

Interactive comment on “Optimal design of a climatological network: beyond practical considerations” by G. S. Mauger et al.

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Authors' responses to **Anonymous Referee #1**

For clarity:

Reviewer comments are in italics

Authors' responses are in bold

FIRST: We would like to thank the reviewer for her comments on the manuscript.

SECOND: Before responding to the specific comments, we note that these are
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the same comments that were provided by reviewer #1 during the “pre-review” phase of our submission, and that the current version of the manuscript reflects our attempts to address the reviewer’s concerns.

In the manuscript is described and applied a method to design optimal climatological networks to monitor climate that objectively locate most valuable stations for any given field based on the idea of adaptive observations by means of the Kalman update equation approach. This is really a subject of interest that has so far received not too much discussion.

Certain parts of the text show some difficulty of understanding. We just have to bear in mind that the information the authors enter on the paper should be of interest to a lot of practitioners. Remember that the reader isn't as familiar with it as the authors and thus is very taxing to be flipping pages trying to keep track of what is being discussed.

A number of sentences are unclear, making the manuscript challenging to read and understand. The paper needs some work, in the sense of explaining clearly what the message of the work is and needs to be friendly for readers.

The authors have reviewed the text for readability and added clarifications where needed. In addition, we have sought to clarify the main conclusions of the work in both the abstract and concluding sections of the manuscript.

I judge this paper suitable for publication, believing that is a good study, but its presentation falls below the standard of the journal. Authors are encouraged to read the manuscript critically, and that's why, with the interest of that serves to them as orientation, I present some comments to them below.

For this reviewer it is not clear what kind of data is finally used; for instance, in pag.5 the authors explain that the data used are monthly data; however, Figure 2 shows results

for regionally averaged annual data and in the remaining Figures and their explanations it is not clear what data are using. In regionally averaged annual data, what really is the “regional” average that is made to obtain this Figure?

The authors have edited the text on page 201 (previously page 5) to clarify that annual data were used (lines 4-5). Although the definition of “regionally averaged” is already included in the text (p. 200, lines 8-9), the caption of figure 2 has been clarified to specify this as well. The caption for Figure 2 has also been edited to clarify that the regional average is used as the target metric – i.e., the monitoring goal for which the network is designed.

According to the authors, on page 5 (line 276), it seems that PRISM is created by gridding point observations using an interpolation scheme. What utility does a network created by means of an interpolation have to use it (with its inherent errors) instead of using directly the GHCN? Is there a reason that cannot be perceived from a reading of the text?

First: The reviewer is correct that this is a limitation of using the PRISM dataset, a fact that we acknowledge on page 207 (line 1).

Second: Although the calculations can be performed using GHCN data, such results are strongly biased by the locations at which such stations exist. As we note in the introduction (page 195, lines 10-15), existing observing networks are biased towards lower elevations near populations centers, and are thus not necessarily representative of regional climate.

Third: The NARR dataset has similar limitations, since it also assimilates surface observations. Furthermore, the NARR data are particularly limiting since data are only available from 1979 to present, making it difficult to obtain robust statistics on the results. We nonetheless present the NARR results (a) for the reasons the reviewer highlights, and (b) to highlight their broad-scale

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similarity with the PRISM results.

As mentioned above, the interpretation of the results is not clear enough on some occasions. For example in Figure 3, it should be explained in more detail the exact meaning of the dots represented against the stations not represented.

All of the stations are represented on each map in Figure 3. As stated in the caption: “Each dot denotes the location of a GHCN station, and is shaded according to the weighting obtained for the given dataset”.

With reference to Figure 2 authors should explain it a little more in regard to its final message. For example, could it indicate that more stations in the highlighted areas should be added to the network and that they are not needed in the rest of the domain under study? Authors should enrich the text of the manuscript in this sense.

We agree with the reviewers that such details are necessary and important for the reader. These questions are addressed in the text as follows:

Assumption about existing stations:

Page 200, lines 17-18: “For simplicity we assume that we are designing the network from scratch. . .”

Method used to summarize results (in Figure 2 and all subsequent figures):

Page 204, line 15 – page 205, line 6: “We summarize the results by producing maps. . .”

Summary of the results from Figure 2:

Page 205, line 24 – page 206, line 9: “The results shown in Figure 2 highlight several regions that are important for capturing the regional signal of climate variability. . .”

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Another aspect that is not sufficiently emphasized in the text is the one involving the question of whether the method works for annual data and not for daily and monthly data.

The theory of the method is established (page 196, line 25 – page 198, line 6), and the methods used are independent of time scale (e.g., equations 1-7 are general: no specific time-step is implied). A note has been added to this effect in the text (page 3, lines 176-179).

I really think that a weakness of the method is that forces to some extent to obtain better results with PRISM than with NARR due to the regridding of the surface observations as they mention in the text.

As noted above and in the text:

- 1. NARR results are subject to their own important limitations, and**
- 2. the NARR and PRISM results are quite similar.**

Since R2 is extracted from the daily observations of ECMWF, I suggest that at least the authors, for better understanding by the readers, explain with a little more detail how they determine the error variance in terms of the autocorrelation time scale.

The error variance is not determined in terms of the autocorrelation time scale. Instead, an empirically-obtained daily error variance is obtained from ECMWF (page 202, lines 17-20) and adjusted to annual based on an assumed autocorrelation time scale (page 202, lines 20-27). For clarity, we have highlighted the fact that the autocorrelation time is used to adjust from daily to annual error variance.

I wouldn't like to complete the manuscript's review without asking the authors to spell

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out briefly the contribution that their results suppose against the bibliographic references provided.

As we note above and in the text, the theory of ensemble sensitivity analysis is well-established, and has been applied to adaptive observations. However, we know of no published results in which the method has been applied to the design of an observational network. The purpose of this manuscript is to describe the approach and use an illustrative example to highlight sensitivities and practical considerations.

Interactive comment on Geosci. Instrum. Method. Data Syst. Discuss., 3, 193, 2013.

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