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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A modelling study of the impact of cirrus clouds on the moisture budget of the upper troposphere" *by* S. Fueglistaler and M. B. Baker

Anonymous Referee #1

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This is a paper with useful, interesting results. I recommend publication with revisions. I did, however, quite often find the paper hard to understand and not very accessible. It is also not written in a "friendly", physically transparent way, and I have a number of comments along these lines. My main concern relates to the layer depth h. From reading the paper, I could not get a conceptual idea on what this parameter represented. I would strongly suggest modifying the model formulation in such a way as to not be required to make any assumptions on its value. In the long run, I think the paper would be much more useful to the community if this were done.

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1) The layer depth h: it is defined as "the scale of the layer in which we assume particles to nucleate, grow and eventually leave through sedimentation." This definition is vague. The most natural vertical distance is the distance an ice particle falls after leaving a cloud, falling through an unsaturated environment, and completely evaporating. This is the most relevant length scale to dehydration because it is roughly equal to the vertical distance over which the irreversible water transport has occurred. The disadvantage of using this fall distance is that it depends on the size of the particle, the RH of the background atmosphere, and the existence of any vertical motion in the background atmosphere. But in any case, is this the length scale that is being suggested (unlikely)? The paper says that h does not equal the cloud thickness, so this would seem to be the only other possibility.

2) However, I think the most relevant parameter the cloud model should calculate (which does not depend on a number of other environmental factors), is the fall speed of the particles once they reach their maximum size. This is the parameter which is most relevant to a given physical situation, and does not require making an assumptions of what h might be. For example, in TTL dehydration, one wants to know whether the fall speed is comparable with the background upward vertical velocity. Or in the mid-latitude situation, one would want to know if this fall speed gives rise to a locally negative tendency on RH that is comparable with that due to large scale subsidence. I don't see how knowing the P value or dehydration efficiency in these examples directly helps you. These are all adjustable depending on h, a variable for which there is no explicit recipe.

3) The temperature change was applied to the air parcel isobarically rather than conserving potential temperature, which would be more physically realistic. Any reasons for this choice?

4) Terminology: I think homogenous nucleation still represents for some people the condensation of vapor to make new particles, rather than in this context, the freezing of supercooled aerosols. Some clarification may be useful (i.e. "homogeneous freezing"

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nucleation). (Was also puzzled as to why there was no neccessity of any assumptions on the initial aerosol density. Wouldn't this affect how many ice crystals were produced?)

5) If one is going to use P, it would seem simpler to not have this 2 pi factor in its definition, so one could refer to a P $<$ 1 or P $>$ 1 regime.

6) A number of the plots have no label on the vertical axis. I realize that these quantities are nondimensionalized, but the variables these axes represent should still be there.

7) In some formulas - I am thinking of A4 in the Appendix - where the ice particle number density n changes as dn/dt = -n v(r)/h, it does seem like h is being interpreted as the vertical interval over which the ice particles exist, and n is the layer average, despite statements that h is not to be interpreted as the cloud depth. It would seem more appropriate to formulate a "lagrangian" model in which your air parcel followed the falling ice crystals (possibly assuming a background lapse rate), rather than having to make any assumption about h.

8) To help the reader who is unfamiliar with saturation point temperature, perhaps note that it reduces to frost point temperature, for air parcels with no condensate, if this is correct.

9) page 9783 - wasn't clear from the context what "nucleation threshold" was.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9769, 2005.