Answer to Dr. Lamont Poole

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

This is an interesting paper describing cirrus cloud occurrence frequencies, vertical distributions, and optical depths derived from lidar measurements at Zurich and Jungfraujoch Research Station, Switzerland, and Jülich, Germany. These results are compared with those from some earlier studies and are also used in a simple radiative transfer model to compute shortwave, longwave, and net cirrus radiative forcings. The paper is generally well written and the results are presented rather clearly. I do have a number of specific comments that the authors need to address before the paper is published in ACP.

Specific comments

The authors either are not aware of or have ignored some earlier papers describing ground-based lidar measurements of cirrus clouds obtained during the ECLIPS (Experimental Cloud Lidar Pilot Study) program. These papers include Platt et al., Bull.Amer.Met.Soc., 75, p.1635, 1994; Vaughan and Winker, Atmos.Res., 34, p.117, 1994; and Pal et al., J. Appl.Met., 34, p.2388, 1995. The authors should also mention how their new results compare with findings from these papers.

Response:

Thanks for your comment. We were not aware of these particular publications. We have added some of the results from these papers to our manuscript and compared them to our measurements.

Changes in the revised manuscript are marked in blue:

Lines 24 on page 2:
Lidar (LIght Detection And Ranging) measurements can be used to establish long time series of aerosol or cloud measurements (e.g., Platt et al., 1994).

Lines 2-3 on page 12:
agreeing well with the CALIPSO measurements discussed by Sassen et al. (2008) and being slightly smaller than the 18-19% measured during the ECLIPS campaign by Winker and Vaughan (1994).

Lines 11-12 on page 15:
These τ values agree well with the ECLIPS-campaign (Pal et al., 1995), where most detected cirrus clouds had optical depths smaller than 0.1.

Pages 4-5: There is no discussion of the possibility of cross-talk between the co-polarized and cross-polarized channels of the lidar and the effect that might have on any results.

Response:
We have added information about the possibility of cross-talk to the manuscript.

Changes in the revised manuscript on page 5, lines 1-6 are marked in blue:

We assume an ideal lidar system, which means that there is no cross-talk between the co-polarized and the cross-polarized channels. Rolf (2012) has examined this for the lidar used in Jülich. He found that for the parallel detector every 2000th detected photon is actual perpendicular polarized and for the perpendicular detector about every 570 detected photon is parallel polarized. While this justifies our assumption of an ideal system for the Jülich lidar, we found considerable cross-talk in the Swiss lidar, depending on certain maintenance conditions. However, cross-talk influences in particular the perpendicular channel which we use mainly for cloud detection but not for optical depth retrieval.

Page 5, line 26: The particulate lidar ratio can also be determined directly from high-spectral resolution lidar (HSRL) measurements.

Response:
Thanks for this remark, we have added this information to the manuscript.

Changes in the revised manuscript on page 6, lines 2-3 are marked in blue:

It can be obtained directly from Raman lidars that allow for an independent measurement of particle extinction and backscatter coefficients (Cooney, 1972; Giannakaki et al., 2007; Radlach et al., 2008; Reichardt et al., 2002; Achtert et al., 2013) as well as from high-spectral resolution lidar (HSRL) measurements (e.g., Burton et al., 2012).

Page 6, lines 17-18: It is not clear how the total uncertainty is computed. I don't think it should be the "sum" of the individual contributions as stated here. Is it the square root of the sum of the squares (RSS) of the individual contributions?

Response:

We combined the uncertainties in such a way that we assess the "worst case" of uncertainty. We have added this information to the paper with the remark, that a Gaussian error would be smaller.

Changes in the revised manuscript on page 6, lines 25-27 are marked in blue:

To assess the total, maximum uncertainty, we combine the individual contributions to provide an upper bound of the uncertainty. We calculate the largest possible error, which usually is larger than the error calculated by a Gaussian error (square root of the sum of the squares (RSS) of the individual contributions).

Page 7, line 1: Is the boxcar filter a moving average boxcar?

Response:

Yes, this information has been added to the manuscript.

Changes in the revised manuscript on page 7, line 9 are marked in blue:

"further smoothed using a moving average boxcar filter"

Page 7, line 24: What is meant by "a set of lidar ratios (5:5:40)"?

