Atmos. Chem. Phys. Discuss., 8, S935–S938, 2008
www.atmos-chem-phys-discuss.net/8/S935/2008/

Atmospheric Chemistry and Physics Discussions

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8, S935–S938, 2008

Interactive Comment

Interactive comment on "The roles of convection, extratropical mixing, and in-situ freeze-drying in the tropical tropopause layer" *by* W. G. Read et al.

Anonymous Referee #2

Received and published: 25 March 2008

This is an interesting paper. The idea that extratropical mixing could undermine cold point temperature control of stratospheric entry water vapor mixing ratios is interesting. (It should be kept in mind that the extratropical mixing timescale is not very well known, and its specification in the model is quite ad hoc). The comments on the necessity of convective mixing in the TTL in order to give rise to an upward propagation of the semi annual signal in CO were also interesting. My main difficulty with the paper was that it was not very readable to me. This is due, I think, to a variety of reasons, but these include (1) the complexity of the model, (2) that it wasn't motivated as well as it could be, (3) the writing was unclear at times, and (4) the extensive use of acronyms. I don't have very many specific recommendations for improvement, other than possibly getting rid of the Gaussian cold trap and driving the model using observed MLS temperatures alone. I think the paper is publishable. But I also think it would be more influential and understandable if changes were made to address the concerns listed below.

Model Structure: Some of the difficulty that I had understanding the paper comes from the difficulty I had in understanding the structure of the model. It wasn't clear to be how or why the model temperatures were prescribed the way they were. The following comments relate to this aspect of the model.

I had difficulty understanding the paragraph starting with "Section 2 CCT-TTL Model Description". For example, p. 3964, line 25 mentions that the horizontal domain of the model is 18,000 km, in 14 equally spaced bins. I gather that there was no attempt to match a model bin to a particular latitude range, even though the use of real time MLS temperatures could have been used to do this. However, from what I understand, all MLS temperature profiles between 12S - 12N from a given day were averaged to produce a daily average profile, which was then used at the 147 hPa and 68 hPa levels at every grid point. I am not sure that the paper ever specifies what the model pressure levels are.

As an example of aspects of the writing that could be improved, I had a lot of difficulty with this sentence: "The daily temperature profile at the 147 and 68 hPa levels is the tropical average." Does this refer to all 14 bins? How can a profile be defined at a level?

Initialization of the 100 hPa temperature: It seems very strange to arbitrarily define the 100 hPa daily temperature to be the warmest MLS temperature. Why? And why then artificially impose a cold trap when you have observed temperatures available? The model is trying to explain observed MLS water vapor mixing ratios. However, it does not seem to be making a serious effort to impose an observed temperature structure.

Every model tries to explain some observed variable in terms of some combination of physical assumptions, plus variables that are constrained by observations. I had difficulty understanding the philosophy guiding this model.

ACPD

8, S935–S938, 2008

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For example, I viewed it as unfortunate, due to its design, that the model was not able to make comparisons with the MLS data in latitude/longitude, either climatologically or in real time.

Why include vertical diffusion? This process would be intended to describe mixing by sub gridscale processes. But what is meant by sub gridscale would depend on the grid size, and the horizontal and vertical transport that is included in the model. I would suggest removing this term, especially if it makes no difference, as the choice of numerical diffusivity would be to some extent model dependent. Or is it needed as a numerical smoother?

1/td = dw/dp + 1/ta. Two of these have to be determined independently in order that the third may be determined. It is worth reminding the reader here that the vertical variation of ta is prescribed from HG01. It appeared that td was then calculated from w (omega) and ta, but maybe make this explicit.

page 3966, line 11: "extratropical supply" -> "extratropical mixing ratios"?

page 3968, line 26: "non-condensible" -> "insoluble"

page 3969, line 1: "In the CCT - TTL model, CO is represented by the time tendency of the vapor equation and all terms involving condensed phases vanish." Better expressed as: "In the CCT - TTL model, the rate of change of CO with time can be represented by an equation similar to that given for the time tendency of water vapor, except that all terms involving condensed phases are removed."

page 3971, lines 1 - 3: did not understand this sentence. Perhaps a diagram, or equation, would help.

page 3972, lines 13 - 15: The 147 hPa Aura MLS H2O and CO would only be input to the bottom of the SA version, presumably, since that is the version where air is rising at the bottom (?). In the other two versions, air is sinking at 147 hPa, and a bottom boundary condition would not be needed? Also, it seems odd that a 147 hPa CO value

ACPD

8, S935–S938, 2008

would be used for a convective input mixing ratio. Would it not be more appropriate to use a boundary layer CO value, as this would be more consistent with the assumption of no mixing during convective ascent (which is invoked elsewhere).

page 3975,line 20: The presence of subvisible cirrus does not necessarily imply dehydration, e.g. the ice crystals may be too small to fall out.

page 3975, line 28: previous authors have argued that convective dehydration, can, at least in principle, explain the annual cycle.

page 3978, lines 19 - 20: "Therefore it appears difficult to simultaneously achieve good agreement with H2O and dD with convection mixing 100% RHi vapor in the TTL". The mass of an ice crystal at 100% RHi will be constant; i.e. no net evaporative or condensational growth since the evaporative flux equals the condensational flux. However, the water molecules on the surface of an ice crystal at 100 % RHi continue to evaporate from it, so that the isotopic signature of the surrounding water vapor can still be influenced by the presence of the ice. I am not sure if the model took this effect into consideration. I agree that in the absence of net evaporation of ice, the exchange of water molecules might be restricted to the topmost layers of the water molecules, so would be slower than with unsaturated RHi $<$ 100% air.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3961, 2008.