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Climate of the Past Discussions

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

Anonymous Referee #2

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The authors present a high-resolution sedimentological record from a maar in central Europe. From this record they infer changes in easterly wind activity during MIS 3. Due to its temporal resolution and location the record has a great potential to serve as a yardstick for testing both, climate models and our understanding of regional atmospheric dynamics during glacial times. The manuscript is in principle well suited for CP. However, there are a number of major issues with the current manuscript that should to be addressed before the manuscript can be considered for publication.

(1) The calibration of the algorithm was done in the range 10-90 % (Fig. 2b). In contrast, most reconstructed values lie well below 10 %. How confident are the authors

that they are really measuring a signal and not noise (it seems plausible to assume that the signal-to-noise ratio should decrease as the carbonate content drops)? The ms. lacks a critical discussion of this issue (and the underlying extrapolation beyond the range of the linear regression model). The ms. would also greatly benefit from an error estimate for the reconstructed carbonate concentrations. I know that this is not straightforward. However, the authors should at least provide an estimate based on the uncertainty resulting from the calibration (and extrapolation if it cannot be avoided).

(2) Using 100-year bins, the frequency of easterly wind is discussed. The ms. would greatly benefit if the corresponding data would be graphically presented (e.g., it should be possible to read off the 160 storm events mentioned on p. 2166, line 26 directly from a graph). The ms. also lacks a statistical quantification of the mentioned differences in storm-event occurrences.

(3) The authors should be more careful in using the phrase "paleo wind direction". It is somewhat misleading since the only inference that can be drawn from the data is on the frequency of *easterly* winds (of a certain minimum strength), which is not the same as *direction* (as the title suggests). Basically the proxy record is binary (on/off) and the key information is on the frequency (unless the authors can convert the percentages into wind strength – but I guess this is far from being trivial). This should be clarified throughput the ms.

(4) The discussion of the east-wind events in a climatological context is not really consistent with the data. For example, increased frequencies during H1 and H2 are not visible in Fig. 3 (there is a peak at the *end* of H1, while during H2 a max. can be seen at the *beginning*). Moreover, none of the Heinrich stadials stands out in terms of variability (I think this an important result, which needs to be corroborated by some statistics).

(5) The presentation lacks clarity in many sections (pose a clear question at the outset; don't mix results with discussion). Although I'm not a native speaker, I have the

impression that language needs considerable improvement.

Minor comments:

- the authors should use the updated GICC05 timescales instead of ss09sea

- description of the method is generally difficult to follow (e.g. p. 2162, line 9, p. 2163, line 25ff)

- non-geologists will benefit if the carbonate outcrops in Fig. 1 will be clearly marked

- Fig. 3 and 4 are too small

- Use "point" as decimal separator (Fig. 2-4)

Interactive comment on Clim. Past Discuss., 5, 2157, 2009.