Replies to Reviewer Comments: Anonymous Referee #2

The Brief Communication uses a new dataset, APP-x, to examine trends in the annual mean absorbed flux of shortwave radiation in the polar regions from 1985-2014. It addresses relevant scientific questions within the scope of the journal. The conclusions are appropriate for a Brief Communication. The article is clearly written and easy to read. However I have some criticisms.

We thank you very much for the constructive evaluation of our work!

I felt the authors could improve the description of the motivation for the study. For example, why consider only shortwave radiation and not include the emitted longwave radiation? The stated goal is "to determine to what extent the increased absorption of solar shortwave energy caused by losses in Arctic summer sea ice can compensate for the decreased absorption caused by modest increases of sea ice extent in the Antarctic." But why? I assume the reason for doing this is to assess the contribution to the overall global energy balance? If so, please comment on how you think how longwave radiation would contribute to the story?

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", so we feel the urgent need to counter this with a reasonable scientific answer. 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.

We added some discussion on longwave radiation. However we want to keep the manuscript focused on shortwave and do not attempt to describe the full energy balance of the polar regions as this would be too large for a brief communication. We focus on only shortwave because besides its major role in sea ice melt (solar heating & melting) it also impacts many other aspects of the sea ice system, such as biological production.

Commas are placed in unusual places and sometimes seem to be missing; eg p. 1, line 9 insert comma after "sea-ice". Please check throughout the document.

We will check the manuscript for typographic errors throughout the text.

p. 1, Line 19: Please give a reference for the debate.

We reformulated for clarity, as this is not really a debate.

p. 2: I think that the AAP-x data set calculates ice thickness using a 1D OTIS model with satellite-derived input variables. Is there any validation of these AVHRR ice thicknesses in the Southern Hemisphere?

We agree, that ice thickness is hardly validated due to a lack of reference data. However ice thickness was only used for ice detection, so it does not impact our calculations.

As an aside, the one-dimensional thermodynamic ice model (OTIM) is described in Wang, X., J. Key, and Y. Liu, 2010, A thermodynamic model for estimating sea and lake ice thickness with optical satellite data, J. Geophys. Res.-Oceans, 115, C12035, doi:10.1029/2009JC005857.)

p. 2, Line 22: More snow on Antarctic sea ice is also likely to give a higher albedo in the Southern Ocean.

Yes, but snow on sea-ice is not separately accounted for in the APP-x dataset. We added another mention of the effect of snow on albedo in the revised version

p. 3: It is excellent that the authors compare sea ice concentration derived from the APP-x data with passive microwave concentration data. But I would like to have seen some sort of quantitative comparison. This would increase the reader's confidence in the new dataset.

As the ice thickness retrieval is thresholded internally by the NSIDC ice concentration, our APP-x ice extent is effectively a regridded NSIDC ice extent. Thus we do not see the need for a further comparison. For your reference, we provide a version of figure 1A with passive microwave ice extent from the University of Bremen at the end of this document.

p. 3, Lines 10-15: I would like to have seen some estimate of the error in the quoted measurements.

We added confidence intervals as determined during the trend-fitting to all numbers.

p. 3, Line 26-27: I did not understand this sentence. Do you mean "this does not significantly affect our analysis of energy fluxes, as the largest uncertainty in the albedo occurs with low fluxes, subsequently leading to a low uncertainty in the time-averaged energy flux."?

Yes, we rephrased accordingly to make it clearer

p. 3, Line 29: ahha – here snow cover is mentioned. Please support with a reference.

We added a reference.

p. 4, line 1-2: Is the APP-x surface albedo not used to obtain ice thickness through the OTIM model? How does the lack of independence of surface albedo and ice thickness affect the calculation of shortwave energy flux?

We agree, but as we did not split solar energy deposition into ice and ocean compartments all that counts is albedo. Albedo retrieval is independent of the ice-thickness retrieval, while the modelled ice-thickness is of course dependent on the retrieved albedo.

p. 4, Line 3-4: Do you really know the annual shortwave energy flux to 0.1 in 200 Wm-2? Please make it clear that you have considered the accuracy of your results.

Thank you for pointing out this inconsistency. While these results are numerically correct according to the APP-x product, the accuracy of the entire measurement is of course less. We thus rounded these numbers to the significant digits in the revised version.

p. 4, Line 6-7 states that "Average Southern Hemisphere absorption remained relatively constant throughout the satellite record" while on line 13 it states "In the Southern Ocean energy absorbed by the ice-ocean system south of 50oS also increased : : :" I am confused by this apparent contradiction and I suspect that I have missed a subtlety.

