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Geoscientific Instrumentation Methods and Data Systems
Discussions

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 #2**

For clarity:

Reviewer comments are in italics
Authors' responses are in bold

We would like to begin by thanking the reviewer for their comments on the manuscript. Along with these responses, we are attaching a revised version of the manuscript, which includes many of the suggestions offered by the reviewer.

The paper addresses a very interesting issue about how to design an optimal environmental observational network. Nowadays the increase of the computational power is creating more high-resolution analysis and forecast for meteorological or climatological purposes, but the number of the stations on the network are not growing at the same speed. Even the number of the grown observing station is decreasing versus remote sensing techniques. This method can help to place new stations and discard the stations were the information is redundant on an observational network.

Although this work shows an interesting approach some points of the paper must be clarified for the final publication. So I suggest proceeding with this interactive discussion to perform minor corrections.

Specific Comments:

I suggest a different structure of the paper to facilitate the reader comprehension.

The Section 2 "Background: network design" could be removed, moving the first paragraph (Pag 196 Lines 11-24) to the Section 1, and the rest of the contents to de section 3.

The Section 3 "Methods" must be restructured. The section introduces the databases that were used to study an optimal design a climatological network over the Pacific Northwest. One table with the type, resolution and years used to perform this work, and other with the information of the stations included in the analysis could be introduced for clarity.

The authors appreciate the reviewer's thoughtful comments regarding the struc-

turing of the paper, and have applied the majority of the proposed modifications. Specifically, the Background and Methods sections have been combined, and broken into subsections that better highlight the different topic areas. We have also added two tables: one listing the 3 datasets, and another describing the GHCN stations used.

The method to take into account the measurement error must be clarified. The relationship between the errors obtained in de ECMWF for the analysis procedure on IFS model initialization is not clear for the networks that were used on this work. If the IFS T1279 model has been used for this estimation, the subgrid scale representativeness it responds to the errors below 16 km approximately, and PRISM database resolution is 4 km. So it is not clear that the errors have been represented correctly. This affirmation is confirmed by the authors on Section 4 (Pag 206Lines 14-16) and in paper Conclusions (Pag 209 Lines 1-4).

Although sensibility test have been performed and the results have showed strong sensitivity to R2 (Section 4 Pag 207 Lines 27-30 Pag 208 Lines1-14), the error source has been chosen from ECMWF arbitrarily, with not a clear relation with resolution and quality of the data bases used during this study. A better justification of the use of ECMWF estimation must be introduced.

The reviewer is correct that it would be ideal to estimate R^2 independently. In practice this is difficult, and is beyond the scope of this study. This is the reason for the wide range of R^2 (two orders of magnitude) used in the sensitivity tests, which – as the reviewer correctly notes – show that the results are comparable over a wide range of estimates.

Nonetheless, the authors have included the results of an additional verification of the ECMWF values used. The manuscript previously included just one spot check on R^2, obtained by comparing values from two observing stations

in the vicinity of Seattle, WA. The new calculation uses a fine-scale PRISM climatology to estimate the representativeness error across the entire study domain. Both calculations reveal estimates of R^2 that are closely in line with those obtained using ECMWF – well within the range of the sensitivity estimates.

The results show that "high spatial autocorrelation across the region (in particular annual at annual time scales), the first station explains a majority of the variance . . .. Top 3 stations in temperature and top 2 in precipitation are sufficient to 95% of variance" (Section 4 Pag 205 ines 14 -20). This suggests that little information is added with new station. The use of the variance as the way of the regionally averaged annual Temperature and Precipitation are well represented by the network only allows obtaining information that you can obtain of the observational data. The use of other metrics (J) can create a more realistic network for general uses and the observational network seams more valuable. It could be interesting that the authors comment how this methodology can solve the allocation of stations for extreme values, model initialization or other uses that are a clear interest for the scientific community.

Agreed. It is not our intent to argue that we need fewer observing stations, but simply that we should pursue our observing goals optimally. The following sentence has been added to the text: "Note that this information can be used in one of two ways: (1) install only 5-10 new observing stations instead of the total number planned, or (2) pursue other monitoring goals (e.g., monitoring local climate, seasonal climate, or probability of extremes).".

Please also note the supplement to this comment:
http://www.geosci-instrum-method-data-syst-discuss.net/3/C66/2013/gid-3-C66-2013-supplement.pdf

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