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Interactive Comment

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 #1

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The criticism of Rousseau et al. (2011) that Kadereit and Wagner raise with this paper links in to a wider fundamental disagreement over the interpretation of loess deposits. The question of the proper use of an independently derived radiometric timescale for loess proxy records, and perhaps more controversially, the precision and accuracy of that timescale, is often addressed in very different ways between different groups of authors. This paper is a welcome contribution to that debate and makes some very important points with specific regard to correlations between European sequences and the North Atlantic. The paper raises some important questions regarding a) the use of known-age marker horizons or dated levels to correlate sequences and b) the degree to which precise age information can be interpolated between those known age points.

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In essence the criticism of the Rousseau et al. (2011) age model from Stayky in the Ukraine lies in the former, but the paper also comments on the precision aspect too.

Generally the paper is quite well written but at points the paper could be easier to follow as it misses some basic information. For example in the Introduction it mentions the Lohne Soil and the likely stratigraphic equivalents throughout Europe as terminating the ‘lower section’, but does not mention what this ‘lower section’ is. No stratigraphic diagram is shown so we need to look at the Rousseau et al. (2011) paper to see where this lies in the site in question. The paper really needs a diagram showing the different stratigraphies of the key sites mentioned and summarising the names of the soil units in the different places. It is all too easy to get lost in the sea of different, country or even site-specific names for different soils! The devil is in the detail but the detail of the soil names and age assignments can get hard to follow without a summary diagram to help.

On the whole I agree with most of the findings of this paper. However, I comment below that in some of the detail I would apply a slightly different approach and have a different view over some of the theoretic assumptions. Firstly, I am not necessarily convinced that the soils mentioned above may be pedostratigraphic marker horizons across Europe, not unless they have been dated as the same age using radiometric means, although this is a point made by the authors. It is far too easy to miss-assign equivalents between soils across climatic transects, as the previous debates on the ages of soils in both Hungary and Serbia demonstrate (Marković et al., 2011 QSR 30, 1142-1154; Novothny et al 2008, QI 198, 62-76). As both the Lohne and Vytachiv soils have been dated here this is more a wider, philosophical point but it is important to note. There is also a significant assumption that the changes in stratigraphy above the Lohne Soils (and equivalents) correspond to millennial-scale events linked to Dansgaard-Oeschger events/Greenland Interstadials of 1500 yr frequency. This is an assumption also made in Rousseau et al. (2011). However, other authors (e.g., Stevens et al., 2008 Geology 36, 415-418; Schmidt et al., 2010 QG 5, 137-142; Újvári

CPD
9, C1451–C1456, 2013

et al.,2011, QSR 29, 3157-3166; Schatz et al., 2012; QG 10, 68-74) have been less eager to make this assumption in loess work because generally, independent dating does not have the precision, nor is applied at high enough sampling resolution, to resolve these cycles in loess deposits. As such, it is equally plausible that these soils represent changes in local conditions (drainage, sediment accumulation, vegetation, microclimate etc) that are unrelated to D-O or GIS events but that have approximately the same frequency. This was rather eloquently expressed in Wunsch (2006 QR 65, 191-203) for all climate archives. That said, it does seem reasonable, especially at sites more proximal to the North Atlantic such as Nussloch, that the loess sites will experience climate modulation via D-O/GIS events, but the landscape, depositional, pedogenic and soil hydrological responses to these influences may not be straight forward. With a precision of 5-10%, luminescence dating does not allow resolution of 1.5 kyr cycles as at 30-20 ka 1σ errors will be 1-6 kyrs. Indeed, the four dates in question here (those published in Rousseau et al. 2011) have errors of between 3.1 and 1.6 kyrs. In fact, there are two groups of dates that overlap within errors. These bracket approximately 10 kyrs of loess deposition, allow it is not clear how accumulation will have progressed between these intervals as numerous studies have shown variable accumulation rates when dating loess sequences at high sampling resolution of 10-50 cm (Stevens and Lu, 2009; Sedimentology 56, 911-934; Buylaert et al., 2007; QG 3, 99-113; Lai, 2010 J. of A. Earth-sci, 37, 176-185; Stevens et al., 2011 etc) Other studies have shown gaps in loess records (Lu et al., 2006 CSB 51, 2253-2259 Buylaert et al., 2007; Zhu et al., 2007 GRL 34, L17306). All this means that resolution of the precise age of millennial scale events and their relationship to North Atlantic events is difficult, and that interpolation of even broad scale changes in loess climate proxies and soil ages between known-age dated horizons is fraught with difficulties. This is especially the case where there are relatively few published independent dates (as here) and arguably the only way to attempt to confirm the validity of precise age between independent ages is via much higher sampling resolution dating. This is not to say that the assumptions presented both here and in Rousseau et al. (2011) are unfounded, but that they are quite significant assumptions, given the limited number of dates used and the relatively poor precision of those dates.

