11, accordingly to what?
4. P14714, line 14, how much better confidence
5. P14714, line 16, 'since' is incorrect as satellites were already measuring the ozone hole but it was not recognised.
6. P14714, line 21, UV irradiance *at the ground.*
7. P14714, line 23, what is the 'climate impact of the ozone changes'?
8. P14715, line 3, layers are 3 km thick but altitude resolution is of the orde of 12 km, see for example the reference paper of Meijer et al. 2006.
9. P14715, line 3-8, this is one sentence and should be shortened/split.
10. P14715, line 12, change to **self-**calibration and add horizontal before spatial sampling.
11. P14715, line 13-14, is it with continuous coverage or it comes in bands.
12. P14715, line 21, 10% per what?
13. P14715, line 22, '..in the last decade' or since 1995?
14. P14716, line 1, move Bertaux reference to after explaining GOMOS acronym on page 14715.
15. P14716, line 12, remove 'firstly'.
16. P14716, line 12, remove 'already' and 'in' after described.

17. P14717, line 4, 'that provide'?
18. P14717, line 13, 'The star temperature and magnitude. . . ', this should be explained better as this can only be understood by insiders.
19. P14717, line 15, 'quite variable' is very subjectively formulated.
20. P14718, line 6, remove second mention of prototype and state to what it is equivalent to in terms of official ESA release. In general throughout the paper a consistent manner of mentioning and cross-referring would be beneficial.
21. P14718, line 9, remove ', in the sky'
22. P14718, line 21, add a reference to better explain the South Atlantic anomaly.
23. P14718, line 21, why the CCD temperature has increased?
24. P14718, line 25, is it proportional or is there a square root relation with the signal correction?
25. P14719, line 18, again the example of ESA versions mixed with GOPR versions. In addition, the term level-1b is mainly known to insiders.
26. P14720, line 1, add 'in that altitude range' after retrieval.
27. P14720, line 5, 'monitored too much'?
28. P14720, line 22, is there possibly a reference to a paper of van Gijsel in the same special issue to validation of GOMOS ESA v5 data?
29. P14720, line 24, add systems after microwaves.
30. P14720, line 25, why only the brightest stars have been selected? This will be a severe limitation the conclusions of this paper.

31. P14721, line 5, add that the paper of Keckhut et al. is a review paper which is probably the reason why this paper is referenced and not all the previous individual ones.

32. P14721, line 16, remove 'rpimary'.

33. P14721, in general the selction of GOMOS data is not clear; are data flags used, what selection filter is used for dark/twilight and bright limb data, what is done with the reported error in both data sets?

34. P14722, line 7-8, '..small amounts. . . differences of 100%..'. I conclude that GOMOS ozone in these levels were 0.

35. P14722, line 27, add 'range' after dynamic.

36. P14723, line 1, change energy to signal.

37. P14723, line 6, remove 'high' as also lower amounts of aerosol loading will affect the ozone retrievals.

38. P14723, line 7, add 'horizontal' before spatial resolution.

39. P14723, have the OHP lidar data been validated?

40. P14723, line 22, change to '..most profiles AT 40 km..'

41. P14724, line 4, remove '' operated at'.

42. P14724, line 10, '..exhibiting weak signals or large noise levels.' This conclusion is not substantiated with evidence. Did you make a correlation between these profiles and the related parameter? Also the phrase 'These biases may be due to. . . ' is a weak conclusion.

43. P14724, line 16, what are 'known dangerous stars'. Please reformulate or explain better.

44. P14724, line 18, please replace 'implanted' with 'tested in the prototype'

45. In the reference section there is a mixed use of writing all co-author names and writing 'et al.', please be consistent and possibly this is regulated by ACP.

46. P14730, caption, change CDD to CCD and add 'measurement' at the end

47. P14731, please add GOPR before the versions and again CDD to be replaced with CCD. Possibly also add that the observed discontinuities are ok.

48. P14734, add version numbers in the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14713, 2010.