We reformulated for clarity and corrected the wrong exponent (see below).

p. 4, Line 12-13: I think you say the total annual shortwave energy in the northern hemisphere increases at a significant rate, while in the Southern hemisphere it did not. Yet the numbers seem similar (8.77 X 1025 Jyr-1 compared with 6.14 X 1025 Jyr-1). Please justify. Again I may have missed a subtlety but, if so, perhaps you could make this clearer.

Thank you for pointing this error out. It was of typographic nature as the number in the exponent was wrong. We corrected this in the revised version.

Fig 1a): Confidence would be increased in the dataset if the slope from another reliable source (eg NSIDC) was added to the figure.

As explained above, the data is derived using a widely accepted NSIDC product, so we do not see the need for further comparison. Adding further lines into the figure would confuse it. For your reference we just provide a version of the figure with black dotted / dash-dotted lines representing the according ice extent data from the University of Bremen below. As mentioned in the text, the magnitude is affected due to the different resolution, but the trends are very similar.

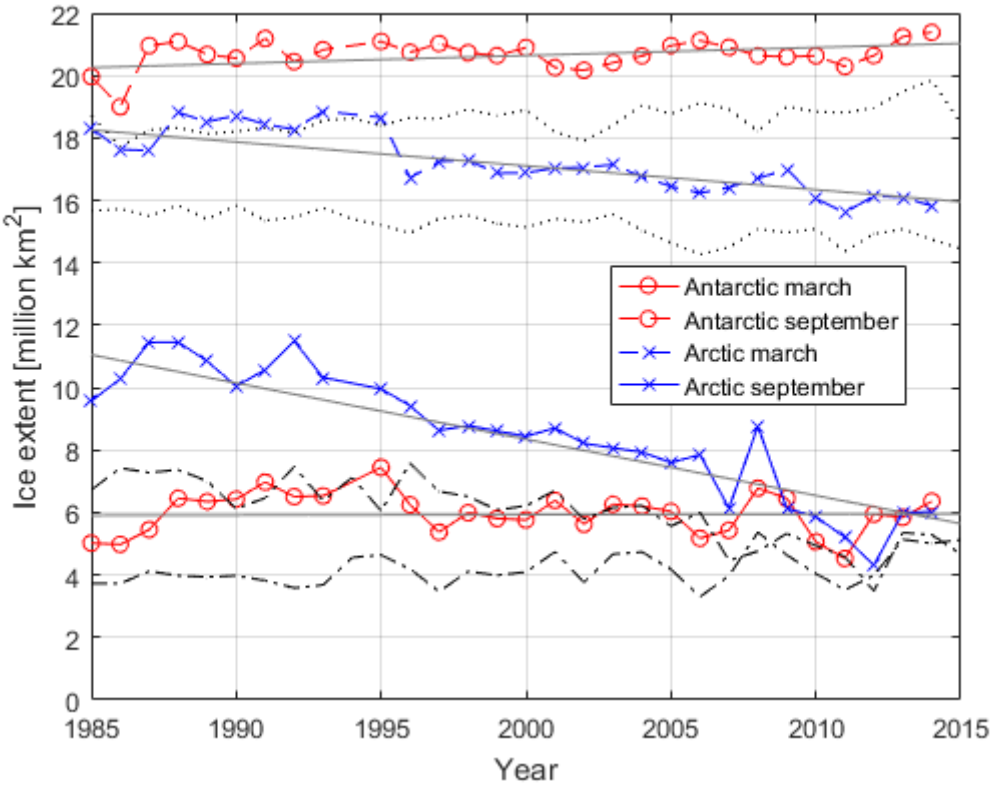

Fig 1b): What are the intriguing jumps in the data?

We thank you for pointing out these jumps. As this stayed visible also after accounting for NaN retrievals and changing the cut-off latitude, we found that these patterns are related to drifting equator crossing times during the lifetime of individual satellites. Thus we introduced an additional correction scaling the retrieved fluxes according to solar zenith angle during the actual satellite overpass. We rescaled the fluxes to zenith angles corresponding to 0:00 and 12:00 local solar time to achieve best possible estimates of daily mean fluxes.

Fig 2: The trend should be in Wm-2yr-1. Remove comma after “both”

Corrected accordingly

Fig 3: Caption and figure do not agree.

Corrected accordingly