

Interactive comment on “Albedo of the ice-covered Weddell and Bellingshausen Sea” by A. I. Weiss et al.

Anonymous Referee #1

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General comments

The manuscript describes very valuable and interesting data, which are very much needed by the scientific community. The three main goal of the paper are: 1) to determine regional differences in the sea surface albedo in the studied area, 2) to calculate area-averaged albedo values representative of each region, 3) to study the relationship between temperature and albedo by applying commonly-used albedo parameterizations.

The first two objectives were reached through a reasonably well made analysis of the data. My main concern refers to the lack of proper error analysis (see detailed comments). Moreover, I would discuss more Figure 4, which presents very nicely the albedo results. I think that it would be good to stress more in the conclusion the poten-

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tial utility of these observations for the modeler community. The authors should also discuss about the representativeness of their dataset. The presented data were collected during two summers, and it appeared that the ice conditions were quite different at least in the Bellingshausen Sea. Would the dataset allow a direct comparison between the two summers in the three main study areas? If yes, it would be very nice to show it. Are the ice conditions during these two years representative of the recent decade?

The last objective was reached less convincingly. The discussion of the parameterizations needs to be rethought, addressing some evident limitations of the methods that were not discussed. And again, an error analysis is missing. See the comments below for the details.

The language of the paper is very good in the introduction but much poorer in other sections. Therefore a thorough language check should be made. The text should also be shortened and made more compact.

In conclusion, I recommend the editor to reconsider the paper after a major revision.

Specific comments

1. In section 2.1 it would be nice to spend few words on the cloud cover conditions during the flights. Cloud cover affects the surface albedo causing a maximum increase of about 0.07 from clear to overcast conditions. Therefore, even if cloud cover is not taken into account in the present surface albedo analysis, it is relevant to explain if, for example, the flights were carried out during prevailing clear skies or not. In case the flights were performed in a variety of cloud cover conditions, the spread of the albedo data caused by the cloud influence should be briefly addressed in the discussion section.

2. In section 2.2 a quantification of the resolution and accuracy of the infrared thermometer is missing.

3. In section 2.3 it would be very useful if the authors could provide a rough quantita-

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tive estimation of the accuracy of their derived sea ice concentration (for instance, by applying the minimum and maximum salinity values to their algorithm they could obtain a first error estimation).

4. Section 3.1 should be compacted. In my opinion, the description of the average surface temperature and albedo during each single flight is not interesting (and should be dropped) compared to the average values over the specific surface types (open water or ice). Indeed, ice concentration and average ice characteristics during each flight are the only relevant quantities discussed in the paper. Moreover, the physical explanation for the differences in albedo between ice and snow of various metamorphic states can be shortened.

5. Table 1: instead of “median” (written in the table caption) did the authors meant “mean”? There are some irrelevant columns, which can be eliminated without losing any interesting information: 1) starting and ending coordinate of the flights, 2) the median (or mean?) ice and water surface temperature and albedo, 3) the water fraction, which is just the complementary of the ice fraction. What is the meaning of ice fraction > 0? Cannot be quantified? In the first row of the SW Weddell Sea section there is a contradiction, as the ice concentration is defined 100% but still there appears to be some open water data. The temperature values presented in table 1 and in the text are often written with 3 digits. I believe that the last digit is smaller than resolution and/or accuracy of the infrared thermometer, and therefore meaningless.

6. Figure 3 should be dropped, as its main message (no correlation between area mean albedo/temperature and ice concentration) could be explained with a sentence.

7. From Table 2 the first two lines should be dropped. Again, knowing ice albedo/temperature and ice concentration gives enough information to determine the area-averaged albedo/temperature. Or, if the authors prefer, the area averaged value can be retained and the ice values dropped. Indeed, the discussion where authors compare the areal mean albedo/temperature values with the mean ice

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albedo/temperature is trivial; it is enough to write in a sentence that the values are almost identical as the ice concentration is so high.

8. I see Figure 4 as the nicest result of the paper. The authors should perhaps emphasize it more, for instance by discussing the potential utility of the information included in the figure for sea ice modelers who need various spatial resolutions (from mesoscale to global). In this respect, it would be important also to quantify the extensions of the three investigated regions.

