Atmospheric
Measurement
Techniques
Discussions

Interactive comment on "Evaluation of Water Vapour Assimilation in the Tropical Upper Troposphere and Lower Stratosphere by a Chemical Transport Model" *by* S. Payra et al.

Anonymous Referee #1

Received and published: 14 April 2016

This paper reports a technique for assimilation of MLS water vapour in the UTLS into the MOCAGE chemical transport model. In principle this is a fruitful line of research - as is well known measurement of water vapour in this region of the atmosphere has many technical difficulties. Furthermore there still remains significant uncertainty over the relative importance of different processes that potentially affect water vapour and determine the variations in its concentration on daily to decadal timescales.

However after reading this paper I was left wondering what what exactly had been gained by the assimilation process described. To me it seemed as if the overall outcome was that in 'MLS space' (a term which was poorly explained) the assimilated water vapour fields matched the MLS observed water vapour fields. So the assimi-

lation simply seemed to provide a systematic (and perhaps very satisfactory) way of interpolating the MLS observations in space and time.

To me the potential gain of assimilation is that it acts as a kind of filter on a given set of observations - selecting (and gaining value from) those aspects of that set that are not in strong conflict with other observations and the underlying model. Here the 'other observations' are meteorological observations that determine the state of the ARPEGE model and hence provide a kind of the lower boundary condition for the MOCAGE model - but the evidence presented in the paper seems to imply that they play very little role in determining the concentrations of water vapour resulting from the assimilation. Alongside this, the model used as a basis for the assimilation apparently simply treats water vapour as a tracer, with for example, no loss through condensation. This is clearly an unsatisfactory model for water vapour in the tropical UTLS.

So my view is that the authors provide a clearer justification for the procedure they have chosen. Why, for example, have they essentially imposed a discontinuity - across 135 hPa - in the influence of ARPEGE on the one hand and MLS on the other. Would it not be better to have some kind of blend of the two in a transition region. Why did they not include some kind of saturation criterion in the MOCAGE model for water vapour? The authors should also make it clearer what in their view has been gained from the assimilation. (Is it any more than spatial and temporal interpolation of the MLS fields?)

There are many improvements that could be made to the paper on a detailed level - as set out in comments below.

l23: 'hPa hPa' > 'hPa'

l34: 'In the MLS space', 'in the model space', etc - I found it difficult to guess what precisely you meant by the term 'space' - use some more straightforward terminology?

l40: 'prevent to assess' - this is an example of a minor problem with English grammar (MPEG) - should be 'prevent assessment'.

l52: 'and transported from one place to another on the globe' > 'and to transport it from one place to another on the globe' (MPEG)

l53: 'Unlike other greenhouse gases' > 'unlike some other greenhouse gases', or perhaps the sentence is confused by 'additional water vapour' - 'additional' with respect to what?

l61: Seems odd to give Panwar et al., 2012 as a reference for low concentrations of stratospheric water vapour. The fact that stratospheric water vapour concentrations are low has been known for a long time (not just since 2012) and over the years there have been several review papers on this topic (e.g. see reference list of Randel and Jensen 2013).

l80: 'Heggling' > 'Hegglin' (!)

l94: 'Around the tropopause, large gradients in H2O and interplay of transport processes between troposphere and stratosphere, mainly due to rapid change in H2O by deep convection' - re deep convection - are you thinking specifically of the tropical tropopause? Even here it is not clear to me that deep convection is the most difficult process to understand nor the main mechanism for generating large gradients.

l122: 'The changes mostly impacted H2O fields over this period' > 'The changes over this period which have had most impact on H2O fields' (MPEG)

l140: 'there is no lower stratospheric wet bias as suggested in previous studies' âĂŤ of course these studies were not of ERA-I - so needs to be amended to something like 'in studies of earlier ECMWF analysis or re-analysis fields'.

l243: 'bias' in this case presumably means 'difference between MLS and frost-point hygrometer' - i.e. at 83hP and 100Pa MLS is showing larger water vapour concentrations than the frost-point hygrometer? Please clarify.

l249: 'following the three independent vertical layers in the TTL' - do you mean 'vertical levels' - i.e. you are considering MLS-derived water vapour mixing ratios for these

precise levels - or do you mean finite layers (each centred on one of these levels?

l332: It would be useful to know a bit more about the physics of water vapour in ARPEGE. Is there condensation immediately saturation is reached, or is some limited supersaturation allowed?

l356: 'capability to include the effect of the averaging kernel' - this is the first time you have used the term 'kernel' - do you mean the MLS averaging kernel? If yes then I am confused by your previous reference to particular layers (or levels). Again please clarify - perhaps the assimilation uses the MLS observations directly via the averaging kernel, but your subsequent analysis is level-based? (This may be a distraction since later - l386 - you seem to say that you do NOT use MLS averaging kernels - in which case it would have been better to make that clear at ∼l356.

l386: As noted above you seem to say here that averaging kernels were not use - i.e. you use MLS estimates of concentrations on particular levels? Please confirm. (Of course, the MLS estimates on the levels in the end come from averaging kernels - do you understand why the use of averaging kernels in the assimilation causes problems?)

l377: Is this 'simple parametrisation' based on previous experience?

l403: To clarify - the 'free run' is (within MOCAGE) simply treating water vapour as a conserved chemical species?

l410: 'Three levels will be studied in detail: 121 hPa (UT), 100 hPa (TP) and 68 hPa (LS).' - this repeats exactly what you have said earlier.

l419: I'm still unclear on what exactly you mean by 'in the MLS observation space'. Also where does the background profile' in Figure 5 come from? (Indeed where do the background fields displayed in many subsequent Figures come from?)

l453: This now explains what is meant by 'MLS observation space' etc. This explanation should have come much earlier.

l483: To be explicit, when you say 'cannot cope with supersaturation' I think you mean 'allows supersaturation, i.e. does not impose any kind of saturation condition'. Clarification would be helpful.

l509: "At 68 hPa (Fig. 12), the background and the MLS analyses ($\sim$4 ppmv) are very consistent with the MLS observations (Fig. 1), whilst ARPEGE is much drier (< 2 ppmv) and the Free run is much wetter (> 6 ppmv). The assimilation system behaves nominally in the lower stratosphere since the background is no longer affected by the Free Run even outside of the assimilation window when and where the MLS observations are taken into account." - You have previously (Figures 5 and 6) shown that there is excellent agreement between MLS and MLS analyses. So aren't you simply repeating that point.

l663: "Sensitivity studies show the great improvement on the H2O analyses in the tropical UTLS when assimilating spaceborne measurements of better quality particularly over the convective areas." - I assume that this refers to the work reported in 7.2 - but in reading that section I didn't get any clear sense of 'great improvement' - only that there were differences in the analyses when different data versions were used. So 7.2 should be clearer on where exactly the improvement is identified.

l971: Figure 7 and subsequent Figures. Why do you have the boxes (which I guess correspond to the geographic regions defined for Figure 6) marked only on the 'Free Run' panel? This seems odd when the 'Free Run' is not actually a case included in Figure 6.