Atmos. Chem. Phys. Discuss., 8, S2227–S2236, 2008
www.atmos-chem-phys-discuss.net/8/S2227/2008/

Atmospheric Chemistry and Physics Discussions

ACPD

8, S2227–S2236, 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.

W. G. Read et al.

Received and published: 30 April 2008

Thank you Stefan for your careful reading of this manuscript and constructive critique. I will try my best to answer your concerns. I have tried to bring together in a simple conceptual model of the TTL the major mechanisms that are believed important and use observations of three constituents H2O, HDO and CO to identify/detect the action of a given mechanism. Unfortunately each species and each mechanism requires parametrizations that are unique to it and this leads to I agree a large number of parameterizations. I believe the choice of parametrization I chose (except for total water detrained from convection) is based on values used in past literature. I will try to concentrate on making the revised paper focus on the robust conclusions that is model predicitions that are not sensitive to the parameterizations. As I have mentioned in response to other referees concern about the handling of the MLS temperature will

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

be different. The Gaussian cold trap is eliminated and the measured 2 D structure as observed by MLS will be used directly.

Replies to specific comments (Stefan's original comment in *italics*.

P3962/L24: I do not think that there is sufficient evidence to say that stratospheric water vapour increased at twice the rate expected from methane in a matter of fact tone. The recent re-evaluation of the Boulder balloon frostpoint data by Scherer et al.(a paper that may be worth mentioning in this context) yields a substantially smaller trend (up to 40percent smaller) than published by Oltmans et al. (2000) and Rosenlof et al. (2001). The remaining trend then deviates not so much from the methane induced trend anymore. However, their analysis also shows that at present it is virtually impossible to have faith in any trend estimate given the large discrepancies between HALOE and frostpoint data.

I will add the reference and the caveats.

P3963/L14: It probably would be fair to say that the isotope data to date also do not give a coherent picture - the Webster and Heymsfield data look very different from what Kuang et al. derived; and I believe the more recent Harvard data looks different from either of these. As you say later (next page, Line 8) observations can be reproduced by models with different mechanisms, which demonstrates that currently isotopes cannot control dehydration/hydration processes either! Also, I'd suggest to combine the paragraphs that mention isotopes into one.

I agree, I think a lot of people had hoped that the isotopologues would distinguish between convective and in situ processes but instead are consistent with the presence of both processes occuring simultaneously. I don't think the Webster data contradict the Kuang or ACE FTS data. If you consider only the clear-sky data and average the values, the ALIAS data measures -650 per mil in the TTL (figure 2). Certainly the Webster data show a lot of variability but so does the ACE-FTS (see figure 6 of this paper).

ACPD

8, S2227–S2236, 2008

P3963/L20: I do not think that there is any evidence for a transport barrier; certainly the picture that emerges from clear sky radiative transfer calculations can be very misleading (cloud radiative effects do play a role, and latent heating from condensation (in convection) provides sufficient energy to maintain the diabatic mass flux well into the region where radiative heating also under clear sky conditions is positive).

The way the model is set up clouds can be formed in only two ways 1) from convection and 2) in situ condensation. In the absence of these then air at the level of zero radiative heating just sits there. I believe you can see evidence of this in the MLS H2O data. In January near Costa Rica which is far from convection and clouds (except maybe some subvisual cirrus) The MLS H2O profile along with CFH and WB57 H2O show a transitional kink at 150 hPa where the H2O profile gradient changes from steep to shallow. You don't see this in the v1.5 data because of limitations introduced by its coarse vertical sampling. Folkins et al., 1999 shows evidence of this in O3. Latent heating is important below the level of neutral buoyancy. In the Folkins convection scheme (2002) entrainment is proportional to latent heating and this term is essentially zero above 12 km. In his convective parameterization which I am using, latent heating effects are negligible.

P3964/L25: Perhaps the expression that temperature drives the model could be changed to just saying that you use temperatures by AURA MLS?

Sure and the relationship in the revised manuscript and model is more direct.

