

Interactive comment on “Spatiotemporal variability of shortwave radiation introduced by clouds over the Arctic sea ice” by Carola Barrientos Velasco et al.

Anonymous Referee #2

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Here, the authors mainly present data gathered from a areally distributed network of pyranometers deployed on an Arctic sea ice floe during summer 2017. Measurements of shortwave fluxes over Arctic sea ice are sparse, and our understanding of the spatial variability of e.g. the incoming shortwave flux as a function of sky conditions is highly limited. Towards improving this, the paper represents a first step. This reviewer intentionally says "first step" because the paper is structured as a data introduction paper, where relatively limited advances are made towards analyzing the results in depth, perhaps because of missing(?) auxiliary data on e.g. surface conditions on the ice. However, as data papers go, the manuscript is mostly clearly written and the measurement methodology appears sound. Therefore this reviewer considers the manuscript

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to have potential for reaching publishable quality, but a major revision is first required to improve on aspects of the content and presentation of the manuscript.

Major comments: 1. The introduction section feels somewhat disjointed to the reader. A rather broad and unfocused overview of Arctic sea ice changes is followed by a brief mention of the PASCAL campaign, followed by a longer description of projects and measurement campaigns with the same sensors in Germany (which seems quite unconnected to the topic at hand), followed by a relatively suddenly appearing statement that the goal of the paper is the “analysis of the temporal and spatial variability of the atmospheric global transmittance (ATg)”. What is the main point that the authors want to make here? Why is the analysis of ATg relevant, what does it actually relate to, what is the long-term goal behind the work? I recommend a review and revision of the introduction to make it more focused on the task at hand and its scientific justification/background.

2. From pg. 4, ln 12, it appears that all the data presented correspond to the waveband 0.3 – 1.1 microns, correct? So the calculated ATg is not the full shortwave broadband transmittance, but rather the visible-NIR section of it? Which would imply that the wavelength-dependent effects (e.g. Nann & Riordan, 1991 for some discussion) of clouds on the SWIR part of the solar irradiance waveband are not measured and their impacts on ATg variability remains unknown? Why does the discussion section contain no content on this point?

3. The middle part of section 3.1. is confusing when comparing with Figure 3. First, Figure 3 and the later text on this page defines the warm period as June 4-9. The text on pg 7, lines 11-12 is different, why? Second, Figure 3 appears to define the warm period as a period of positive AO, in conflict with the text and present definitions of AO. Figure 3a needs to be checked and revised.

4. The explanation in section 3.7. on the ATg variability causes for broken clouds do not feel convincing. It could easily be argued that the areas further away from floe edge

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have a thicker and dryer snow cover with higher albedo, thus making it possible that enhanced multiple cloud-surface scattering could contribute significantly to observed variability in addition to cloud organization? The case needs to be made better on this point.

5. Section 3.8. is a bit underwhelming content-wise. The finding that irradiance variability decreases with shorter observation time periods is quite self-evident, as is the fact that broken cloud cases exhibit the largest variability, and that thick multilayer clouds show the least variance. This reviewer would at least challenge the authors to take their thinking a step further. What do these results mean for recommendations on how to measure irradiance on snow and ice in the future? How much more “wrong” would you be if you had just a single pyranometer doing the work of the network?

6. Figure 1 has too much content squeezed into a single frame. At the very least, replace Fig 1d with a zoomed in region around Svalbard, with the drift track marked clearly. The sea ice concentration data has little to do with the manuscript. Also considering flipping the photograph so that the relative directions match with subplot b – the best quality would result from combining the two if the photograph has enough auxiliary information to make it a geotiff.

Minor comments (page – line): 2 – 2: extended -> extensive

2-2: “As the surface temperature increases, snow and sea ice melts,…” -> careful here with the wording. While brief warm periods can occur over the Arctic sea ice pack, most melt occurs at close to 0 C because the melt processes eat up the excess energy. Recommend simplifying to “Initialization of snow and ice melt reduces the surface albedo and increases the amount of. . .”

4-15: “Larger than the accuracy achieved. . .” -> a larger accuracy means a poorer measurement quality? Be precise with the terms, please. Consider revising the ambiguous “accuracy” (consists of both bias and precision) with “measurement uncertainty”.

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6-1: ATg presented are the averages from the pyranometer network? Define this in the text, please.

6-9: Lateral cloud edge reflection increases irradiance in broken cloud cases, but that is primarily a single-scattering mechanism. It's not only multiple scattering events, so be precise, please.

Figure 6d: The variability in ATg mentioned on pg 10, ln 1, is invisible without altering the color mapping.

11 – 27: The spatial variation of ATg may be low; ATg itself appears to vary a lot from 0.2 to 0.6. Revise for precision.

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