Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC4
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Comment on acp-2022-395

Anonymous Referee #4

Referee comment on "Investigating the radiative effect of Arctic cirrus measured in situ during the winter 2015–2016" by Andreas Marsing et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-395-RC4, 2022

General Comments:

The cloud radiative effect (CRE) of cirrus clouds tends to be strongest in the Polar Regions since cirrus cloud emissivity tends to be greater than the corresponding albedo, and longwave (LW) radiation tends to dominate over shortwave (SW) radiation in the Polar Regions. This gives Arctic cirrus a potentially elevated status in terms of radiative impact on climate. Moreover, cirrus clouds having visual optical depths τvis between 0.3 and 3.0 have the greatest frequency of occurrence (Hong and Liu, 2015, JClim), have a CRE representative of cirrus clouds overall (Hong and Liu, 2016, JClim), and appear to be most abundant in the Arctic during winter (DJF; Mitchell et al., 2018). Thus, the CRE of winter Arctic cirrus might be particularly strong, making the topic of this journal submission of interest.

However, this manuscript was written with a focus on SW radiation with LW radiation arguably secondary in importance. While the SW radiation is more interesting in many respects, the uniqueness of Arctic cirrus in terms of LW radiation should not be ignored. In the results section, it might be instructive to show net irradiance for these surface albedo (and cloudy vs. clear) conditions as a function of time over a 24 hour period. Relating TOA Fnet (same as CRE) to solar zenith angle is fine but this focus might detract from the fact that most of the time during Arctic winter the sun is not present and Fnet is determined only by LW radiation. A representative latitude (based on in situ sampling) could be selected for this. This would add perspective for those readers seeking a more representative understanding of Arctic cirrus radiative effects.

The paper is well written and organized and presents results that appear to be unique. After some minor revisions, it should be appropriate for publication in ACP.

Major Comments:

1. Figure 9: The results in Fig. 9 (especially 9a) appear to contradict the results in Fig. 17 of Hong and Liu (2015, J. Climate), where Fnet at the surface is comparable with TOA Fnet for the same τvis used here. Please attempt to explain this discrepancy.
2. Lines 352-354: There are evidently some errors in this sentence. The visible optical depths (τvis) for the Jan. and March case studies are 0.65 and 0.68, respectively (line 265) but here it says both τvis are identical. Moreover, τvis = 3 IWP/(ρi De), and multiplying the IWC profiles by a factor of 5 should also increase IWP by this factor, and thus increase τvis by a factor of 5. That being so, the 5-fold τvis stated for these two case studies should be 3.25 and 3.40 (not 2.94 and 2.85 as stated in the text).
3. Lines 379-380: Note this is due only to changes in SW radiation. Please provide an explanation to conceptually understand this. For example, is this due to the greater "effective" optical depth of the cirrus when incident reflected SW radiation enters cloud base at oblique angles?
4. Lines 382-384: But τvis is almost the same for both case studies (0.65 vs. 0.68). Are you sure that a 0.03 change in τvis can account for the shift in the snow albedo curves?

Technical Comments:
1. Line 29: trough => through?
2. Figure 9: Fig. 8 => Fig. 8a,b?

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2022-395/acp-2022-395-RC4-supplement.pdf