Reply to Review#2:

We thank the reviewer for the helpful comments and suggestions. Based on the comments of both reviewers, the paper has been revised. In particular, section 2.2 gives now more details on the effect of potential aerosol detection, cloud inhomogeneities and includes already the introduction of the term surface area density path (ADP) instead of section 5; a couple of new subsections are incorporated in section 3 for comparisons to SAGE II and ground based lidars; a new section 5.7 on the formation process of LMS clouds in the CLaMS model is added.

Please find below the point-by-point response:

Major point:
The formation of cirrus clouds in the lower most stratosphere (LMS) is not addressed in this contribution. However, it is very crucial if the clouds are formed in the troposphere and then are just advected into the stratosphere or if the clouds are directly formed in the stratosphere. The authors should use the model results to investigate this issue at least qualitatively. Although the model might not be able to reproduce the ice water contents in a correct way, the humidity values along the trajectories should give some hints about the formation and even on the location of the nucleation event in terms of stratosphere vs. troposphere.

We followed the suggestion of the reviewer and included an additional subsection 5.7 on *LMS cloud formation* in the CLaMS model in conjunction with trajectory calculations and CRISTA observatons. New Figures 11 and 12 are discussing the temporal evolution of air masses transported from the subtropical jet over the Atlantic and finally forming ice clouds at high latitudes.
Finally we added following paragraph in the summary section 6 on the results of this trajectory study:

Trajectory studies with the cirrus module of CLaMS in comparison with the CLaMS simulations show the importance of mixing for the formation of ice clouds in the LMS. Mixing events at the subtropical jet can generate the entrance of enhanced vapour mixing ratios into the LMS which favour the formation of ice clouds even at high latitudes over Scandinavia in the model in agreement with CRISTA observations.

Minor points:

1. Comparison with former investigations: I miss some former investigations, which might be used for comparison. Actually, there are some recent activities of cirrus climatologies using LIDAR data in the French community (Dionisi et al., 2013; Hoareau et al., 2012, 2013; Dupont et al., 2010, 2011), which might be used for comparison. The SAGE data analysed by Wang et al. (1996) are available as gridded data; thus, they can be easily compared with the CRISTA data in a climatological sense. Finally, Spichtinger et al. (2003) reported ice-supersaturated layers in the stratosphere. Since thin cirrus are often embedded into a supersaturated environment, these data might also be used for comparison.

We followed the reviewers suggestion and included a couple of sub-sections in section 3 on comparisons with SAGE II, specific humidity measurements, and ground based lidars (3.4, 3.5, 3.6). However, the comparisons are not quite quantitative, because most of the analyses are suffering under the lack of an exact tropopause determination and a reanalysis of the datasets would be desirable.

2. Broken cloud fields: The authors mention in the text that CRISTA is very sensitive to homogeneous thin clouds but most cirrus clouds are inhomogeneous. They mention some errors for broken cloud fields; however, these sources of uncertainties should be explained in more details (maybe in the appendix).

We like to refer referee#2 to our Reply 1.2 for referee#1. There we give more details on the effect of broken clouds. In addition, we included additional information into and reworded the last paragraph of section 2.3:

Uncertainties in CTH determination from broken cloud segments along the line of sight in combination with the horizontal integration of the limb information and from the cross track extension of the FOV (30 arcmin, ~15 km) are not considered in the present analysis. Spang et al. (2012) showed for the combination of realistic limb ray tracing through high resolved 2D IWC fields from ECMWF analyses and RTM calculation that due to the integrated view of the limb sounder significant differences in the retrieved cloud top height are not expected for two clouds with constant ADP. For example a large IWC concentrated over a short horizontal distance or even multiple cloud fragments and a low IWC distributed homogeneously over a large horizontal distance result in very similar vertical CI profiles and corresponding CTHs. The study of Spang et al. (2012) showed that the size of the FOV and the location of the CTH in the FOV are the driving factors for the CTH error. However, due to the limb geometry cloud Inhomogeneities along the line of sight can result in an underestimation of the CTH with respect to the true CTH. If the cloud segment is placed significantly in front or behind the tangent point (>150 km for a FOV of 1.5 km) and consequently at higher altitudes then the retrieved CTH will be negatively biased. This error would therefore not explain the cloud observation above the tropopause.

3. Model parameterisation: It is obvious that the model has problems in reproducing thin cirrus clouds. From my point of view, the very simple cirrus scheme suffers from the fact that as soon as ice is formed (at a given threshold of 100-150%) all excess water vapour is transferred to ice. This is not realistic, since the relaxation time is usually quite large depending on the surface area of ice crystals. …

The fact, that all water vapour above the threshold is transferred to ice can indeed lead to the formation of relatively thick cirrus clouds. This effect can quite clearly be seen in the new Figure 12 (bottom panel) of the revised manuscript. While the cirrus clouds in the model case with conventional freeze-out at 100 % evolve slowly over several hours, the cirrus clouds with freeze-out at a certain over-saturation are forming quite rapidly. Furthermore, conventional freeze-out leads in general to more and thinner ice clouds, since they form at higher temperatures. Nevertheless, as for the examples in Figure 12, the results concerning the evolution of the clouds once they have formed are quite comparable between these two cases.

An introduction of a relaxation time on the order of minutes or hours into the global model with its 24 hour time step would not lead to a significant change in the results. Regarding the trajectory runs with a significantly higher temporal resolution this would of course lead to thinner ice clouds, and in the following maybe to a higher potential for the generation of ice clouds afterwards, since there is a chance of less water being removed by sedimentation.

We are currently working on an implementation of higher temporal resolution for the cirrus parameterization into the global model, thus improving the results on the larger scale. In this implementation we will consider relaxation times on scales that we will try to deduce from results with a more detailed microsphysical bulk model (Spichtinger and Gierens, 2009a).

Technical comments:

Figures 1, 4, 5 are heavily overloaded and are hard to read. Please make them much larger and think about reducing the amount of information. Maybe it would be better to produce two figures instead of only one.

In the final version of the manuscript these figures will be significantly enlarged (confirmed by the ACP editorial office). We also reduced the information (wind contours) and changed the symbols in Fig. 5 to reduce the overload on different information.

References:

Spichtinger, P. and Gierens, K. M.: Modelling of cirrus clouds – Part 1a: Model description and validation, Atmos. Chem. Phys., 9, 685-706, doi:10.5194/acp-9-685-2009, 2009a.