

Review on "Brief communication: Increasing shortwave absorption over the sea ice area at both poles"

General comments

In my opinion, the manuscript has improved in readability compared to the previous submission. The authors have made some statements clearer and have corrected shortcomings in their analysis.

I still have some concerns regarding the relatively small amount of robust and novel conclusions in this paper. In the paper, and even more so in the discussion, the authors mention the interest of the media as well as claims by so-called climate sceptics as a motivation for their study. However, a research paper is primarily meant to address other researchers. The main statement that the short-wave absorption is increasing should hence be complemented with an analysis of the causes. In my opinion the article could therefore elaborate a bit more on the potential scientific merits of its approach, and explore the main statements in some more analytical detail.

Specifically, the conclusions about the role of sea-ice albedo and clouds could be supported with some more evidence. From the analysis of sea ice distribution, surface albedo and absorbed short-wave radiation, the authors conclude that changes in cloud properties and ice albedo are responsible for the observed changes in short-wave absorption, but these properties are not shown directly. It would be ideal if the authors could somehow quantify the individual contributions of changes in sea-ice albedo and cloud coverage in order to support their statements. For example, how has the downwelling short-wave flux changed? As several cloud properties are also included in APP-x (for example cloud radiative forcings), it might not be out of proportion for this brief communication to test whether changes in these properties are in line with the author's claims that are based on some other variables of the same dataset.

In the conclusions, the authors repeat their statement that the short-wave absorption is increasing. I would also expect some statements about the scientific relevance of this finding, e.g. implications about our understanding of the polar climate, and how future research should address the questions that have to remain open.

Specific comments

- p. 2, l. 14: It is stated here and also at other places (e.g. p. 4, l. 13/14) that the dataset "takes into account" changes in cloud cover, albedo changes, etc. This seems to relate to the statement that "the energy balance is kept closed" (p. 2, l. 22/23) which seems to involve some kind of model. I suggest to explain this in more detail. Why is it an advantage to use this approach instead of independent observations of different variables? For readers not familiar with the available data, it could be explained in more detail which properties are observed and which are inferred by the authors involving additional assumptions.
- p. 3, l. 8: "accounts for true cloud cover". How? And what is "true"?
- Fig. 1: Why is the short-wave absorption much more variable in the Southern hemisphere? Given these fluctuations, is the trend significant at all? If not, it should not be claimed that the solar absorption has been increasing, and the conclusions might be better based on the correlations between variables instead of the trends.
- Fig. 2: I am puzzled by the fact that the sign of the trends is spatially so uniform, even in places where there is no sea ice. How can this be the case? Could there be a bias?

Minor comments

Main text

- What is new about the APP-x dataset, i.e. what makes it a "novel tool"? Hasn't AVHRR data been available for a long time?
- p. 2, l. 12: remove "large"
- p. 3, l. 11: "slightly larger numbers" – how much larger approximately?
- p. 4, l. 3: rephrase to make clearer: "seasonal Arctic sea ice..., the dataset shows a decrease..., while in the Antarctic, albedo trends show regional differences ... ice concentration."
- p. 4, l. 11: remove "relatively"
- p. 4, l. 15: "The mean annual shortwave energy flux...."
- p. 4, l. 18/19: Why is the absorption more spatially uniform in the Arctic?
- p. 4, l. 24: I suggest to cite the source of this result, or explain how this test was done.
- p. 4, l. 26/27: "energy absorption" – isn't it only the short-wave absorption and not the full energy balance what the authors address here?
- p. 4, l. 30: "does not show a pattern" – I don't understand what is meant here.
- p. 5, l. 11: "Increases in Antarctic sea ice only occur during the Southern Hemisphere winter". Fig. 1a seems to contradict this statement. Anyways, I do not consider this aspect important enough for the conclusions section. Instead, I would prefer to see a discussion of the implications of the main results (see above).

Fig. 1

- Fig. 1: The caption refers to minimal and maximal seasonal extent, but within Fig. 1a it is referred to March and September (by the way, months are written with a capital letter).

Fig. 2

- Caption: trends in "sea ice albedo". Is this really what the figure is showing? As far as I understand it shows the surface albedo change.
- I suggest to use a different colour bar that gives less weight to very small changes and more weight to large changes.
- a) and b) "Arctic" and "Antarctic" are unnecessary details in the variable name because one can see what region it is on the map anyways.

Fig. 3

- This figure is not discussed in the text. I suggest to embed it in the line of argument more explicitly.
- "albedo anomaly". Surface albedo, I guess?!