Review of „Paleo±Dust: Quantifying uncertainty in paleo-dust deposition across archive types" Cosentino et al.

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In recent years, there has been a renaissance in the field of spatio-temporal reconstruction of sediment mass accumulation rates (MAR), probably due to the fact that atmospheric dust does indeed play a significant role in shaping Earth's climate, in both direct and indirect ways. Yet there are considerable uncertainties about the actual role of dust that need to be clarified in order to better understand how the Earth system works under present-day or past (e.g. glacial) climate conditions. Research on MAR reconstructions is taking several directions: 1) synthesis of new data from one or more type(s) of paleo-dust archives, 2) methodological innovations in the calculation of MAR, 3) database construction, and/or 4) focusing on periods that have not been included in reconstructions so far (e.g. Marine Isotope Stage 4-5-6). As such, a new database called ChronoLoess (Bosq et al.) is currently appearing in the ESSD pages, which can be extended in a standardised format. In contrast to ChronoLoess, which focuses on only one archive, the authors of this manuscript have compiled data from a large number of paleodust archives of different types (marine/terrestrial) in the Paleo±Dust database, following a common methodology. I consider the most important advance of the present paper to be that they have discussed the uncertainties associated with the dust mass accumulation rate (DMAR) calculations of each archive type and attempted to estimate them quantitatively, which in many cases is not a simple task. In addition, paleo-dust flux estimates from peat bog cores are included in the paleodust compilation for the first time.
Having read the manuscript, I find it to be neatly written, logical and easy to follow, and I consider it to be a significant step forward in the reconstruction and comparison of LGM and Holocene dust fluxes. The authors have tried to organise the database in a way that it can be used for future Earth system modelling, which is also a positive aspect. I have, of course, some critical comments on the manuscript, which I write about below (and in the annotated pdf file attached), but these do not affect the conclusions to any significant extent and are rather just a further refinement of the methodology. On the whole, the manuscript, with some modifications, can be published in ESSD.

Line-by-line comments

Line 97, εDMAR: This is just a pedantic point, but the error of a quantity is usually denoted by the Greek sigma in statistics, I have never seen the use of epsilon for that purpose. What is the reason for using epsilon here? There is another reason of why I discourage the authors using epsilon: in dust provenance studies the Nd and Hf isotope compositions are often expressed in the epsilon notation (εNd, εHf), so this may potentially cause some misunderstandings.

Lines 98-100: The authors presented uncertainty estimations for MAR and MAR10, which disregard potential correlations between the variables in the MAR equations, so these are not full statistical treatments. I suggest the error propagations below.

Let us rewrite the DMAR equation: $DMAR = \frac{h_{thick} \times DBD \times EC}{t_{bottom} - t_{top}} = \frac{h_{thick} \times DBD \times EC}{\Delta t} = \frac{x \times y \times z}{\epsilon}$ $(\epsilon = \Delta t = t_{bottom} - t_{top} = t_b - t_t)$

The errors (or variances, or standard deviations) of x, y, z and ϵ ($\sigma_x^2 = s_{h_{thick}}^2$, $\sigma_y^2 = s_{DBD}^2$, $\sigma_x^2 = s_{EC}^2$, $\sigma_\epsilon^2 = s_{\Delta t}^2$) are known. Obviously, x (h_{thick})and ϵ ($\Delta t = t_{bottom} - t_{top}$) are correlated ($C_{x\epsilon} \neq 0$, $C_{x\epsilon}$ is the covariance of x and ϵ, and $C_{x\epsilon} = \varrho_{x\epsilon}\sigma_x\sigma_\epsilon$, where $\varrho_{x\epsilon}$ is correlation of x and ϵ), while no correlation exists between the other variables: $C_{xy} = C_{yz} = C_{y\epsilon} = C_{z\epsilon} = 0$. DBD and EC may be correlated, this can be a matter of debate, but we suppose zero correlation here. With this, the uncertainty of DMAR is given by:

$$\sigma_{DMAR}^2 = f^* \times M \times f$$

, with $f^* = \left(\frac{\partial_{DMAR}}{\partial x}, \frac{\partial_{DMAR}}{\partial y}, \frac{\partial_{DMAR}}{\partial z}, \frac{\partial_{DMAR}}{\partial \epsilon}\right)$, $f = \left(\frac{yz}{\epsilon}, \frac{xz}{\epsilon}, \frac{xy}{\epsilon}, -\frac{xyz}{\epsilon^2}\right)$, $M = \begin{bmatrix} \sigma_x^2 & C_{xy} & C_{xz} & C_{x\epsilon} \\ C_{yx} & \sigma_y^2 & C_{yz} & C_{y\epsilon} \\ C_{zx} & C_{zy} & \sigma_z^2 & C_{z\epsilon} \\ C_{\epsilon x} & C_{\epsilon y} & C_{\epsilon z} & \sigma_\epsilon^2 \end{bmatrix}$

$$\sigma_{DMAR}^2 = \left(\frac{yz}{\epsilon}, \frac{xz}{\epsilon}, \frac{xy}{\epsilon}, -\frac{xyz}{\epsilon^2}\right) \begin{bmatrix} \sigma_x^2 & 0 & 0 & C_{x\epsilon} \\ 0 & \sigma_y^2 & 0 & 0 \\ 0 & 0 & \sigma_z^2 & 0 \\ C_{\epsilon x} & 0 & 0 & \sigma_\epsilon^2 \end{bmatrix} \begin{bmatrix} \frac{yz}{\epsilon} \\ \frac{xz}{\epsilon} \\ \frac{xy}{\epsilon} \\ -\frac{xyz}{\epsilon^2} \end{bmatrix}$$

$$\sigma_{DMAR}^2 = \left(\frac{yz}{\epsilon}\right)^2 \sigma_x^2 + \left(\frac{xz}{\epsilon}\right)^2 \sigma_y^2 + \left(\frac{xy}{\epsilon}\right)^2 \sigma_z^2 + \left(\frac{xyz}{\epsilon^2}\right)^2 \sigma_\epsilon^2 - 2\frac{xy^2z^2}{\epsilon^3} C_{x\epsilon}$$

It is worth testing the differences between the outcomes of the two error propagation equations and deciding whether the original one is retained or the latter one proposed here.

Line 131: It is worthwile adding that this relation is true if the bottom/top ages of a segment is given as negative ages in the past calculating from e.g 1950 BP, or b2k. If the ages are positive as usually used (e. g. t_{bottom}=23500 years, t_{top}=21000), then this does not hold. This can be easily fixed by changing the direction of the inequality signs.

Lines 236-246: The authors use a dry bulk density (DBD) value of 1.48 g/cm^3 for CLP loess, while 1.45 for others. For European loess, DBD is relatively well-defined (at least for East Central Europe, ECE): Újvári et al. (2010) [Quat.Sci.Rev. 29, 3157-3166] published a DRD value of 1.497+-0.079 g/cm^3 derived from 6 test sample measurements (2 methods), which was later confirmed by e.g. Peric et al. (2020) [Boreas 49, 841-857]. So, this value should be used for the ECE loess sites, and perhaps for others as well considering how close this is to the CLP loess DBD value. I suggest mentioning these studies here, just to provide a less CLP-biased overview of the topic.

Line 381, SBMAR equation: Why is this Eq (1)? We have seen several equations before in this manuscript. Those must be numbered and indicated as well, in my view.

14/09/2023
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