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Climate of the Past Discussions

Interactive comment on "Climatic changes in the Urals over the past millennium. An analysis of geothermal and meteorological data" *by* D. Yu. Demezhko and I. V. Golovanova

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The analysis of borehole temperature data from the Urals has already substantially benefit from recent contributions of the Demezhko and Golovanova. I see two major improvements of the presented work with respect to, for example, [1, 2], which enhance the statistical reliability of the results: a larger number of considered boreholes and an advanced method for the ground surface temperature history (GSTH) reconstruction. However, this should be more clearly mentioned in the text, including a higher number of citations of recent papers and a more focussed presentation of the novel results.

Geothermal Data. The authors state that for their selected boreholes, the temper-

atures increase almost linearly with depth. In general, however, the heat induction equation involves an additional quadratic term (see, for example, [3]) which becomes important at larger depths. A comment on the magnitude of error which is expected for data from deeper layers due to this linear interpolation would be welcome. Moreover, the authors claim that the depth of recordings was less than 700 m (page 3, line 12), whereas Fig. 2 shows data up to a depth of 900 m.

It is not clear how the inversion algorithm of [4] copes with the fact that in the presence of non-climatic influences and measurement noise, the GSTH reconstruction is an ill-posed inverse problem. In particular, I expect that a proper reconstruction requires a suitable regularization [3]. The paper would substantially benefit if the authors could give some details on their particular inversion approach and briefly address the regularization problem.

As far as I have understood the paper, for the interval estimates method, only the minima and maxima corresponding to the little ice age and medieval warm period have been taken into account. If so: How is the uncertainty of the LIA and MWP positions within the reconstructed data taken into account? Are there estimates about the possible errors induced into the reconstruction due to the interpolated time scale? I would like to encourage the authors to discuss these points in some more detail, explaining what is actually meant by ”signal-to-noise ratio” (page 5, lines 4-5) in this context.

As a suggestion for further methodological improvements in the future, I think that methods similar to the synchronization of different records based on cross-recurrence plots [5] would allow to derive a more reliable time-scale for every borehole and, consequently, yield information about the actual profile of the thermal diffusivity in the material. This would be a good cross-check of the statement that there are ”no sharp contrast of rock thermal properties” (page 3, lines 12-13). Is this statement in general accompanied by direct measurements along the considered boreholes? (If yes, what is the magnitude of changes of a?)

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Statements about the absolute temperature during the MWP and LIA should be accompanied by error bars in Fig. 4, which should involve not only the variations of the estimated values between the different boreholes, but also the uncertainties of the single reconstructions which are probably largest in the older part of the record. In addition, for the general audience of Climate of the Past, it would be interesting to systematically compare not only temperature estimates from different borehole studies, but also some complementary reconstructions based on other palaeoclimatic proxies.

The asymmetry of the warming and cooling phase of the LIA may structurally resemble those of glacial-interglacial cycles, but is likely caused by different mechanisms. Hence, I recommend to avoid the corresponding statement (page 7, lines 10-12). If at all, there may be some relationship with the millenial scale variability during the last glacial cycle found in terms of Dansgaard-Oeschger events.

Meteorological Data. [1, 2] have already studied some time series from the region taken from the Global Historical Climatology Network. In the presented work, a corresponding reference is missing, which does not allow to evaluate the homogeneity and reliability of the considered records. In general, the homogenization of long time series from historical records is a challenging and complex task itself (see, for example, [6, 7]). If the authors claim "the reliability of instrumentally measured data is beyond doubt" (page 6, lines 13/14), this statement should be accompanied by an appropriate reference.

The averaging procedure firstly applied to the meteorological data involves the substraction of an individual temperature value. The authors should mention how these values have exactly been derived. In general, Fig. 3 should be accompanied by error bars taking the uncertainty of this "master curve" into account.

The by far most serious concern of mine is related to the analysis and interpretation of slope regression coefficients from the meteorological data. In particular, the consideration of coefficients obtained from 11-year periods is statistically meaningless as

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the corresponding differences over this time interval are clearly within the natural variability of the entire record. This is also underlined by the obvious 11-years period of the derived slope coefficients themselves in Fig. 5. It is thus inappropriate to derive a trend ”per 100 years” from an 11-years subsample: in Fig. 5, the uncertainty of the slope coefficients is larger than the amplitude of their fluctuations. In my opinion, the 31-year window is the absolutely smallest interval from which substantial information about climate change might be inferred (interestingly, the corresponding figure shows a slightly increased warming rate over the last 70 years of the record).

Conclusion. I do not doubt that in general, the work of Demezhko and Golovanova is worth being published in Climate of the Past, however, in its present form, the manuscript requires a major revision.

References

[1] Štulc, P., Golovanova, I.V., Selezniova, G.V., 1997. Climate Change in the Urals, Russia, inferred from Borehole Temperature Data. Studia geoph. et geod. 41, 225-246 (1997).

[2] Golovanova, I.V., Harris, R.N., Selezniova, G.V., Štulc, P., 2001. Evidence of climatic warming in the southern Urals region derived from borehole temperatures and meteorological data. Glob. Planet. Change 29, 167-188.

[3] Hartmann, A., Rath, V., 2005. Uncertainties and shortcomings of ground surface temperature histories derived from inversion of temperature logs. J. Geophys. Engin. 2, 299-311.

[4] Demezhko, D.Yu., Shchapov, V.A., 2001. 80,000 years ground surface temperature history inferred from the temperature-depth log measured in the superdeep hole SG-4 (the Urals, Russia). Glob. Planet. Change 29, 219-230.

[5] Marwan, N., Thiel, M., Nowaczyk, N.R., 2002. Cross Recurrence Plot Based Synchronization of Time Series. Nonlin. Proc. Geophys. 9, 325-331.

[6] Butler, C.J., Garcia Suárez, A.M., Coughlin, A.D.S., Morrell, C., 2005. Air temperatures at Armagh Observatory, Northern Ireland, from 1796 to 2002. Int. J. Climatol. 25, 1055-1079.

[7] Camuffo, D., Jones, P. (Eds.), 2002. Improved understanding of past climatic variability from early daily European instrumental sources. Climatic Change 53, 1-392.

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