

Interactive comment on “Changes in chemical composition of the middle atmosphere caused by sudden stratospheric warmings as seen by GOMOS/Envisat” by V. F. Sofieva et al.

Anonymous Referee #1

Received and published: 20 September 2011

The paper presents changes in middle atmosphere for ozone, NO₂ and NO₃ in case of sudden stratospheric warmings, using GOMOS data. Since the GOMOS measurements are very accurate and often underexploited, it is interesting to presents such results. The methodology is correct and the paper is easy to understand. Nevertheless, some important points are missing and there are some errors in the analysis; thus major revision is mandatory.

The title is too vague. The changes presented here concern only ozone, NO₂ and NO₃. It seems that “Case studies of . . . (giving the dates) . . .” will be more appropriate.

The main criticism concerns the references. There are 23 auto-references among the

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52 references, which correspond to 44%! It is a record . . . I am very surprised that the authors systematically ignore the work done on GOMOS by other teams.

I think this subject was already studied in the past with other satellite data. Can the author provide a survey of what have been done previously ?

Page 23322 line 14: For the GOMOS validation, the authors give 2 auto-references that are only a compilation of the works done by other teams. This is not a fair approach for the teams who have really performed the work. The authors must refer to: Brachen et al., Adv. Space. Res., 36, 5, 855-867, for comparison with other satellite data. Marchand et al., GRL, 31, L10107 for O₃, NO₂ and NO₃ for self-consistency of the measurements using data assimilation. Meijer et al., JGR, 109, D233052004, 2004, for ozone. Renard et al., JGR, 113, A02302, 2008, using balloon instruments.

Page 23327 line 3: The vertical descent of NO₂ as well as its spatial distribution is well documented in the paper by Renard et al., JGR, 114, A12323, 2009.

Page 23327 line 13: The NO₂ low abundance inside the polar vortex has been known since a long time. For measurements in such geophysical condition and at the time of GOMOS observations, the authors can give the Berthet et al., JGR, 112, D21310, 2007 reference.

Page 23328 line 3: If I well understand, “only” 9 cases are documented, and 2 cases do not follow the trend. What kind of conclusion can be derived, taking into account the previous studies on such phenomena?

Pages 23329 line 9 and 23330 line 17: The strong temperature dependence for the NO₃ production was first demonstrated by Renard et al., J Atmos Chem, 51, 65, 2005 using balloon measurements. Thus, the fact that the NO₃ enhancements are due to change in temperature is not a new result.

Page 2331-2332: The discussion with the negative and positive correlation is partly unclear. We don't really understand which conclusions the authors want to propose.

C9142

Page 23331 line 12: The fact that the SIC model does not include the heterogeneous chemistry on aerosols is a problem. The NO₂ concentration is very dependent on the amount of sulfate aerosols, and on the occurrence of PSCs within the polar vortex prior to SSW periods. At least, the author must estimate the error induced by such approach.

Page 23332 line 10: The fact that the January 2006 and January 2008 events differ from what is observed during the other events can give some troubles on the general rules the authors want to propose.

Figures 3-4: Why the ozone climatology is stopped at a lower altitude of 20 km, and the NO₂ and NO₃ climatologies are stopped at 25 km? Is it based on validation works? In this cases, please give the appropriate references.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 23317, 2011.