Authors response to the comments by Anonymous Referee #1

The manuscript describes an improved cloud detection algorithm for MERIS, developed especially for a sequential retrieval of melt pond fraction (MPF) in the summer Arctic, denoted as MECOSI. A clear improvement with respect to the previously used algorithm is demonstrated. That is, a significant progress is reported. On the other hand, the study needs to be motivated and presented more clearly.

The authors appreciate the effort of the Anonymous Referee, the positive review and constructive comments!

First of all, the cloud mask from AATSR is here used as reference and is assumed to have a 100detection algorithm for MERIS seems to be an increase in the swath width for the MPF retrievals, with respect to if the cloud masking would have solely been based on AATSR. The application of retrieved MPF is not stated. If the aim is to derive climate data, I would say that close to perfect retrievals (AATSR is assumed to give perfect cloud masking) over the smaller swath is to prefer, than significantly less accurate data over the broader swath. That is, I found the motivation to be weak, or unclear.

This certainly is a valid concern. One needs to note that not only AATSR has 512km wide swath as compared to 1150km MERIS swath, but also AATSR coverage in the polar region is limited (compared AATSR and MERIS in Fig. 9 of the manuscript). That is, MERIS does provide a better global coverage and is preferable for the presented study. The motivation behind is twofold:

1. To the knowledge of the authors, at the time of writing no climate model includes melt ponds on top of sea ice. Although field measurements of melt ponds have been performed and published since a long time, i.e. an assimilation into a climate model within a limited spatial range as the referee suggests would have been long possible, melt ponds nevertheless present a challenge for climate modeling due to unknown global spatial distribution. Although air temperature at the surface is available also over sea ice covered Arctic ocean, melt pond fraction is not linearly linked to the air temperature but also depends on the ice topography and its internal macrophysical properties as density, porosity etc. Satellite datasets of possibly global coverage help understand not only local events but spatial dynamics in general, which may eventually lead to successful inclusion of melt ponds into climate models.

2. Although most of the field campaigns and in situ measurements of the sea ice covered Arctic ocean are available during Arctic summer, the links and feedbacks between the rapidly evolving sea ice surface, the atmosphere and the underice ecosystem are not yet fully understood. The appearance of melt ponds on sea ice during melt onset causes a drastic change of its albedo and transmittance which affects the surface energy balance and facilitates lateral, top, bottom and internal sea ice melt, i.e. affects the sea ice volume. Only recently the suggestion that melt ponds during melt onset might be connected to the sea ice area during the sea ice minimum has been published (Schröder et al., 2014). In order to understand these processes, a long-term global coverage record of sea ice parameters, among others also melt pond fraction, needs to be available to the community. That is, the presented cloud screening routine and the resulting melt pond fraction dataset can be used in independent studies of sea ice processes and not only in climate models.

The corresponding explanation and motivation are added to the text.

OLCI seems to be used as motivation in the abstract, but this sensor is not discussed at all in the text.

As both sensors MERIS and OLCI are similar with OLCI being a successor of MERIS, OLCI is mentioned as means to provide a long-term melt pond fraction data record as continuation to that of MERIS. However, the presented cloud screening method has been developed specifically for MERIS sensor and the authors like to highlight that the general problem of cloud screening over snow for ENVISAT sensors, e.g. SCHIAMACHY (see e.g. Schlundt et al., 2011), has now been updated and advanced.

The corresponding explanation is added in the new version of the manuscript.

That AATSR should give a perfect cloud masking sounds to good to be true. The limitations of the AATSR cloud detection should be discussed. And presumably, the error of the AATSR retrievals should be considered, both when setting up the MERIS Bayesian scheme and when evaluating the performance of MECOSI.

Indeed, no cloud screening routine is 100% reliable. The AATSR cloud mask, its limitations and validation are presented by Istomina et al. (2010). They highlight the challenge of cloud screening validation, with in situ point measurements (e.g. lidars) being precise but giving very limited spatial coverage, and with comparisons to other cloud masks being compromised by the time difference between the satellite overflights. The comparison of the AATSR cloud mask to the lidar data has proven its robustness (95% correct cloudy/clear detections with remaining 5% of cases connected to thin clouds on a sample of ~100 scenes).

