

Interactive comment on “Long-Term Global Ground Heat Flux and Continental Heat Storage from Geothermal Data” by Francisco José Cuesta-Valero et al.

Anonymous Referee #1

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Summary:

The manuscript presents an estimation of past ground heat flux and past surface temperatures over the last few centuries based on measured borehole temperature profiles. The main objective of the analysis is to estimate the history of vertical heat flux into the ground, in the more general framework of the global energy fluxes perturbed by anthropogenic climate change. The methodology of deriving past surface temperatures from borehole temperature profiles is well established. The novelty in this study is threefold: the shifted focus towards the surface heat fluxes, the estimation of uncertainties, and the expansion of the available data base. The main conclusion is that the

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ground heat flux estimate from borehole profiles has larger than had previously estimated. The authors claim that this component of the energy fluxes is important within the climate system.

Recommendation:

Some revisions necessary, but I think this is a valuable contribution to Climate of the Past. The manuscript is generally well written - although some sections would benefit from a revision.

General comments:

1) I found Section 2 too detailed. It will certainly help readers with a more superficial background on borehole climatology, but I think that this section can be compressed, displaying the main ideas and the important technical details that are used later on in the manuscript. For instance, I do not think it is necessary to display equation 11 in such level of detail. A matrix equation should suffice

2) In contrast, section 3 should include the new methodological aspects of the direct heat flux inversion. Here, either I missed something or something is indeed missing. On the one hand, the manuscript alludes to a direct inversion of the flux profiles (equation 18) to heat flux histories, using also the Perturbed Parameter approach (line 266) But the methodology for the direct inversion of heat flux histories is not explained, at least I could not find it in the manuscript. The PPI approach has been explained for the temperature inversions, not the heat flux inversions. Perhaps, it is so obvious that it does not need an explanation, but to me it is not that clear. In case I misunderstood something here, it is likely than an average reader will also get confused. There is an imbalance between the level of detail presented for the temperature history inversions and for the heat flux inversions.

On the other hand, the manuscript also used ground heat flux histories derived from the inversion of ground temperature histories, equation (19). There are then apparently

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two reconstructions of the ground heat flux histories, one by a 'direct inversion method' and one based on the reconstructed surface temperatures. And yet a third estimation for the recent period using the CRU temperatures. If this is true, it should be clearly indicated. Please consider labeling these three products to guide the reader.

3) The approach leading to the weighting scheme in equation 17 can be problematic. I am not saying it is wrong, but a more versed statistician than me may complain. In essence, what the authors are doing is applying Bayesian scheme to estimate the inversion uncertainties. They assume a prior distribution of some model parameters, which are then passed through the model to produce temperature profiles, and these synthetic profiles are weighted by the likelihood (17). The problem is that there are hidden assumptions in this approach that are not explicitly stated. Are the initial model parameters a priori equally probable? Without that assumption it is not possible to attach posterior probabilities to the synthetic profiles and to the model parameters. A more sophisticated, fully Bayesian approach could include a Monte Carlo Markov chain sampling of those posterior probabilities and of the temperature histories, in which their values are varied in a more systematic scheme. In any case, the hidden assumptions that authors are making about the relative probabilities of the assumed model parameters need to be explicitly stated.

A second comment is that I guess that σ in equation 17 is also depth-dependent. If not, please state clearly. If yes, would it have an impact?

4) The main claim of the study is that the ground heat flux cannot be neglected. I miss a more direct comparison with the ocean heat flux, so that the reader gets a clearer idea. Probably, the ocean heat flux is much larger but the authors can more clearly elaborate their point.

Particular comments

5) line 30 'and sea level rise'

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This is the major consequence of increase in ocean heat storage, so it is surprising that it is included with 'The rest of the components in the climate system'

6) line 63 'the model resolution for obtaining stable solutions'.

the vertical resolution

7) line 75' These results also support previous estimates of temperature change since preindustrial times based on meteorological observations and CGCM simulations, using estimates from an independent source of data and considering the most distant period of time to determine preindustrial conditions to our knowledge.

This paragraph is unclear and hard to read

8) line 82 In borehole climatology, the continental subsurface is typically represented as a semi-infinite homogeneous half-space without internal sources of heat, where energy exchanges at the land surface and heat flux from the Earth's interior are considered as the

half-space is not a well-defined term. Please, rephrase this paragraph more clearly

9) line 212 'the 95% confidence interval (two standard deviations) of the anomaly profile

This is only (approximately) true if the distribution is gaussian.

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