Review of ACP manuscript “acp-2022-395”
Title: Investigating the cloud radiative effect of Arctic cirrus
Authors: Andreas Marsing et al.

General comments

This is an interesting study. However, I would think it requires major revisions before it can be considered for publication in ACP. In this paper, IWC profiles of two specific cases are used to investigate the sensitivity of cirrus radiative effects on different parameters using radiative transfer simulations. The resulting dependence of the radiative effects of the cirrus on solar zenith angle and surface albedo is not particularly exciting, this is almost common knowledge. What I found original is the comparison of simulations assuming homogeneous clouds vs. using IWC profiles measured with high vertical resolution. I would consider this the most interesting part of the paper. Therefore, the study could become even more relevant, when additional artificial profiles would be considered. The paper already shows, that measured profiles result in different radiative effects compared to homogeneous cirrus. However, the question is how representative the few measured profiles are. How would the results change if IWC increases/decreases with altitude? This could lead to some more general conclusions of the paper.

Specific comments

1. Introduction

- The title should already indicated, that this is a case study. Otherwise the reader expects more than the paper can deliver.
- The introduction mixes state of the art with data description and even already conclusions from this study. This is confusing and should be clearly separated.
- Line 15: No numbers are given to quantify the importance of the cirrus radiative effect. Switching from negative to positive is important, but not, if we switch between +- 0.01 W m^{-2}
- Line 32: What means “unavoidable”?
- Line 63: Flight hours depend on flight speed and do not reflect the amount and representativeness of the data. Also different instruments sample different volumes during the same time. Horizontal distance, sampled volume, total particle number would be more meaningful.
- Line 68: What means “thin”? IWP or optical thickness measurements should be quoted here. Or you characterize the general cirrus conditions during the campaign earlier.
- Line 73: This is an example of mixing conclusions into the Introduction. This was not shown yet. Needs to be demonstrated.
- Lines 79-80: should be part of the conclusion section.

2. In situ measurements of IWC

- Line 86: How do you measure IWC? The first two paragraphs read as if WARAN is measuring water vapor only. First introduce the concept, which allows also to derive IWP from the instrument (heating, evaporation, etc)
- Line 103: This means, that you assume saturated air within the cirrus. Provide a justification, that this assumption is valid in most of the cases.
- Line 110: What is the inlet velocity and how large is it? Is this measured in parallel or is this combining all the enhancement effects?
- Line 118: Why can FISH serve as a reference? Is it more precise? Why not using FISH data for this study?

3. Statistics from Arctic cirrus sampling

- Fig. 3: Add the legend from the original publication.
- Line 142 ff: Are measurements in mid-latitudes included in this "Arctic" study? Below in the text it is separated. But I suggest to remove all mid-latitude data from the beginning. The study aims for Arctic cirrus. So there is no need to discuss mid-latitude cirrus in this paper.
- Extending Fig. 3 might also not be the main objective of this study. Consider to focus on the Arctic cirrus.
- Line 162: The analysis of Fig. 4 should be better motivated with respect to the main objective of the paper. e.g.: temperature might affect emission - thermal IR radiative effect.
- Line 166: Explain PVU, please.

4. Complete profiles of cirrus at the high latitude tropopause

- Line 204: These very thin cirrus clouds are common for the Arctic: Really? Only 4 profiles are from Arctic locations.
- Table 1: Leave out the mid-latitude cirrus, give IWC in g m^{-3}
- Line 210: What do 18 min mean in distance?

5. Radiative transfer calculations

- First paragraph: Partly a repetition of the introduction. As a reader, I expect a focused and detailed description of what has been done in this section and not another general overview.
- Line 253: The radiative transfer solver is not given in this section. DISORT or two-stream? For SW: UV radiation is neglected? How large is the contribution of UV, which is not covered by the simulations?
- Line 258: More details on this parametrization are needed. What does R_{eff} depend on? Is the Liou parametrization consistent with the parameterization of optical properties by Baum et al?
- Lines 261-262: Even if there are no in situ measurements of crystal shapes available for the cases investigated here, shape effects should at least be discussed. You may refer to the following two papers that would fit here:
    - For the thermal-infrared: Wendisch, M., P. Yang, and P. Pilewskie, 2007: Effects of ice crystal habit on thermal infrared radiative properties and forcing of cirrus. J. Geophys. Res., 112, D08201, doi:10.1029/2006JD007899
    - For the solar: Wendisch, M., P. Pilewskie, J. Pommier, S. Howard, P. Yang, A. J. Heymsfield, C. G. Schmitt, D. Baumgardner, and B. Mayer, 2005: Impact of cirrus crystal shape on solar spectral irradiance: A case study for subtropical cirrus. J. Geophys. Res., 110, D03202, doi:10.1029/2004JD005294.
- Line 276: It seems, that broadband albedo data of fixed values are used. In that case, I don't understand, why not simply changing surface albedo in 0.1 steps between 0 and 0.9? Such simulations can be attributed to specific surface types afterwards.
- Line 281: Snow and ocean do not reach an emissivity of 1.0. There is no need to make this approximation.
- Line 286: It is not clear, what temperature and humidity data are finally used for the simulations. HALO, IFS or GEMS? Or did you merge the data?
- Line 300: Are the measurements only used to validate the model or are they merged with the model for the final input of the UVSPEC?
- Eq. 4: I always thought that net irradiance is the difference between downward and upward irradiance. I understand that you define upward to be negative, this seems odd, at least from a measurement point of view (negative irradiances?).

- Line 315: Using symbol "*I*" for irradiance is uncommon. Now you use "*F*" as a second symbol for the same quantify. Net irradiance has the same unit as the irradiance. I recommend to use only one symbol.
- Line 333: What is the motivation to use different surface albedo for the second case? Hard to compare the impact of IWP of the two clouds, when the albedo is different.
- Line 353: Both cirrus have almost the same optical thickness. What is the gain to analyze a second case with similar optical thickness? What was different between both days? Should be motivated when introducing the cases.
- Line 369: To me the conclusion looks different: There is simply no significant solar radiative effect by the cirrus, if the surface albedo is high. Thus, no SZA dependence is visible.
- Fig. 11 very similar to Fig. 6, please combine them.
- Line 392: similar discussion for Fig. 6.
- Line 393: Temperature profile is not discussed. But this might have an impact on LW radiation.
- Line 438: 4.47 k day-1: What is the layer thickness the heating rates are related to? The absolute values strongly depend on this. Without knowing the thickness, the values are not comparable to other studies.
- Fig. 12: The heating rate profiles do not seem to fit to the net irradiance profiles. Decrease of net irradiance should always result in a negative heating rate. Or is this only the cloud contribution to the heating rate?
- Line 446: This should be discussed with the differences between solar and IR wavelength. The heating rates likely are dominated by the IR irradiances. There is no chance, that the surface albedo makes a big difference.
- Line 449: If Fig. A3 is discussed here in the main part of the paper, then the figure should also be placed within the main text. Readers should not be forced to move forward to the appendix while reading.

6 Discussion and outlook

- Why no conclusions?
- Discuss, if the measurement strategy/flight pattern affects the conclusions given here. The IWC profiles are not measured at a single location and might be affected by horizontal inhomogeneity of the cirrus. This should be mentioned and discussed here.
- The effect of the temperature profiles and the location of the cirrus with the profile was not discussed. As the longwave heating is dominating over the solar effect, this is more important than changing surface albedo.

Appendix

- As mentioned above: I would prefer to have the plots in the main text, where they are obviously needed.