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Climate of the Past Discussions

Interactive comment on "Paleoclimate and weathering of the Tokaj (NE Hungary) loess-paleosol sequence: a comparison of geochemical weathering indices and paleoclimate parameters" *by* A.-K. Schatz et al.

A.-K. Schatz et al.

ann.schatz@uni-tuebingen.de

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Dear Referee #2,

Thank you very much for your comments on our manuscript in CPD. There seem to be three main issues about the content which need to be discussed and clarified.

1. / The paleoclimate transfer functions are mainly from Sheldon et al. (2002), they are derived from a database consisting of North American soils only and the regression quality is low

The transfer functions used in our study are indeed mainly from one main author (N.D. Sheldon), yet from several publications between 2002 and 2013 and with different co-authors. The quantification of paleoclimate parameters from paleosols is a relatively new field. Neither have many transfer functions (by multiple authors) been published yet, nor has one function evolved as the most suitable one. We tried to include all the relevant transfer functions into our study.

The soil database used to derive these transfer functions is a large dataset of North American soils. Ideally, a dataset used as a basis for transfer functions specifically for (Southeast) Europe would contain local soil data. Such a dataset is not available. The North American soils from Sheldon et al.'s database, however, have both a very large variety in formation times, formation processes and parent materials (which might explain some of the large variation, see below). We therefore assume that they are representative for European soils as well and that, therefore, the transfer functions can be applied to them, too. This argumentation will be included in the revised manuscript.

You mention in your comment that the regression quality of the derived transfer functions is very low and the data define points of clouds instead of nice correlations. The correlations are certainly not perfect, but we wouldn't expect this for such a diverse dataset. Re-evaluating the R^2s from the publications again, we find R^2s of 0.66, 0.59 and 0.72 for Sheldon et al.'s MAP functions (XRF1, XRF2, XRF3-MAP) and R^2s of 0.96, 0.37 and 0.57 for XRF1-, XRF2- and XRF3-MAT and would argue that these values are not too bad given the highly diverse dataset. However, we agree that this discussion should be included in the revised version.

2. / Large errors on paleoclimate estimates, differences between estimates

We are aware of the large errors associated with the MAP and MAT estimates. Part of them might be explained by the large variety of soils in Sheldon et al.'s database, as mentioned above. We suppose that the errors should be smaller for loess/paleosol samples only, because using data from soils of one parent material should produce

smaller errors. But this needs to be investigated further in order to be able to derive reliable quantitative paleoclimate data from paleosols in the future. Our study contributes to this development by applying and evaluating the currently available transfer functions. Despite the methodological shortcomings, we are able to show that most results from different transfer functions overlap within standard errors and indicate a trend towards lower temperature and decreased precipitation for loess. We agree that the large errors and the need for further improvement and development of methods should be discussed in more detail in the discussion section. We are going to do so in the revised version of the manuscript.

3. / Is the extrapolation to loess possible?

Kühn et al. (2013) used two different equations (one for Mollisol horizons and one for Bt, Bw horizons) and received similar results for loess deposits for MAP and MAT. MAP were reliable, whereas MAT data were clearly overestimated. They argued that the transfer functions of Sheldon et al. (2002) can also be applied to loess, since it is the parent material of the paleosol and it should include a genuine climate signal as well. This is true for Tokaj, too, assuming heterogeneity of the loess through the profile. The results of Kühn et al. (2013) and our study have shown a clear trend towards lower temperatures (but most probably still overestimated) for loess than for the paleosols and MAT/MAP estimates that are comparable to those obtained from other proxies, which encourages the application of the transfer functions to loess. We agree that we need to include this discussion about the validity in our manuscript and are gladly going to do so.

Sincerely,

Ann-Kathrin Schatz & Co-authors

References: Kühn, P., Techmer, A., and Weidenfeller, M.: Lower to middle Weichselian pedogenesis and palaeoclimate in Central Europe using combined micromorphology and geochemistry: the loess-paleosol sequence of Alsheim (Mainz Basin, Ger-

many), Quaternary Science Reviews, 75, 43-58, doi: 10.1016/j.quascirev.2013.05.019, 2013. Sheldon, N. D., Retallack, G. J., and Tanaka, S.: Geochemical Climofunctions from North American Soils and Application to Paleosols across the Eocene-Oligocene Boundary in Oregon, The Journal of Geology, 110, 687-696, doi: 10.1086/342865, 2002.

Interactive comment on Clim. Past Discuss., 10, 469, 2014.