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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Water Vapour and Methane Coupling in the Stratosphere observed with SCIAMACHY Solar Occultation Measurements" *by* Stefan Noël et al.

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We thank the reviewer for the comments and will consider them in the revised paper as described below. In the following, the original reviewer's comments are given in *italics*, our answer in normal font and the proposed updated text for the new version of the manuscript in **bold** font.

General comments

- *This paper describes a water vapor data set derived from SCIAMACHY solar occultation measurements. It covers the altitude region from 17-45 km and the latitude region from 50-70N over the time period Aug 2002 to Apr 2012. The authors describe the method, the data set and then attempt trend analysis and describe the co-relationship between their CH4 and H2O data. I think a new data set is a valuable contribution, and the validation comparing to ACE and MLS is also valuable. The analysis of variations related to the QBO and discussion of the BDC is repeating work that has already been done, much going back to studies from measurements taken by UARS or LIMS/SAM. I think the paper could be significantly shortened into a data description/validation paper and much of the QBO and total hydrogen (or potential water) discussion eliminated.*

  The aim of the paper is not only to present and validate the new SCIAMACHY H2O data set. We also want to show the usefulness of the H2O SCIAMACHY data in combination with other data, e.g. in the context of dynamical studies. The results obtained related to BDC or QBO are indeed not new, but we can confirm them with the new SCIAMACHY data. Therefore we prefer to keep the discussion on dynamical effects in the paper, but will clarify this in the revised version (see also answers to comments of other referees).

- *General comment: Please have the native English speaking co-author edit the text when revised.*

  Will be done. Therefore, the updated text might change slightly in the final revised version.

Specific comments:

- *Abstract, line 13-15, I would think that at lower altitude, water vapor is largely impacted by the stratospheric input value (so tropical tropopause temperatures). The "balance" hasn't had time to be established with young lower stratospheric air.*

  Agreed, "balance" is misleading. We will reformulate this as follows:

  The SCIAMACHY data confirm, that at lower altitudes the amount of water vapour and methane are transported from the tropics to higher latitudes via the shallow branch of the Brewer-Dobson circulation. Further, the increasing methane input into the stratosphere due to the rise of tropospheric methane after 2007 may contribute to the increased water vapour.

- *Page 1, Introduction, L17-18, the climate of the planet is determined by many factors, not just greenhouse gases. Please rewrite this sentence.*

  Agreed. New text:

  Water vapour (H2O), methane (CH4) and carbon dioxide (CO2) are all greenhouse gases.

- *Page 2, line 3, the sentence "Most of the water vapour is of natural origin and located in the troposphere."and then change "It enters" to "Water vapor enters"*

  This part will be rewritten:

  The amount of water vapour in the troposphere is very large compared with that in the rest of the atmosphere. Water vapour enters the stratosphere mainly through the tropical tropopause layer ...

- *Page 2, line 8, I don't think this is an entirely accurate statement, in particular that the BDC controls the freeze drying process. The BDC is a zonally aver-*

aged construct, and freeze drying (and the associated microphysics) is a local process.

The term "controls" indeed might not be accurate here w.r.t. to freeze drying. We will reformulate the text as follows:

The Brewer-Dobson circulation controls the tropical upwelling, i.e. the transport of air masses from the troposphere into the stratosphere (both water vapor and methane) and influences the freeze-drying, i.e. the process through which the tropopause acts as a cold trap such that water vapour partly freezes out before reaching the stratosphere.

- *Page 5, figure 2; (and related text). Some descriptions as to what the improvements made in the algorithm between the 2010 product 2.0.2 and the current one is warranted (rather than simply referring to the 2016 methane paper).*

  We will add the following information:

  This is due to the improved retrieval method as described in Noël et al. (2016). The most relevant changes are:

  - **Use of a weighting function DOAS based fit at each altitude.**
  - **Better consideration of altitudes below the actual tangent height.**
  - **Improved selection of measurements.**
  - **Use of improved input spectral data (better pointing information and calibration).**
  - **Use of an updated radiative transfer model (SCIATRAN V3).**
  - **Updated error calculation.**

- *Page 8, line 3&4..i think you mean biennial not bi-annual*

  Indeed. Will be changed.

