Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2019-1-AC1, 2019
© Author(s) 2019. This work is distributed under
the Creative Commons Attribution 4.0 License.

Earth System Dynamics Discussions
Open Access

Interactive comment on "Millennium-length precipitation Reconstruction over South-eastern Asia: a Pseudo-Proxy Approach" *by* Stefanie Talento et al.

Stefanie Talento et al.

stefanie.talento@geogr.uni-giessen.de

Received and published: 26 April 2019

We thank the reviewer for her constructive comments and suggestions. We have given full consideration to the comments in the revised manuscript.

Please find below a point-by-point reply to the questions raised. A marked-up manuscript version (with tracked changes) converted into a pdf is also uploaded.

Interactive comment on "Millennium-length precipitation Reconstruction over South-eastern Asia: a Pseudo-Proxy Approach" by Stefanie Talento et al. Tine Nilsen (Referee) tine.nilsen@uit.no Received and published: 12 March 2019

Printer-friendly version

Discussion paper

CC BY

General comments: This manuscript is interesting and an important contribution to the development within climate field reconstruction methodology. The results are obtained using advanced and novel methods, and the quality of the presentation is high in all parts of the manuscript. This manuscript present pseudo-proxy experiments for millennium-long hydroclimate reconstructions over South-eastern Asia using two Bayesian reconstruction methods, in addition to the classical Analogue method. The pseudo-proxies are perturbed with Gaussian white noise of different levels, and reconstructions are constructed with both annual and decadal temporal resolution. The study is thorough with respect to testing reconstruction skill using different metrics. The results show that the Bayesian techniques perform better than the Analogue method for most of the scenarios studied. Proxy-density is important for the reconstruction skill, but is not the only factor. The innovation of the manuscript is twofold: 1) the application of a Bayesian Hierarchical model (BHM) climate field reconstruction method to target fields of precipitation over South-east Asia, with relevance for the South Asian summer monsoon. 2) The novel approach of combining the BHM reconstruction technique with clustering is important to potentially reduce computation time for the BHM reconstruction method. The manuscript is well-written with a good structure. Given the extent of the study the length is appropriate, although I miss some relevant information (see comments below). In terms of language and preciseness, the authors should go through the formulations in the manuscript, checking that the most important information is presented early in the sentences and paragraphs, and that the sentences themselves have a logic structure. Specific comments: - The title is informative and precise for the content. The same goes for the abstract. A minor point is that both the terms “hydroclimate” and “precipitation” are used in the abstract and intro, without a clear precision of what hydroclimate is or how it differs from precipitation. Perhaps a sentence could be included to make this clear.

Agreed. In the Abstract we restrict to only use the term ‘precipitation”.

- P. 3 l. 36, intro: you write that you use four different methods, but from this text I would

CC BY

say you use three different methods: 1) BHM, 2) BHM with clustering, 3) analogue method. This appears to be a matter of definition. It is perfectly fine to separate the 5- and 10-clusterings in the figures, but the method is the same, or did I misunderstand something? Arguments would be needed for why the methods are different. Note also a typo in this sentence.

Agreed. We changed the description to “three methods” and corrected the typo.

- P. 4 l. 32, model: in figure 1 you have plotted the precipitation both over land and ocean, do you use also the ocean precipitation values in your analyses? I find this figure less visually intuitive than later figures because the land/sea boundary is not clear.

Agreed. We modified the figure to only include information over land.

- p. 5 l. 4-7, data: I miss a justification for why these particular pseudoproxy sites are selected, that is, why are the networks of Chen et al. (2015) and Ljungqvist et al. (2016) favoured. Are they particularly high-quality, with low noise levels, or chosen because of their suitable spatial distribution? For a real reconstruction, would you choose this particular proxy network?

Yes, the selected sites would be the ones used in a real-world reconstruction. The criteria for the selection of records was: millennium-long (with start date before 1000CE), at least 2 values per century, located over land, published in the peer-reviewed literature and described as indicator of local variations in hydroclimate. We added a clarification for this in the text under the section “Proxy Data locations”.

- p. 6, l. 5-7, PPE: consider rewriting as that the word “trend” might confuse the reader, since you refer to non-significant values for the evolution of precipitation. After all, non-significant values might be considered natural variability.

Agreed and modified accordingly.

- P. 6-8, BHM: the methodology description of the BHM reconstruction method is nice.

You might help assist the reader by specifying with a few more words the meaning of phi and sigma.

Agreed. We added that 1/phi is the e-folding distance and that sigma is the spatial persistence parameter (homogeneous in space).

- P. 7 l 18-22: are these data detrended before performing the Kolmogorov-Smirnov test? Trends can affect the Gaussianity of the record. Note the same remark for figure A1 as for Fig 1 for land/ocean values.

Trends are not emoved before the test. However, we repeated the analysys after de-trending and the results are unchanged (the trends are non significant in this data). Figure A1 was modified to only include information over land.

- P. 7 l. 31-34: long sentence with interesting information, consider rewriting as the important information is presented at the very end of the paragraph.

Agreed and modified accordingly.

- P. 8 on the Prior level: you refer to the priors in Tingley & Huybers 2010, are they identical to those you use? If not, you may consider including the list of priors in an appendix, especially since your target variable is precipitation and not surface temperature. You have not provided a statement on availability of data and code, the information on the priors could also be provided externally for verifiability.

Agreed. Yes, the prior we use are as in Tingley and Huybers (2010), this was clarified in the text.

