Atmos. Chem. Phys. Discuss., 9, C7339–C7341, 2009
www.atmos-chem-phys-discuss.net/9/C7339/2009/

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.

Anonymous Referee #1

Received and published: 20 November 2009

Review of "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:

This paper provides an estimate of solar heating in the Arctic Ocean over the 1983-2007 time period. It is novel in its use of a radiative transfer model forced with observations that avoids use of reanalysis data for the downwelling shortwave flux. The main finding of this work is that the sea-ice solar forcing (difference in shortwave radiation absorbed with and without sea ice) is significant, even on the global scale, and that trends in net solar flux are positive, although smaller than estimates made by recent work by Perovich et al. 2007.

The principal strength of this paper is that it derives an estimate of the net surface shortwave flux while avoiding problematic downward fluxes obtained from reanalysis products. It appears to be a useful estimate of the bounds for sea ice forcing, and should be widely useful to large scale coupled modeling efforts.

My concerns about the paper stem from questions about the resolution and precision of the ISCCP data on which the work is based. 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 am also concerned about the accuracy of monthly estimates during times when the surface albedo changes rapidly (daily). 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...". 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. The authors are careful to state that this type of study is limited since it does not employ any coupled modeling, and this is certainly true.

I believe that with the addition of minor amounts of descriptive material, as mentioned above, this paper would be a widely useful contribution. Additional minor points are as follows:

The term "radiation transfer model" is used in several instances. I believe the term "radiative transfer model" is more common and familiar to readers. p. 21042 line 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. p. 21042 line 21: "Climatologically, Arctic sea ice...." p. 21043 line 17-19: "changing ice properties"– does this mean changing ice concentration? or some other change in the ice? It's not clear. p. 21043 line 21-23: annual net radiative forcing increase of 2W/m2 in a narrow coastal band? over what area? certainly not globally! p. 21044 line 9-10: No statistically significant trend where? over the Arctic? Globally? p. 21050 line 28: Experiments aren't "hypothetical", so how about "In another experiment,..."? p. 21050 line 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" "Buffin" Bay should be "Baffin" Bay, and this name appears incorrectly in several places in the paper

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21041, 2009.