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.
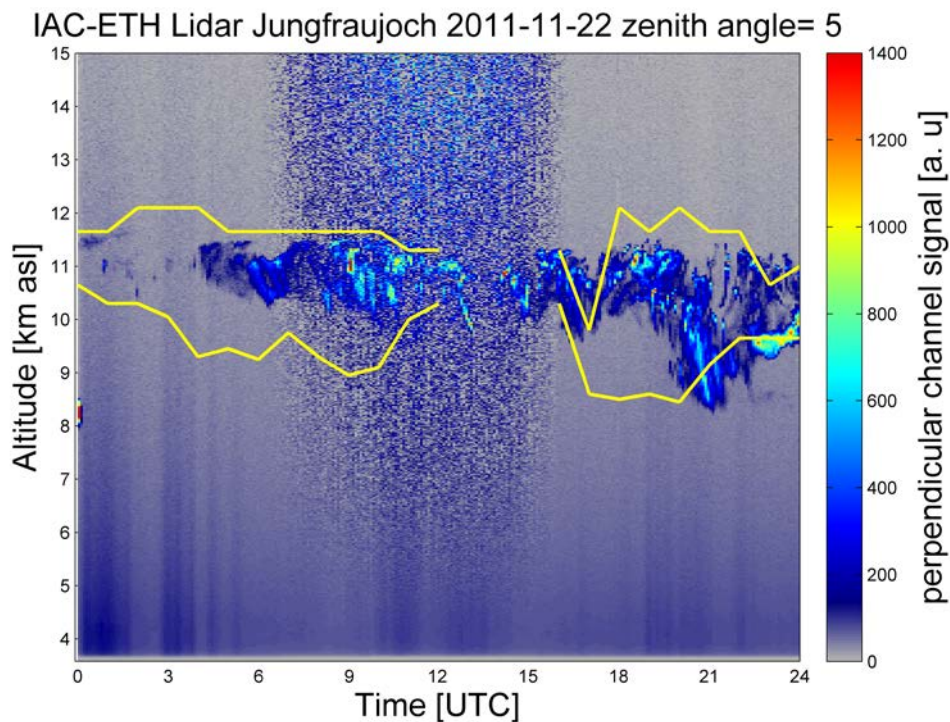
Page 7543, lines 11-13: Please state the vertical resolution in the cloud layer. Given the thickness of the cloud (1:5 km), please comment whether such vertical resolution is sufficient.

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 RHice 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.

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 Jungfraujoch (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 fmax (currently the units are 1=(s m)).

We corrected this.

6. PP 7751: please explain why w2 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.

Answer to Anonymous Referee 3

The authors are grateful for the time and thought that Anonymous Referee 3 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 3 are in italics and our responses in plain text.

In this study the authors investigated the influence of input data uncertainties on the simulated cirrus cloud properties over Jungfraujoch using a microphysical trajectory box model. They looked at the impact of trajectory resolution, unresolved updraft velocities, and the assumed IN number concentration on the simulated accuracy. Not surprisingly, they found higher trajectory resolution and the addition of small scale temperature fluctuations helped to improve the agreement between model and observation. On the other hand, the higher sensitivity to the specified initial humidity than to the unsolved temperature fluctuation is interesting. My major comment is that the observational data (lidar retrievals) used to the evaluate the model result are too limited in time (20min). This made the case study too specific and perhaps not applicable to other conditions. In general, the paper is well written and easy to read. However, I agree with reviewer 2 that the information presented in figure 10 is not very clear and should be improved. Some of the figure indices are mismatched and need to be carefully checked before final publication.

The representativity was addressed by Reviewer 1 as well. In the manuscript we emphasize that our results may not be representative for all atmospheric conditions. To examine further atmospheric conditions, the more active case in the Appendix was added. We have additionally performed simulations for different times during 2011-11-22, with similar results as those presented in the manuscript. Therefore we presented only the results of the chosen time slot. A discussion on this issue was added in the Conclusions.

Specific Comments:

P7536L18: Typo "bysignificantly"

Done

P7537L11: Remove "in turn"

Done

P7540L3: Would be better to note that the reported IN concentration in DeMott et al. (2010) is in per standard liter, not per liter under the ambient state. What's the unit (L−1 STP or L−1 under ambient state) used in the IN sensitivity simulations?

This has been noted in the manuscript. Our unit used in the simulations is in 1 per volume of ambient air.

P7541L22: How does ZOMM represent the size distribution of ice particles?

