Dear Dr. van den Broeke

With regard to the requested corrections/modifications to the initial submission of manuscript tc-2014-107, we are happy to resubmit a revised version of our manuscript. The manuscript has undergone a substantial revision in response to the comments of the reviewers. In the attached letters of reply we provide a list of changes made to the paper followed by point by point answers to the reviewers comments. We hope it is now acceptable for publication in TCD; however, we can edit further in response to the reviews, if necessary.

Regards

Dmitry Divine

Norwegian Polar Institute,

Tromsø, Norway

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 \alpha^{r} for the regional aggregate bootstrap albedo only, whereas \alpha^{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

Response to the comments of Reviewer 2

We would like to thank the reviewer 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:

7) Manuscript title was changed to "Regional melt pond fraction and albedo of thin Arctic first-year drift ice in late summer"
8) 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.
9) We added two additional figures showing the latitudinal distribution of melt ponds/leads and the bootstrap albedo inferred from all 6 flights.
10) 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.
11) 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. One co-author (A. Renner) who processed these data joined the manuscript.
12) Both Introduction and Conclusions were modified to comply better with the goals and results of the study.

We note that 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.

Response to Reviewer 1 major comments:

1) Both reviewers indicated that the paper required some restructuring in order to highlight the major subject for the study – the spatial variability in sea ice surface properties and/or the methods of data analysis. With the present format of the manuscript no specific emphasis is placed on the presentation of the technique(s) which are now all moved to the Data and methods section. We actually believe that there was nothing specifically novel in the methods which were adapted from other studies; the manuscript primarily focuses on the regional properties of first year sea ice cover during melt. Despite the limitations in both space and time the presented results provide a good sketch on the state of thin (70-80 cm) first year ice during the peak of the melt period. In the revised version of the manuscript we present the upscaled regional albedos with and without the MIZ as well as the regional albedo of melting sea ice only. We also demonstrate in two new figures how the degree of surface melt and the associated albedo changes varies across the margin of the ice pack.
2) The list of citations in the revised manuscript was extended to comply with the changed content. It also concerns the relevant references on the past aerial studies of melting sea ice

pack. One have to stress, however, that the volume of available literature on the topic is rather extensive and the 100% complete citation list is simply unrealistic. We note also that some of the papers we cited already provide a good overview of the literature on the study subject.

3) We agree with the reviewer. In the revised version "the advanced stages " is substituted with the "advanced stage".
4) Please see the list of major changes and our response to your major comment (1).

Reply to other comments (your comments are highlighted italics)

Abstract and Introduction

P3699/L3: Correct 'advanced stages of melt' to singular.

Done

P3699/L1-21: Is the main result you are highlighting the regional albedo estimate of 0.40?
The main results are the melt pond coverage together with regional and sea ice albedo inferred from the surveys and upscaling.

P3699/L24: 'ocean-sea ice-atmosphere' used here, 'atmosphere-sea ice-ocean' used later. Choose one and abbreviate it if necessary

we use the notion of "atmosphere--sea ice—ocean"

P3700/L14: "..up to 70% of the surface". Is would be more appropriate to refer to level (shorefast) FYI here if you also include value(s) for drift ice.

We refer the "level first year ice" as a high melt pond fraction (>70%) was observed on both level landfast and drift first year ice.

P3701/L19-27: The seasonal evolution of sea ice albedo and its relationship to ice topography and heat balance have been well studied, e.g. during the SHEBA study, prior the references dated 2012 and 2013 here.

Yes, the authors are aware of this, we simply referred to the papers that provide some review of the earlier studies. Two more relevant references on the original studies on the topic (Eicken et al., 2002, Perovich et al., 2005) were also added.

Data and Methods

P3702/L12: How was aerial photography used to assess the representativeness of ice thickness?

It was not. We actually referred to the unpublished EM-bird data from these flights. In order to corroborate the results, the data on ice thickness is now presented along with the melt pond coverage statistics.

P3702/L13-14: The latter stage of melt is interesting. It looks like you may be observing the point at which there is an increase in pond fraction due to flooding by seawater. It would be nice to have this expressed within the context of the expected temporal evolution of pond fraction for this ice type.

The water in the ponds was indeed salty, we however don't not speculate here about the proportion of the sea and the sea ice melt water in the ponds, same as the potential role of the sea water in melt pond evolution. Formally speaking some inference on this could be done if we took the samples of the melt pond water for salinity in parallel with the sea ice coring. Although the latter was done during the campaign, no samples of the pond water were collected.

P3703/L19-21: Describe the technique or appropriate reference instead of the software used.
P3704/L7: Again, appropriate to describe method but no need for software used.

Note that we actually refer to the original studies describing the methods. Indicating the type of the software used in numerical analysis is a common practice, we don't understand why we should refrain from following this.

P3706/L17: ": : :underestimates melt pond coverage: : :". Here and elsewhere in the paper – adopt either the symbology or full text and stick with it throughout the paper.

Done. We only left the double notation in Figure 3 for convenience of a reader who browses through images before reading the paper.

