A modelling case study of a large-scale cirrus in the tropical tropopause layer

February 13, 2016

We would like to thank the reviewer for the insightful evaluation of our work. Please find below our point-by-point reply.

1. ***Reviewer*** — 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 initialization" (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.

   Authors — We understand the concern of the reviewer. Actually, the development of cirrus clouds in the South-East part of the domain is not entirely spurious; on earlier Calipso tracks, cirrus clouds are also seen in this region, as illustrated on figure 1 (included in this response, see below). It remains that the opacity of those clouds is likely overestimated by the model.

   However, we expect that this only very marginally affects the clouds analyzed at the center of the domain. Indeed, backward trajectories launched at the time of CALIOP observations (January 28, 10:00 UTC) on the 360 K isentrope, shown on figure 2 (also included in this response), show that : 1) only a limited portion of the air in the cirrus has transited in the South-East part of the domain (this can also be seen in figure 2 of the paper) 2) none of the air parcels went further than 15°South

   At last, we also emphasize that nested runs initialised with a larger domain (from 27.5° to 27.5°N and from 158°W to 102°W) also showed this cloud development in the South-East of the domain (see also figure 1 in the response to reviewer 3).

   The limited influence of spurious boundary conditions is now mentioned in the Model set-up section, and the nested run is presented in the sensitivity to initial conditions section.

2. ***Reviewer*** — 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)?

Authors — We agree with the reviewer that, if possible, ensemble simulations are a very appropriate tool to characterize the sensitivity to initial conditions. However, it is difficult to conduct such an experiment in our case, for two reasons.

First, the numerical cost of the simulations would be too large, if there were a significant number of ensemble members. Second, there is a major difficulty regarding the preparation of an ensemble: the relevance of ensemble runs depend on the ability of the ensemble members to represent adequately the uncertainty in the region and process of interest. The ensembles developped by operational centers (e.g., ECMWF) are chosen to maximize the variability in the evolution of midlatitude tropospheric perturbations, not in the Tropical Tropopause Layer (TTL). If we started an ensemble of WRF runs from the ECMWF ensemble, there is no guarantee that it would span the right uncertainty in the meteorological fields in the TTL. This choice of ensemble members is really the major caveat. We have used here only a few simulations, carried with the ECMWF operationnal analysis, ERA interim. We have also carried experiments using NCEP CFSR reanalysis as initial conditions :"Consistently, a simulation with NCEP-CFSR winds and temperature conducted in early stages of this work lead to a cirrus field with significant differences."

To clarify this, we have stated in the text that the sensitivity shown here is more illustrative than a quantitative evaluation. Such sensitivity tests are common practice when carrying out case studies.

3. ***Reviewer*** — 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?

   Authors — We want to emphasize that the effect of the clouds in the TTL simulation presented here may seem small, but are actually very significant. In this region of low positive heating rates, the cloud lower the mean LZRH and nearly double the heating rate at 100 hPa, which are very significant effects. This has been stated more clearly in the text.

   Regarding the influence on the dynamics, indeed the simulations do not show a strong influence of cirrus radiative heating on circulation. However, we don't think that our case study alone can be used to draw a general conclusion, as it is specific to the environment in which our cloud develop. For instance, the cloud temperature is around 190 K, which is higher than some TTL cirrus, and limits the heating rates. Other real case studies of cirrus clouds in the TTL in different environments would help to settle this issue. We have added a sentence on this in the conclusion.

4. ***Reviewer*** — Page 31091, line 3: "upwelling trends" might be "upward trends".

   Authors — We meant the long-term trends in tropical upwelling. We have changed to "tropical upwelling".

5. ***Reviewer*** — 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?

   Authors — We agree that this may be a problem, but analyses are the only option to initialize such a large domain. The good comparison with cloud observations gives confidence that the initial conditions are sufficiently well represented in the analyses.

6. ***Reviewer*** — 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.

   Authors — More description has been added in section 2.2 following this and referee 1 comment.

7. ***Reviewer*** — Page 31094, line 13: Correct "the the domain".

   Authors — Corrected.

8. ***Reviewer*** — 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.

   Authors — We have adopted the reviewer's suggestion.

9. ***Reviewer*** — 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.

   Authors — We agree that the important quantity is the temperature change following an air parcel. In the adiabatic limit, it is directly related to the vertical displacement following an air parcel. Regarding the vertical velocity, it gives the instantaneous cooling rates, while the displacement is its integral over some time (since the beginning of the simulation here) and, importantly, following an air parcel. This is now more detailed in the text. The temperature on an isentrope is directly linked to its height, as now shown in figure 4; however, the Lagrangian evolution of temperature cannot be easily predicted from height and wind fields only those fields are not stationary and evolve during the time of the simulation (see fig.2).

10. ***Reviewer*** — 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 ~ 1.6 to 1.0.

Authors — We agree with the reviewer, ice nucleation depends on the absolute value of relative humidity (and this is how it is implemented in the microphysics code). The cloud field already illustrates the crossing of the nucleation threshold. Here, our Delta RH aims at evaluating the impact of the vertical motion on (total) relative humidity increase, this increase causing eventually to cross the threshold. The figure hence shows that this change of RH due to the ascents is a good predictor of cloud location. Nonetheless, as noted by the reviewer for the limited area of initially dryer air parcels in the South-East part of the domain, this increase is not always sufficient for ice nucleation.

