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Discussions

Interactive comment on "Homogenization of Portuguese long-term temperature data series: Lisbon, Coimbra and Porto" *by* A. L. Morozova and M. A. Valente

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Authors examine the homogeneity and homogenise long monthly temperature records of Portugal. If they find statistically significant breaks supported also by metadata, they correct the biases, but otherwise they do not apply corrections. I agree with this practice, because in case of low number of time series for comparison, corrections easily might worsen the data quality. I am convinced that the quality of examined data has been improved, and the study merits publication.

However, I see many problems with the details.

I/ Certain pieces of the results suggest that the data had not been checked for outliers before homogenisation and/or that QC was not very effective. The study does not

contain anything about general QC. —- The suspicious pieces of the results: a) The monthly variation of spatial correlations (Table 4) is rather hectic, b) seasonal changes of corrections terms have strange structures, which hardly can be explained with any physical phenomenon apart from a low signal to noise ratio whose likely origen is random large errors in the data.

II/ Justified scientific facts are mixed with hypotheses. I think that the inclusion of hypotheses is allowed, but it should be clear from the text that they are hypotheses (e.g. by using "we think", "we suppose", "likely", etc. If sg. is only a hypothesis, then it is better not to repeat that too frequently.

Only hypotheses and not justified facts are:

i) Volcanic effects on inhomogeneity-biases: Volcanic effects usu. last 1-4 yr, with sharply lowering intensity from the second year. In contrast, the study deals with inhomogeneities whose biases last for one or more decades. If Authors suppose that short-term effects sometimes could provoke long-lasting changes in the climate systems and consequently in the observed data, first they should write clearly their hypothesis. This hypothesis has little evidence, thus I suggest reducing the references to volcanic effects throughout the paper.

ii) DTR is a better indicator than Tmax or Tmin. It has more evidence than long-term colcanic effects have, but is still a hypothesis only. I suggest changing the wording to: "non-climatic jumps [of DTR] often can be seen more clearly" (bottom of page 532)

iii) "The use of tests of different type (parametric, non-parametric, ... helps to obtain more significant results" —- It is often believed, but it is in fact a hypothesis only.

III/ Some statements are imprecise, and the meaning becomes misleading:

i) In the definition of Tn and Tx: I am sure that they are not monthly minimum and monthly maximum as it is written in the study, but they are the monthly averages of daily maxima and minima.

ii) Pieces of the results that no non-climatic breaks are found with 95% significance transformed to the text (in several places of the study) to: there is no non-climatic break with 95% confidence. However, from the former statement does not follow the later. —- Such statements of confidence should be deleted, they are very misleading, in fact the corrected series likely still have inhomogeneities but the signal to noise ratio does not allow their detection and correction with adequate confidence.

iii) Measurement error 0.1°C — mentioned several times in the paper. However, it refers to the expected measurement errors, which sometimes might be strongly exceeded (e.g. by personal error). Please write first that the standard measurement error is +/- 0.2°C, and there is no need to repeat that later.

IV/ There are too much repetitions throughout the paper, e.g. the correction method is written well in Sect. 2.2, but its principles are repeated again and again when the corrections for a selected station data are discussed.

V/ "...four of the most popular homogeneity tests have been used" (p. 525). Popularity is not a scientific argument when statistical methods are selected. As in this study the role of statistical tests is secondary relative to the use of metadata, I suggest explaining it in the introduction. Then it can be written that the inclusion of some simple and widely used statistical tests are appropriate to the examinations of the study.

Minor comments:

i) "homogeneity breaks" — it is not a usual term (breaks or change-point are more usu.), I suggest writing first "homogeneity breaks (hereafter: breaks)" then using "break".

ii) Metadata lists are often incomplete and it would be nice to mention that in the paper. Note: Metadata list can be incomplete even with high level data-management, since it is hardly possible to realise and document all the possible changes in the influencing factors (changes of environment, of the treatment of natural vegetation, in the

cleanliness and reflectivity of the screen, habitual personal errors, etc.)

iii) At the bottom of p. 534 the composition is strange, because from November to December and from July to October of the same year is simply from July to December, so maybe that something is incorrect here.

iv) "unbiased homogeneity tests" (p. 541): I suggest writing statistical tests or statistical homogeneity tests instead.

Interactive comment on Earth Syst. Sci. Data Discuss., 5, 521, 2012.