*P3965/L1-12: The construction of the temperature at 100hPa is quite awkward. I understand that you need to idealize the temperature field, however the decoupling of the flow variations and temperature variations is one of the truly weak points of the Holton and Gettelman model. Quantitative estimates of such a model are, strictly speaking, not overly meaningful (i.e. the model allows to show that *if* the flow is as prescribed, then a certain water vapour results from the temperature field; however, the crucial point is *how* air is advected through the spatio-temporally varying temperature field).*

ACPD

8, S2227–S2236, 2008

Interactive Comment

Another point of concern for a quantitative estimate of water vapour is that you only have 100hPa temperatures, which are almost always higher than the true cold point, and you therefore have inevitably a moist bias.

Again MLS temperature will be used directly without constructing a hypothetical Gaussian "cold trap" region. Being a 2D model with idealized horizontal transport, air will sample the full variability of the measured temperature field which means the coldest grid box if saturated (100 % RHi) will determine the stratospheric entry VMR. I recognize that many details of the atmospheric transport through the tropical temperature field such as amount of air exposed to the coldest temperatures versus percentage that by-passes those regions and variations in the entry concentrations of H2O into the TTL are generally neglected by this model. The statement that only having 100 hPa MLS T is always warmer than the true cold point inevitably leading to a moist bias is not always true because of compensating errors. One error being the requirement within the 2D model that all air passes through and is potentially freeze-dried in the coldest grid box. In the real atmosphere, each global air parcel experiences an ensemble of minimum temperatures some which may be colder or warmer than the MLS minimum temperature. Whether this model produces a moist or dry bias depends largely on where the MLS minimum temperature falls in the ensemble of Lagrangian trajectory minimum temperature. Without losing sight of my objective here, the main goal is to reproduce with reasonable accuracy, the seasonal cycle behavior of H2O, not necessarily the exact amount. Again this is an investigation of how different mechanisms affect the tracers.

P3966/L10: I am not quite sure how realistic these mixing timescales between tropics and extratropics are. Surely the general consensus is that mixing just above the jets but below the tropical pipe (i.e. around 19km) is very effective? How sensitive is your model to the choice of the values of these parameters?

My values come from a analysis of the tape recorder signal by Mote et al. 1998 also used by Holton and Gettelman, 2001 paper. I will explore its sensitivity and yes it is an

ACPD

8, S2227–S2236, 2008

important effect as I have discussed in a few places in the paper.

P3967/L1-5: From this description it is not quite clear how you treat convective detrainment: does it detrain into the layer that is then advected in the style of HG01? Do you detrain into the coldest region, or uniformly in your horizontal domain? Perhaps I have missed something - but a very clear description is required here. Also, do you have a steady detrainment, or is it stochastic? (My guess is steady.)

Convection detrains in all horizontal and vertical grid boxes at the rate shown in figure 1. The detrainment is steady. I will clarify this better.

p3967/L11: In a recent paper (Fueglistaler and Fu, 2006), we concluded that it is very unlikely that thin cirrus lead on average to net diabatic cooling. It could be mentioned here that the Hartmann et al. explanation for the âstratospheric drainâ has been questioned.

I will mention this. Thanks for pointing it out.

P3968/L5: Is 150hPa in your model already in the upwelling region; Figure 1 suggests that it is not - so 150hPa seems to be not a good boundary condition (it would sink down). Perhaps I miss something? Perhaps you could show in a cartoon how the model is built: bottom, top, lateral boundaries, and horizontal domain; and where convection detrains (see above).

For the runs where convection is activated, the 150 hPa is in a sinking region so you are correct, 150 hPa is not a meaningful boundary and the model results don't depend on it. For the slow ascent case where convection is neglected (except for providing large scale upwelling for P<= 150 hPa) then the 150 hPa boundary condition is important.

P3968/L19: I'd be interested to see how you calculate the contribution from methane oxidation - I thought the increase of water vapour in the tropical lowest stratosphere arises also (mainly?) from in-mixing of stratospherically older air, not only from in-situ oxidation?

