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Climate of the Past Discussions

Interactive comment on "Modeling dust emission response to MIS 3 millennial climate variations from the perspective of East European loess deposits" *by* A. Sima et al.

Anonymous Referee #3

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The interesting study by Sima et al. uses a climate modeling approach to better understand loess deposits in eastern Europe. The study is very useful in terms of placing eastern European loess records in a larger climatological context. The manuscript is based on the same model setup and boundary conditions as the earlier study by Sima et al. (2009), but focuses on a slightly different region. Therefore some of the conclusions in this ms. (e.g. importance of seasonality and vegetation) can already be found in the earlier study. Taking into account that the manuscript is a regional extension of the earlier study I am surprised by the somewhat lengthy presentation. Apart from the regional extension (which is logical given the loess sequence) the manuscript adds only little to the insights provided already in the earlier study.

I am also concerned with regard to the boundary conditions of the model, which are a mixture of glacial boundary conditions (partly outdated: ice sheet configuration), including somewhat arbitrary SST anomalies in the North Atlantic to represent a Heinrich stadial. As the authors mention there are several simulations with coupled ocean atmosphere models available for MIS 3 and it is surprising that the current study does not make use of this information.

As mentioned above the study is of clear interest from a regional paleoclimatological perspective and certainly deserves to be published. However, I would strongly recommend to condense the manuscript substantially (specifically, introduction, results and conclusions). The discussion reads to some extent as an excuse for the use of a mixture of boundary conditions. However I do not find the line of argument very convincing with regard to having a setup that is representative of MIS 3. To me it would be more logical to present the study as a sensitivity experiment for some mixture of glacial boundary conditions (that were available at the time their model experiment was been conducted).

Minor comments: p. 145, l. 9: Testing a correlation should be rephrased since it implies a statistical test.

p. 145, l. 14: Consider replacing H-events by H-stadials throughout the text because it is the cold phase and not the IRD input that determines the dust deposition anomalies.

p. 149, l. 18: F is not defined.

p. 150, l. 19: Based on Sima et al. (2009) it is surprising that annual mean values are analyzed in the first place given that the seasonality was found to be very important in that study.

p. 151, l. 22: It is interesting that the margin of the Fennoscandian ice sheet is discussed in this context. Is this also reflected in the ICE-4G reconstruction?

p. 154, l. 22: Avoid “a bit lower”.

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p. 168, l. 17: Replace hypotheses by assumptions.

Fig. 1: Mention in caption that blue/gray colors indicate elevation. This and most other figures: axis titles for latitude and longitude are missing.

Fig. 6: Panels are much too small.

Fig. 7/Fig. 8: Indicate statistical significance of differences.

Interactive comment on Clim. Past Discuss., 9, 143, 2013.