9. When commenting Figure 5, the authors write that “all three panels verify the tendency for the mean sea ice albedo to increase with decreasing surface temperature”. I do not agree, the uppermost left panel shows that in the Bellingshausen Sea the lowest albedo was observed with the lowest temperature. By the way, why in figure 5 there are no data points for temperatures below -7 °C in the Bellingshausen Sea, while in Figure 2 there appear temperatures down to -11 °C?

10. In Table 3 the authors should convert Kelvin to Celsius degrees.

11. The whole discussion related to Figure 6 and Table 3 is inadequate. First of all, there are not any quantitative estimations of the ability of the parameterizations to fit to the data (see my comment below). Second, some important shortcomings of the listed parameterizations are not properly addresses. These are 1) the setting of the minimum allowed ice albedo to typical Arctic values, 2) the lack of discrimination between snow-covered and snow-free ice (actually, Ross and Walsh parameterization include the discrimination, but the authors do not apply it, I guess because the dataset does not allow it). The implications of these shortcomings on the performance of the parameterizations should be discussed.

12. At the end of section 4 the authors describe the calculation of the linear temperature/albedo functions that fitted the three specified sea ice areas, but they do not comment them, except for concluding in section 5 that no linear parameterization can predict the sea ice albedo with sufficient accuracy. This conclusion seems uniquely

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based on the qualitative impression given by figure 6. What is the “sufficient accuracy” that they believe is required? The authors need to quantify with some statistical error (i.e. root mean square error and bias) how well all the parameterizations listed in table 3 and shown in Figure 6 fit to the data. Then, a discussion should be made on the implications that such errors can have in weather prediction and climate models (for instance on the shortwave radiative budget at the surface). Only then the authors can evaluate if their new parameterizations can bring substantial benefit to the model simulations or not compared to the existing parameterizations. In case they do not bring significant benefits, it is not relevant to write them in the paper.

13. In section 5 The authors state that: “commonly only the sea surface temperature is used to parameterize the albedo in climate and weather prediction models”. I would eliminate that “only”, as more and more models are using also other quantities in the snow/ice albedo parameterization. This does not decrease the value and relevance of the present investigation on the relation of albedo with temperature, as the correlation between these two quantities has been shown in many other studies over different areas.

14. From line 15 to line 25 of p. 3276 (section 5) the authors discuss the effects of ice thickness and snow cover on the albedo-temperature relationship. However, the dataset does not provide any direct measurement of ice thickness or snow cover. If the authors want to extract a rough estimation of these quantities from the combination of temperature and albedo data, they should state it clearly and discuss the limits of these estimations. Presently the discussion is not well based, as the derived quantities are used to interpret the data from which they are obtained.

Technical corrections

1. On p.3263, line25: “angel” should be corrected to “angle”
2. On p. 3275, line 25: “We determined frequency distributions of sea ice albedo values and of averaged albedo values for the three sea ice areas...” . You actually did

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not calculate the frequency distribution of the ice types, but rather the percentage of area covered by sea ice and its averaged albedo in each of the three areas.

3. In my opinion there are variations in the writing style inside the manuscript. While the introduction is written in excellent English, some other sections (in particular section 3 and 4) flow much less smoothly and require improvement. I list just three examples of poorly written sentences: - p3267, line 26: “The Ronne Polynya is the result of the prevailing wind in this area which is a mostly southerly to south-easterly wind, resulting from cold air draining from the continent. . .” - p3269, line 24: “Our data show that the sea ice concentration in the Weddell sea was always very high during our observations, i.e. Cice > 95% whereas in the North-Eastern Bellingshausen Sea the sea ice concentrations showed also lower values and we observed Cice > 73%.” - P3270, line 10: “Other studies showed that with decrease in sea ice cover and increase water fraction the sea ice albedo decreases. This was shown by Brandt et al. (2005). They showed on the basis of satellite data that. . .”

4. P3270, line 16: “for the three sea ice areas” is a repetition and should be dropped.
5. P3273, line 23: “Welsh” should be “Walsh”
6. Figure caption 5: in “. . .number of data points with surface temperature” replace “with” with “versus”.

Interactive comment on The Cryosphere Discuss., 5, 3259, 2011.

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