

Interactive comment on “The EUMETSAT sea ice climate record” by R. T. Tonboe et al.

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General comments: The content of this publication can be considered very important contribution to the creation of consistent several decades span of sea ice cover data sets. This article reads like it was written by several different people. Up to the section 2.1 the writing style and terminology used do not explain clearly the data sets properties used in this project. The list of recommendations below will improve the readability of these sections. Starting with section 2.1 the writing style and the clarity greatly improves and very few minor clarifications are needed. But, very important section 3.3 Ice chart and ESICR comparison discussion is not easy to read. The comparison should be better organized. The authors should spend some time on deciding what will be the primary use of these long term data sets. The spatial and temporal resolutions, as well as the accuracy of retrievals are usually driven by these applications. The criteria for comparisons with existing data sets and/or ice charts are also dictated by who will

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be using this new and improved sea ice cover information. Algorithms designed for processing spaceborne measurements are developed for specific applications. Some of these algorithms are very simple and set up to provide yes/no indicators to mask ice covered (>threshold value) areas. Some other algorithms, especially to be used in near-real time for navigational support, are more complex and are region and season specific. The regional and seasonal variability of sea ice cover can be partially compensated by using seasonal, regional algorithm parameters and sea ice signatures. These parameters can be derived using RTM. The complexity of the microwave radiation from the sea ice cover must be accommodated. The authors do state that understanding what contributes to the emission from the ocean surface (e.g., snow free ice floes, ice flows covered heavy wet snow, young saline ice, etc..etc.,) is very important but do not carry out their evaluations region by region. The “new” hybrid algorithm should have region specific “flagging” to allow global processing. The authors should dedicate more discussion time in planning their future work to regional seasonal variability of the ice cover and how that impacts microwave and optical brightness temperatures. The authors have selected a hybrid algorithms that uses fixed coefficients multiplied by observed brightness temperatures. It is not clear how using NWP is incorporated into calculations of these coefficients or in computations of dynamic tie-points. All throughout descriptions of different algorithms the authors discuss items they call “uncertainties”. It is not clear what this particular quantity represents. Is it a statistical value or derived value related to variability (or accuracy) in measured brightness temperatures or other measured (or derived) parameters? In a real world there are very few instruments that can claim to have accuracy of less than 2%. The sea ice cover data sets generated using MODIS and/or SAR provide higher spatial resolution data but the authors should not forget that the algorithms used to generated ice cover imagery may contain sensor/processing algorithm specific errors, e.g., ocean surface roughness effects on SAR imagery; cloud cover on MODIS The authors have acknowledged the need for the algorithms to accommodate pack ice and marginal ice regions. It is not clear if the hybrid algorithm will be tested for these diverse ice cover regions.

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It is also not clear how different coefficients used in this new algorithm can be derived using physical properties of different types of ice and weather conditions. The ice cover in marginal ice zones can change very quickly depending on the wind (e.g., read about icebreakers trapped in Barents ice; ships stuck in Weddell sea) and in some parts of the world the extent of the ice cover is dictated by the thermal currents (e.g., sea ice cover in Labrador and Newfoundland waters). The authors have discussed “smearing” of ice cover location but did not address “smearing” effects of orbital data gridding onto a fixed grid. These effects can be determined by comparing retrievals from ascending and descending satellite overpasses looking at ice edge that was not changed (in 10 to 12 hours between satellite passes) by prevailing winds. The authors should have more systematic plan for testing their chosen algorithm. Signature areas of predominant sea ice type cover (e.g., Sea of Okhotsk for seasonal ice cover; old ice covered areas; mid-winter Labrador Sea ice cover) are ideal for testing algorithms. .

. Section specific comments: In the abstract 1. Replace: American by NASA, NOAA, or NSIDC depending which agency was responsible for the satellite sensor, satellite launch, and satellite data processing Sea ice area by sea ice covered area 2. Sentence starting on line 16: “The methodology.and ending on line 21 is not clear 3. Are the authors saying that in preparation of this sea ice dataset : 1) NWP and RTM were used to reduce impact of the atmospheric conditions on the measured brightness temperatures; 2) to reduce the inter-sensor comparison bias dynamic tie points were used in retrieval algorithms, and 3) hybrid algorithm was created using Bristol and Bootstrap algorithms. This algorithm was used in estimation of the spatial and temporal variabilities in sea ice concentration retrieval accuracy. 4. Who generated “sea ice charts from the Arctic and the Antarctic” (lines 22 and 23) 5. What are the “intermediate ice concentration” 6. Line 32: see comment #1 7. Pg. 2, line 1: by “Here the sea. . .” The authors are saying In publications referenced above the sea ice. . . . 8. Pg. 2 line 8: replace “atmospheric parameters” by weather conditions such as wind generated ocean surface roughness and cloud cover 9. What is “structural uncertainties” 10. Line 15 Define “noise” 11. Why “climatic trends” are “artificial trends” ??? 12. Be more

