

Interactive comment on “Assessment of the interannual variability and impact of the QBO and upwelling on tracer-tracer distributions of N₂O and O₃ in the tropical lower stratosphere” by F. Khosrawi et al.

Anonymous Referee #2

Received and published: 31 October 2012

Overall comment: There are several awkwardly constructed sentences in this manuscript. (there are too many for me to detail each one.) It needs editing by one of the native English speakers who are listed as authors.

Here is an example of just one instance: Intro, second sentence starting on line 25...missing a subject? This needs to be rewritten.

Specific questions:

Page 22634 states “The atmospheric tape recorder is caused by the imprint of the

C8801

tropopause temperature on trace gases as e.g. H₂O, CO and HCN and their transport into the stratosphere with the upwelling branch of the Brewer-Dobson circulation.” This is incorrect. Only the water seasonality is due to an imprint of tropopause temperatures, the others have to do with seasonality in tropospheric sources. A reference could be included here....I believe there is a paper by Schoeberl et al. that discusses this (in either GRL or JGR)

Page 22635: How well do the satellite O₃/N₂O relationships agree with those derived from aircraft measurements at the levels of interest? Also, do uncertainties in temperature measurements from the satellite measurements play a role? Also, it appears that you are deeming Odin/SMR to be the most reliable or accurate measurement. Can you show that is the case? (Via a discussion of the validation exercises for each instrument considered.) If you’re going to be combining different data sets, have you show there are not discrepancies between different O₃ and N₂O data sets considered?

Page 22646 states “and vertical velocities are overestimated by models in the lower tropical stratosphere (Ploeger et al., 2010)”; Is this really true for all models?

Page 22646 also states “Though a satisfactory agreement between models and observations was found at 650 ± 25 K (differences generally within $\pm 20\%$) unusually (unrealistically high N₂O mixing ratios (N₂O > 320 ppbv) were found in the Odin/SMR data that were not found in the model simulations.” Aren’t those values unrealistically high not just relative to model simulations, but based on surface measurements? Should they not be thrown out before the analysis is even started?

Figures 3-6...it would be useful to be able to compare the o3/n2o relation for all instruments considered on the same plot...it looks like instrument differences are significantly larger than interannual differences.

Page 22647: discussion of QBO...Given the variation between instruments, I have a hard time being convinced that you are actually seeing a QBO signal in the small variations at the ends of the n2o/o3 curves.

C8802

Page 22648: how do a greater number of observations lead to a lower standard deviation? (discussion of MLS data). Is the issue here really that Odin/SMR has a much lower precision than MLS? And how does coarser spatial resolution lead to lower inter-annual variability? I'd be more convinced if you could demonstrate this with synthetic data compared using different sampling and vertical weighting.

Page 22649 states "In the SD-WACCM simulation the QBO is realistically represented and arises solely from the nudging of the WACCM dynamics with GEOS5 meteorological fields. As in our recent model evaluation study the curves of monthly averages of N2O and O3 derived from SD-WACCM are at 500 ± 25 K steeper (but not as steep as E5M1 and KASIMA) than the ones derived from the satellite data which can most likely be attributed to a stronger tropical upwelling in the model simulation than observed." Could you at least overplot the MLS curves on the WACCM (figure 6) curves? It looks like MLS has the 500K relationship as steep as the model, whereas ODIN/SMR and MIPAS are not. I'm also having a hard time seeing that the curves extend to much lower N2O values...picking one month and overplotting the model and satellite derived curves however may help in that demonstration. It may very well be that the real amount of mixing is not well represented in the model; however, I find it hard to see that in the plots and discussion given here.

Page 22650 states "In the tropics, monthly averages of N2O values at 650 ± 25 K were much higher derived from Odin/SMR observations (reaching up to 330 ppbv) than simulated by KASIMA and E5M1. These values are even higher than the highly accurate ground-based observations of N2O (319 ppbv in 2005) derived in the troposphere (Forster et al., 2007). In the Odin/SMR data these high N2O mixing ratios occur solely in the tropics and with a seasonal cycle." Isn't this telling you there is a problem with the Odin/SMR measurements?

Figure 8: Do you really mean "equivalent latitudes" from 10N-10S? How do they differ from geographic latitudes and how were you able to calculate equivalent latitude for the tropics?

C8803

Page 22651: states "Further, the N2O fields for the stratosphere (Fig. 8 third panel) show that due to a stronger upwelling N2O was transported higher up in 2002, 2004, 2006, 2008 and 2010 which is in agreement...." This paper could use a simple description of how the secondary circulation varies according to phase of the QBO...to better explain why you're expecting a variation in upwelling. What might even be more useful is to see if you can determine what the QBO easterly vs westerly upwelling difference might be based on the N2O measurements, and if it's consistent with theory.

Page 22651 states "The fact that lower N2O averages than Odin/SMR (330 ppbv) are found in the Aura/MLS observation is likely caused due to the coarser vertical resolution of Aura/MLS as can be seen from Fig. 9." Is that really the case, or is there a problem with Odin/SMR. As I noted previously, you can actually test this by using synthetic data at applying the appropriate averaging kernels.

In regards to measurements in the stratosphere greater than 330 ppbv....are these even remotely realistic? I just looked at the NOAA GMD surface measurements (see http://www.esrl.noaa.gov/gmd/dv/hats/cats/cats_conc.html) and nothing is this high over the time period in question. If the tropospheric average is only 319 ppbv (as stated on page 22652) I question the value of discussing these high biased measurements in detail. It appears that these values are a result of a high bias, which therefore brings some of the analysis discussed in this paper in question.

Page 22654: states "The QBO in SD-WACCM is realistically simulated and in good agreement with the QBO derived from Odin/SMR (not shown)." How is the QBO derived from Odin/SMR? Do you just mean the pattern in N2O, or some derived upwelling estimates?

Page 22657: states "We found that the inter-annual variability is low and can easily be distinguished from model deficiencies." However, the inter-instrument variability does not appear to be low. Can it "easily be distinguished from model deficiencies"?

Page 22658: states "We attribute the steeper correlation in the model simulations to

C8804

a incorrect simulation of tropical upwelling which is due to a missing or incorrect simulation of the QBO." You should describe in detail what manner of incorrect tropical upwelling would change the O3/N2O correlations on a potential temperature surface, and in what manner. That has not been done clearly in this paper.

Page 22659 states "Such a high positive bias was not found in validation studies performed applying Odin/SMR N2O observations.." Where did validation data for 650K in the tropics come from? Were there balloon tropical N2O measurements?

General assessment: It's difficult to understand the main point the authors want to make with this paper. If they want to do a detailed study of how the QBO impacts N2O and N2O/O3 correlations, it needs more work. It seems that currently the purpose of the manuscript is to attribute anomalously high values of N2O observed in the tropical stratosphere to the QBO, and state that models aren't doing it correctly. However, what the authors need to do is demonstrate that those anomalously high values of N2O are actually real.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 12, 22629, 2012.

C8805