- *Page 8, discussion of the "inverted behavior" (or anti-correlation) between water and methane. This is well known behavior and probably doesn't need the extensive following discussion regarding the QBO.*

  As mentioned above, we would like to keep this discussion on QBO in order to show the capabilities of the SCIAMACHY data.

- *Page 11: line 14. You don't have a long enough time series to talk about 5-6 year oscillations, just delete that comment.*

  We agree that it is difficult to tell if this 5-6 year periodicity is real from our data, as we state in the text. To clarify this we will reformulate this sentence to:

  This implies a periodicity of about 5 to 6 years, but due to the limited length of the time series, this can only be confirmed in the future.

- *Page 12: trend discussion: the data set under consideration is just 10 years. That is not long enough to talk about trends. The so called trend noted on line 8 (Urban et al 2014) is really a step function like feature, not a trend. With 10 years, you can look at interannual variability, and perhaps should stick to just that. Show a time series, not a linear trend.*

  Indeed 10 years is too short for a trend in the climatological sense. Therefore, what we present here are essentially estimated changes over this time interval. Knowing their limitations, these changes can nevertheless provide interesting information. We therefore would like to keep the "trend" results in the paper, but we will add a clarification at the begin of the trends section:

  The time series of SCIAMACHY data covers only ten (nine complete) years. Consequently it is not possible to derive from these data long-term trends. In this sense, the trends shown in the following have to be interpreted as changes over the corresponding time interval 2003 to 2011. To derive these changes, a linear regression has been fitted to the water vapour anomalies ...

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- *Page 12, line 13 "an estimate" is duplicated*

  Will be removed.

- *Page 12, line 14. It is not true to say that if potential water is conserved, the trend should be zero. You could have a trend in water vapor entry value, thereby allowing a potential water trend. You could also have a trend in the input of methane, again allowing a potential water trend.*

  We are referring here to the trend in potential water, not then individual CH4 and H2O trends. A trend in the H2O or CH4 input would indeed result in a corresponding potential water trend, but then potential water would not be conserved (unless both trends balance, which is not expected for tropospheric trends). On the other hand, if potential water is conserved, there should be no trend in potential water.

  For clarification, we will reformulate this sentence:

  If potential water is conserved, the potential water trend should be zero.

- *Page 12: I really don't understand the point of this sentence "Considering this error, the combined trend above about 20 km is in a statistical sense not significant, meaning that the assumption that all water vapour is produced from methane via the net reaction (R2) is not disproved by the measurements." One should keep in mind that all water vapor is not produced from methane (ie, the average entry value is on the order of 3.5 ppmv, current methane is* $\sim$*1.8 ppmv, so if all were oxidized you could get a contributions of 3.6 ppmv, so at most you could get half of water vapor from methane. It may be that here the authors are trying to assess contribution to the trend. Rohs et al, 2006, JGR, determined for the 78-03 trend in stratospheric water vapor, only 25% can be due to a trend in methane. A similar analysis could be done here, for the SCIAMACHY period.*

  Indeed, since we are looking at anomalies, we refer here to the changes of water vapour and methane, i.e. stratospheric production/loss. As suggested by referee #1, this sentence will be changed to:

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Given that the trends in potential water between 21 and 45 km lack statistical significance, there is no evidence that water vapour is produced in the stratosphere by any mechanism other than methane oxidation.

The analysis of Rohs et al. requires as input in addition to stratospheric CH4 and H2O trends also the tropospheric CH4 trends and information about age of air. It is not possible to derive tropospheric trends and age of air from our data, and the stratospheric trends we derive are very small and often not significant (as are the tropospheric trends during this time period). Therefore we think it is not reasonable to include results from such an assessment in the manuscript.

- *Page 14, line 26-30: this description of the processes going on is in error. In the upper altitudes, water vapor changes are anti correlated with methane, and simply reflect age of air variations; the QBO signal is not "carried by methane".*

This paragraph has been reformulated for clarification (see also comments of other referees):

The QBO signal is observed in both methane and water vapour at higher stratospheric altitudes. In contrast, the tropospheric methane entering the stratosphere via the lower branch of the Brewer-Dobson circulation is not impacted by the QBO at lower altitudes. The QBO signature in the upper altitude data can be explained by a QBO-dependent modulation of the transport to higher latitudes via the deep branch of the Brewer-Dobson circulation, similar to the variation in tropical aerosol extinction coefficients as seen by Brinkhoff et al. (2015) at 30 km.

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