Results

The section headings for 3.1, 3.2, 3.21, and 3.2.3 should be shortened.

The paper structure was revised and the corresponding headers were shortened accordingly.

P3708, L21: Methods regarding EM-bird calibration are not needed.

Omitted in the revised version.

P3708/L25: Why have you chosen flight 2 out of the 5 pack ice flights?

Flight 2 was long, conducted in the middle of the drift and covered a large area providing a good statistics on the sought variables. This was also a reason to choose specifically this flight for control analysis using the technique of Renner et al.

P3709/L2: ": : :the results are similar: : :". Based on the authors' experience or using any supportive data, can it be said that conditions were similar as well?

The meteorological conditions were routinely measured during the entire drift (see data in Hudson et al., 2013) . The overcast conditions prevailed with a relatively weak wind during the experiment. The ice thickness from EM-Bird measurements for 5 flights presented here also supports a homogeneous ice cover in the study area.

P3709/L16-19: Sentence "The results of in situ measurements : : :". Re-write for clarity.

Modified. See the first paragraph in Section 3.2

P3710/L3-5: "This suggests : : : negatively biased." I don't see evidence of this from the boxplots in Figures 5 & 6 which show (mean values) ai > as and ai _ as, respectively.

The negative bias in the mean should be expected when dealing with the random variable sampled from a distribution skewed to the left. In former Figure 5 (now Figure 7) the effect is not as apparent as in Figure 6 (now Figure 8) where the set (i.e. image-based) mean (0.32), median (0.36) and bootstrap (0.36) albedo are substantially different.

P3710/L21-P3711/L9: Methods out of place in results and discussion section.

The paper is now reorganized, all methods moved to the respective section.

P3711/L21-23: "This suggests : : : regional-scale estimate of the surface albedo." This statement is not well justified, i.e. how does the between-flight similarity in swath-based aggregate albedo values improve their use in providing a regional estimate? Also you have purposely left out flight 6 due to a different ice cover state, but is that not part of the region? Again I would suggest the focus is placed on ice type/condition rather than region.

In the revised version we indeed put more emphasis on the ice type, yet the regional albedo with and without the MIZ is provided too. The argumentation behind the similarity in the state of ice cover and higher quality of the regional albedo estimate is fairly straightforward. The use of the bootstrap technique implicitly relies on the spatial and temporal homogeneity of the state of ice cover in the area. If this condition is not met, the regional estimate will be biased towards a value that represents (i.e. derived from) the data subset with a higher relative weight in the combined data set.

Sections 3.2.2 and 3.2.3 are well written, though better explanation/justification for 3.2.2 is needed for readers not familiar with issues of autocorrelation in spatial analysis.

We hope that restructuring the manuscript made it more comprehendible for a reader. The sections corresponding to former 3.2.2 and 3.2.3 were slightly modified but we decided to avoid any further extension of the technical details: the references already provide all necessary information on the subject. In particular Nychka's et al., (2000) manuscript would be rather easy to understand for anyone with knowledge of basic university courses on Calculus.

P3715/L1: delete "small scale features such as" and "entirely"

Done

P3715/L11-17: Are you implying that the observations in this study are unique? It would appear so based on Section 2.1. So why not mention this earlier and be more explicit?

Yes, this is what the authors actually meant to demonstrate. We moved this paragraph to Introduction, it is also emphasized throughout the text, including the title and abstract that the study focuses on thin first year ice.

P3715/L20: change "melt pond coverage and open water fraction" to "melt pond and open water fractions"

Done

P3716/L22-P3717/L14: See general comment #2 above.

We have substantially extended the list of cited literature to cover as much as possible from what is available on the topic.

P3717/L24: delete "Further"

Done

P3717/L15-16: replace “the regional albedo estimate as it was defined in our framework” with “our regional albedo estimate”

Done

Technical Corrections:
P3700/L8: ‘adequate representations’
P3701/L20-21: ‘geographical setting’; delete ‘used in the study’
P3705/L11: ‘sea-ice’ is used here, ‘sea ice’ elsewhere; be consistent
P3706/L9: delete ‘or leads’ since it is implied open water
P3710/L9: “available” not “avaialbe”
P3717/L25: “a detailed

Done

Table 1: shorten description

Done

Figure 1: figure is too small, especially the text.

Figure 1 was modified according to suggestions made. Note that the font size in the final version will depend on the figure format (one or two-column wide).

Figures 3-4: percentages are used here for melt pond fraction but not in Figures 5-6. Maps of flight tracks are redundant. What is the purpose of ‘c’ in these figures if along-track data is not discussed in the results?

All these figures are now modified; panels rearranged and flight tracks removed. The along the track data are indeed not discussed in the text in details, although these data were used to assess the autocorrelation for the block bootstrap technique. In general we believe the data presentation is necessary to demonstrate the along the track variability in f_mp and f_ow. This was also a motivation to present the two contrasting cased of flight 2 and flight 6.

Regards

Dmitry Divine

Norwegian Polar Institute

Tromsø, Norway