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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.

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Dear Referee #3,

thank you very much for your feedback. We would like to answer your questions point by point:

1./ Focus on the upper part of the profile

In this study, we decided to focus exclusively on the upper part of the profile for two reasons. First, in order to evaluate our paleotemperature estimates, we needed data

from the literature to compare our results with. There is plenty of data available for the Carpathian Basin und other parts of Hungary, but the only paleotemperature data for the Northeastern part of Hungary and Tokaj, specifically, are the malacothermometer paleotemperatures from Sümegi and Hertelendi (1998). Unfortunately, they are only available for the upper half of the profile. We could have proposed paleotemperatures for the lower half of the profile as well, but wouldn't have been able to evaluate them and, as a consequence, they would not have the same scientific quality. Second, the chronostratigraphy (Schatz et al., 2012) for the upper half of the profile is more detailed than for the lower half. There are only 3 OSL ages available for the lower loess unit. For the lower paleosol there is only a very rough minimum age estimate. Paleoclimate information would be rather useless for a part of the profile that is not well-constrained with regard to chronology.

However, we agree with you that these considerations should be included and discussed in the manuscript. We are going to do this in the revised version and also include a discussion on the parent material of the paleosol. You noted correctly that since the paleosol was derived from the underlying loess, we need to explain that the lower loess is geochemically similar to the upper loess. While it is true that grain size varies between the two loess layers, more detailed geochemical characterization (Sr-Nd isotopes, major/trace element fingerprints) shows that the two loess layers are geochemically very similar, indeed. Details of this are to be discussed in a forthcoming paper to be submitted in April.

2./ Missing and/or confusing information in the methods section

As pointed out in our reply to the second referee, details about the methods used to obtain the new data presented in this manuscript can be found in section 3.1 (p. 474, l. 22-24). We used mainly XRF and some additional measurements and calculations to correct the data. An overview of the results can be found in the appendix. Some additional information and/or clarification will be provided in the revised version. The MS and carbon isotopic data we used in this manuscript were previously published in Schatz et al. (2011) and measured on the very same samples (p. 476, l. 23). However, as suggested in our previous comment, we are going to clarify this in the text and provide an extended data table including XRF, MS and δ13C data in the revised version of the manuscript for ease of reference.

3./ Susceptibility-based MAT and MAP

We agree that a short discussion about the validity of applying the Maher et al. (1994) to loess/paleosol sequences outside of China should be included in our manuscript, as it has previously been done by e.g. Buggle et al. (2009) and Panaiotu et al. (2001) for SE European loess. Both authors used the same formula to reconstruct paleotemperatures from loess/paleosols. Also, Maher et al. (1994), Maher and Thompson (1995) and Maher et al. (2002) have argued that the formula should work for loess/paleosols of other temperate zones as well. Moreover, Maher and Thompson (1995) have shown that not only a strong correlation exists between susceptibility enhancement and precipitation, but that there is also a statistically significant correlation between susceptibility enhancement and temperature (winter, mean) However, it seems like our methodological approach needs to be expanded. Thank you very much for providing additional, more recent references. We are going to analyze these additional equations carefully and, if applicable, include them in the revised version of the manuscript as well.

4./ The 25 cm sampling intervals are too coarse depth plots should be changed accordingly

While a 25 cm sampling interval is rather coarse for some paleoclimatic analyses as e.g. detailed mineral magnetic studies, it is a very common spacing for paleopedological investigations and comparable with e.g. 20-30 cm reported by Újvári et al. (2014), 40 cm by Varga et al. (2011) or 10-50 cm by Buggle et al. (2011, with details of sampling in 2008). All of them use the standard data/depth plots with discrete data points joined by a smooth line.

5./ Thank you for the additional remarks. We are going to correct the wrong table reference on p. 482 and replace "MS" with the more specific “X“ (small chi).

Regards,

Ann-Kathrin Schatz

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