Clim. Past Discuss., 9, C2568–C2571, 2013
www.clim-past-discuss.net/9/C2568/2013/
© Author(s) 2013. This work is distributed under
the Creative Commons Attribute 3.0 License.

Climate of the Past Discussions
Open Access

Interactive comment on "Geochronological reconsiderations for the Eastern European key loess section at Stayky in Ukraine" *by* A. Kadereit and G. A. Wagner

Anonymous Referee #2

Received and published: 14 November 2013

The manuscript "Geochronological reconsiderations for the Eastern European Key loess section at Stayky in Ukraine" by Kadereit and Wagner highlights the difficulties encountered when proxies of terrestrial archives of climate change such as loess palaeosol sequences are tentatively correlated to the more fine-tuned proxies of ice core chronologies. The manuscript is very well written and the criticism raised to the paper of Rousseau et al., 2011 (Clim. Past. 7, 221-234) seems logical and well justified. Thus, the present paper, actually coming as an accompanying contribution to the Kadereit at al. paper in QSR 2013 (QSR 59, 67-86) discussing Nussloch key section in Central Europe, is a welcomed contribution for Eastern Europe, commenting on the paleoclimatic interpretation of the master loess-paleosol section at Stayky in Ukraine.

For the Nussloch site, Kadereit et al., 2013 (QSR 59, 67-86) showed that a correlation of Lohne soil with GIS 8 can be excluded and extrapolate their finds to Vytachiv soil, previously correlated by Rousseau to GIS 8. The authors tend to correlate the formation of this paleosoil to earlier Greenland Interstadials, most likely to GIS 5. The comparison of the IRSL ages quoted by Rousseau et al., 2011 (Clim. Past. 7, 221-234) with the different global ice-core scales available, especially with the nowadays golden standard NGRIP_GICC05 presented by Kadereit and Wagner looks very convincing, regardless of the fact that the authors argue themselves that “it does not seem wise to obtrude marine and ice-core schemes onto terrestrial archives”. Also, the authors argue that the inherent weakness in event stratigraphy between loess paleosol records lies in the visual correlation of records and recommend “ a priori placement of time markers and the application of computer-aided time-series analyses” in order to overcome this limitation. The reviewer agrees well with these statements as well as the general logic of the manuscript. At the same time however, the reviewer strongly beliefs that both manuscripts share a common weakness, namely the lack of well dated absolute markers. Thus, my major comment regards the IRSL chronology itself. The IRSL ages presented by Rousseau et al., 2011 (Clim. Past. 7, 221-234), may in the reviewer‘s opinions held more tribulations than pointed out by Kadereit and Wagner in the present manuscript. The reviewer does not intend to infer that applying MAAD – IRSL techniques, which are considered obsolete, should lead to incorrect ages. These techniques may lead to correct ages, but form the information available in Rousseau et al., 2011 (Clim. Past. 7, 221-234), one could never tell whether this is the case or not. Many questions could be raised regarding the luminescence methodology implied by that study, out of which the most important addresses whether fading tests have been carried out or not. If these samples have not been monitored for fading, than, the reviewer highly doubts they are the most reliable chronometric information attainable. Anomalous fading may not be ubiquitous, but is certainly very common, and highly likely when working on a polymineral fraction. The fact that the study of Lomax et al., 2013, QI in press, presents ages obtained without a fading correction us-

CPD
9, C2568–C2571, 2013

CC BY

ing IRSL that fit within error limits the ages obtained using SAR-OSL on fine quartz at Krems-Wachtberg is under no circumstances an argument solid enough to be applied to Stayky. It should be taken into account that even in the study of Lomax et al., 2013, QI in press, an age underestimation can be noticed, while Schmidt at al., 2010 (QG 5, 137-142) and Stevens et al., 2011 (QSR 30, 662-681) present significant fading rates for polymineral fine grains of Serbian loess (g2days around 3 %/decade). In the case of Romanian loess Vasiliniuc et al., 2012 (QI 293, 15-21) calculated an average (on 71 aliquots) g2days of about 4% per decade for IRSL signals of polymineral fine grains. In the reviewer's opinion, the argument presented by Kadereit that "it would need a severe underestimation of at least ca 2.5 ka. . .or even 5.5 ka (1 sigma error bar) to justify on grounds of chronometry a matching of the Vytachiv Soil with GIS 8. Hypothesizing such a large age-underestimation would outrange the . . .results at Krems-Wachtberg" is not solid enough. For an age of 30 ka, a correction of 5 ka can be easily obtained in the case of a fading rate even lower than the usually reported 2-4% per decade on loess studies in Central and Eastern Europe (see for example Schmidt at al., 2010 (QG 5, 137-142), Stevens et al. 2011, (QSR 30, 662-681), Thiel et al., 2011 (QI 234, 23-31), Vasiliniuc et al., 2012 (QI 293, 15-21) as well as China Buylaert et al., 2007 (QG 2, 9-14). Indeed, Lomax et al., 2013, QI in press, comment that the magnitude of the observed fading can depend on the detection window, although they admit that this deserves more investigation in order to be proven. Unfortunately, one can only guess the detection window employed in the IRSL investigations of Rousseau et al., 2011 (Clim. Past. 7, 221-234). I think that Kadereit and Wagner should take this information into consideration, and discuss this possibility in more detail. Also, I suggest that the authors present a stratigraphic diagram of Stayky section along with Nussloch and maybe Schwalbenberg II as well. It is very difficult for a reader that is not very well familiarized to the particular names of all the pedostatigraphic markers mentioned to follow the text without such a diagram. All in all, the present article brings an important contribution in correlating loess-palaeosol section in Western and Eastern Europe and presents a solid analysis of the possibilities of correlating rapid climatic events recorded

CPD
9, C2568–C2571, 2013

CC BY

by marine and ice cores to the events recorded in the terrestrial archives. For the interpretation of Kadereit and Wagner to be confirmed, the section needs to be sampled at a better resolution and a more solid chronology needs to be provided. Loess could be used, under favorable circumstances to reconstruct synoptic-scale paleoclimatology over millennial timescales, but in order to do this a reliable and as precise as possible chronology needs to be available. In the case of luminescence, this should imply in the reviewer's opinion multiple techniques. A combination of quartz and feldspar dating, on multiple grain sizes that would hopefully yield consistent results would lead to final ages where the error could be reduced (hopefully to allow resolving the millennial Dansgaard-Oescher cycles), considering that these different grain sizes and minerals are coeval (Aitken 85, Appendix B). Although this is time consuming, and might prove difficult, as it can be seen in Vasiliniuc et al., 2012, (QG 10, 75-80), Constantin et al. 2013 (QI, in press), Youn et al., 2013 (Catena in press), this approach should, at least in principle work for such young samples as this study concerns (<40ka). Unless such a chronology will be provided in future studies, the reader will remain entitled to wonder whether the interpretation in Rousseau et al., 2011 (Clim. Past. 7, 221-234), happens to actually give the right answer for wrong reasons (i.e. lack of consideration of anomalous fading).

Interactive comment on Clim. Past Discuss., 9, 2629, 2013.

CC BY