The authors agree that this has not been addressed enough in the manuscript and add the corresponding explanation into the text.

Sections 1 and 2 needs to be restructured. At least I fail to see a clear logic in these sections. The introduction should more clearly focus on motivation and goal of the study. For example, objective/goal is now formulated in the middle of Sec. 1 and at start of Sec 2. The information around line 21 on page 1 and line 17 on page 2 is very similar, that indicates that the order is not optimal.

The review of available cloud screening approaches (Sec 1.1) is nice, but causes distraction as placed now. I would suggest to reformulate the title of Sec. 2 somewhat, and then move the review to Sec. 2.

The authors are grateful for this comment and agree that the manuscript can be better structured. In the new version of the manuscript, we take special care to avoid repetitions and keep the text concise and clearly structured, the Sections 1 and 2 have been reformulated as suggested.

There is a quite heavy use of acronyms, and you assume that many are understood by everybody. Note that this includes all names of satellite sensors. Is needed to use VIS and NIR? What is SGSP? Is RMSD the same RMS? MPF is defined in the abstract, but I would say that it needs to be defined in the Introduction as well.

This problem has also been highlighted by the second referee and the authors agree that the usage of the acronyms has to be reconsidered. In the new version of the manuscript, we define MPF also in the abstract, and take care to spell out all the remaining acronyms. VIS and NIR are removed.

Minor comments:

Page 4, line 18: "R11/R10<0.27" This needs further explanation.

This is a manually derived threshold which stems from the visual analysis of several dozen of MERIS scenes and was described and used in Zege et al 2015 (Eq 17 therein). The corresponding explanation and reference are added into the new version of the manuscript.

Page 4, line 19: Writing "small fraction" is misleading as cloud systems in the Arctic typically are very shallow. In fact, are not low clouds a special problem for using oxygen A-band in this way? Probably what you mean on page 7, line 11, but this requires a more careful discussion/analysis.

Of course, what is meant here is “short path length” and not “small fraction”. This has been corrected in the new version text.

Page 5: Add information about resolution of AATSR.

The text “The spatial resolution of AATSR is 1km at nadir.” is added on Page 5 Line 30

Page 6, line 5: What is the maximum distance of mismatch in position. That is, what is the maximum nearest neighbour interpolation?

The mentioned here regridding has been done with the python package pyresample. The radius of influence for the nearest neighbour interpolation is 1.5km. This value has been added to the text.

Page 6, line 6: This sentence needs further explanation.

The following text has been added as explanation: “As the AATSR and MERIS data have different spatial resolution, the two datasets have been gridded to a single grid (the coarser grid of MERIS). This might have affected the pixels at the borders of clouds in a way that earlier fully covered pixels now become partly covered which the binary AATSR cloud mask cannot fully reflect. Therefore we exclude the 2 pixel border from the study.”

Start of Sec 3.3.1: Seems to be quite some repetition from Sec 2.1. Can be avoided.

Indeed, the authors agree that the beginning of Sec. 3.3.1 has already been mentioned in Sec 2.1The sections 2.1 and 3.3.1 have been now restructured correspondingly.

Page 9, line 10: The equation below defines b as a mean, not an integrated value.

For the sake of clarity, the sentence at Page 9 line 10 has been changed correspondingly:

“The brightness b is a spectral integral over the reflectance. As the spectral resolution of the sensor is quite coarse with only 13 used channels, the brightness can be represented by the following equation:”

Page 9, line 14: I don’t understand what "l = [1, 14] {11" means.

The authors meant “in ascending order from 1 to 14 except for 11”. As the same is basically said in words in the corresponding sentence, this equation is obsolete and for the sake of clarity is removed in the new version of the manuscript.

First paragraph of Sec 3.4: This needs further/better explanation.

The first paragraph of Sec. 3.4 has been rewritten, with the following text as a substitute:

“The cloud probabilities for each given set of features (Section 3.2) were compiled into binary masks in order to compare the results to the binary AATSR masks. The masks are created by normalizing the cloud probability P(**F**,C) to the range [0,1] and splitting the dataset at a probability threshold 0.45 to introduce binary values. An operation of morphological closing and opening was then applied to the cloud and snow/ice pixels in order to remove single pixels.”