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Interactive Comment

Interactive comment on "A modelling case study of a large-scale cirrus in the tropical tropopause layer" *by* A. Podglajen et al.

Anonymous Referee #2

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This paper tries to simulate the TTL cirrus clouds previously reported in literature by a mesoscale numerical model and to discuss the effect of such cirrus on the radiative budget and water transport to the stratosphere. This subject is relevant to the scope of ACP, and worth publication even though it is only a case study. I recommend publication in ACP after the following points are cleared.

General comment

1. Model set up:

The set up of the model domain needs justification. The cloud formation along the south-east boundary (Figures 2, 3, 4, and 7) is probably spurious. It may be related to the large deviation from the analysis field of "up to 3 K at 16 km, 36 h after initial-

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Interactive Discussion

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ization" (page 31096, line 16). The comparison shown in Figure 7 indicates that the initialization by ERA Interim, rather than ECMWF operational analysis used for "reference simulation," has suffered less from this problem. Some expansion of the model domain may not solve this problem. Considering that the south-east boundary corresponds to the upstream of the cirrus clouds under consideration, it is necessary to examine the effect of the boundary carefully.

2. Ensemble simulations:

The simulations are repeated by changing the initial and boundary conditions using ECMWF operational analysis and ERA Interim data set as well as by switching the microphysical parameterization scheme as "sensitivity tests." Isn't it necessary to conduct ensemble runs to get firm result if there found "the strong dependence to the choice in initial and boundary conditions" (page 31092, lines 20-21)?

3. Generalization of the results:

The authors conclude that the cirrus clouds have a small effect on radiative budget and do not significantly influence dynamics. Can it be a general conclusion from this particular case study? If not, what is the limitation of this study and what kind of study are needed in the future?

Specific comment

Page 31091, line 3: "upwelling trends" might be "upward trends".

Page 31092, line 12: "in a region where analyses may present significant errors". If so, is it appropriate to rely on the analysis field for initialization and boundary condition?

Page 31093, line 21: "bulk microphysics scheme of Thompson et al. (2004)" Some descriptions on the treatment of supersaturation and homogeneous/heterogeneous ice nucleation will help reader to understand.

Page 31094, line 13: Correct "the the domain".

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Page 31097, line 3ff: The color points do not make sense. An alternative will be: set initialization points surrounding the cirrus of interest on the panel for 10:00 UTC on 28 January, and trace the location of those points following the back trajectories until the time of initialization. What we see from the sequence of panels will be the difference in the location of cloud against that of air parcels initially (in a backward sense) surrounded the cloud.

Page 31098, line 18ff: "there is no clear correlation between w and the cirrus cloud in most of our simulations" The Eulerian vertical velocity is not an appropriate variable to see in situ cloud formation. The cooling rate following the atmospheric motion will be the best. Some more explanation on the difference between "adiabatic upward vertical displacements" and "upward velocities" (lines 21-22) will help interpret the temperature distribution on an isentrope combined with horizontal wind velocity field. In addition, there may be some contribution of the moisture flux from the west near the southern boundary of the simulation region.

Page 31098, line 26ff: "The cirrus location is clearly associated with the strongest vertical uplift." Figure 4 is not consistent with this statement. The largest upward displacement is found near the south-east corner of the model domain, while the cirrus clouds are found near the center. In addition, there is some confusion. The authors emphasize the difference between the cloud location and the horizontal displacement of air parcels (page 31097, line 16ff). Then the cloud location could not be compared with the Lagrangian vertical displacement since initialization. Further, assuming the typical time scale of ice nucleation being a few hours, the uplift since the start of the simulation (36 hours) will not be a good measure.

Page 31099, line 3ff: I am skeptical about the usefulness of Delta RH because the ice nucleation depends on the absolute value (not the relative change) of RH. It will not be consistent with the consideration of supersaturation that does not cause ice nucleation. Another cause of confusion is the reduction of RH after ice nucleation as the cloud formation will be accompanied by the decrease of RH from $\sim$1.6 to 1.0.

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Page 31100, line 2: Which part of the symmetric signal is an equatorial Rossby wave? How can it be identified?

Page 31100, line 5: What is Yanai wave? Is it Rossby-gravity wave?

Page 31108, lines 4-5: The comparison of the short wave heating between ERA interim and WRF results could be done by estimating the "3 h average that include the sun rise" in WRF simulation.

Page 31112, bottom line: "1000 m in 30 h" What about the corresponding cooling rate in the unit of Kelvin per day?

Page 31113, line 26: "TOA" has first appeared without explanation.

Figure 1: The time of observation (top left) and simulation (top right and bottom right) should be identified. Slightly different horizontal/vertical ranges among the top left/right and bottom right panels should be adjusted.

Figure 2: I understand the CALIOP observation over the cirrus was at around 10:00 UTC on 28 Jan. 2009. The simulated result of this particular time should not be missed along the time evolution of meteorological fields.

Figure 3: I don't understand why the distribution at 20:00 is shown rather than 10:00. The left panel, being the same as one of those shown in Figure 2, could be omitted or possibly be changed to illustrate pressure or height of the 360 K isentrope.

Figure 4: Again I don't understand why the distribution at 12:00 is shown rather than 10:00.

Figure 10: The standard deviations or the confidence intervals of the total water change in the domain should be indicated as the estimates of uncertainties.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31089, 2015.