



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

Anonymous Referee #1

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General comments In the manuscript is described and applied a method to design climatologically optimal 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 in the paper should be of interest to many 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

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for readers. I judge this paper may be accepted for publication after revision, believing that is a good study, but its presentation falls below the standard of the journal. Authors are encouraged to read the manuscript critically. That's why, with the interest that serves the authors orientation, I present some comments to them below. Specific comments For this reviewer it is not clear what kind of data is finally used; for instance, in page 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 enough what data they are using. In regionally averaged annual data, what really is the "regional" average that is made to obtain this Figure? 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 (with its inherent errors) instead of using directly the GHCN? Is there a reason that cannot be perceived from a reading of the text? 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. 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. 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. 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 regriding of the surface observations as it is mentioned in the text. Since R2 is extracted from the daily observations of ECMWF I suggest that, at least, the authors should, 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. I wouldn't like to complete the manuscript's review without asking the authors to spell out briefly the contribution that their results suppose

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against the bibliographic references provided.

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