

Interactive comment on “Mapping and Assessing Variability in the Antarctic Marginal Ice Zone, the Pack Ice and Coastal Polynyas” by Julienne C. Stroeve et al.

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Received and published: 29 March 2016

General comments

Julienne Stroeve and her co-authors compare the results of two popular algorithms using passive microwave satellite data to classify ice types in the Antarctic sea ice zone, the NASA Team and the Bootstrap. They show that sea ice extent estimates are largely consistent between the two algorithms. They differ, however, in the proportion of consolidated pack-ice versus marginal ice zone (MIZ) and polynyas, with greatest discrepancies in the contribution of the MIZ. When applied to biological datasets, in this case the breeding success of snow petrels *Pagodroma nivea*, however, these discrepancies can lead to opposite conclusions. This manuscript presents a highly desirable

critical cross-validation of satellite-derived data. In particular, the inclusion of a biological dataset adds high value to both physical and biological communities, highlighting the importance of such exercises in this under-studied cross section of disciplines. Furthermore, it was a pleasure reading this manuscript, since it manages to present a complex topic in an easily understandable language, even to non-physicists. Having said this, there are a few things that should be improved: 1) The results and discussion section are not well separated. To preserve the excellent flow of the manuscript, I recommend to merge them into a “Results and Discussion” section. If this is not possible, speculations and literature references should be consequently moved from the results section to the discussion. 2) In my view, the greatest weakness of the approach chosen is the definition of the ‘polynia’ ice type. Just using a proportional ice coverage of 0.8 near the coast as a criterion may incur confusion with other more open ice situations. It may well be that the two algorithms ‘see’ completely different things, and therefore result in a large difference in the seasonal occurrence of ‘polynias’. 3) There is little information provided on the statistics of GLMs looking at the breeding success of snow petrels. A convincing statistical approach, including model selection, is the fundament of any conclusions concerning the seemingly opposite outcomes of the two algorithms.

Specific comments

Introduction LI 44-66: When addressing regional variability, the strong decline in the WAP should be mentioned with respect to the final objective to discuss ecological implications. Also, previous declines in Antarctic SIE should be mentioned. For example, Flores et al. (2012) note that "This growth, however, has so far not compensated for a decline of the average sea ice coverage between 1973 and 1977, which accounted for $\sim 2 \times 10^6$ km² (Cavalieri et al. 2003, Parkinson 2004). Reconstructions of the position of the ice edge in the pre-satellite era give strong evidence that the overall areal sea ice coverage in the Southern Ocean declined considerably during the second half of the 20th century (Turner et al. 2009a)".

M&M LI151-152: Could be problematic with respect to ecological interpretations, be-

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cause it also includes areas with loser pack ice and wakes behind icebergs that are not polynyas and thus do not necessarily feature the same biological dynamics. Also: What is "near" the coast precisely?

Results LI 413-526: More details on the model statistics are needed. Were the slope terms and intercepts significant? Using differences in AIC alone for model selection can be tricky. I recommend testing the 'better' model against the 'next worse' using ANOVA or F statistics, depending on the model applied. It would be useful to see the model validation plots in the supplementary material. To me it is not clear why the model using pack ice and BT is preferred.

Discussion L619: Another way to validate algorithms would be the ASPEcT ship observation data

Conclusions LI643-646: I have the feeling that the results of the polynia estimation are blurry in both algorithms, resulting in this large variability of timing of polynya maxima. This may be due to inaccurate definition of polynyas in the analysis.

Technical corrections

Abstract L14: replace "biological" with "biologically"

Introduction LI 40-43: Split this sentence in two. L59: I believe it should say "maxima"
L94: Did you mean "continuously"? L114: replace "mattes" by "matters"

Results L197: Better say "Results and discussion"? L215: delete "(e.g. the shading)"
L373: replace "significantly" by "significant L375; delete "(e.g. spring)" Figure 5: What is the variability measure here indicated by the shaded areas around the curves?

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-26, 2016.

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