Where this paper is innovative and significantly departs from Rousseau et al. (2011) is that it makes the point that significant errors can occur in assignment of soils to climate events if chronologies at key sections used as reference points are incorrect. Based on a reinterpretation of the chronology of the Nussloch site (that is used in Rousseau et al., 2011 to assign an age model to the Stayky profile in the Ukraine) presented in Kadereit et al. (2013), the authors reinvestigate the chronology at Starky. They propose that the ages presented in Rousseau et al. (2011) do not support that paper's ultimate chronological interpretation of the site. I completely agree with the authors that event stratigraphic approaches require a good, independent chronological basis, for the reasons the authors state in the manuscript. They also point out inconsistencies in the presentation of the age-data in Rousseau et al. (2011), and I would further add that there is little information about whether these samples showed anomalous fading and how, if at all, this was corrected for, the basis for the choice of the IRSL protocols used, and the specific criteria used to pick ages for presentation of the age model (see below for more on this). However, the key point here is that using the most up to date Greenland age models for the timing of GIS events, the start and end points of the age model or Stayky presented in Rousseau et al. (2011) are inconsistent with the independent ages published in that same paper. This conclusion is well expressed in Figure 2 and from the data shown, to my mind it is hard to disagree with Kadereit and Wagner's conclusions. This is an extremely important finding as it undermines many previous chronostratigraphic interpretations of loess sequences.

However, I would also argue that while Kadereit and Wagner are in my mind entirely correct to point out that the published IRSL ages point to a different age model to that presented in Rousseau et al. (2011) I am more cautious in accepting the fine detail of the newly proposed ages of the soils and grain-size changes at Stayky. As the authors point out, the IRSL ages are not sufficiently resolved to allow assigning soils to GIS or

CPD
9, C1451–C1456, 2013

other events (a glance at the error bars on the ages in Fig. 2 is sufficient to demonstrate this), and it is unwise to assume that the marine and terrestrial records show the same detailed characteristics. The authors then must rely on counting the soils and tentative stratigraphic supporting evidence, e.g., from pollen, to assign a detailed new timescale to the ages of the embryonic soils. As stated above, due to the vagaries of loess deposition this is extremely difficult to do, and much of this is indeed pointed out by the authors. It is also important to point out here that the IRSL dates presented in Rousseau et al. (2011) themselves may not be reliable, a possibility considered but largely discounted by Kadereit and Wagner. The paper mentions briefly the possible shortcomings of dating of loess using IRSL and points out that no information is given in Rousseau et al. (2011) on possible anomalous fading that is often seen in IRSL signals (Auclair et al., 2003, RM 37, 487-492). This would lead to significant age underestimation if not adequately corrected for (see Roberts 2008, Boreas 37, 483-507 for summary). Furthermore, the multi-aliquot methods used in Rousseau et al. (2011) are now largely considered obsolete in luminescence dating of loess, often replaced by single-aliquot regenerative (SAR) protocols (c.f. Murray and Wintle, 2000, RM 32, 57-73) that have greater precision. Dates on quartz using the SAR protocols are generally considered the quality standard in luminescence dating, at least until 40-50 ka (Roberts, 2009; Buylaert et al., 2007). It is not clear why these methods were not used at Stayky in Rousseau et al. (2011). A further problem is that no independent checks or information on the signals or choice of parameters in the protocols used in Rousseau et al. (2011) is presented in that paper. This is regarded as a standard for luminescence dating (Murray and Wintle, 2000; Roberts, 2009) and as such the reliability of the IRSL ages cannot be determined fully. I would therefore urge caution in making detailed age models from the dates published in Rousseau et al. (2011) as there is not sufficient detail given to judge their accuracy, not to mention the errors in the presentation of the data in the original paper. As such, while I still think that the contribution here is important, I would urge the authors to be even more tentative in their age model assignment from the published dates. I would also urge the authors to sample the site at high resolution and date using more widely accepted SAR protocols on quartz, as mentioned has been done by Lomax et al. (in press) for Krems, with published quality control checks. The authors do note this and also state it as desirable for Nussloch. Indeed, this could form the basis of a detailed age model from which a more robust age assignment of soils could be made, notwithstanding the limitations imposed from precision on luminescence dates. However, I would not rely on the overlap within errors between uncorrected IRSL and OSL ages at Krems to justify the likely accuracy of the IRSL ages at Stayky. Indeed, Stevens et al. (2011) and Vasiliniuc et al. (2013 QI 293, 15-21) by contrast show that uncorrected IRSL ages significantly underestimate quartz OSL ages in Serbia and Romania.

Despite these issues I completely agree with the authors' general conclusions that fine-scale correlations between loess sites should be considered as working hypotheses and that the age model of Rousseau et al. (2011) requires revision. I would just argue that more of a sound chronometric basis than the four IRSL ages is needed for any working hypothesis. Nonetheless, I consider that this paper is a very important contribution as it raises significant question marks over approaches used to consider millennial scale climate variation in loess sequences.

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