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specific in “for the properties that we are able to quantify,????? 13. Please explain what are the “dynamical tie-points” 14. “residual uncertainties” for which parameters and please, define uncertainty is it - accuracy.

In section 1.1 Line 14 after “84°.” Should be replaced by: “SMMR data were acquired every second day” In Section 1.2 “The SSMIS data (used in this project?) are from the L2B near real time data-stream” received from??? In section 1.3: Please clarify your statement on lines 7 and 8. Was sea ice coverage included in NWP? If yes, where did that information come from? In section 1.4 line 15 insert “The coarse resolution of the passive microwave brightness. . .” Line 22 and line 24 “Brightness above 220 (units?) 60 (units?) “We use 6 hourly data at a resolution of 1.25 degrees.” Why this temporal and spatial resolution was used?

Section 1.4 Line 15” add passive microwave in front of “brightness temperature”

Section 1.5 What does it mean by: “The operational sea ice charts from the National Ice Center (NIC) are a relatively independent . . .” this contradicts what you write in the text below these lines. See also in section 3.3 “The NIC ice charts are produced manually on the basis of satellite and reconnaissance data” Which satellite “pixel grid” are you referring to? Section 2.1 The title of this section is misleading. Microwave (and thermal) emission received by satellite sensors is attenuated by the atmosphere. In addition, emission from the atmosphere contributes to the radiation measured by the spaceborne sensors. Over the old sea ice and open water this contribution could be significant in comparison with the radiation from the sea surface. Line 26 What is “wind shear” ? Liquid water has much greater contribution to the measured passive microwave brightness temperatures than water vapour. For the sake of clarity: Section 2.2 should be placed before section 2.1 Section 2.2 Brightness temperatures for what type of ice are selected to be considered for a tie-point ? Section 2.3 How are the tie-point brightness temperatures are used to derive parameters in the Bootstrap and Bristol algorithms? Section 2.5 It is not clear what the authors call “uncertainty”. Is it a quantity representing statistical variabilities or derived quantity deviation from an

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accepted value? “ice concentration uncertainty of 1.4 % for the Bristol algorithm, and 1.7 % for the Bootstrap algorithm in frequency mode” compared to what? At what spatial and temporal resolutions?

Is this quantity relevant when discussing temporal and spatial variability and trends?

2.14 Level 4: Gap filling by spatial and temporal interpolation

This section contains interesting description of filling in missing data. The gap filling is required if the proposed data set to contain high temporal resolution sea ice information. If the goal is to have data sets for climatology analysis, is it necessary to go through these additional computations? In sections below, the authors discuss the hemispheric comparisons. Did they actually filled temporal and spatial gaps in their data sets to carry out this analysis?

Section 3.1 Where did the sea ice climate data records come from? The authors claim that the discrepancy between the ice cover values they derived and NIC ice charts are due to the “atmospheric noise”. They could have taken a look at historical weather or AVHRR/MODIS cloud imagery to confirm or deny that is the main cause for the differences. The increase in the discrepancy for the spring/summer seasons are very likely due to the algorithms do not accommodate seasonal variability in the emitting layer, e.g., melting snow, melt pond, change in the ice salinity. Statistics does not compensate for the physics of microwave radiation from different surfaces and propagation through the atmosphere.

Part of the section 3.1 is dedicated to discussion of differences between NIC ice charts and ESICR data sets. Why then there is a separate section “3.3 Ice chart and ESICR comparison discussion”? The last paragraph in section 3.3 should be placed into the conclusion and summary section

Section 3.4 Do the hemispheric sea ice cover and open trends described using ESIGR generated data sets differ significantly from those observed by using Bootstrap or

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Bristol algorithms?

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/tc-2016-34/tc-2016-34-RC1-supplement.pdf>

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-34, 2016.

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