

Interactive comment on “Remote sensing of sea ice: advances during the DAMOCLES project” by G. Heygster et al.

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The manuscript summarizes advances in sea ice remote sensing during the DAMOCLES project. The topic is naturally very suitable for the DAMOCLES special issue. The manuscript summarizes and evaluates previously published results and presents some new results. I don't see problems in this combination. The authors have done an extensive and valuable work: a lot of important information is summarized in a way that is mostly easy to follow even for a reader who is not a specialist in remote sensing techniques. The text is generally well written, although the quality varies between different parts of the manuscript. I suggest that the manuscript should be accepted subject to the following revisions.

Main comments:

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1. Some of the previously unpublished results should be described in more detail. These include Section 5.3 on sea ice drift from ASAR observations and Section 5.5 on sea ice deformation. Also the in situ measurements of snow reflectance seem to be new results, but these are already explained with sufficient detail.

2. Although the manuscript is submitted to the DAMOCLES special issue, I believe that most readers would be more interested in a review on advances in sea ice remote sensing during recent years than during some individual project. I do not propose to broaden the scope of the paper, but it would be very good to include in the Conclusions an evaluation of the advance made in DAMOCLES compared to the general advance during the same period. What has been the contribution of DAMOCLES in the worldwide advance in sea ice remote sensing?

3. Many Sections start with a good introduction to the historical progress and state of the art in the field, followed by a review on the recent advance. In the latter part, it is, however, not always clear what has been achieved in DAMOCLES and what in other recent projects. For example, in Section 6, is the work by Forsström et al. (2011) a DAMOCLES achievement?

4. The manuscript misses some DAMOCLES achievements in sea ice remote sensing: the study by Riihelä et al. (2010) on remote sensing of albedo with validation against Tara data should be reported in Section 4.3. Also the DAMOCLES paper by Maksimovich and Vihma (2012) includes some aspects of sea ice remote sensing. In particular, how to apply remote sensing data to distinguish between the true onset of snow melt on sea ice from the appearance of open water due to ice drift divergence.

Riihelä, A., Laine, V., Manninen, T., Palo, T., and Vihma, T. (2010) Validation of the Climate-SAF surface broadband albedo product: comparisons with in situ observations over Greenland and the ice-covered Arctic Ocean. *Remote Sensing of Environment*, 114, 2779–2790.

Maksimovich, E., and T. Vihma (2012), The effect of surface heat fluxes on interannual

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variability in the spring onset of snow melt in the central Arctic Ocean, accepted with minor revisions to *J. Geophys. Res.*

4. Section 6 on remote sensing of sea ice thickness is very interesting, but I am not convinced about the conclusions on the main uncertainties. The text addresses the importance of snow depth in converting freeboard measurements to ice thickness, and the differences in snow depth between first-year and multi-year ice are well demonstrated. The authors point out that the Warren et al. (1999) snow climatology does not cover first-year ice. In the end of the Section they conclude that the uncertainty in multi-year ice density is the major source of error for the ice thickness estimation. It remains unclear why the uncertainty of snow load is no more considered a major issue. The same comment holds for the Conclusions, where the ice density and measurements of freeboard are considered the main sources of uncertainty.

Minor comments

5. Introduction: - The papers of Rampal et al. (2008) and Andersen et al. (2007) are not the best references for effects of leads on the heat flux from the ocean to atmosphere. - something is missing from the following sentence. Perhaps start with "Due to the combination of thinning ...". - line 22: ... more over sea ice than over open water .. - Page 40, lines 7-8: confusing text: snow surface temperature (IST, ice surface temperature) - lines 17-18: there are much more factors causing variability in snow albedo: e.g. solar zenith angle, cloud cover, surface tilt (e.g. sastrugi), and even air humidity (e.g. Pirazzini 2004, JGR).

6. Page 42, lines 3-5: which years were studied? Line 9: SHEBA took place in 1997-1998.

7. Page 43: a schematic figure on the inverse method might be helpful for a reader.

8. Page 44: the concept of the effective temperature could be explained more clearly.

9. Page 46, line 3: replace "significantly affects" by "strongly interacts with"

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10. End of Section 4.1: Do the authors have any idea about the reasons for the variations in snow grain size in Ross Ice Shelf (a flat, very homogeneous region)?

11. Title of Section 4.2: add "of"

12. Page 50, line 3: "measure the snow reflectance"

13. Page 51, line 28: "it is only 30°C"

14. Page 52, line 7: the sentence is not finished

15. Is there any validation for the results presented in Figure 9?

16. Page 54, lines 1-2: It is worth mentioning that, via rafting and ridging processes, sea ice dynamics is very important also for the thickness distribution of sea ice.

17. Page 56, line 23: "during sunlight periods, when"

18. Page 59, line 17: "drift and deformation generate leads"

19. Page 61, lines 1-13 and Fig 12: The authors seem to associate wind drag to the ratio of drift speed and wind speed, but the term wind drag (or wind stress) means the momentum flux from air to ice. I don't see basis for the statement: "changes in the wind drag on sea ice show a clear increasing trend since 2002." Perhaps the authors mean that the response of ice drift to wind drag has increased (due to ice becoming thinner). See also Vihma et al. (2012, GRL).

20. Page 63, line 15: "Eq. (3)"

21. Page 65, line 18: "Sect. 2.1"

22. Table 2: add years covered by various data sets

23. Figure 6 is nice, but if you want to show one photo of measurements, it would be more suitable to show a photo on measurements on sea ice instead of a hill top.

24. Figure 11: move "(black and grey lines, respectively)" right after "convergence".

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Also, in the upper-right corner of the figure, one of the “wind div” should be “wind conv.”

Interactive comment on The Cryosphere Discuss., 6, 37, 2012.