

Replies to Reviewer Comments: Reviewer 2 (S. Bathiany):

Please find our replies to the reviewer's comments in blue print.

General comments

In their brief communication, the authors focus on the surface short-wave balance in high latitudes. They use a new observational dataset (APP-x) to analyse trends over the recent decades and analyse the reasons behind these trends. I consider the format of a brief communication and the journal as a suitable choice to publish these results. Also, in the light of the large ongoing changes in the Arctic, the topic is of high relevance for science and the public. The general outline of the analysis is clear, and the relevant steps are explained.

We thank you very much for the positive evaluation of our work. We are happy that you also consider a brief communication in "The Cryosphere" as the right outlet for this research.

However, I also see two major issues that in my opinion should be addressed before the paper can be published:

1. General motivation for this paper

I had some problems to understand the motivation behind this analysis. This is related to the fact that the authors aim to compare the hemispheres to each other. I am wondering why exactly the comparison of the hemispheres is useful or relevant. Is there anything we can learn about how the energy balance works? Or are there implications for predictions or other practical aspects? The analysis of trends in sea-ice albedo, seaice area (or extent) and cloud cover, and their combined effect on the short-wave (SW) absorption at the heart of this study. This is done individually for both hemispheres, and important differences are mentioned. The authors make the interesting point that SW absorption increases in the Southern Ocean, despite the slight increase in sea-ice area. To my taste, this could be communicated as the main result (if it is new) in title and abstract because it is much more straightforward than comparing hemispheres. Although the authors do not add energy balance terms from both hemispheres together, their wording sometimes suggests this. I suggest to not say that solar absorption in one hemisphere "does not compensate" what is going on in the other hemisphere. I find this formulation confusing. I expect different effects at a certain location to be able to compensate each other, but not in two very remote regions that do not communicate directly.

Indeed our motivation for this study is mainly coming from requests from the public (and media) that we frequently get. We agree that the concept of putting both hemispheres in one pot is somewhat far-fetched in a scientific way of thinking alone. However, this comparison is frequently made by "climate sceptics", where increases in Antarctic sea ice cover are used as an argument to counter the decreasing trend in Arctic sea ice. We feel the urgent need to address this through an analysis of not just the changes in sea ice in the two polar regions, but also the impact of those changes on the surface shortwave radiation budget, which in turn impacts sea ice. Furthermore, the discussion paper has received significant attention in the community (article metrics) and related media requests show that the comparison of both hemispheres is a relevant topic. Having said that, the reviewer makes a good point that the finding of increased absorbed solar radiation in the Antarctic is interesting and important. We will emphasize this point in the revised manuscript.

Another problem with the title is that it even suggests a comparison of two unrelated processes or units. This is of course not true but confronting "sea ice gain" in one hemisphere with "increased solar absorption" in the other hemisphere is unfortunate in my opinion. Alternatives for the title could be "Increasing short-wave absorption in southern high latitudes despite increasing sea ice area" or "Short-wave absorption is increasing over both of Earth's poles" or something similar. In general, the implications of this finding could be made clearer.

We agree with your view that the title is comparing apples and oranges somewhat, so we suggest an alternative title combining your suggestions "Increasing short-wave absorption over the sea ice area at both poles"

2. There seems to be a problem with the numbers, at least in Fig. 1b and the associated text. As pointed out in the interactive comment by W. Eschenbach, the short-wave absorption values are not in agreement with other datasets. Some error might have been made in the calculations? The authors should correct it and check if their claims still hold.

Indeed there was a mistake in our calculations. Cells with incorrect retrievals are labelled with NaN in the App-X product. Unfortunately NaN cells were ignored during averaging, leading to way to high numbers in our manuscript. We fixed this by attributing zero fluxes to NaN values, as incorrect retrievals originate from too little sunlight in winter. Consequently we set NaN albedos to 1. Our numbers lie now in the range cited by W. Eschenbach, considering the known underestimation of absorbed fluxes by CERES data (Riihelä 2017, JGR, in press). However our conclusions stay unaffected by this calculation error.

Specific comments

1. Abstract: I think that it can be written more clearly (also see main comment 1).

We reformulated the abstract in the revised version

2. Fig. 1: Also as pointed out by W. Eschenbach, the temporal evolution of the fluxes in Fig. 1b is surprising. I understand from Fig. 3 that despite the low sea ice in specific years, we cannot automatically assume a large peak in SW absorption in these years, mainly because of confounding effects of cloud cover fluctuations? However, I am also wondering about the peculiar oscillation-like pattern in the Arctic time series, with a rapid increase every 5 years, followed by an accelerating decrease. This pattern is repeated several times with increasing magnitude. The authors may want to check if this behaviour is real.

We thank you for pointing out the oscillation-like pattern with jumps. As this stayed visible also after accounting for NaN retrievals and changing the cut-off latitude, we found that these patterns are likely related to drifting equator crossing times during the lifetime of individual satellites. The App-X radiative flux product is already corrected for this bias but somehow some bias remains. We will investigate this potential bias further and try to correct it in the revised version.

3. Fig. 3: The peaks in Fig. 3c seem to not coincide with those in Fig. 1b, or are at least shifted in time. For example, in Fig. 1b, 2000 is a year with low absorption in the Arctic and 2001 is a year with high absorption. But in Fig. 3c, both years have negative anomalies, whereas 2002 has a positive anomaly. Also, year 1994 is missing. I understand from the text why it is missing in the southern hemisphere, but it could be included in the analysis of the northern hemisphere.

This seems to be due to the drifting equator-passing times (see above comment). We updated the description in the text. Actually the data outage also affects the northern Hemisphere in 1994.

4. What is the reason for the increased downwelling SW radiation over the Southern Ocean? The authors could elaborate a bit on the role of cloud cover. Is this signal expected in the light of anthropogenic climate change, or is it random (internal variability)? So, what could be expected for the future?

We added some discussion on the reasons for the increased downwelling SW radiation. This seems to be caused by cloud cover. However one would rather expect an increasing cloud cover with retreating ice, thus we speculate that this is driven by local effects and interannual variability.

5. Methods: Why is it necessary to first calculate the cumulative absorbed energy in the whole region over one year and then convert it into fluxes again for the figures? The dataset already seems to contain fluxes. The potential calculation error mentioned above may be related to this.

Of course flux averages are calculated directly on the fluxes given by the dataset. We clarify this in the revised version.

Is there any good reason why the sea ice extent is defined differently than in other studies?

We added some explanations as to why we used this threshold of ice-thickness bigger than zero. APP-x does not contain any ice-concentration field and to stay consistent we did not want to use any alternative source. However the APP-x dataset uses NSIDC ice concentration >15% internally during the retrieval to determine whether a pixel is ice-covered. Thus all pixels with sea ice concentration > 15% have a valid ice-thickness and pixels below the threshold don't. So we are indeed using the same definition as in other studies. Differences to other studies just result from the rather coarse resolution of APP-x