Response:

We use lidar ratios between 5 and 40, with steps of 5 in between. The sentence has been changed in the manuscript.

Changes in the revised manuscript on page 7, line 32 are marked in blue:

"lidar ratios between 5 and 40 sr, in steps of 5 sr …"

Page 7, line 31: Why is the temperature -38° C used to ensure pure ice clouds? Can the authors provide references?

Response:
We chose this threshold as it is the threshold for homogeneous nucleation to take place. We ensure that we detect pure ice clouds and exclude the mixed-phase clouds. We have added references to the manuscript.

Changes in the revised manuscript on page 8, lines 8-9 are marked in blue:

"Temperature has to be lower than -38°C (e.g., Pruppacher and Klett, 1997; Koop et al., 2000; Krämer et al., 2016) to ensure pure ice clouds and avoid detecting mixed-phase clouds (this…"

Page 12, Table 2, footnote (6): The text is confusing as written. Did the authors intend to say that relative uncertainties in their mean optical depths are comparable to "monthly mean values of 10-20% from ISCCP"?

Response:

The mean values of tau compare reasonably well with monthly mean tau values of 0.1-0.2 from ISCCP. We have changed the sentence accordingly in the manuscript.

Changes in the revised manuscript on page 13, table 2, footnote 6 are marked in blue:

"Mean values of τ compare reasonably well with monthly mean τ values of 0.1-0.2 from ISCCP…"

Page 14, lines 9-19: It would be good if the authors did some statistical analysis on the optical depth distributions in Figure 4 and could state whether the various distributions are significantly different from a statistical point of view.

Response:
We have used a Wilcoxon rank sum test to test this. We find p-values smaller than 1e-9, which clearly indicates that the distributions are significantly different.

Changes in the revised manuscript on page 15, lines 12-13 are marked in blue:

A Wilcoxon Rank sum test reveals that the optical depth distributions of the different sites are significantly different from each other.

Page 20, line 3: From Table 3, I conclude that CRFSW at 50°N from ISCCP is about an order of magnitude than the present results, but the CRFLW at 50° N from ISCCP is only a factor of 1.5-3 larger.

Response:

Thank you for pointing this out. We have provided this information to the manuscript and also added some explanations.

Changes in the revised manuscript on page 21, lines 15-16 and page 22, line 1 are marked in blue:

"1.5-3 times larger radiative forcing in the longwave, CRF_{LW}, and one order of magnitude larger radiative forcing in the shortwave, CRF_{SW}. The difference in the CRF_{SW} can only be explained in terms of a…"

Page 23, lines 8-10: I don't understand what is meant by "radiative forcing of the lateral boundary" of cirrus clouds? It would be good if the authors could provide a brief explanation.

Response:

In this cited paper the authors (Li et al., 2014) discussed the radiative effect of observed cirrus cloud edges. With lateral boundary they describe the first 10 km horizontal from outside cirrus clouds with optical depth of 0 towards small optical depth less than 0.3. In this transition region, they found still a CRF_{LW} of 10 W/m2.

This information has been added to the manuscript.

Changes in the revised manuscript on page 23, lines 29-31 are marked in blue:

The radiative effect of observed cirrus cloud edges is discussed. In the transition region of large cirrus, defined as their optically thin rim ($\tau < 0.3$), which is often missed by satellite passive optical sensors such as MODIS, the CRFLW found to be still substantial (~ 10 Wm^{-2}).

Page 23, lines 22-23: I don't understand the last sentence of this paragraph. What did the "close examination of CRFNET" with respect to cloud τ show?

Response:

Thank you for pointing to this ambiguity. We have removed the sentence, since it belongs to the paragraph below and is repeated there.

Changes in the revised manuscript on page 25, lines 8-9 are marked in blue:

The sentence was removed.

Technical Corrections

Page 1, line 2: It would be better to say that cirrus "…affect the water vapor budget …" not determine it.

Page 1, line 15: Reword to say "… thus enabling lidar measurements of higher ..."

Page 2, line 3: The word “subvisible” is misspelled.

Page 20, line 9: Reword sentence to say “Cirrostratus clouds with $\tau < 3.6$ occur particularly in this altitude range.”

Page 26, line 1: The word “subvisible” is misspelled again.

Response:

These corrections have all been implemented.