Clim. Past Discuss., 10, C63–C66, 2014
www.clim-past-discuss.net/10/C63/2014/

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.

Anonymous Referee #3

Received and published: 5 March 2014

The objectives of the present manuscript are to estimate the weathering intensity within the paleosol developed during marine oxygen isotope stage 3 and the overlying loess unit of the Tokaj sequence via 14 different indices found in the literature, to compare the calculated weathering intensities and to calculate mean annual temperature (MAT) and mean annual precipitation (MAP) using already published transfer functions for x-ray fluorescence spectrometry (XRF), magnetic susceptibility, carbon isotope proxy data. For the studied segment of the Tokaj sequence all 14 weathering indices give appreciably the same result and MAT and MAP estimates based on magnetic susceptibility

proxy data appears to corroborate best with rates found in the literature. Objectively, there is a major conceptual flaw in the design of the study and a cruel lack of details in methodology and data provenance which prevents a critical review of the manuscript's discussion and conclusions. These major points are outlined below:

1) It is stated on p.473 lines 9-11 "Since most of the paleoclimate proxy data are only available for the upper half of the profile, i.e. the upper loess layer and the upper paleosol, we concentrated primarily on this part of the section (Fib 1b)." From the Schatz et al. 2011 Quat. Int. paper we know that the Tokaj sequence is about 16 m thick. In the present manuscript, only the upper 8 m excluding the modern soil are considered. From the above sentence this choice is one of convenience and not driven by the scientific question at hand. There lies the problem given that the focus is on weathering intensity. How can one evaluate the weathering of a paleosol without knowledge of the state of the parent material from which the paleosol was formed? The present study should have considered the underlying loess unit OR argue that the nature of the overlying loess unit is exactly the same as the underlying loess unit and therefore may serve as a proxy for the parent material. However, looking at Figure 3 in Schatz et al. 2011, this argument would be very difficult to support. The loess unit underlying the paleosol studied here is characterized by 40 to 50 % particle size greater than 30 μm, while the overlying loess unit is characterized by more than 60% particle size greater than 30μm. This is a significant difference in particle size of the parent loess material (change in provenance? atmospheric circulation and/or intensity?) which in all likely hood is accompanied by differences in mineral assemblage (component and/or relative concentrations). Can the authors explain or argue the design of the study which seems inappropriate given the stated objective?

2) The methods section would benefit from a more transparent presentation of the field sampling and its relationship with previous studies. For example are these the same samples as for Schatz et al. 2011. What data are new (first publication here) and what data were previously published and where? Loss on ignition data are reported in the

results but analytical method not mentioned in the section 3.1. Magnetic susceptibility data? Carbon isotope data?

3) MAT and MAP from magnetic susceptibility proxy data are calculated following Maher et al. (1994) and Han et al. (1996). The validity of applying these empirical formulas (constructed from modern soils developing on Chinese loess) to the Tokaj sequence (Hungary) needs to be discussed. While a physical mechanism linking magnetism and precipitation has been proposed and validated (but read below), it is not the case for temperature and magnetism. Since these earlier works, it has been demonstrated that the Maher et al. 1994 transfer function constructed from Chinese loess Plateau modern soils is not universal to all soils developing on loess parent material. Maher et al. 2003 proposed the following transfer function from Russian steppe modern soil: MAP = 86.4 Ln(Xb-Xc) + 90.1 Geiss and Zanner, 2007 showed there was yet another transfer function for the US Great Plains.

More recently, Orgeira et al. 2011 proposed a magnetic enhancement proxy which may be universal. At the very least, by incorporating a soil moisture term, the difference between Chinese, Russian steppe and US Great Plain transfer function is significantly subdued.

The Maher et al. 1994 transfer function is of the same form as the example above. Nowhere in the present manuscript (nor its supplemental data table) are magnetic susceptibility values given. The form of the transfer functions requires that a magnetic susceptibility enhancement (Xb-Xc) of the soil (or paleosol) B-horizon with respect to its underlying parent material (C-horizon) be calculated to obtain a MAP estimate. Moreover the calculated MAP estimate will be for the time period of soil formation of the B-horizon considered. How did the authors go about doing this (what values? which depths?)? How do they calculate a magnetic susceptibility based MAP for the loess interval?

An additional minor point is:

4) Given that the sampling was conducted continuously over 25 cm depth interval (these are thick sampling intervals), the data plots versus depth in Figures 2 and 3 would be better drawn by a step line displaying the obtained value across the sampling depth instead of discrete data points joined by a smooth line.

I am of the opinion that in its present form, the manuscript should not be published.

I have not done an in depth editorial review of the manuscript. However,

- p.482 line 23: Table 4 is referenced but it should be Table 5;

- Throughout the text, I would strongly recommend to replace the acronym MS for magnetic susceptibility by the correct acronym X (small chi) if mass specific magnetic susceptibility (units of m3/kg) or k (small kappa) if volume specific magnetic susceptibility (units are dimensionless but reported as SI).

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