ZOMM uses a log-normal size distribution. It is initialized with 100 logarithmically spaced size bins. The number and radius of each size bin is allowed to change during the model run. We added this information to section 2.3. For further details we refer to the work of A. Cirisan, 2014 (cited in the manuscript).

P7542L16: Why only 20min's data were used? Why not using more lidar data and including more trajectories in the analysis?

We chose this time window because we manage to hit Jungfraujoch with a sufficient number of online trajectories at this time and because the underlying NWP-model produced a cirrus cloud at this time. We therefore assume that the NWP-model correctly models the larger scale temperature and moisture field at that particular time, so that we can rely on the p and T-fields that we need to force the microphysical box model. We have analysed different time windows on this day. Since the results did not differ significantly for other time windows, we decided for brevity to only show the results for this particular time. In addition, it has to be considered, that for each trajectory type (20 s, 1 min, 5 min), we performed 25*21=525 different simulations, adding up to a total of 1575 boxmodel runs.

P7544L24: If more trajectories were included, do you expect the result would change?

If including more trajectories for the same case, we would not expect to see major changes in the results as to the sensitivity of the simulations to initial moisture, cooling rate statistics or IN number density. The major reason is the fairly homogeneous large-scale situation during this period. If the same analysis would be conducted for other case studies, we expect somewhat different results, particularly in the relative importance of uncertainties in the small-scale temperature fluctuations and other variables. However, we anticipate that the simulations will be still sensitive to all of the parameters.
We would like to highlight again that a more general assessment of the relative uncertainty for a variety of different large-scale situations is beyond the scope of the current study, as the number of required box-model simulations is quite significant and the calculation of online-trajectories – ensuring a suitable coverage- is quite demanding.

P7544L11: What is the number of solution droplets assumed in the model?

We assume 250 sulfate particles per cm^3. Their sizes are distributed log-normally with a mode radius of 0.05 micrometers and a sigma of 1.4.

P7545L13: "according to the formulation of : : :" this part is a bit misleading.

We reformulated this sentence.

P7545 section 2.3: more details of the ZOMM model are needed. For example, apart from the nucleation process, which other processes are considered in the model? How these processes are coupled? And what is the microphysical time step?

ZOMM takes uptake and release of water vapour by ice crystals as well as solution droplets into account. In addition, sedimentation of ice crystals is treated. We added the time step scheme in the manuscript: "In the model, we apply a dynamic time step. The composition of liquid solution will change maximal 0.1 % in the nucleation regions and 1% for other regions during one time step."

We included this information in section 2.3. For more details on the of the ZOMM model we refer to section 3.4 in the paper of A. Cirisan from 2014.

P7545L18: Offline trajectories are based BACKWARD calculation, while the online trajectories are based on FORWARD calculations. Will this make a difference in the box model simulations?

No this will make no difference at all. If only the grid-scale wind field is used (as in the present study) and turbulent motions are not explicitly taken into account, trajectories calculated forward or backward in time will have an identical path, if the forward trajectories are started from the location at which the backward trajectories end (or vice-versa). Therefore also the evolution of temperature and pressure along the trajectories will be identical and hence the information that will be used by the boxmodel. The microphysical box model is run forward in time for all trajectory data-sets. We included the later information in section 2.3 of the paper.

P7545L19: Could you elaborate more about the sedimentation treatment? The current statement is not clear to me. Do you take the sedimentation flux from the host COSMO model? If so, do you consider the same ice particle size distribution in COSMO and in ZOMM?

No, we do not use the sedimentation flux from COSMO. Sedimentational fluxes to lower parcels are based on ZOMM simulations along higher level trajectories. We reformulated the respective sentences in section 2.3.

P7555L7: Do you mean Fig.8 here?

Thanks for the hint. We mean Fig. 9 but the correct color on line 6 should be blue.

P7555L12: Doesn't the green curve in fig8a indicate a cloud?

It indeed does. In lines 10 and 13 it should be Figure 9 instead of 8. Please excuse for the confusion.

P7556L1: Do you mean Fig.8c?
No, 9c

P7556L24: Do you mean Fig.8a?
Yes

P7557L21-228: The discussion here is a bit hand-waving. Would be nice to plot the supersaturation (as figure 6 and 7) before and after the microphysical calculation and the ice crystal size to facilitate the discussion.