Regarding the reduction of RH because of water condensation, this was actually taken into account because the RH used to compute Delta RH was not exactly the difference in relative humidity, but the ratio of ice q_{ice} plus water vapour q_{vap} mixing ratio over the ice saturation mixing ratio q_{sat} :

$$\Delta RH = \frac{q_{vap}(\underline{X}(t),t) + q_{ice}(\underline{X}(t),t)}{q_{sat}(\underline{X}(t),t)} - \frac{q_{vap}(_0),t_0) + q_{ice}(\underline{X}(t_0),t_0)}{q_{sat}(\underline{X}(t_0),t_0)} \tag{1}$$

Thus, water phase changes will not affect the numerator $q_{vap}(\underline{X}(t),t) + q_{ice}(\underline{X}(t),t)$ along an air parcel trajectory because of the Lagrangian conservation of total water if we neglect sedimentation and diffusion. Only temperature changes will have an impact on the denominator and hence ΔRH. The text was previously misleading and missed to explain that point and this has been corrected.

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

    Authors — It is difficult to clearly delimit an equatorial Rossby wave in the simulations because of the superposition of many modes and the complex response to the PV intrusion. However, equatorial Rossby modes with PV signature is expected by the PV intrusion.

12. ***Reviewer*** — Page 31100, line 5: What is Yanai wave? Is it Rossby-gravity wave?

    Authors — Yes. This has been clarified in the text.

13. ***Reviewer*** — 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.

    Authors — We thank the reviewer for this suggestion, that has pointed us out to a mistake in this paragraph. With more investigation, we found that the SW contribution was not at all sufficient to explain the observed difference. Most of the difference actually arises in the long wave. We do not have a clear explanation for this since the temperatures and water vapor are comparable at that altitude. The text has been corrected.

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

Authors — -8 K/day, this has been added in the text.

15. ***Reviewer*** — Page 31113, line 26: "TOA" has first appeared without explanation.

    Authors — Corrected.

16. ***Reviewer*** — 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.

    Authors — This has been added. We emphasize that the time of the observation is not exact and only for indication, because it takes about 15 minutes for the satellite to cross the domain.

17. ***Reviewer*** — 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.

    Authors — We have changed the timing of the successive panels following the reviewer's suggestion.

18. ***Reviewer*** — 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.

    Authors — We have followed the advice of the reviewer, and replaced temperature by height.

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

    Authors — No particular reason, except that 12:00 is a more standard analysis output time. This has been changed for 10:00, but the patterns are similar.

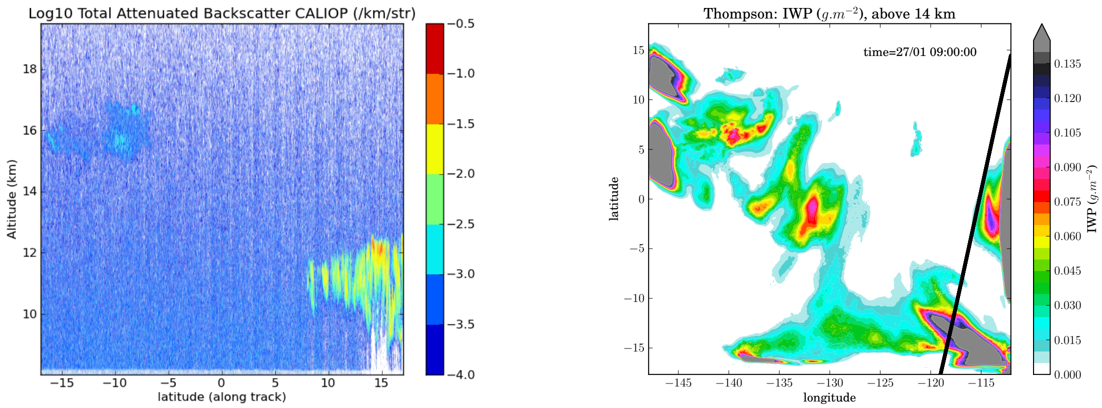

Figure 1: (Left) CALIOP observations over the Eastern Pacific on January 27, 9:00 UTC. (Right) Ice Water Path above 14 km in the simulation on January 27, 09:00 UTC. The black line on the right panel corresponds to CALIPSO track.

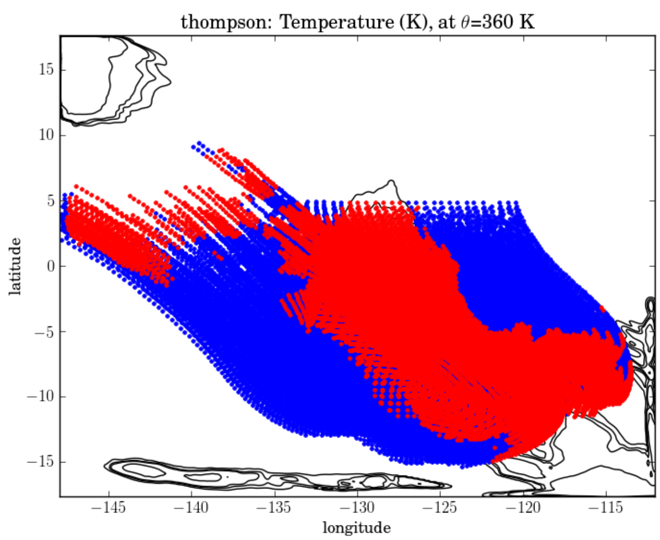

Figure 2: Backward trajectories of parcels initialized at $\theta = 360$ K on January 28, 10:00 UTC. The (backward) initial position position of the parcels span the area between 12.5°S and 5°N and between 134.°W and 120.°W, with a resolution of 0.5°. Trajectory points are colored in red if the air parcel was within a cirrus (IWC $\geq$ 0.02 ppmm), in blue if it was in cloud free air (IWC $<$ 0.02 ppmm).