

Answer to Anonymous Referee 1

The authors are grateful for the time and thought that Anonymous Referee 1 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 1 are in italics and our responses in plain text.

This work examines the uncertainties of Lagrangian parcel modelling of cirrus clouds to the details of model configuration (resolution, small-scale temperature fluctuations, initial water vapour content, and nucleation mechanism). The analyses are carefully done and scientifically sound. The manuscript contributes to progress in cirrus cloud modelling, and it is suitable for ACP. I recommend publication of the manuscript, subject to revisions.

In general, the manuscript is difficult to read. For instance, the abstract provides too many technical details and does not clearly highlight the main results, which are (based on my understanding):

- The model calculations are sensitive to the temperature fluctuations, upstream specific humidity, and nucleation mechanism, and the uncertainties associated with these factors are highly non-linearly linked.*
- High resolution is required in order to account for the small-scale (high-frequency) temperature fluctuations.*

I suggest the authors to rewrite the abstract to communicate these points more effectively to the readers.

We have rewritten the abstract to more clearly highlight these results. We think that a major outcome of the study is the illustration of the large day-to-day variability of the vertical velocity variance, which needs to be taken into account for the construction of small-scale temperature fluctuations in future studies. This remains an important point in the abstract.

In addition, although the main results (as stated above) are important, they are not especially new (I expect these results before reading the manuscript). Also, I am concerned that some of the model results may not be robust, i.e. very specific to the particular cloud studied here. Thus, the additional case (currently in the appendix) is helpful. Having these two cases, the authors could focus the discussions on the results that are robust (or not robust), and by doing so clarify the main conclusions of the paper.

We have so far found no publication where these uncertainties are systematically examined for a Lagrangian perspective, though they have been demonstrated separately for a variety of case studies. The systematic comparison of these uncertainties for a single cloud is new. We emphasize in the paper, that the main result might not be representative for all atmospheric conditions. Therefore we added the more active case in the Appendix. During this work we have done simulations for different times during 2011-11-22, which behaved more or less similar. Therefore for brevity only the results for this single time slot are shown. We have added some discussion on the representativity of the particular case in the conclusions.

Please see my specific comments below:

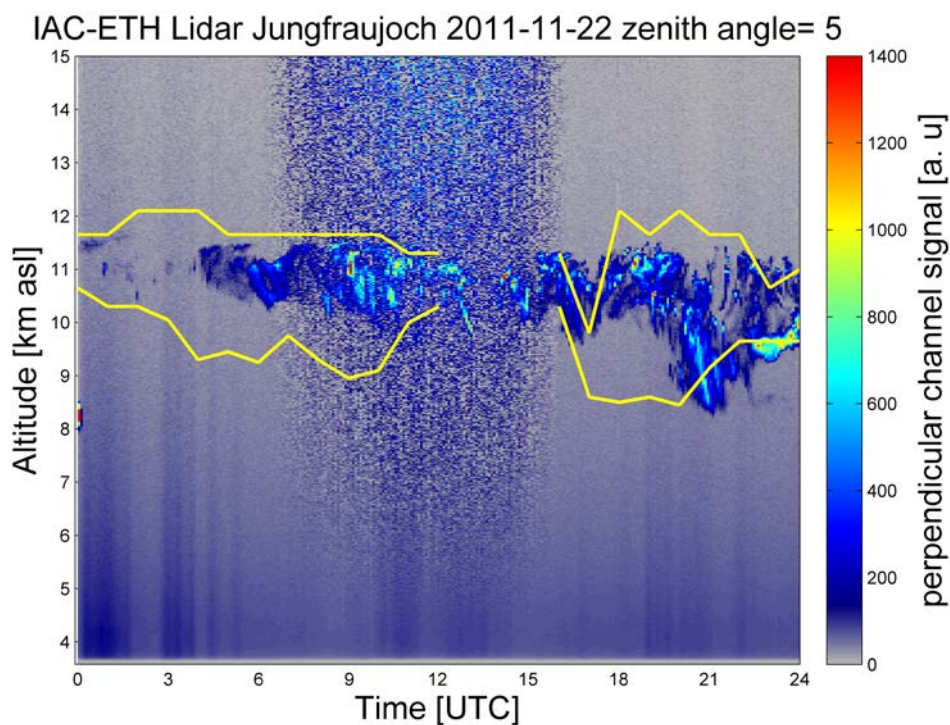
- Page 7538, line 15: “ice nuclei number density” is not quite correct. The simulations*

were carried out with homogeneous nucleation only, and with both homogeneous and heterogeneous nucleation with varying ice nuclei number densities. In section 2.3, please provide the number density of solution droplets used for the calculation of homogeneous nucleation.

This was corrected.

Section 2.2.1: It would be very useful to carry out a simulation of the cloud in the Eulerian domain using the COSMO model. The Lagrangian parcel calculations are subject to additional uncertainties (treatment of shear and particle sedimentation) and thus would greatly benefit from the comparison with the cloud simulation in the Eulerian domain.

The cirrus cloud was also simulated in the Eulerian domain using the COSMO model. One motivation for choosing this particular case was the presence of a cirrus cloud above JFJ in the Eulerian model, which implies that the large-scale temperature and humidity field is well-represented in the driving model. We added some sentence regarding this issue to section 2.2.1. In the figure below we show the lidar raw data of this day. The yellow isolines indicate a cirrus cloud being present in COSMO-2. Clearly, COSMO nicely captures the cirrus cloud observed by the lidar.



Page 7543, lines 11-13: Please state the vertical resolution in the cloud layer. Given the thickness of the cloud (1.5 km), please comment whether such vertical resolution is sufficient.

This is a good question. The vertical resolution of the COSMO-model in the region of the cloud is about 500 m, which is rather coarse. We added this information to section 2.2.1. However, a cloud is formed within the COSMO-model at the time of the observations, which

suggests a rather good representation of the large-scale temperature and humidity field. For the box-model approach, we used a vertical resolution of 100 m. The meteorological data and initial RH_{ice} are based on the COSMO-2 model. In the box model, we interpolate vertically the COSMO output to 100 m resolution. Indeed, the sedimentation of ice particles requires high vertical resolution. We tested the effect of different vertical resolutions on simulated cirrus clouds. The 100 m of vertical resolution is chosen here for affordable CPU time and still reasonable small artificial numerical effects.

I suggest referencing Spichtinger and Krämer (2013) and Dinh et al. (2015).

These papers have discussed specifically how small-scale, high-frequency temperature fluctuations affect ice nucleation, and thus are particularly relevant here.

Also, the high sensitivity of ice number density to the initial water vapour content of air parcels has been studied in Dinh et al. (2015, see their section 5.3).

These references have been added. We would note here, however, that the paper by Dinh et al. (2015) was submitted after our paper.

The radiative-dynamical effects (see e.g. Dinh et al., 2010; Schmidt and Garrett, 2013), which have not been considered here, could explain why the current model calculation underestimates the cloud extinction, especially at the cloud top in the active case (figure 14). Indeed, the radiative heating rate could be quite significant in the active case. The radiatively induced updrafts and water vapour flux convergence could help to maintain the cloud, and produce a higher cloud top and cloud thickness (see Dinh et al., 2010, their figure 7). Such features would be consistent with the lidar measurements in figure 14.

Thank you for your helpful comment. We have added some sentences about the radiative-dynamical effect, including References, to the Appendix. However, the inclusion of radiative effects is not straightforward in a Lagrangian framework and it is beyond the scope of this study to include this effect.