

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 #2

Received and published: 26 February 2014

This paper presents a batch of major elements as well as Ba, Rb and Sr concentrations for about 35 samples from a loess-paleosol sequence from Hungaria. The authors use these data to calculate past temperatures and precipitations at two different time intervals, the loess deposition period and the paleosol formation period. I am afraid that the whole story stands on very shaky grounds. The temperature and precipitation evaluations are done using equations that come from publications by other people. Most equations were published by Sheldon et al. in 2002. Looking at this paper, several things appear clearly: Sheldon et al. used a large dataset of soils from North America,

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



[Interactive
Comment](#)

collected in places where both the temperature and precipitations were known. They calculated empirical correlations between chemical indexes and temperature or precipitations and they discuss the robustness of their approach. Already in the abstract, they explain that some indexes might be correlated with precipitations but that the case was less clear for the temperature. Looking at the quality of the regressions in the various figures of their paper, it is very clear that data define clouds of points and not nice correlations. The error on both slopes and intercepts are very large and the authors recognize it. Given this background, I do not understand how the authors of this manuscript can use the equations, not even mention potential errors of the calculated values, and conclude without discussion (see both abstract and figure 4) that they can constrain the mean annual temperatures and mean annual precipitations during loess deposition and during paleosol formation. The first point is that nowhere they evaluate whether the regressions calculated by Sheldon et al. using soils can be extrapolated to loess deposits. However, this is an issue that should have been addressed. In addition, given the errors on each individual determination and given the differences between the various estimates calculated using the different indexes, the most obvious conclusion seems to be that they cannot determine temperature and precipitation using these indexes. If the authors want to convince readers that their estimates are statistically significant, they need to demonstrate it properly. It is not presently the case. I suggest that the authors reevaluate their entire discussion and conclusion before they consider resubmitting their paper to a Journal.

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)