Referee comment on “A mechanism for dust-induced destabilization of glacial climates” by B. F. Farrell and D. S. Abbot

The paper presents the hypothesis of a mechanism that might explain the abrupt change in climate states between stadial and interstadial periods during glaciations, as recorded by paleoclimate archives. The authors' approach makes use of a climate model (SCAM) to reproduce the magnitude of the dust cycle under varying dust loads deduced from observations, and point to the modeled dust-induced changes in the atmospheric stability and the hydrological cycle as the possible drivers for such climate state changes, and advance some speculation on possible feedback mechanisms involving dust. Also, the authors use a stochastic model to simulate a time series showing the transitions between climate states and their timing.

In my opinion the idea behind this paper is very interesting, as well as the general approach. Nonetheless, I see two major potential issues that may flaw the results and interpretations of this work. First, as pointed out by N. G. Heavens, the authors should either clarify if and how they are considering dust-longwave interactions or include them in their simulations. Given the well-suited purpose of analyzing the effect of dust on the radiative balance and the hydrological cycle and atmospheric stability, a full representation of dust optical properties would be ideally included. If it was not feasible to include this mechanism, this should be stated clearly and taken into account throughout the discussion. Second point, the choice of the range of values for the “dust factor” should be more accurate, or at least the authors should be more cautious in their assumptions on the magnitude of the variations in the dust cycle. I would say that an extensive comparison with observations (e.g. Kohfeld and Harrison, 2001; Maher et al., 2010) in order to evaluate the reliability of the proposed range is missing.
In synthesis, I found that the rationale behind this work is very interesting and noteworthy, and I think it is worth exploring following the authors' approach, but the work in its present form would clearly benefit from a refinement of both the model setup (or documentation, if the setup already included dust-longwave interactions) and the use of observations to determine realistically the plausible range for the “dust factor”. I would definitely encourage the authors to follow on their effort to test their hypothesis.

Specific comments

p. 1723, l.1-3: The text “Greenland ice cores...” and reference “Lambert et al., 2008” don't match, please adjust.

p. 1723, l. 3-5: “atmospheric dust co-varies with precipitation on the same time scale”. Precipitation where? Please clarify

p. 1723, l. 7-11: it seems rather unclear which the link between these sentences, and the relation they have in the context of the paragraph, as they refer to disparate time scales than the main discussion

p. 1723, l. 11-15: yes it may be consistent, but is it the only factor contributing?

p. 1724, l. 26 – p. 1725, l. 4: please deal somehow with the dust-longwave interactions “problem”

p. 1725, l. 1-2: please invert the upper and bottom panels in Figure 1, according to the order of appearance in the text

p. 1725, l. 12-16: A 2-8 increase in dust input into the atmosphere cannot be simply labeled as laying "well within the the range of estimated dust source increase seen in the proxy records during cold Pleistocene periods". This statement is at least incomplete for a few reasons, which would contribute to "cut down" the estimate proposed by the authors. In particular, there is information on the spatial variability of those estimates, which show how the magnitude of polar ice cores ratio may not be extrapolated to represent a global average.
First, as also discussed in Fuhrer et al. (1999), and reprised from different perspectives e.g. in Fischer et al. (2007) or Mahowald et al. (2011), there is a debate whether concentration or deposition flux in ice cores is the better parameter to describe the glacial/interglacial or stadial/interstadial variations. While the concentration may be the best option for Greenland, where wet deposition of dust dominates, this does not hold for Antarctica (EDC: EPICA Dome C), where dry deposition is thought to dominate (Wolff et al., 2006), which implies that deposition flux is the best parameter, with peak ratios of ~25 in the LGM/Holocene comparison.
Second, in Antarctica the stadial/interstadial variability is much less reduced compared to both glacial/interglacial ratios, and compared to Greenland: compare Fuhrer et al. (1999) and Lambert et al. (2008).
Third, compilations of dust deposits around the globe in the LGM/Holocene comparison (Kohfeld and Harrison, 2001; Maher et al., 2010) do not compare directly to the stadial/interstadial ratio, but still clearly show that on average the LGM/Holocene ratio globally is about 2-4, with places showing a ratio close to 1. The peculiarly high ratio shown by polar ice cores may be related to a number of causes debated in the literature, here are just a few recent examples: McGee et al. (2010); Li et al. (2010); Albani et al. (2012).

p. 1726, l. 1-2 – p. 1727, l. 1-7: the "Observables" section should be expanded by comparing to the relevant literature on the three predictions.

p. 1727, l. 9-10: not yet, in my opinion, pending on a reply on the major comments raised

p. 1727, l. 13-15: while large uncertainties exist both in the observational estimates of dust deposition fluxes and in models, the ratio in Mahowald et al. (2006) is evaluated over a broad range of deposition fluxes worldwide (see also Mahowald et al., 2011), and is in relatively good agreement with the observations. Such kind of comparison should probably be included into this work to evaluate the plausibility of "realization" of the proposed range of the "dust factor". This does not rule out at all the proposed mechanism.

p. 1728, l. 4-7: current generation climate models do have the option of a prognostic treatment of dust (e.g. Neale et al., 2010), but it is true that (especially for emissions) some small-scale processes are not resolved, which is a challenge for a full prognostic treatment of the dust emissions in response to climate-status changes.

p. 1728, l. 24-29: same as the temperature bipolar seesaw, given the temperature-dust association both in Greenland and Antarctica, dust maxima in the alternation of stadial/interstadial paeriods are in anti-phase in Greenland and Antarctic ice cores (e.g. EPICA community members, 2006). Please discuss this fact, and if the authors implied that the mechanism may be controlled by one hemisphere in particular, this should be noted explicitly and discussed more.

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