

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

Anonymous Referee #2

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I see on the discussion site that an anonymous review for this paper was submitted on June 21, and the authors submitted a response on July 8. I have NOT read either the review or the response, and therefore the following review is not influenced by either one.

This work uses monthly-averaged passive microwave sea-ice concentration data to examine the area of open water in two broad regions of the Arctic – the Pacific sector and the Atlantic sector – for the period 1979-2014. The researchers find that the time series of open water area in September in the Pacific sector undergoes a significant shift in its mean value in 1988 and again in 2007. The Atlantic sector shows shifts in the same years, though weaker. The researchers call into question the idea of fitting linear trend lines to time series of sea ice or open water.

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I share the researchers' contention that linear trend lines are not necessarily the best way to fit time series of sea ice or open water, and in fact I have looked at alternative curve-fitting options myself, including breakpoints, so I am sympathetic to this basic point.

Unfortunately this paper suffers from multiple fatal shortcomings. We are not told how the breakpoint years (1988 and 2007) are identified in the data. The descriptions of the regression models are impossible to follow, and no equations are given. The analysis appears to compare regression models with different numbers of free parameters, and therefore it's not clear whether the better fit is simply due to more degrees of freedom. The work analyzes open water area, which is just the additive inverse of sea-ice area, which has been extensively studied using the same data in the same geographical locations – so why don't we see breakpoints in the time series of sea-ice area? (e.g. see the figures in Parkinson and Cavalieri, *J. Geophys. Res.*, 113, doi:10.1029/2007JC004558, 2008); some discussion is warranted. The self-organizing maps and their application are not well explained. There are many odd sentences in the paper.

Main Comments

We are not told how the breakpoint years are identified. Page 5 line 32 says, "There is an ostensible breakpoint at 1988..." and page 6 line 2 says, "A second breakpoint can be identified in 2007..." How? This is a key part of the analysis, but we are left completely in the dark.

The description of the regression model (page 6 line 5) says, "A model comprised of a constant, a trend variable, and a variable with the three means for each period..." What does this mean? A simple equation would probably clarify everything, but there are none. How many free parameters does this model have?

Page 4, lines 22-30. This is the paragraph about the self-organizing map analysis of sea level pressure. Lines 23-24: "daily sea level pressure anomalies (that is, summer monthly differences...)" – this is really confusing. Also, why are the maps arranged in

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a 5 x 4 array, as opposed to (say) a 4 x 3 array or a 6 x 5 array? What do the rows and columns of the array represent? Page 8 line 21, how is Node (3,1) constructed, for example?

Page 6, lines 5-10. Here different regression models are compared, with one having a higher "adjusted R^2 " than the other, but it's not clear whether the model with the better fit simply has more free parameters. Page 6 lines 12-34, impossible to figure out what's going on without equations to help.

Minor Comments (in page order)

Page 1, lines 23-24. This is an extremely strange sentence, the meaning of which is unclear.

Page 1, lines 30-31. Krupnik & Jolly, AMAP, and Liu & Kronback are not listed in the References. Page 5 line 14, Meier 2005 is not listed in the References.

Page 2, line 15. What does "artifacts" refer to here?

Page 3, lines 23-25. A map of the regions would be helpful.

Page 4, line 3. IABP needs a reference.

Page 5, line 2. "The open water fraction area..." Which one, fraction or area?

Page 5, line 30. I know that R^2 is the squared correlation of the fit, but what is the "adjusted" R^2 ? Page 7 line 16: "had a negative adjusted R^2 " – strange that squared correlation can be negative; what sort of adjustment is done to R^2 , and why?

Page 7, line 2. What does it mean for a time series to be "temporally uniform"?

Page 7, lines 26-27. Concerning possible errors in the SSM/I data, the authors cite personal communication and an article in the Washington Post. Aren't the errors actually documented somewhere?

Page 8, lines 1-2. Regarding the ice age anomalies in the first 6 years of the record,

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"this is likely an artifact of the data product's initialization" – isn't this documented somewhere?

Page 8, line 33. "A long-term, high quality and temporally consistent record of Arctic open water remains an elusive goal." This is a strange sentence that needs further discussion.

Page 9, lines 9-10. "the processes associated with these shifts can only be tested independently using a modeling approach" – hasn't this (modeling approach) been done before?

Figure 1. The scale bar is too small to read.

Figure 2. The caption says "open water (%)" but the vertical scale runs from 0 to 0.8, suggesting that it is fraction rather than percent. The legends are too small to read. Why is some data plotted as lines and other data plotted as points? The colors are difficult to distinguish.

Figure 3. The axis labels are too small to read easily. The vertical axis is labelled "Open Water Area [%]" but the vertical scale runs from 0 to 0.6, suggesting that it is fraction rather than percent.

Figure 5. I can't figure out what the message is here. What is the reader supposed to notice? The colors are difficult to distinguish.

Conclusion

It is impossible to follow the analysis in this work, and I have low confidence that it is done correctly. There should be some discussion of why breakpoints are found for open water area but other researchers have not found breakpoints for sea-ice area. Parts of the Introduction and Discussion appear to be irrelevant, speculative, or just plain strange. I must emphatically recommend that this paper be rejected.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-108, 2016.