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Atmospheric Chemistry and Physics Discussions

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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 #5

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Review by S.Fueglistaler (s.fueglistaler@damtp.cam.ac.uk).

General

Read et al. present a modelling study of transport across the TTL and into the stratosphere. They combine two conceptual models (one intended to mimick slow radiative ascent, one to represent convective detrainment) which is capable to simulate 3 processes that have been suggested to be of importance: radiatively balanced ascent, convective detrainment, and convective detrainment at a detrainment altitude/temperature that is above the level of neutral buoyancy. By prescribing suitable boundary conditions and physical behaviour for a range of tracers - water vapour, water isotopologues, and carbon monoxide - and subsequent comparison with observations, they try to identify which processes are crucial for the TTL. The questions addressed by this paper are under discussion for now about six decades, which in itself illustrates how difficult the problem is. The attempt to gain insight from a combination of idealized processes (rather than, as often done, just one model of an idealized process tuned until it matches observations) should be thus very welcome, and I recommend publication of this very interesting paper after consideration of the following concerns. As a general remark, I consider the very strength of the paper (the combination of idealized processes) also in some way its weakest part: idealized models usually serve to demonstrate that a certain process gives results in qualitative agreement, but are poor in terms of quantitative analyses. The model as presented in this paper depends on a large set of parameterizations (for example in-mixing time scales, isotopic composition of gas and solid phase, carbon monoxide boundary conditions, and many more) such that the reader quickly loses oversight, and the specific advantage of an idealized model (a small number of free parameters, and a good understanding of the mechanistics of the model) is lost. Moreover, many of these parameterizations are quite ad-hoc, and it is not quite clear how much results depend on the specific choice of values. A good example of where the approach clearly reaches its limits is also the awkward mapping of observed temperatures to the Holton-Gettelman cold trap model. Below, more detailed comments are given that the authors hopefully may find useful.

Specific comments (Page number/Line number)

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

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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.

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.

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).

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

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). Another point of concern for a quantitative estimate of water vapour is that you only

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have 100hPa temperatures, which are almost always higher than the true cold point, and you therefore have inevitably a moist bias.

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?

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.)

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 'stratospheric drain' has been questioned.

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).

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?

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

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pattern in the TTL.

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.

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.

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

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).

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!

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.

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.

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

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thin cirrus cloud statistics as they occur throughout the year; as you also state later).

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?

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).

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.

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 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.

Additional references

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Discussion Paper

Fueglistaler, S., Q. Fu (2006), Impact of clouds on radiative heating rates in the tropical lower stratosphere, J. Geophys. Res., 111, D23202, doi:10.1029/2006JD007273.

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Scherer, M., H. Vömel, S. Fueglistaler, S.J. Oltmans, J. Staehelin (2008), Trends and variability of midlatitude stratospheric water vapour deduced from the re-evaluated Boulder balloon series and HALOE, Atmos. Chem. Phys., 8, 1391-1402.

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

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