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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diagnosis of processes controlling water vapour in the tropical tropopause layer by a Lagrangian cirrus model" *by* C. Ren et al.

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The paper presents a physically improved representation of dehydration and possibly re-hydration in Lagrangian simulations of the TTL, resulting in a model named "LACM". The dehydration is described with a quite detailed, previously published, nucleation/deposition microphysical scheme, with reasonable approximations, combined with a simple model of sedimentation. Rehydration is fetched indirectly from the underlying dynamical model (ECMWF/FLEXTRA), from which the trajectories are also calculated. Prober representations of microphysics in such simulations are indeed needed. Previous trajectory studies aiming at quantifying the troposphere to stratosphere water vapor transport modes have relied on unrealistic assumptions, an this paper does as well rely on at least unverified assumptions, in spite of increased physical detail. In recognition of the fact that water transport in the TTL is a hard problem to assess I think that the paper could be published in ACP in spite off some problems with the scientific method, detailed below, provided that the assumptions and limitation of the conclusions is made explicitly clear in the article.

Before going into technical details, I will make a general remark about the way I look at this work: I appreciate the idea of improving representation of dehydration in trajectory studies. I also understand the motivation for diagnosing sedimentation and convection solely from the trajectories. However, it is not obvious to me that the ECMWF analysis, or anything derived solely from it, should be able to capture water vapor mixing ratio resolved good enough for evaluation of different dehydration schemes against specific in situ measurements in the TTL. In other words: I am not yet convinced that the tests performed can actually be used for rejection or acceptance of the LACM. Basically I believe that it would be better to use all available information, including e.g. GOES infrared images, to prepare the trajectories if the purpose is to validate a parameterization of dehydration/rehydration against field measurements. I think that the idea deserves to be published, despite the discouraging results, and I would like to encourage the authors to consider other ways of validating their model.

Major comments:

p 5519 l 17-21 It is unclear to me whether FLEXTRA uses the vertical wind directly from ECMWF, or calculates it in some different way, like for instance radiative heating calculation? In either case I miss a discussion of the quality of the vertical wind input in relation to the conclusions. The vertical velocity is the most important controlling parameter in cirrus nucleation. Particle number densities, and therefore sedimentation velocities, are very sensitive to this parameter.

Also, information about which measurements are actually assimilated into the ECMWF analysis around the TROCCINOX area would be appropriate. For example one might ask if some of the campaign data used indirectly to produce the trajectories?

p 5520 l 20 The calculation of τ_q leaves out some details besides not taking into account non-spherical particle shape. For instance correction for ventilation, correction for sub-mean free path kinetics to D_v and accumulation of deposition heat is not included. I shall not advocate that all this should be calculated also, that would be an overkill when one considers other uncertainties like e.g. vertical wind and convection, but it would be appropriate to include some considerations about to which degree and in which direction the τ_q (and maybe the dehydration velocity) is biased by leaving out these details.

p 5523 l 5 In line with the first referee I am sceptic here. It is assumed that number density (per volume) is constant everywhere between the point of interest and the cloud top, and that the particles are mono-disperse. Which physical mechanism justifies this? One would not expect a justification of the mono-disperse assumption, which I regard as choice made for computational reasons, though some discussion of how far this is from reality would be nice. However, the constant number density must rely on a physical argument (internal convection/turbulence?), which then would conflict with the dynamical equation (eq. 12), in which fall speed is assumed to control the sedimentation. I do appreciate, that equation 12 is a simple dynamical equation depending on variables extracted from the model, but it needs more physical justification.

p 5524 l 16 The re-hydration scheme relies on the data assimilation system and of course on the parameterization of convection in ECMWF. The paper needs more discussion on the validity on the ECMWF water vapor field close to convection. The Geophysica results do not seem to confirm the ECMWF mixing ratios on the "golden day". So my question would be: If the ECMWF analysis cannot resolve tropical convection on a, say, 200 km scale why then should one expect variations on that length scale to be meaningful just because the air parcels have been advected along model trajectories for a couple of days after the convection took place? Maybe this could be made more clear by some numbers: On which length scale is ECMWF expected to resolve water vapor fields in the TTL? It could actually happen that the LACM is sound, while it is impossible, at the present, to validate it directly against tropical in situ measurements.

p5530 l 2 I feel a little uneasy about eq. 15. I accept the premise, that the comparison is more fair on a logarithmic scale due to the large range of values of q_t. But it looks like an overcompensation to first take the differences on logarithmic scale and then normalize each residual separately with $1/\ln(q_{toj})$. In this way residuals at small scale are weighted higher than residuals at large scale. To me it looks as if the method would for instance hide a case of systematic performance reduction everywhere except in coldest regions. It could be misinterpreted as if the authors are trying to fix the comparison method in favor of their model, which I don't believe is their intention. Either there could be an explanation covering this question or there should be a reference to some work documenting the "validity" of eq. 15 in this or equivalent context. A third possibility would be to compare saturation ratios instead of mixing ratios.

p5532 l 12 The spikes in the red and blue curves of figure 8 hints that the LACM re-hydration is smearing the water vapor field. The claim that "LACM always improves on the instantaneous dehydration approach" should be considered in connection to this. As I read it (correct me if I am wrong), the instantaneous nucleation runs are done without re-hydration, which could imply that they show more variability. As the authors also states elsewhere, more variability may lead to worse correlation with measurements, even though the overall performance may be better. So, is it just a smearing which gives the improvement?

p5533 l 5 Of course the sort of study presented in section 3.3 is one of the goals of this work. Considering the uncertainties of the method it is still premature to derive any quantitative conclusions about convective influence on cloud coverage and humidity in the TTL, and the authors are also modest in their conclusions. I am not sure about this section. I would like to see more validation against observed global cloud abundance in the TTL e.g. [Liu and Zipser(2005)] or

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Interactive Comment

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[Dessler et al.(2006)Dessler, Palm, and Spinhirne]. As it stands now it appears somewhat weak.

p 5535 l 2 As mentioned, I am not yet convinced about this conclusion.

Minor comments

p 5524 l 3 "saturation ratio 0.8": I suppose that this number does not influence the results?

p 5527 l 10 "Fig 2" -> "Fig 4"

p 5528 l 8 "With the exception... " for some reason I don't understand this sentence.

p 5542 Generally the figures are too small. This is often a problem in ACPD. Especially figure 6 and figure 8 are almost unreadable.

p 5548 fig 7 It puzzles me a little how the ECMWF interpolations can show saturation ratios above unity?

References:

Liu, C. and Zipser, E. J.: Global distribution of convection penetrating the tropical tropopause, Journal of Geophysical Research (Atmospheres), 110, D23 104, 2005.

Dessler, A. E., Palm, S. P., and Spinhirne, J. D.: Tropical cloud-top height distributions revealed by the Ice, Cloud, and Land Elevation Satellite (ICESat)/Geoscience Laser Altimeter System (GLAS), Journal of Geophysical Research (Atmospheres), 111, D12 215, 2006.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 5515, 2007.