

Interactive comment on “Impact of maximum borehole depths on inverted temperature histories in borehole paleoclimatology” by H. Beltrami et al.

Anonymous Referee #2

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Manuscript: Impact of maximum borehole depths on inverted temperature histories in borehole paleoclimatology

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The paper by Beltrami et al. is well written; newcomers to the field of geothermal paleoclimatology will find a great deal of information in it. Generally speaking the background information to understand the manuscript is well presented. In terms of form, the title reflects the subject of the manuscript and the abstract introduces the reader to what is the general purpose of the manuscript in an easy way. The introduction, even though is well written and the ideas well presented, is too long. And talking about the size of the introduction I have to say that, in my opinion, the number of references cited (and therefore the list of references) is also too long (for the reviewer, to check if the

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references in the text agree with the list of references is a nightmare). I would say that the manuscript is a good introduction to borehole paleoclimatology.

However, the question I put myself after reading the manuscript several times was (I read the manuscript several times because I was looking for something really important or new in a paper that has been submitted for publication in a journal with the reputation of *Climate of the Past*): is this a research paper or just a calculation to put a number on something that researchers working with geothermal methods, in general, and borehole paleoclimatology, in particular, are already aware of? In my opinion, the answer to that question is that the manuscript is, in fact, just a calculation; a calculation of a minimum depth that boreholes should have to allow performing GST reconstructions from temperature logs that are reliable. I am sure that every researcher working on borehole paleoclimatology is aware of the fact that “the deeper the better” in terms of GST reconstructions; the problem is that most of the times they do not have deep boreholes and to have one drilled with the appropriate depth would imply money they do not have. So, as the authors say, opportunity wells are the most used boreholes for paleoclimatic studies/reconstructions. The main problem I see with the use of shallow boreholes (and by shallow I mean boreholes shallower than 200 m) is that some researchers are assuming the same degree of accuracy for GST reconstructions from shallow boreholes that one would expect for boreholes as deep as 500 or 600 m.

So, is the manuscript by Beltrami et al. a research paper or not? My answer (read my opinion) to that question is that the manuscript is a calculation, an exercise performed using known theory and algorithms. Is it a bad manuscript? No it is not; however, I would not classify it as a research paper; I see it as an interesting research note. Technically speaking the manuscript is alright: there are no formal errors or erroneous concepts; besides the length of the introduction and the length of the list of references the paper is very readable.

So if I had to decide about the publication of the manuscript I would say that it should be published but as a research note and not as a research paper.

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