Ocean Sci. Discuss., 12, C60–C62, 2015
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Ocean Science Discussions
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Interactive comment on "Atmosphere–ocean interactions in the Greenland Sea during solar cycles 23–24, 2002–2011" *by* P. E. Binns

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Received and published: 26 March 2015

I have not completed a full review of this paper because although the techniques used are interesting, I cannot see that it presents any convincing evidence for systematic or significant changes of the "variability" (see below) that requires an explanation, nor does it provide any clear evidence (or any prior reason to suppose) that such changes might be associated with solar activity, rather than just arising from decadal variability. Detailed comments of the methodology and text would therefore be out of place. The use of cluster analysis to investigate spatial patterns and possible changes thereof is an interesting alternative (or complement) to the more common use in oceanography and meteorology of of EOFs (Empirical Orthogonal Functions); Cheng & Wallace (1991) provide an interesting account of their relative advantages & disadvantages.

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It relies on some measure of the difference between spatial patterns (the root-mean-square point-wise deviation of temperature in this case, I think), that can be further analysed in various ways, including as here assessing the magnitude of changes over time ("variability"), and grouping and clustering of similar patterns. However, I do not find the argument that there are changes of pattern that are inconsistent with natural variability to be persuasive, for the following reasons 1) The changes of area-mean sea-surface temperature in Fig 3, which the author describes in section 3.3, and claims are both systematic and associated with solar activity, have not been convincingly shown to depart from random fluctuations 2) The physical significance of the overall point-wise temperature deviation ("variability") that is used as the basis for the investigation (and the cluster analysis), is not explained, and the claimed association with solar activity (Fig 5b) is not convincingly demonstrated. Visually there do appear (to me) to be 4 or 5 stronger annual cycles of "variability" during 2005 to 2009, with a decrease of the mean from 2006 to 2008, but these changes have not been shown to depart from chance, and in any case they precede the solar minimum of 2008/9. 3) The author asserts (section 5) that "three observations indicate a relationship between the level of solar activity and the day-to-day variability of the SST field". These observations are stated to be a) "First: the lowest variability coincides with the period of lowest solar activity. b) Second: the timing and character of the changes in variability during the transition from summer to winter varies with the level of solar activity c) Third: the forms of the late summer SST fields during the period of lowest solar activity are unique to this period, whereas those in the years before and after are similar, forming an apparent symmetry about the low solar years"

Regarding these; a) a running (e.g. annual) mean of "variability" is not given, but by eye it would probably fluctuate little except for an increase followed by a decrease in the period just preceding the solar minimum (see comment 2 above): no formal correlation analysis is given to support the assertion: the Kolmogorov-Smirnov plot & statistic (Fig 6b and in the Supplementary material) show that if there is a change during the solar minimum it is small (around 10% of the mean level): this may be (just) statistically

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significant, but is it of any practical significance (and if so, what is it) ?; b) the evidence for these changes is unconvincing to me, and their practical/physical significance is not clear; c) the cluster analysis (Fig 8) does appear to show that the patterns are somewhat different during 2007-2009 compared to before or after this period: whether or not this is an artefact of the distance value chosen to discriminate clusters or a chance occurrence is not discussed: whether or not it has anything to do with the solar minimum with which it coincides is speculation. In any case an association based (as here) on a single exceptional cycle of change would not be very persuasive, in the absence of a clearly articulated physical mechanism.

It is arguable that this paper presents some circumstantial evidence that leads to a hypothesis, i.e. that temperature patterns in the area may be systematically different during solar minima. Such a hypothesis could conceivably be tested using data from earlier (or future) solar minima: it cannot be tested using the data that were used to construct it. Even if a convincing correlation should be established, that would of course not imply causation. Some exploration of any plausible mechanisms by which such a linkage could arise would therefore be desirable to support any suggestion that this is more than just a chance coincidence.

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Reference Cheng, X. and J. Wallace (1991). "Cluster Analysis of the Northern Hemisphere Wintertime 500-hPa Height Field: Spatial Patterns." J Atmos Sci 50(16): 2674-2696.

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