We provide the supersaturation figures below. We do not think that there is a need to include those figures in the paper. However, we reformulated this paragraph in the article to be more precise.
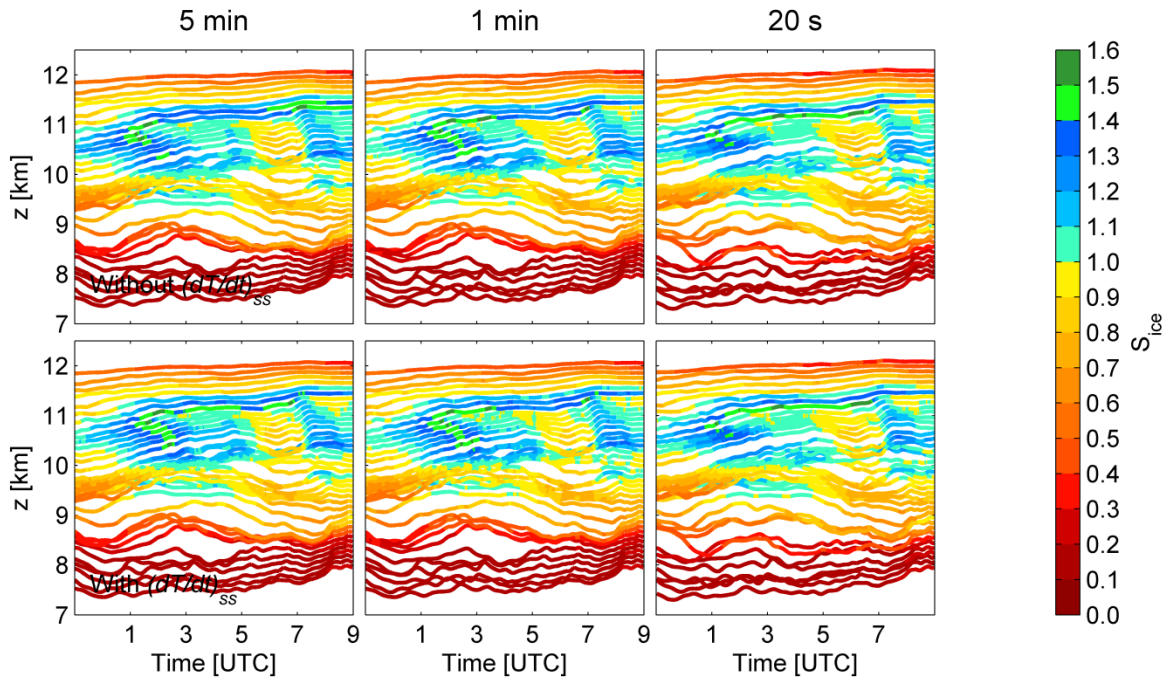
Answer to Anonymous Referee 4

The authors are grateful for the time and thought that Anonymous Referee 4 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 4 are in italics and our responses in plain text.

This manuscript presents a research study to investigate the influence of uncertainties in iput data on the simulated cirrus cloud properties. The study is interesting, and the paper is well written. I suggest publication of the manuscript after consideration of some mostly minor comments.

General comment:
Considering the evaluation of the model with lidar measurements I agree with reviewer #3 that one case study with observational data of 20 min may be too specific and the results may not be comparable to other conditions.

This issue has already been addressed in the answers to Reviewer #3.

Specific comments:
p. 7536, l. 15: Typo - 'bysignificantly : : :'

Done

p. 7546, l. 9: What about the extinction calculated from lidar measurements? Is this property sensitive to the retrieval and input parameters?

In Fig. 8 & 9 as well as Fig. 14 the uncertainties in the lidar evaluation (uncertainty in lidar ratio, in the signal itself as well as the molecular properties, see p. 7542 line 6-11) is shown.

P. 7546, l. 18: Typo - 'compares compares : : :'

Done

p. 7547, l. 19: Do you mean differences in the on- and offline trajectories?

Yes, we added this information to the sentence.

p. 7551, l. 6: Can you explain these differences?

The differences at long-wave length are very likely related to differences in the large-scale meteorological situation, i.e., smaller or larger gravity wave activity. There is a number of potential sources for this variability as for instance: differences in the stability of the lower atmosphere changing the vertical propagation of gravity waves induced in the lower atmosphere (e.g.,), differences in the wind direction and strength modifying the generation of terrain-induced gravity waves, the presence or absence of deep convection or fronts, which may induce gravity waves.

p. 7551, l. 27: Do you mean 'ascent data'?

Yes, this has been corrected.

Figures 6 and 7: labeling/time scale is inconsistent (upper and lower panel) for 1m and 20s cases.

Thanks, this has been corrected.

Figures 11–14: Maybe the order of the figures should be adapted following the argumentation in the text.

We are not sure what the referee means, since the order of the figures is identical to the sequence they are mentioned in the text.