

Comments from reviewer on the article (discussion paper of os-2013-57.):

Possible signals of poleward surface ocean heat transport, of Arctic basal ice melt, and of the twentieth century solar maximum in the 1904-2012 Isle of Man daily timeseries

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1. *Presentation quality:* The title of the paper gives me associations and expectation about what kind of topic the paper is going to analyze: An analysis of the (causal) relation between the poleward heat transport and melting of the Arctic ice and that the solar maximum influences on the heat and melting process. According to the title and introduction, the paper is empirically oriented and uses data/time series to analyze the topic. It is an interesting paper full of information. However, in my opinion, the authors should in the intro of the paper make clear what they are going to show/analyze. In my opinion readers prefer a clear presentation of the problem in first part of the introduction.
2. *Scientific quality:* The paper shows that the authors have a lot of detailed information about the dynamics in the sea and atmosphere (locally and globally) and also knowledge about the interaction between these mediums. These detailed information and descriptions are indeed interesting and valuable, but I have a couple of critical remarks to the approach:
 - a. My impression is that they combine theory and observation on local, regional and global level to support theirs arguments. That's fine. However, methodologically this is not strictly proofs, just indications, because they do not say anything about (potential) observations which could indicate nothing or even the opposite. The authors can partly present general propositions and partly find single observations which support the general propositions. I think it is important to make clear in the paper what kind of scientific method is applied in the paper to "prove" (or falsify) the scientific objectives. I also read they the authors use the word "possible signals" in the title of the paper.
3. As already mentioned the paper is based on interesting observational data and even though the authors do not clarify which method they use, is my impression that this is basically a descriptive analysis. I suggest that the authors include information about their methodological approach to the problem in the intro of the paper.
4. *Scientific Significance:* Due to my background, I don't know whether the manuscript represents a substantial contribution to the scientific progress within the scope of Ocean Science. But I think the authors bring in some new data and new information to the topic.
 - a. As mentioned, I miss a clear presentation/description of the method applied in the paper. Let me give a couple of examples: The authors emphasize that the temperature and other climate indicators have developed through three different time-regimes; 1904-1939, 1940-1986, and 1986-2012, respectively. I think it important to clarify what kind of method is applied in identifying these regimes. What kinds of criteria are applied when saying that a particular period or climatic regime is different from another? The authors are regularly referring to shifts/changes but they do not say anything how to "prove" that these shifts have taken place. Is it possible to prove that a shift has taken place? I'm aware of that it could be that it is not possible, but anyway, I think it is important to clarify the status of the argument for drawing the conclusion of the three regimes.
 - b. In the paper the authors mention and discuss many times trends (negative, positive or no trend), but the authors do not present the statistical results and they do not evaluate the validity of the statistical analyses. I think this is the most important thing the paper is lacking.
 - c. The authors argue that there is a relationship between changes (probably also the level) in sea temperature and sun-spot activity. They focus, if I have understood them correctly, on the extreme values when they discuss this relationship and it is of course important. I miss that the authors discuss whether sunspot activity in general, and not only in the extreme situations, has any effect on the sea temperature. The relationship between sunspot activity and temperature can be quantified and tested statistically, and I wonder why the authors don't do that. I'm aware that it will be time and space-consuming, so maybe that is the reason for not doing this kind of approach to the problem. Anyway, I suggest the authors discuss whether there is any general causal link between sunspot-activity and surface temperature – and why only "extremes" play a special role for the surface temperature.
5. *Presentation quality/scientific quality:* The authors do not make clear in the beginning (in the introduction) how the paper is organized. Figures and language are OK. The scientific conclusions and results are clear, but I guess the readers will feel that this is the "truth". As a reader I miss critical remarks to the conclusion. What are valid results and what part of the methods and findings can be associated with uncertainty?

1. Does the paper address relevant scientific questions within the scope of OS?
 - a) Yes, my impression is that the authors present topics relevant for OS.
2. Does the paper present novel concepts, ideas, tools, or data?
 - a) No, not genuine new aspects but I assume the regional data applied in this context is new.
3. Are substantial conclusions reached?
 - a) No, in my opinion is the method applied on the data and scientific problem too vague to obtain a «substantial conclusion». The summary-section should have included remarks on the validity of the analysis and on the conclusions.
4. Are the scientific methods and assumptions valid and clearly outlined?
 - a) No, especially is there a lack of discussion of method and on the validity of the results.
5. Are the results sufficient to support the interpretations and conclusions?
 - a) Due to vague application of methods and weak evaluation of the results, the validity of the results will also be reduced.
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
 - a) What the authors do in the paper is possible to reproduce.
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
 - a) Yes, that's my impression.
8. Does the title clearly reflect the contents of the paper?
 - a) Yes, the title does, but it is possible to use alternative (shorter) titles.
9. Does the abstract provide a concise and complete summary?
 - a) The authors could include information about the method they have chosen.
10. Is the overall presentation well-structured and clear?
 - a) It is satisfactory, but in my opinion should the authors in a much earlier stage of analysis/paper present the problem to be analyzed and include how they will analyze the scientific problem (method).
11. Is the language fluent and precise?
 - a) It is satisfactory.
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
 - a) It is basically a descriptive analysis without technical signs/equations. The authors should also check out whether abbreviations are defined properly.
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
 - a) No, my impression is that the figures and text are OK.
14. Are the number and quality of references appropriate?
 - a) It is sufficient.
15. Is the amount and quality of supplementary material appropriate?
 - a) It is sufficient.