- P. 8-9, Section 2.2 on the description of the BHM+clustering method: an extended text would be appreciated since this is a novel method, either in the main text or as a supplement. I am interested in the authors point of view on the following:

1) From the text description, I understand different clusters are treated independently by the BHM technique. Hence, locations that are in close proximity to each other may

CC BY

be part of different clusters, and their mutual spatial correlation is then zero when using the BHM. Is this correct? How realistic is this in the physical sense? Your interpretation of cluster separations as representing topographic boundaries such as mountains help with the understanding of the methodology, but I am curious on how representative this is for the true variability of precipitation in the region.

Yes, the BHM is applied on each clusters in an independent manner. We modified 1 sentence in the description to make that claerer for the reader.

Clusters are created according to the instrumental-period information. Groups are formed by sites whose precipitation time-series are highly correlated. Therefore, it can happen that close locations are grouped separated and then in the BHM there is no exchange of information between these neighbor clusters. However, if a similar precipitation-correlation structure holds also prior to the instrumental period it is expected that the clusters wouldn't be too different and, then, no much information is missed in the BHM.

Clustering is an objective unsupervised learning technique, based on data evidence. On the contrary, separating a region by NW, SW, etc. is artificial and not data-based. Then, clustering is truly representative of the precipitation variability (according to the instrumental-period information).

We extended the text in the manuscript to clarify that this technique does not require any expert-knowledge.

2) Do you find it relevant to justify the number and spatial division of the clusters using expert-knowledge on the known precipitation patterns in the study region? future users might be less interested in the relation between the true geographical constraints and the cluster division when applying the time-saving simplification of the BHM reconstruction. If no expert-judgement is required, how can users decide on the number of clusters necessary?

CC BY

Agreed, we clarified this in the text. No expert-judgement is needed, this is an automatic technique (see also previous answer).

The criteria for the selection of the number of clusters was that most of the clusters should include pseudo-proxy locations (if a cluster does not include pseudo-proxy information the BHM scheme only uses instrumental-period data). While this condition is met without problems for 5 Clusters, with the 10 Clusters division for both the annual and decadal cases one of the clusters is disjunct with the pseudo-proxy network and, therefore, a higher number of clustering divisions was not attempted.

We added this clarification in the text.

3) Related to (2): why do you choose 5/10 clusters? What happens if you use other variations? Fig A2 shows the clusters, how do they relate to the confined regions defined in the Results section? (East China, North-Western Arid China, India, Mongolia, Tibet and so on.)

Agreed (see the previous answer). The regions mentioned in the Results section are not obtained via clustering, the simply arise as distinctive after plotting the different skill measures of the reconstructions.

- P. 10 l. 16-17, Analogue method: did you also try other values of N? I wonder how(if) the reconstruction skill of the analogue method would change if the number of analogues were different?

Yes, we made some test using between 15% and 40% of the analogues in the pool. Using more analogues makes the reconstruction to drift towards an instrumental-period mean, while too few analogues forces the reconstruction at a certain time-step to be exactly a single year (or 10-year period) in the instrumental era. We selected to use the 20% of the total number of analogues as a compromise between those two extremes. Although results do change with other selection, 20% seems to be around the optimal selection for both annual and decadal resolutions.

CC BY

We added a sentence in the text clarifying this aspect.

- Results section reads well. - P. 14 l. 38: replace “as the latter” with “as the quality” to avoid any potential confusion. In the next sentence (last in results section) I don’t understand the main message. The sentence is unclear, and could be rewritten so that the main point is stated first in the sentence.

Agreed and modified.

- P. 15 l. 11: this last part of the sentence is unclear.

Agreed, the sentence was modified.

- P. 15 l 23-28, summary: the first half of this paragraph can be developed. In the methods-description of the BHM you write that the model assumes a Gaussian distribution for the climate variable , but there is no such assumption for the Analogue method. Why would the Analogue method perform worse in reconstructing non-normal values than the BHM? How far from gaussian are the precipitation values in the arid and semi-arid regions ref. Fig. A1?

We provide no proof of the non-gaussianity being the cause for a poor Analogue Method performance. It is simply a proposed hypothesis, given the geographical distribution of the skill of the method. Proving this will require the design of new experiments (more theoretical ones, with different distributions of the input data for the Analogue Method) and is out of our scope in this manuscript. We clarified this aspect in the text, moving this brief discussion to the “Results” section.

Regarding the gaussian test, the p-values in the NW Asia area are close to 0, indicating that the probability of the data coming from a gaussian distribution is very low. We modified Figure A2 to show the p-values in addition to the rejection/acceptance of the Kolmogorov test.

- P. 15 l. 37: rewrite last part of the sentence.

CC BY

Agreed, the sentence was removed.

Techincal corrections: - p. 4 l. 28, model: first time use of JJA should have the abbreviation written out. - All figures: please increase the fontsize to a readable size for plot titles, labels, tickmarks and colorbar indicators. - Figure 2: Please use a different color combination than blue and black, as the curves are hard to distinguish.

Agreed.

Specific examples where text clarifications are needed: - P. 2 l. 23-24: "Proxy distribution in space and time is heterogeneous with decreasing numbers back in time. archives vary with respect to temporal resolutions." -> Consider rewording "decreasing numbers", to something more precise. For the second sentence: I would avoid using "varies" in this case, and instead write something like: "the temporal resolution is different between archives".

Agreed.

- P. 6 l. 19-20: "the approach splits the complex relationship model into three basic components." -> Use a more precise term than complex relationship model.

Agreed.

Please also note the supplement to this comment:
https://www.earth-syst-dynam-discuss.net/esd-2019-1/esd-2019-1-AC1-supplement.pdf

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2019-1, 2019.

CC BY