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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Zonal asymmetries in middle atmospheric ozone and water vapour derived from Odin satellite data 2001–2010" *by* A. Gabriel et al.

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Response to Referee #2

We thank Referee #2 for helpful comments and suggestions. Following the recommendations of Referee #2 the text is improved and some figures are updated, as listed below. The results and conclusions being more tightened now.

Response to General Comments

1. We would like to note that our analysis do not use a lot of unjustified assumptions from our point of view. Basically we use geostrophic horizontal winds and quasi-geostrophically balanced equations, which are a well known approach, and a linear solution of the transport equation neglecting second order tendencies for the zonally asymmetric perturbations (leading to Equations (3) and (6)), which is also conventional. In the revised manuscript we tried to make this point clearer from the beginning. We agree with Referee #2 that the derivation of wb* have uncertainties, mainly because vertical winds derived via a quasi-geostrophic approach are usually weaker than observed (which is also well known). However, the spatial structure of the wave one pattern in middle atmospheric w* is captured. For a more critical assessment of our winds we add a new figure with the wind components v and w* of ERA Interim (Figure 3.2 in the revised manuscript), which illustrates the uncertainty of the approximated winds more clearly. However, for the linear solutions of the transport equation we use the approximated winds because they allow a homogeneous approach for the whole altitude range of the middle atmosphere. The intension of the linear solution of the transport equation was exactly to provide information on the most important processes generating the wave patterns. Please note that, in our first manuscript, we intend to focus on the extraction of the processes that could be derived from the used data alone, i.e. without too much externally prescribed parameter like chemical loss rates. However, following the recommendation of Reviewer #2, we provide now more new figures (Figures 4.1-4.2, 5.1-5.2 and 6.1-6.2) that give more information on the individual terms in the tendency equation including also some rough estimation on the effect of zonal asymmetries in chemical loss rates. Note here that we cancelled therefore some plots which seems to be less important (old manuscript: SON in Figures 4.1-4.2 and 5.1-5.2 and JJA in Figures 5.3-5.4) in order to avoid too much plots in the paper.

2. The paper is improved and somewhat reorganized. In particular, explanations and interpretations of the processes included in Section 2 (old manuscript) are now shifted to Sections 3 and 4 where these processes are discussed in detail. The paper may be now more tightened and better readable.

3. For the Northern Hemisphere, the terms in the balance equations for O3 and H2O and the related first guess solutions are now given individually (see Figures 4.1-4.2, 5.1-5.2 and 6.1-6.2). It may be much clearer now that the zonal asymmetry in horizontal transport by geostrophic winds is one of the most important factors controlling the wave structures. As described below, we included additionally zonally asymmetric chemical loss rates, which are obviously of less importance for the primary generation of the wave patterns, but which can – in case of ozone – modify the spatial structure of the wave pattern.

4. The primary source for the zonal asymmetries in middle atmosphere is the propagation of ultra-long planetary waves forced from troposphere, and not the loss rates. Because the loss rates are linearly to the field itself, a zonally asymmetric field (e.g. T*, O3* or H2O*) is precondition for an effect of zonally asymmetric loss rates, therefore they cannot be a primary source for the zonal asymmetries. However, we agree with Reviewer #2 if he/she wants to state that the loss rates may have a substantial feedback to the wave patterns. Following the suggestion of Reviewer #2 we incorporate a rough estimation of the chemical loss rates for O3 and H2O in the transport approach, given in terms of temperature-dependent chemical reaction rates and prescribed mean profiles of the involved chemical species (see revised manuscript, Figures 4.1-4.2, 5.1-5.2 and 6.1-6.2, and related discussion). We found indeed an effect modifying the wave structure of stratospheric ozone. However, please note that we do not want to overstate these rough estimations of the loss rates because more investigations based on chemical transport models or circulation models with comprehensive chemistry (including detailed photolysis and highly non-linear catalytic ozone destruction cycles) are needed. In the revised manuscript we tried to make this point as clear as possible.

Response to Specific Comments

1. The matching procedure of the data sets of H2O and T at the specific altitude of 50 km has definitively no influence on the described feature of the change in phase of H2O* and T* with increasing height, which occurs over an altitude range of lots of

kilometres. This switch leads only to a thin perturbation line in the plot indicating that the data do not match exactly. For illustration we add Figures of H2O* and T* without applying this matching procedure in Appendix A of the revised manuscript.

2. We understand the critical comment of Reviewer #2 concerning the uncertainties in the determination of wb*. However, as we now demonstrate in the revised manuscript, the quasi-geostrophic approach leads to a robust picture of the wave one pattern in middle atmospheric w*, although the amplitude is somewhat too weak. Following the suggestion of Reviewer #2 we have included a damping term for zonally asymmetric temperature which lead to some improvements, but which do not change the characteristics of the wave one pattern substantially (note that the related changes in wb* are less than about 10%). From our point of view it is not appropriate to derive wb* in a similar way from transport of ozone or water vapour, mainly because the vertical gradient of the tracers varies much more with altitude (including change in sign) than the global mean temperature gradient used in the quasi-geostrophic approach. We have done some attempts for specific altitude ranges but the results are not suitable to provide an elaborated comparison. Generally the derivation of the wind components from observed tracer distributions via an inversion of the transport equation needs a more sophisticated algorithm, which could be an issue of a further project. Instead – as mentioned above – we have included a comparison of the derived wind components vg and wb* with the wind components v and w* retireved directly from ERA Interim (Figure 3b in the revised manuscript), which may illustrate the uncertainty of the approximated winds sufficiently.

Response to Minor Comments

We have revised the text according to the recommendations. Also captions for Figures 1 and 2 are revised. Thank you.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 4167, 2011.