Response to the comments of Marcel Nicolaus (Reviewer 1)

We would like to thank Dr. Nicolaus for his constructive comments and criticism given to the manuscript, which, we hope, has led us to improve our work. We have changed the paper content in accordance with your advice and advice given by another referee.

We start with a description of major modifications we made to the paper in response to the comments provided by the reviewers:

1) Manuscript title was changed to "Regional melt pond fraction and albedo of thin Arctic first-year drift ice in late summer"
2) The manuscript was restructured to accommodate the methods in Section 2 only. This made Section 3 more focused on the presentation of the results and discussion.
3) We added two additional figures showing the latitudinal distribution of melt ponds/leads and the bootstrap albedo inferred from all 6 flights.
4) We extended the analysis of the available images using a more advanced but labour intensive image processing technique of Renner et al. (2013) to refine our correction scheme for the rest of the data.
5) The EM-bird data on ice thickness from 5 flights were processed and analyzed to demonstrate the spatial homogeneity of the ice type in the study area. We also added a co-author (A.Renner) who processed the data
6) Both Introduction and Conclusions were modified to comply better with the goals and results of the study.

Response to the reviewer's major comments:

1) Both reviewers raised the issue of using a different surface classification technique instead of the algorithm of Renner et al. (2013) which was available at hand for the authors. We fully understand the reviewers concern but can provide argumentation sufficient to advocate for the use of the method presented in the manuscript. Our major motivation for using a simplified 3-class object detection -based technique is related to a large volume of data we had to process. As stated in the data section in total some more than 10000 images had to be classified. The actual choice of the technique was a trade off between the time available for the raw data analysis and the scientific outcome of the work. We do acknowledge a higher quality of the results one can get using the method of Renner et al (2013) which however comes at a price of a much higher labour intensity. While our simplified approach to image processing have required some 6 weeks of work (including the method elaboration in Matlab), using Renner's method conditioning that all the images are supervised and manually corrected would take months of work. Note that we also had to produce and test a new image classification training set that takes into account a change in the setup we used for this study. We, however, did use the improved method of Renner et al (2013) for the quality control of our simplified technique and this is indicated explicitly in the manuscript. Our relatively minor modification of the method applies to the supervision step of the algorithm and aims at a more efficient elimination of smaller scale misidentified textural features. Fig.4 in Renner et al., (2013) provide a good example for such misidentifications when shadows from the surface topographic features (like roughness of the snow surface) are interpreted as melt ponds or light marks due to ripples on the water identified as sea ice.

Note that panels c in Renner et al., (2013) demonstrate that this issue is not alleviated in the final result; this is also shown in Table 1 that suggests relatively high false detection rate for melt pond pixels. Our modification of this method allowed a manual selection of the regions with the scattered misidentified pixels or pixel clusters during the supervision step. It made it possible therefore to have the images classified with an almost absolute accuracy. However, as this would imply too long processing times, we analyzed using Renner et al. (2013) technique only about 15% of the data set and used these classified images for the quality control and error models on the variables derived from the whole image data set.

2) (a) The reviewer raises a rather common question of how the field data can be directly integrated into the models and/or used to improve their performance. The general answer would be to say that our understanding of the physical processes at work (also for the case of the sea ice processes) and the way they are presented/parameterized in the models to a great extent based on the analysis of the data brought from the field. Although one has to admit that a single case study may not provide an in-depth answer, this a routine field work that boosts the progress in the modelling studies. We however may also see how the field data used directly in the models, e.g. for improvement of the seasonal scales sea ice forecasts (see e.g. Lindsay et al., 2012, GRL or Castro-Morales et al., 2014, JGR). Specifically our data representing a higher resolution study of the sea ice surface properties conducted on a regional scale can directly be used in validation of the remote sensing algorithms for retrieval of ice thickness, concentration, melt pond coverage. From our upscaled estimate of the regional albedo we also immediately see that the treatment of melting first year sea ice in a number of models (see e.g. Table 1 in Johnson et al., 2012) is inadequate calling for a reconsideration of the seasonal cycle of the first year ice albedo.

(b) We don't consider the question as relevant in the context of the albedo studies alone. It is much more general yet the answer is rather straightforward. Any empirical variable, (measured directly or derived from the measured values), regardless of what this variable is later to be used for should be presented together with its uncertainty bounds. The lack of this knowledge precludes any subsequent efficient use of this result.