ACPD

8, S2227–S2236, 2008

Interactive Comment

Let me look into this more closely. The parameterization I used was derived from the tropical H2O gradient in the stratosphere and the rate of ascent. I initially used a value from Sherwood's 2001 paper but it was 3.3 times larger than my analysis. According to this value it will take about 650 days to add 1 ppmv or 0.13 ppmv in 90 days (from 83 to 68 hPa). Therefore I agree with you, any significant increase must come from extratropical mixing.

P3969/L9: Perhaps you can briefly state why you think that transport patterns (e.g. migration of ITCZ, monsoon convection) is not important for understanding the CO pattern in the TTL.

The nothern hemisphere monsoons don't appear to have a strong seasonal cycle so its effect from would be to dilute the tropical one. The ITCZ tends to be across the Pacific ocean which usually has low CO all year long so it should have virtually no annual cycle and like the NH monsoons, tend to dilute the seasonal cycle seen in the zonal means.

P3969/L10-27: I would suggest a slight rearrangement, and give the fractionation of evaporating ice here rather than on page 3971/Line 27. Also, one should add that these values of delta-D of ice are highly uncertain.

OK I will consider that. I agree that delta-D of ice is uncertain but valuable property to measure

P3971/L9: I am not a specialist in convection, but no entrainment from the cloud base to the TTL certainly is not quite realistic. I see there is no way to include entrainment into the convective cells without introducing even more poorly known parameters, but I think one should at least mention that this assumption is extreme.

I recall Keith had discussed this approximation in his 2000 paper. I will mention this.

P3972/L13-L15: I don't understand what is meant here.

All this is saying is that after removing a bias, coincident comparisons between MLS T and I believe the GEOS 5 analysis show a quasi random 1K variability. This could be

ACPD

8, S2227–S2236, 2008

Interactive Comment

caused by MLS noise or atmospheric variability not captured in the analysis.

P3972/L21: Given that the two water vapour retrievals do not give the same results (if I understand Figures 2 and 3 correctly), it would be worth saying a few words here (and to what extent this observational uncertainty affects the rigor with which you can determine model performance).

In the revised manuscript, I will only consider v2.2 H2O and T. I have done a test run and the v2.2 H2O show better agreement with the latest model run than it did when I used the v1.5 temperatures. I think this is because the v2.2 T captures the temperature structure near the cold point tropopause better than v 1.5.

P3973/L15: The claim that ACE-FTS HDO agrees well with data shown by Kuang et al. and by Webster and Heymsfield is pretty bold given that these two data sets arguably hardly agree with each other!

See comment above. If you look at averages in the TTL and consider only the clear sky points I think the agreement is good. The ACE-FTS data indeed show considerable variability perhaps not as much as the aircraft but certainly much more than its uncertainty estimate.

P3973/Section 4: As a general remark, it may be less confusing to refer simply to "the model" rather than the "CCT-TTL model", and have perhaps slightly more easily identifiable acronyms for SA, C-NOICE, and CSDO1-ICE. The reader gets easily confused.

OK I will do that.

P3974/L15: One difficulty I have with the term "cold trap" is that it is not quite precise in what it means: one can consider the entire cold point as the cold trap, or, as is probably intended here, just the coldest location. The statement as it stands however would also be true if simly the tropical mean cold point was referred to.

I intended it to mean the coldest region in the tropics. I will be more clear in the revised version.

ACPD

8, S2227–S2236, 2008

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

P3974/L29: I am admittedly not convinced that in-mixing of stratospheric air really leads to a subsaturated tropopause region (as a dominant feature of the season), and I think we lack any other evidence for that (clearly one cannot argue, for example, from thin cirrus cloud statistics as they occur throughout the year; as you also state later).

I believe this is true also. After looking at this, the problem in the model is originating in the amplitude of the annual cycle in the MLS temperature. In version 1.5, MLS is producing an 8K annual cycle whereas the ERA analysis (either cold point or Lagrangian) is 4.5K. It is because of this, extratropical in mixing is capable of reducing the H2O mixing ratio in July-August to subsaturation. I will take a close v2.2 T and and GEOS 5 temperatures and see if this is still the case.

P3976/L13ff: I cannot follow why lack of convective mixing leads to failure of producing a semi-annual cycle. Are the subsequent sentences explaining this?

