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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Arctic sea-ice extent and its effect on the absorbed (net) solar flux at the surface, based on ISCCP-D2 cloud data for 1983–2007" *by* C. Matsoukas et al.

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We thank Referee #1 for the valuable suggestions and corrections. We have tried to address the raised points one by one. Please find our response below, with the referee comments in italic.

If surface (e.g., snow and ice) albedo values were derived from the ISCCP data, then the manuscript needs to contain a more quantitative description of these products... I also found details about the modeling to be scant. Information about surface roughness, cloud optical depths, cloud heights, and the associated errors in these *properties should be more comprehensively described.*

Surface roughness is not used at all by our model. Cloud optical depths, heights and sea-ice extent come directly from the ISCCP dataset without any error estimates. Because of the scarcity of cloud datasets as comprehensive as ISCCP and the resolution related problems against point measurements, it is hard to provide an estimate for the errors in optical depths and cloud heights. We can say however that we have been using the ISCCP cloud and surface data for many years (see our publications in "References") and we obtain consistently fluxes in agreement with measurements both at the top of atmosphere and at the surface. Hoping to shed some more light to the ISCCP methods, we have included the following description in our manuscript:

ISCCP initially summarises 30 km×30 km cloud data every 3 hours on an equal-area map grid with 280 km resolution and merges the results from separate satellites with atmospheric and ice/snow datasets to produce global coverage at each time. The Stage D2 data product is produced by further averaging over each month, first at each of the eight 3 hr time slots and then over all time slots. The cloud top temperature is derived from the infrared radiances, while the cloud top pressure from the vertical temperature profile of the atmosphere. We use the cloud optical thicknesses in the visible (0.6 μm), retrieved in ISCCP after comparison of the visible radiances with pre-calculated values from their radiative model. For more details on ISCCP polar clouds, the reader is referred to Hatzianastassiou et al. (2001).

ISCCP also provides surface data, such as snow/ice cover and albedo. According to the ISCCP documentation on the ice data product: "The original sea ice dataset used by ISCCP through 1991 is a digital version of the weekly analyses prepared by the U.S. Navy as paper maps since 1972. The weekly sea ice analyses combine data from shore station reports, ship reports, aerial reconnaissance and satellite image analysis. The satellite-based information constitutes 90-98% of the total and comes from visible/infrared imagery from the operational weather satellites and microwave imagery from experimental and operational satellites (when available). If new data do not arrive during the analysis cycle, older values are retained. In 1993, NOAA ceased preparation of the digital sea ice cover dataset for an indefinite period; the last year of data available is 1991. The new sea ice dataset is based solely on a daily analysis of microwave measurements from the SSM/I on U.S. Air Force DMSP weather satellites using the "NASA Team" algorithm (Cavalieri et al., 1984)."

I am also concerned about the accuracy of monthly estimates during times when the surface albedo changes rapidly (daily).

The referee's concern about the monthly resolution of the dataset is a valid one. We don't have a definite answer for the Arctic region, but we have investigated this issue for downwelling fluxes, both shortwave (globally in Hatzianastassiou et al. 2005, over the Mediterranean in Matsoukas et al. 2005, over the Red and Black Seas in Matsoukas et al. 2007, over Greece in Fotiadi et al. 2006) and longwave (globally in Pavlakis et al., 2004). These studies show small differences between the monthly, $2.5^{\circ}\times2.5^{\circ}$ dataset used here and a 12-year long, daily, $1^{\circ}\times1^{\circ}$ dataset provided by the GEWEX Surface Radiation Budget project. The surface radiation fluxes differ by usually less than 6 Wm^{-2} (globally, over the Mediterranean and Black Seas, over Greece) and their correlation coefficient is larger than 98%. Considering that the downwelling shortwave flux for the Arctic Ocean is around 200 Wm^{-2}, the 6 Wm^{-2} difference due to the above resolution issues corresponds to an error of 3%, which can be translated to the sea-ice forcing.

I find the title of the paper misleading- the paper really has nothing to contribute regarding new information about the Arctic sea-ice extent, so it seems misleading to title it "Ice extent and its effect on...". A more appropriate title could be "The effect of Arctic sea ice extent on the absorbed (net) solar...".

The title of the manuscript is now "The effect of Arctic sea-ice extent on the absorbed (net) solar flux at the surface, based on ISCCP-D2 cloud data for 1983–2007", as suggested.

Minor points:

- *The term "radiation transfer model" is used in several instances. I believe the term "radiative transfer model" is more common and familiar to readers.*
  The term “radiative transfer model” has replaced “radiation transfer model”.

- p. 21042, l.9: *“at the Arctic Ocean surface”, is this defined by a latitude boundary or ? please describe. Also, is this increase averaged annually or over sunlit period? Please clarify.*
  The calculation of surface forcing includes any 2.5°×2.5° cell northward of 60° N, reported as sea-ice covered (even partially) by ISCCP, at least for one month in the period July 1983 – June 2007. This area is shown with the coloured pixels in Fig. 1. The forcing for the Arctic Ocean surface is the sum of all 2.5°×2.5° cell forcings, divided by the Arctic Ocean surface area, equal to 14,056,000 km^2. If this sum is instead divided by the planet surface area, we derive the forcing for the whole planet. The words “annually averaged” have been inserted in the abstract in order to clarify the temporal frame.

- p. 21042, l. 21: *“Climatologically, Arctic sea ice. . .”*
  “Arctic” has been added.

- p. 21043, l. 17–19: *“changing ice properties-” does this mean changing ice concentration? or some other change in the ice? It’s not clear.*
  The relevant phrase now reads “changing ice cover”.

- p. 21043, l. 21–23: *annual net radiative forcing increase of 2W/m2 in a narrow coastal band? over what area? certainly not globally!*

The flux perturbation of 2 Wm^{-2} is definitely local, as explained by Stone et al. The word "local" has been inserted in our manuscript.

- p. 21044, l. 9–10: *No statistically significant trend where? over the Arctic? Globally?*
  The area of interest for the Wang and Key paper is the Arctic. We have added a few words in the manuscript to clarify this point.
- p. 21050, l. 28: *Experiments aren't "hypothetical," so how about "In another experiment,...? "*
  The word "hypothetical" is now removed.
- p. 21050, l. 29:*I think "melting "is a misleading term; the sea ice is often melting in summer. It would be better to say "September total Arctic ice disappearance".*
  "Melting" is substituted with "disappearance".
- *"Buffin" Bay should be "Baffin" Bay, and this name appears incorrectly in several places in the paper.*
  The correct "Baffin Bay" is now used throughout the manuscript.

References

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Interactive Comment

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