Ocean Sci. Discuss., 12, C144–C148, 2015
www.ocean-sci-discuss.net/12/C144/2015/

Ocean Science
Discussions
Open Access

Interactive comment on "Atmosphere–ocean interactions in the Greenland Sea during solar cycles 23–24, 2002–2011" *by* P. E. Binns

Anonymous Referee #3

Received and published: 5 May 2015

This submission concerns sea-surface temperature (SST) variability in the Greenland Sea. There is significance in the topic, for example its relation to convection and the meridional overturning circulation. The variability is discussed from the viewpoint of the different water masses, but also possible linkages with atmospheric forcing (cyclonic activity detailed from satellite images etc. and possibly represented by variable pressure and/or NAO) and ultimately to solar forcing and the sunspot cycle, which had a minimum in 2008.

Some of the linkages are plausible or better. 1) That cyclonic activity causes variability in SST is well known. Indeed, cases of summer storms mixing down the seasonal thermocline and so cooling SST are well documented. [A shallow summer thermocline with large vertical temperature difference might be a candidate for showing large variability

in these circumstances; this could be a reason for the "precursor peak" discussed in section 5.2.1.] 2) Weather systems correlate with NAO and evidently cause variability in atmospheric pressure. The latter is a matter of dynamics and (although I am not expert) I am fairly sure the former is true. 3) There are papers (as cited) on relations between the atmosphere and the solar cycle. I cannot comment on these as it is not my field, but I am happy enough to accept that.

However, I have severe problems with this submission:

a) What exactly is the new message? Three points are made in the abstract, possibly the clearest statement of the author's intent. (i) A statistically significant difference in the day-to-day variability of the sea surface temperature field between the period of lowest solar activity and the remaining periods. I think the statistics on this may be OK owing to the $\sim$3400 realisations of day-to-day variability, but the difference in variability is actually rather small on average. (ii) During the transition from summer to winter, systematic, inter-annual changes in the day-to-day variability of the sea surface temperature field. (iii) Forms of the late summer temperature fields exhibit symmetry about the years of lowest solar activity. With only a few years evidence, statistical confidence in (ii) and (iii) must be weak.

b) There is a lot of discussion of correlations between different features; this rather obscures the author's intended message with a lot of detail, the relevance of which is not made clear when presented.

c) There is much apparently arbitrary selectivity. [The prime example is the chosen area of the Greenland Sea.] Again this may be a matter of the relevance not being made clear. However, for statistical confidence estimates, only random selection is allowed, and selection on the basis of a good fit or correlation to make a case is definitely not allowed!

d) Some apparent arbitrariness may be the result of inconsistencies of which many are apparent in the detailed comments below.

Detailed comments on science.

Section 2.1 Page 108 line 7. Another recent paper is “Recirculation in the Fram Strait and transports of water in and north of the Fram Strait derived from CTD data”, M. Marnela, B. Rudels, M.-N. Houssais, A. Beszczynska-Möller, and P. B. Eriksson. Ocean Sci., 9, 499-519, 2013

Section 4.1, page 109 line 22. “exceptional” is distracting. I think it means a greater fall in temperature from summer to winter than in the other sub-areas. But not by much; figure 3 should be referred to.

Section 5.1. Page 111 lines 23-25. This sentence is not strictly “Furthermore” as it is more-or-less a consequence of the previous sentence. What both sentences and the figure 5b caption ignore, is that from 2002 to 2005 sunspot activity decreases while variability increases, i.e. the opposite trend. The ten tests might have been a fairer comparison or more convincing if the numbers had been unequal in about the same proportion as SSN<10 to SSN>10. However, those tests show fairly convincingly that the inequality of means between SSN<10 and SSN>10 is significant, albeit rather small.

Section 5.2.1. Page 112 line 17. The figure in the supplement shows that this “precursor peak” in 2003 is in (early) September. Also the timing of the “Months forming the precursor” in the supplement figure vary greatly; the end time is the beginning of November in 2003 and the beginning of August in 2005, i.e. a whole season different. Is like being compared with like?

Sections 5.2.1 and 5.2.2. The statistical significance of statements based on only 3 years with small SSN versus 6 with larger SSN cannot be very strong, even assuming that the years are independent.

Section 5.2.2. Since the warmest month is July in all cases in figure 7b, the timing of “tracking” by variability is entirely dependent on the trend in variability. Except perhaps

for 2007, reference to the Supplement figure 7 is hardly supportive of the September “step up” in variability. The detail shows a lot of week-to-week changes in variability; these changes introduce uncertainty to monthly values.

Section 6.1, page 114 line 5. See comment on Supplement pages 12, 13 about R = 42%. Is 18% variance reduction really $p < 0.01$? Page 114 lines 7-8. But why take the winter months? None of the argument so far has related to winter. And what period does this apply to? Statistical confidence estimates do not allow one to make selections of the data that give a best fit. If you want to present the DJF figure you must show the other seasons as well and find the probability that one of the four has a correlation coefficient as large as 54%. [In any case 54% or $\sim$ 29% variance reduction is not so great even if taken over the whole period with 27 realisations: 9 years x {D, J, F}].

Section 6.2 page 114 lines 14-15. The text here says July-February in each case but figure 9 says June to May which if true does not relate closely to summer-winter transitions. Page 115 lines 3-5. December 2008 – February 2009 may indeed have been the deepest part of the solar low but another obvious reason for low variability (despite cyclones) is that all the sea was cold so there was little spatial variability of temperature as a basis for temporal variability.

Figure 8a. 2008 is far removed from 2007 and 2009 which undermines the whole argument.

Supplement pages 12, 13. The period marked as “solar low” on the time-series plot on page 12 does not correspond with that of either of the “scatter” plots. For statistical confidence estimates it is essential to choose the period a priori and not adapt it to improve correlations. In any case R = 42% for the 29 values July 2007 to November 2009 is not very strong; this means a reduction in “unexplained” variance of only R**2 or 18%.

Technical corrections

Section 2.1 Reference is needed again to figure 1, especially for the area covered (not stated in main text despite the detail – page 106 lines 8-10 – about grid areas when calculating the mean). Page 106 line 13. What about the data from June to September 2011? Page 108 line 26. I think it must be the very end of October at 74N.

Section 6.3 page 116. Line 4. "This indicates (how?) an association (of what?) with variations in wind-driven sea ice." Please clarify the meaning of this sentence. Lines 7-9. Sentence "Both . . export." When is this supposed to apply?

Section 7. Page 117 line 24. Where is the "decrease in temperature" supposed to apply?

Figure 2 and others later. The cluster areas shown do not correspond with the area covered as outlined in figure 1. Figures 8a and 8b are inconsistent. There are more colours for spots in 8b (several not shown in 8a).

Figure 10 caption says "northwestwards" but to my eye the cyclone has moved somewhat to the northeast in the lower image.

Supplement. For the year's analysis, entries for the number of clusters and number of transitions (by month) are missing (c.f. the January analyses).

Supplement pages 7, 8. Why no colour coding for some clusters within the "symmetry years": clusters 2, 17, 15, 23, 10, 14, 6, 19?

Supplement page 15. The spreadsheet has 7 or 8 pairs of dates shaded but only 5 triplets of images on which the resolution is insufficient to see which dates they represent.

Supplement page 16. It is not stated what the entries mean; especially the meaning of the numbers in red and negative entries is unclear.

Interactive comment on Ocean Sci. Discuss., 12, 103, 2015.