The simple answer is in this model without convective mixing it takes 90 days for air to rise from the model bottom to the CPT. Extratropical mixing rate which is about 30 days (increasing with height) washes out the semiannual cycle. Even without the extratropical mixing. A semi-annual CO oscillation at 150 hPa would tilt-over tape recorder like producing a similar cycle lagged a few months later at 100 hPa. The MLS data clearly doesn't show this. It would be interesting I think to add CO to your trajectory model and see how it does. Since the ECMWF trajectories include both rising and sinking parcels at faster speeds than the bulk mean average I use in my model it should do much better. But as I understand correctly the rising trajectories are a consequence of the convective scheme in the ECMWF model. The main point being that CO provides direct evidence for convective influence in the TTL. H2O alone isn't particularly sensitive because of the effectiveness of the condensation process.

P3977/L2-4: It would be nice to have at least a few sensitivity calculations to back up this claim (i.e. it is a bit awkward that it is first said that something is important, and then to say that it is actually something else that really matters).

ACPD
8, S2227–S2236, 2008

OK I will do this.

P3978/L20: This sounds like an interesting point, but it also suggests that I may have missed a point: what else than 100should convection mix-in if it is also loaded with ice? On P3979/L9 I find something like an explanation, but I admittedly have troubles understanding it. Is it possible that the model has a problem to have subsaturated regions in the TTL because of the Holton-Gettelmann-type setup of temperature and circulation? In the trajectory studies one always finds subsaturated regions in the TTL due to the spatio-temporal variability of the temperature and circulation fields, and any detrainment into such air masses leads to moistening and presumably isotopic enrichment with having to assume any desiccation of the TTL due to convective overshoot.

The issue is whether convection detrains 100% RHi computed at the grid box temperature (along with its Rayleigh distilled HDO) or 100% computed at the much colder overshoot temperature which effectively dehydrates the air by dilution with dry air. The latter point being that 100% RHi at say 180 K in the overshoot is drier than 30% RHi in environmental air at 190K. What happens of course there is no net removal of HDO supplied by convection as its simply moved from vapor to ice. Upon detraining into warmer air, much of this ice evaporates returning the HDO fraction to the amount specified for total water in convection. This is a simplified representation of how cloud resolving models appear to simulate convection in the TTL. The other view that convection “relaxes” a grid box to 100% RHi (computed at the grid box temperature and ignoring microphysical impacts e.g. Dessler et al. 2007) even with ice detrained is less effective for raising the HDO/H2O ratio, hence the need to use a higher supplied fraction. I don’t see any problem having subsaturated air in the model. I will admit, the data (or at least the way its been presented) doen’t uniquely distinguish between these views and is a interesting issue for further investigation.

P3982/L14: You may add also a reference to Notholt et al. (2005). I think the main problem of all of these studies (as we argued in Fueglistaler and Haynes, 2005) remains that the sheer magnitude of the trend as proposed by Rosenlof et al. (2001) is

ACPD

8, S2227–S2236, 2008

Interactive Comment

virtually impossible to achieve without assuming really dramatic changes at the tropical tropopause. If I understand your Figure 8 correctly, then your mechanism has exactly the same problem: an increase from 3.7ppmv to 3.9ppmv over 45 years corresponds to a trend of about 1 permille, which is full order of magnitude smaller than what has been suggested by Rosenlof et al. (2001). I think this should be at least mentioned.

Will do. The mechanism I propose could not produce a trend as steep as Rosenlof (2001). The only way I can think of to produce a trend like that would be through Sherwood's microphysical connections. In the context of my model one would trend convections ice retention factor from small to large in the CSD01 representation. If I set the ice retention factor to zero and ignore extratropical mixing you will get annualized H2O stratospheric entries less than 1 ppmv.). If that happened convection could have transitioned from a dehydrating to a hydrating influence in the TTL which would initially produce a strong increasing trend followed by a decreasing trend when the CPT T trended condensation takes over. I know that in a recent paper by Grosvenor et al., 2007 they looked at sensitivities of the microphysics to aerosol pollution in their cloud resolving model.

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