

Interactive comment on ““Changes” of the thermal continentality in Central Europe between the years 1951 and 2013: case study – Slovak Republic” by J. Vilček et al.

Anonymous Referee #1

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1. Does the paper address relevant scientific questions within the scope of ESD? Yes
2. Does the paper present novel concepts, ideas, tools, or data? No, except perhaps data for the analysis
3. Are substantial conclusions reached? Not too substantial
4. Are the scientific methods and assumptions valid and clearly outlined? More or less yes
5. Are the results sufficient to support the interpretations and conclusions? Rather not
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Not attempt to cover more overview within introduction, which might even identify some other methods for the analysis (Minetti, 1989, Continentality indices.

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Methodological revision and proposition, and others, one can found many others relevant references with more sophisticated methods of indexing the continentality just using google search
8. Does the title clearly reflect the contents of the paper? Yes
9. Does the abstract provide a concise and complete summary? Yes
10. Is the overall presentation well structured and clear? Yes
11. Is the language fluent and precise? More or less yes, as there is proof reading included in the process, no problem
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes, actually, nearly none
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No
14. Are the number and quality of references appropriate? No, as said above
15. Is the amount and quality of supplementary material appropriate? Not available.

The paper is dealing with study of changes in thermal continentality in Central Europe between the years 1951 and 2013 in the case study for selected stations of the Slovak Republic. No doubt it make a sense to study the development of continentality especially with respect to the climate change development, but the study presented seems to me to be too simplistic. Although this kind of assessment is worth to publish, there are several caveats of this very simplified study, which as my opinion is really not covering the issue in publishable extent.

One of the major objection I have is against the introductory overview of the continentality measures or indices methods as mentioned above. From such overview even more options for analysis could result which (when compared) might make the study more sophisticated. Another major issue concerns the comparison with the climate change signal based on climate model results. There is not complete reference on the model used as well as no indication of the scenario, but one could find that this will not be the up-to-date version. However, todays trend should be to use more models, actually, now available from CMIP6 experiments with new scenarios as well as even the use of downscaled results, e.g. based on EuroCORDEX activity results. Comparison of the individual scenarios as well as the assessment using the ensemble of the

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simulations would provide much more solid conclusions.

Further comments: As rather major revisions are necessary as my opinion, I am not giving too detailed individual comments concerning the text clarity, just remarks for content.

p. 2, l. 17 not correct citation of the reference, it should be rather Sobisek et al.

p. 3, l. 26 Did Melo make in Melo (2002) any model simulations?

p. 3, l. 27 missing references for the models

p. 4, l. 13 Supan method – missing reference

p. 4, l. 16 extreme months of individual years? What if happen December within the same winter period is colder than January?

p. 4, l. 19 actually, is there any difference with respect to the previous (used) definition?

p. 4, l. 21 model reference, scenario indication missing

p. 5, l. 17 inversion position / what is that? I could accept something like ...place with often inversion appearance ... or something like this. Moreover, it might be rather a matter of cool air due to catabatic flow along the valeys slopes then real inversion situation fully developed in the boundary layer

p. 5, l. 22 no such a reference in the list

p. 6, l. 20 no indication of the scenario again

p. 6, l. 26 Actually, even this study confirms some increase, although not so much significant. Based on the study results one could rather ask why we have for future from the model the decrease? However, for rather limited selection of the stations and just one model, one could hardly develop any solid conclusion from these results.

p. 7, l. 12-13 It is in contradiction to the statement of the results analysis parts where decrease revealed (p. 6, l. 21)

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p. 7, l. 17-21 Yes, why not covered in this study in such more details? Of course, bioclimatological conditions are depending not only on the continentality (thermic) itself, but on the mean temperature and humidity as well. Moreover, what should be as well studied, is eventual change of the shape of the annual course of monthly mean temperature etc. as follows from the other available works.

p. 8, l. 12 It is in contradiction to the statement of the results analysis parts where decrease revealed (p. 6, l. 21)

p. 8, l. 20 wrong reference (see above)

Interactive comment on Earth Syst. Dynam. Discuss., 6, 1261, 2015.

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