

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.

- *This study nicely presents the SCIAMACHY H₂O and CH₄ measurements and their relationship. The SCIAMACHY measurements are a very valuable addition to the available H₂O and CH₄ measurements in the middle atmosphere over the*

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period 2003-2012, and the results shown here are scientifically valuable. However, in much of the text the authors seem to be trying very hard to create a mystery where there is none. There is (1) a QBO signature in H₂O crossing the tropical tropopause and (2) a QBO signal due to changes in transport (age-of-air) which causes a variation in the amount of CH₄ that has been oxidized to produce H₂O. The authors repeatedly overemphasize the importance of small tropospheric CH₄ variations on the observed variations in stratospheric H₂O. While gradually increasing anthropogenic CH₄ is a very important driver of long-term change in H₂O, variations in CH₄ entering the stratosphere are only marginally relevant to the variations observed in these measurements, which span a decade. Small changes in tropopause temperature are a far more important driver of interannual changes in H₂O entering the stratosphere as has been shown by many authors (e.g. Dessler et al., JGR 2014).

We agree that some of the statements/formulations in the manuscript may be misleading. We do not aim to propose new dynamical processes or explanations. Our intention is to present the new SCIAMACHY H₂O data set and show via the combination with CH₄ that information about atmospheric dynamics can be derived. This is not necessarily new information, but it shows the usefulness of the SCIAMACHY data.

We will clarify this in the revised version of the manuscript (see answers to the following comments and also our answers to the comments of referee #1).

- *Figure 11 is appropriate for a review paper on atmospheric dynamics, and might be appropriate if the authors were running a dynamical model to compare with their measurements, but it seems inappropriate here.*

We agree that Figure 11 does not present any new results. However, it summarises the different dynamical processes discussed in the manuscript and is therefore considered as helpful especially for the non-expert reader. We therefore prefer to keep this figure in the manuscript but will move it to the (modified)

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introduction.

- On page 14 line 7 they state: “This is not the case for methane, which could explain the missing QBO signature in the methane time series at 17km.” There is no need for a “could” here. The H₂O entering is governed by tropopause temperatures, and the CH₄ is not.

Agreed. We will remove “could”.

- In paragraph following this (and in the last sentence of the conclusion) they again try to overemphasize the importance of CH₄. There is nothing inherently wrong with pointing out that changes in CH₄ may play a small part in the observed changes of H₂O, but an increase of 8 ppbv/yr in CH₄ over 4 years would yield only at most ~0.064 ppmv of H₂O over 4 years. This looks small when compared to the observed variations in potential water, and if CH₄ were the major driver of these variations potential water would not show decreases. Only finally, at the end of this paragraph, do the authors mention that: “However, from the current data set an additional influence of varying tropospheric water vapour input on the observed increase of potential water cannot be ruled out.” This is certainly the primary driver of the variations in potential water, as is well understood. In the last sentence of the manuscript the authors again seem to only reluctantly accept that “possibly in combination with changes of water vapour” are important. Presumably this refers to changes in water vapour entering the stratosphere, but even that is not clear.

Actually, the referee is right here. An increase in CH₄ due to tropospheric trends alone cannot quantitatively explain the observed increase in potential water.

We will therefore reformulate this part:

Schneising et al. (2011) estimated for the time interval 2007 to 2009 a tropospheric increase of methane of about 8 ppbv year⁻¹ following a period of no significant change from 2003 to 2007. Taking into account the delay

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between the tropospheric and a possible stratospheric trend related to the age of air (about 2–3 years between injection into the stratosphere at the tropics and measurement at 17 km at higher latitudes according to Haenel et al., 2015) explains part but not all of the increase of potential water at lower altitudes after 2009/2010 shown in Fig. 9. An additional influence of varying tropical tropospheric water vapour on the observed increase of potential water is therefore likely. Prior to the end of 2011 the positive potential water anomaly extends to higher altitudes. This is in agreement with the increasing age of air at higher altitudes.

The conclusions will also be adapted:

The increase of tropospheric methane after 2007 reaches these lower stratospheric altitudes with a delay of about 2 years. This contributes in part to the observed increase of potential water after 2009, but additional processes such as changes of tropospheric water vapour input are required for a quantitative explanation.

- Then, in the final paragraph of the discussion they say: “A remaining open issue is the QBO signal observed in both methane and water vapour at higher stratospheric altitudes ... Therefore the QBO signal has to be carried by methane, but as can be seen at lower altitudes the methane entering the stratosphere is not varied by QBO.” This is all well understood, as the authors finally admit in the second half of this paragraph.

As requested by reviewer #1, the introduction of the revised paper will contain more information about known dynamical processes.

For clarification, we will also reformulate this part as follows:

Above 20 km, in the region of the deep branch of the Brewer-Dobson circulation, air is older. This enables oxidation of methane to water vapour to be completed rapidly. As a result variations of both gases are in phase and

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potential water is essentially conserved (Fig. 7). Consequently at these altitudes water vapour changes can be concluded to be determined by the oxidation of methane. 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 abstract is similarly unnecessarily confusing. First, the phrase “SCIAMACHY methane and water vapour time series reveals that stratospheric methane and water vapour are strongly correlated”. The implication seems to be that this is a new result. Please rephrase this as “reveals [or, better yet, “shows”] the expected anticorrelation between methane and water vapour”.*

We will rephrase this sentence as suggested:

The combined analysis of the SCIAMACHY methane and water vapour time series shows the expected anti-correlation between stratospheric methane and water vapour and a clear temporal variation related to the Quasi-Biennial-Oscillation (QBO).

- *The next sentence reads: “Above about 20km most of the water vapour seems to be produced by methane, but short-term fluctuations and a temporal variation on a scale of 5–6 years are observed.” First, there is no reason for a “seems” here. The authors should be able to calculate how much of the observed water vapour is produced by methane. Secondly, I do not understand how the second part of this sentence follows from the first following a “but”.*

This part of the abstract will be reformulated accordingly:

Above about 20 km most of the water vapour is produced by methane. In addition, short-term fluctuations and a temporal variation on a scale of 5–6 years are observed.

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- *I finally have to admit that I do not understand what new point the authors are trying to make in the last sentence of the abstract.*

There is indeed no new finding here. We only want to state here, that the described effects can be seen in the SCIAMACHY data.

We will clarify this:

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 have contributed to the increased water vapour in the extratropical lower stratosphere as observed by SCIAMACHY.

- *A few minor additional points in the text: I don't understand the statement on page 2 line 19: “roughly conserved in the stratosphere if no changes in mixing of air masses occur”. What does “changes in mixing of air masses” mean?*

This refers to additional production / loss processes others than via the net reaction (R2), like production of H₂O by oxidation of other hydrocarbons, but these are indeed rather negligible (as e.g. stated by Nassar et al., 2005). We therefore will remove “if no changes in mixing of air masses occur”.

- *On page 9 line 6: “the remaining sensitivity of the retrieval method to aerosol” is rather a roundabout way of saying “errors in the water vapour retrieval due to aerosols”. This is essentially what the authors say in the next line.*

To clarify this, we will reformulate this sentence as follows:

Note that this observed reduction of water vapour after the Sarychev eruption may be introduced by errors in the water vapour retrieval due to the remaining sensitivity of the retrieval method to aerosol.

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