

## ***Interactive comment on “North Atlantic abrupt climatic events of the Last Glacial period recorded in Ukrainian loess deposits” by D.-D. Rousseau et al.***

**D.-D. Rousseau et al.**

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We acknowledge the comments of reviewer 1 who we thanks for pointing elements of the text that required to be clarified

Reviewer 1 –p. 1959, title: suggest changing to "Abrupt climatic events of the North Atlantic in the last glacial period may be recorded in Ukrainian loess deposits"

R yes, done

–p. 1961, introduction: in commenting on European loess studies, I suggest citing three recent review papers: Frechen et al., 2003, QSR; Rousseau et al., 2007, EQS;

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Zoller, 2010, CEJG.

R yes done

–p. 1962, line 4: is Smiley et al. (1991) really the best reference here??

R yes as this is the paper in which Kukla is presenting the correlations between Chinese, European and USA loess series

–p. 1964, lines 3-6: this is not always the case: note that in Siberia and Alaska, MS measurements show precisely the opposite trend. I suggest qualifying this statement accordingly.

R In the text we indicated that this is generally the case and refer to Kukla et al 1988. Of course we do know that in some regions like Alaska, Siberia and New Zealand, this the contrary due to the enrichment of the sediment with volcanic particles.

–p. 1965, line 17: readers do not yet know what the Bug loess is: either explain here, or wait to refer to it until after you have explained what it is.

R we followed R1's suggestion by removing Bug from the text

–p. 1965, line 21 and elsewhere in the manuscript (especially right below, on p. 1966): where you say "allocated to," I think you mean to say "correlated to" or "assigned to."

R this is correct, we changed to "assigned to" sorry for the Frenghish

–p. 1965, line 23: upper case "M" on "Mollisol", unless you intend to mean a "mollic" horizon, which is lower case "m", but is "mollic", not "mollisol."

R ok we changed to "mollic"

–p. 1966, lines 16-18: what is the basis for interpreting these as "tundra gley" soils? If they are grey colors, please give the Munsell soil colors you see, such that readers can tell what you mean by "grayish matrix", for example, you should see 2.5Y or 5Y hues, with relatively high chromas and intermediate-level values if they are truly gleyed. Even

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if they ARE gleyed, how do you know you had a tundra vegetation? No E horizons, therefore not boreal forest?

R This description fits with the general stratigraphy proposed for the last Pleniglacial in the Kyiv area based on the stratigraphical, pollen and mollusk analysis of 23 sequences described in Gerasimenko and Rousseau (2008). Quaternaire, 19: 293-307 to which we refer in the revised text

–p. 1968, lines 7-28: these interpretations all assume you have only a single loess source: do you know that for certain?

R We did not make this assumption. We are describing grain size stratigraphic zones, discussing possible related wind intensity changes, but our discussion of grain size variations does not involve the assumption of a unique source area.

–p. 1969, lines 21-27: I agree that the sand content is very low, but you do have measurable amounts of it. If winds were from the west during all climate phases (stadial, interstadial, and Heinrich events), and if the sand is from the Dnieper River (to the east of Stayky), how do you explain it? Rare easterly winds? What would cause them throughout the whole glacial period?

R The wind directions shown in fig. 8 are annual means (calculated on 6-hourly data from 20 years of simulation for each of the tree climate states: GS, HE and GIS). They indicate that winds were predominantly – but not exclusively! – from the west. Certainly, the wind was blowing at times from other directions, among which the east, bringing coarse material from the Dnieper valley to our studied site. An analysis of the synoptic conditions corresponding to the different wind directions at Stayky is beyond the purpose of the present manuscript.

–p. 1970, lines 10-22: I am a bit confused by the GSI as presented here. In Antoine et al. (2001), there is nothing on the GSI, although that paper IS about Nussloch. The GSI ratio IS, however, in Rousseau et al., 2002, QSR, which is also on Nussloch, so that

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needs to be fixed. Nevertheless, in Rousseau et al., 2002, the GSI ratio is between  $50-20\mu\text{m}/<20\mu\text{m}$ . In lines 13-14 of p. 1970, it is said that the GSI ratio is between  $52.6-26\mu\text{m}/<26\mu\text{m}$ , but in your figure, it is given as  $63-20.7\mu\text{m}/<20.7\mu\text{m}$ . All this needs to get fixed for consistency.

R Concerning Antoine et al 2001, you are right and the reference was removed. The confusion relies on the fact that the 2002 paper was using measurement performed classically with a pipet while the new measurements have been performed since then with a laser coulter which is not providing exactly the same grain size fractions.

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Interactive comment on Clim. Past Discuss., 6, 1959, 2010.

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