(c) We are not quite sure what the reviewer actually meant behind the "dependency of aggregate albedo on observational scales". We suggest this is somehow related with the effect of inclusion/elimination of the MIZ in computations. If yes, we present the relevant numbers and some discussion in the revised version of the manuscript.

(d) The issue of choosing the bootstrap technique for the data analysis is actually discussed in the manuscript. Processing a large collection of spatially distributed images with an inference on some sought parameter is equivalent to sampling from a random data field with the unknown probability distribution and covariance structure. Having generated a large set of samples the bootstrapping is a natural choice when one want to derive some general statistics from the entire set.

3) The next three general questions are discussed throughout the text:

"What does this study and their results mean for models and other studies" is briefly discussed in Conclusions

"What are the results that advance this field and how can they be used" is briefly discussed in Conclusions

"Why should this method be used instead of the existing ones" is discussed in Data and methods sections

Answer to Rev. 1 specific comments.

Title

The authors don't make any specific stress on the presentation of the technique(s). We believe this is nothing particularly novel in the methods which were adapted from other studies. We still focus on the analysis of the regional morphological properties of first year sea ice and derivation of the regional scale albedo, so we decided to modify the existing title to emphasize our main findings. We however prefer to retain "drift ice" to discriminate from fast ice that may have different optical and morphological properties due to different formation conditions and higher sediment load.

Abstract

The abstract was re-written to better present the scope of the work; the sentence about the relevance for the modelling was omitted as we don't present any particular results on this topic. Yet this issue is discussed in Conclusions.

Introduction

1) We mention the aspects of the remote sensing of sea ice in summer in Page 4, Lines 8-10.
2) The explicit expression for the aggregate albedo is introduced in the "Data and methods" section. In the Introduction we simply provide a relevant reference where this term appears for the first time in the text. The term "regional albedo" refers to the spatial scale of this variable, while the term "aggregate albedo" indicates the method this estimate was made. We do include the open water in the regional estimate, since the leads is a part of the surface. The upscaled ponded sea ice albedo is, however, provided too.
3) We improved the two last paragraphs of Introduction to make sure the goals for the study are more clearly presented. However, we would like to keep the description of the manuscript structure too.

Data and Methods

1) As stated in the text ICE camera was developed as a part of the photogrammetric setup. The frame shooting rate was set to ensure at least 70% overlap between the successive images provided typical EM-bird flight velocity and altitude. For the goals of this study such a frame overlap is redundant. The every second image from one of the cameras (efficiently every fourth captured image) ensures minimal or almost no overlap between the selected images.
2) Please see our comment referring to the motivation behind the use of the simplified image processing technique. Computing time has never been a critical aspect in this work, rather the time required for supervision and correction of the results. We are aware about the link between the colour of the ponds and a state/thickness of the ice beneath the pond bottom; this is explicitly mentioned in the text. As the two types of ponds are characterized by a distinctly different colour (albedo) we are keen to keep this differentiation in the upscaling

scheme. Analysis of the images from a single track classified using Renners method demonstrates the areal relationship between the two types is rather stationary; we therefore extend this ratio to the entire data set. Submerged ice in this study is treated as melt ponds.

Results and Discussion

Figure 1 was modified in accordance with your suggestion. In addition we show a km scale on the two new figures for the latitudinal changes in the melt pond/open water coverage and calculated albedo.

Conclusions

1) We reserve the notation α^{r} for the regional aggregate bootstrap albedo only, whereas α^{s} is used for any arbitrary set based bootstrap albedo. We believe this is a fairly efficient way to avoid any mess with the notations in the text.
2) The respective paragraph in Conclusion is now reformulated to emphasize our results and present the explanation of a systematically lower value for sea ice albedo we have derived. Note that follow suggestion of Reviewer 2, when comparing with other studies we discuss the ponded first year sea ice albedo rather than the regional albedo. This eliminates the open water from consideration and put more focus on the ice itself.
3) Yes, the authors are aware of different geographical settings in the study of Lu et al. (2010) what again highlights a difficulty of making any very detailed intercomparisons between the studies scattered in time, space and having different methodologies.

Figures and tables:

1) Figure 1 was modified in accordance with your suggestions. Bathymetry information was eliminated replaced by the sea ice chart for the period of the drift.
2) It must be a print issue: on Panel 3b the difference between the shades is clearly visible.
3) Done
4) Done

Regards
Dmitry Divine
Norwegian Polar Institute
Tromsø, Norway