Clim. Past Discuss., 8, C559–C561, 2012
www.clim-past-discuss.net/8/C559/2012/

Climate of the Past Discussions

Interactive comment on "A mechanism for dust-induced destabilization of glacial climates" *by* B. F. Farrell and D. S. Abbot

Dr. Ditlevsen (Referee)

pditlev@gfy.ku.dk

Received and published: 15 June 2012

In the paper a mechanism for explaining the jumps between the cold dry stadial state and the warm wet interstadial state observed in the paleoclimatic records is proposed. The idea is that dust pumped into the atmosphere by winds can change the stability of the tropical atmosphere, such that convective heating is strongly reduced and the climate falls into a stable, cold state. Two reasons for this are proposed. They are both related to the absorption of incoming short wave radiation by the dust aerosols: Firstly, the reduced radiative heating at the surface results in reduced evaporation, secondly, the heating of the atmosphere from aloft results in a more stably stratified mid-troposphere, such that convection is reduced. Two factors for stabilizing this increased dust load state are proposed: By reducing the hydrological cycle the residence time of

the dust in the atmosphere is increased, since rainout is less efficient, secondly with colder and drier conditions more source areas for dust become available. (The last part I also find interesting, as it is in line with the findings in an old (un-noted) paper by myself: Marsh and Ditlevsen, Climate during glaciation and deglaciation identified through chemical tracers in ice-cores, GRL, 24, 1319-1322, 1997 !) The quantitative estimates of this mechanism are based on the NCAR radiative-convective column model. Even though there are caveats in the modeling approach, especially in not including the circulation, the authors convincingly show that the order of magnitude of the observed changes in dust concentrations could be enough for the proposed mechanism to work. I find that this is a very interesting proposal, which should be published. The main results are presented in figure 1. The authors propose three states of the system, which should correspond to the stadial and the interstadial states (State 3 and State 1 respectively) and an intermediate state (State 2). I suggest, for this to be consistent with equation (1) that State 2 be identified with the unstable fixed point at x=0.5 (in figure 2). Going back to figure 1, I'd say that State 2 should be for 'Dust Factor' (=y) approximately 25 (where precipitation has an inflection point) . The explanation for the instability of State 2 then goes: If y becomes a little less than 25, the convection and thus the rainout increases and y decrease further and if y increase, rainout is reduced and y increase further. This is (almost) the same point where the difference in potential temperature between mid-troposphere and surface change behavior. I must admit, I do not understand how this difference can be negative (for $y < 25$). Please clarify.

Minor suggestions: Figure 1: Units for temperature are Celcius and not K. Consider changing the State 1-3 bar. For readability it would be helpful with curves connecting the plotting symbols (I quess we can safely assume that the model will produce smooth curves). I'd personally like labels ('precipitation','Theta(515)-Theta(surface)' etc) in the plot. Figure 2: This is only for easier understanding: When defining x=log_10(y), then have it comparable with figure 1, such that x=0.5 is scaled to x=log_10(25) etc. And if the time scale of jumping is meant to resemble the DO-events, the mean should be 3000 years rather than 1600 years.

Interactive comment on Clim. Past Discuss., 8, 1721, 2012.