

Interactive comment on “The reconstruction of paleo wind directions for the Eifel region (Central Europe) during the period 40.3–12.9 ka BP” by S. Dietrich and K. Seelos

D.-D. Rousseau (Editor)

denis.rousseau@lmd.ens.fr

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Dear colleagues, The manuscript that you submitted to Climate of the Past got 2 of the expected 4 reviews I was waiting for but received also two comments from CP readers.

The two reviewers are raising comments and concerns that require clarification and improvement and referee #2 request serious corrections that I ask you to consider. The two comments from CP readers also raise points that I ask you to take into account in order to improve the quality of your manuscript before publication in Climate of the Past. On my side I have several points that I would like you to consider also.

First of all, there are papers indicated in the manuscript as in press or submitted. What

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is their present status and contrary to the submitted status, I would prefer evaluated data.

In the introduction of the paper, you refer to climate model resolutions, which better value seems to be 2°. Please consider the recent paper by Sima et al 2009 in which the LMDZ model is used with its zooming module over Europe which increases considerably the resolution, contrary to that obtained with ECHAM and GENESIS.

I would also suggest moving the first paragraph of the Lithology description, i.e. the location of the studied core on top of the methods. Still in the methods, you indicate that the sample interval rather corresponds to a mean time resolution of 4 months. What is the error? Can the end of the method paragraph be related to Fig. 2a?

In the 2.1 lithology, you mention that the age model is tuned to pollen zones. Can you explain?

Also on lines 171 and 172, "The GIS-2 (19-21m depth) with a certain amount". As this is not that much evident on figure 3 because of a relatively tiny value, can you much more precise than saying "certain"?

At the end of the 2.2 part, you indicate, "If the climate situation changes to warm and wet conditions, the content of dust decreases at the same time." Following what was said before, if warm and wet conditions, do you notice any aggregation of the grains that the image processing could consider as coarser material?

Results Lines 263- 265. "Thus there is only little accumulation of dust in the Eifel region. H4 (39.4-37.2 ka BP) obviously has no significant. Why obviously? H4 is the largest of the six H events during the last glacial period and it would be obvious to record it don't you think so? Can you comment on that?

Some lines later, why invoking the Laurentide Ice Sheet "g"rowth? What is the reference you refer to?

Lines 271-274 "thereupon. . .east wind activity is moderate". You always interpret in

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term of wind strength but is it the strength or the available source region, which releases more material or both?

Lines 280-283. So in fact you have east winds during both interstadials and stadials? How do you explain?

Lines 285-288. You are addressing the storm regimes and more especially the combination of the frequency of the storms and their strength. If you refer to two main contributor of dust in Asia, Takla Makan and Gobi deserts do not show the same behavior with the former having more frequent dust storms but less strong than in the latter where the frequency is comparatively lower. Interesting also is that you get high values in organics during the very short interstadial 2 between 22 and 21. ka BP, especially when comparing with longer ones.

Line 317, "During GIS-2, the wind system is stable. What is a stable wind system?"

Line 320 "after the H2 event", referring to the figure provided it seems rather after the start of H2.

From a general point of view, I feel uncomfortable with your manuscript because you are using both marine and ice-cores terminology to your own record. And could be the origin of confusion. H events are indeed only marine events and not at all recorded in the ice-cores. . Just consider reading the first sentence of part 3.1: "both, the Greenland stadials (GS) before and after the Heinrich-3 (30.2-28.7 ka BP) are characterized by the highest number of east winds during MIS2".

Line 360-361. Katabatic winds are particular phenomenon requiring various conditions that a single assumption as given in the manuscript does not help. If addressing this issue, please provide more information.

Concerning Fig. 1, the elevation is not that easy to read Can it be possible to plot the elevation in a second companion figure 1b? This is thus an interesting paper which however requires some more work to fulfill CP requirements for final publications. I

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guess you have enough material in order to revise your manuscript and make it available for final publication. All the very best Denis-Didier Rousseau

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