

Answer to Anonymous Referee 2

The authors are grateful for the time and thought that Anonymous Referee 2 put into the review and comments regarding our paper. We incorporate most of those comments into our revised manuscript, which has led to substantial improvements. Detailed responses to all comments follow below. The original comments from Anonymous Referee 2 are in italics and our responses in plain text.

1 General comments

The objective of the research is to identify and reduce uncertainties in cirrus modelling, which is welcome. The approach is case studies using trajectory box modelling and the uncertainties studied are related to the quality of the thermodynamic fields along the trajectories, the representation of unresolved vertical motions, and the initial values of specific humidity and concentration of ice nuclei. It is no surprise that higher temporal resolution of both the background model and the trajectory interpolation improve the results, and adding small scale temperature fluctuations is an established technique even in Eulerian models. However, unfortunately there seems to be no way how a better initialization of specific (or relative) humidity and IN concentration can be achieved. Unfortunately there is no discussion on these points. Otherwise, the paper is interesting and easy to read. The SAL metric in its current presentation is not of much use, mainly because Fig. 10 is much too small and the symbols cluster together and partly cover each other. The authors should replace the figure with a table giving the respective SAL values.

We have enlarged Fig. 10. We choose not to replace it with a table as the many data points presented (25 per panel of the figure) would result in a large and unclear table.

2 Major comments

1. P 7537, ll 5 ff.: To my opinion, it is too convenient to simply state that "mechanisms are not well understood" and to quote a "low level of scientific understanding". These statements are too general. Please describe what exactly is not well understood. The uncertainties of climate predictions are not necessarily due to cirrus clouds. Sherwood et al. (2014) trace it back mainly to uncertainties related to low clouds and convective mixing.

We have added additional information to this paragraph and rewritten this section of the introduction. The suggested reference was added.

2. Sect. 2.2.1: Please add information on the vertical coordinate and orography treatment in the COSMO-2 model.

We added the requested information to section 2.2.1 of the article.

3. P 7545, ll 20 ff.: The horizontal spread of the trajectories show that the assumption of a vertical stacking of the boxes that arrive together at Jungfraujoeh (JFJ) is not justified at all. While the authors admit that this is a poor assumption there is no discussion on the effect of that assumption. Sedimentation is mentioned to occur before arrival at JFJ and to remove heterogeneously formed ice. This should be no problem for the interpretation. A more important question is whether there are ice crystals falling into

a box from above and consuming the excess vapour in that box. Does this occur? Is this effect represented in the model?

Yes, in the stacked box model, we take the vertical transport of water into account. The total water in the level is increasing due to sedimentation from the level above and decreasing due to sedimentation to the lower level.

The ice particles falling from the level above will grow and decrease the supersaturation, if the air mass is super saturated and vice versa. We added this information to section 3.2.

This is done for both heterogeneous and homogeneous formed ice particles.

As is also visible in Fig. 6, the nucleation event in connected to the observed cloud is in most simulations not the first nucleation event along the trajectories (trajectories arriving at 11-11.5 km altitude in Fig. 6). These former nucleation events may deplete water vapor and IN (in the runs with heterogeneous nucleation) in the parcels. However, since these processes are just tied to the sedimentation out of the parcel, they should not be affected by the horizontal spread of the trajectories. Accordingly the impact on the conclusions is rather small. We added a few sentences on this issue in section 3.1.

4. P 7556, ll 1-4: It is questionable whether the PSD of T is the appropriate quantity for describing an influence of T-fluctuations on the resulting cloud, since it is the cooling rate at the nucleation threshold rather than the temperature that matters. I wonder why you do not look at the pdf of the cooling rates. Can you please discuss this?

We agree with the referee that the cooling rate during the nucleation rate is probably more important than the temperature amplitude of the gravity wave itself. However, the box model takes the temperature time series as input and therefore we think it is worthwhile to show the PSD of temperature. The PSD of temperature is coupled with the PSD of cooling via the relationship .

In addition, we would like to highlight that we show in addition also the PSD for the vertical velocity, which is directly linked to the cooling rate by the adiabatic constant in Fig. 4. The major points we conclude in the paper based on Fig. 3 (cut-off frequency, lower energy of long-wavelength waves compared to MACPEX and SUCCESS) are equally supported by this figure.

3 Minor comments

1. P 7539, l 4: It should be noted that cirrus cloud modelling mostly is done in the Eulerian framework, e.g. in NWP and climate models. The question of the quality of trajectories does not apply to such models and this should explain why not much attention has been paid so far to this question.

We have added some sentences concerning this issue

2. P 7542, l 10: Please rewrite this sentence. Measurement uncertainties never affect the vertical position of any cloud.

The sentence has been rewritten

3. Fig. 2: Please explain thin and thick contours in the plot. (Thick is evident, but could be mentioned for completeness).

This information has been added.

4. P 7549, l 25: change "mediates" into "mitigates".

Done

5. Fig. 3: Colored vertical lines are too thin. Check calculation for f_{max} (currently the units are $1=(s\ m)$).

We corrected this.

6. PP 7751: please explain why w^2 is the velocity variance and not simply the velocity squared. These quantities are the same only if the mean w is zero. Is this assumed? Or is it meteorological parlance?

We appreciate the reviewer for this critical comment, indeed we should use simply w^2 .

In the case of COSMO2 data, we found that $\bar{w} \approx 0.05 \sqrt{w^2}$.

w^2 is then practically equal to the variance .

We changed the variance to w^2 in the MS, when it is applicable.

7. Fig. 5: should be larger. I can hardly read the insert text.

We are not sure to what the referee is referring since the text insert in Fig. 5 is fairly large already.

8. Figs. 8–10 are too small.

We enlarged those figures as requested.