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Discussions

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Interactive comment on "Abrupt transitions in Arctic open water area" *by* M. A. Goldstein et al.

Anonymous Referee #1

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Review of "Abrupt transitions in Arctic open water area" by Goldstein et al.

Summary

This paper presents an analysis of sea ice extent derived from satellite passive microwave data and other sources to investigate changes in open water area in the Arctic. Transitions found in 1988 and 2007, particularly in the Pacific sector. The results suggest that there may be critical thresholds in the Arctic system where a return to a previous state is unlikely.

General comment

Overall, this is an interesting paper and addresses an important issue. The analysis is solid and the paper is well written. I think my main concerns are with the source data. For example, overall, the quality of the passive microwave data is good and there has been considerable effort for the record to be consistent over time. However, some

inconsistencies may remain, particularly regionally. Eisenman et al., The Cryosphere, (2014), for example found potential inconsistencies; these were primarily in the Antarctic, but in supplemental material, potential Arctic inconsistencies are also found.

One thing that is a particular concern is the periods used for the "break" points. How were these chosen? Were they ad hoc, or derived from the behavior of the data. Based on the text, they seem ad hoc. If so, that seems arbitrary. If not, then this should be made clearer in the text when the periods are first discussed (pg. 4, line 19). Also, I wonder about the periods being of different length – 9 years, 17 years, and then 7 years – this also has an ad hoc feel to it.

Getting back to the satellite transitions, these periods are a particular concern because they seem to at least roughly coincide with satellite transitions, particularly the 1988 transition, which roughly corresponds with the switch in sensors from SMMR to the SSMI series. SMMR and SSMI are different sensors, with different channels, so there are some distinct differences in the sensors that may affect sea ice concentrations – e.g., sensor resolution (sensor footprint), frequencies, etc. The algorithms are adjusted to try to account for these differences, but it's not possible to do so exactly. This seems particularly apparent in Figure 3, where the SMMR record for the Pacific section show much lower variability than the SSMI and SSMIS era. I would guess that this is likely an artifact of the different sensors. There was also a sensor transition in 2008, between SSMI and SSMIS, though the differences between the two are much smaller.

The authors do a good job in not simply relying on the passive microwave record, but also looking at the Hadley sea ice and the NIC ice charts. However, both Hadley and the NIC charts rely on passive microwave data as source. Hadley uses passive microwave for the period after 1978. And NIC charts were reliant on passive microwave for a lot of their information from 1978 until the launch of Radarsat in 1995.

I suspect that these issues do not invalidate the results but they may well have some effect. I think these issues should be addressed more in the manuscript.

I think this manuscript is acceptable for publication after minor revisions to address the comments here.

Other comments (by page and line number):

2, 24-25: Note that the CDR simply combines the NASA Team and Bootstrap estimates, so the different products are not completely independent.

2, 27: The "merged" product uses data that underwent manual quality control over its entire record, 1978-2014, not just the period prior to 1988.

2, 30-31: It is true that PM products do not detect ice well at low concentrations, but the 15% concentration threshold is also selected based on the relatively low spatial resolution of the sensors, which makes any ice edge somewhat "fuzzy". I believe the 15% threshold was originally defined because it was found to best match the "true" ice edge during validation studies.

3, 10: I think HadISST2 uses OSI-SAF concentrations since 1979, at least according to Titchner et al., 2014.

3, 13: It's worth noting that Radarsat data started in 1995. This represents a substantial change in NIC's chart. Before then, they used a lot of PM data; after, it was much less. Also it would be good to include a journal reference for the NIC charts, e.g., Dedrick et al., Can. J. Rem. Sens., 2001, doi:10.1080/07038992.2001.10854887.

4, 4: Not sure what you mean by "rasterized"? The source data are already on a grid. Maybe rephrase this sentence.

5, 23: The HadISST does use NIC charts, but only as a bias correction.

5, 25: I think the resolution likely accounts for a lot of the differences between the 100 km HadISST and the 25 km PM records.

7, 15: It would seem more relevant to use the same period as the PM record, i.e., 1979-1988 instead of 1953-1988, etc. Would that potentially affect the analysis?

7, 25: Adding the Eisenman reference (above) somewhere here would be useful

7, 26: What is meant by "errors in the data stream in the first months of the SSMI instrument"? Do you mean the first SSMI, launched in 1987, on DMSP F8? I'm not aware of such errors. The Harvey reference mentions recent errors in 2016, but these don't seem relevant to analysis at hand.

7, 33: The authors say Week 37 is used for ice age. Is this consistent past the sea ice "birthday" – i.e., the week when the ice is aged another year older. The week of this transition varies, depending on when the minimum occurs (I think it generally varies between week 35 and 38). It is important to be consistent throughout the record – either all years before the "birthday" or all after. If you just pick a week in September, you may capture a mixture of years before and after the birthday, which will lead to inconsistencies.

8, 4: The age algorithm doesn't so much "favor" the oldest ice as it effectively records only the oldest ice in a given cell.

Figure 2: The caption says the units are % open water, but the y-axis is 0-0.8. I assume the y-axis is actually sea ice fraction (unit-less, 0-1 range)?

Figure 3: Same comment as for Figure 2. Both figures would also benefit from a bigger font for the axis numbers and legend.

References: Just a style comment and this may be a formatting standards issue, but it would be helpful to indent the Reference list. It's very hard to pick out individual references.

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Discussion paper