

Interactive comment on “On the importance of initial conditions for simulations of the Mid-Holocene climate” by H. Renssen et al.

Anonymous Referee #2

Received and published: 25 July 2006

The authors tackle a relevant issue : the possibility of multiple equilibrium states at the mid-Holocene, and correctly describe the problem in the introduction. The difficulty is the potential model dependence of the results. In the present climate model (ECBilt-Clio version 3), there is one stable solution at 6 k, such that the state reached at the end of a long integration does not depend on the initial conditions. This result is consistent with, the ECHAM-BIOME model (Brovkin et al., 1999). On the other hand, the authors claim that a previous version of ECBilt-Clio had several stable states at 6k. Several remarks need to be formulated about this specific point:

1. First, it is not clear at all whether ECBilt-Clio 2 had several stable states. The very high variability exhibited around 6k precisely manifests a very small hysteresis width, if any. The argument of the authors is a bit weak here: (p. 320) : "In a transient

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experiment [...] this bistability was revealed by [...] an unstable phase that lasted from 7.5 to 5.5". If the states had been stable, the models would not have oscillated between these two states.

2. The problem remains however relevant : is the 6K climate a viable target for GCMs if it is so highly variable and unstable, as suggested by ECBilt-Clio 2. ECBilt Clio 3 shows that, yes, the 6K climate is stable and unique.

3. Why would ECBilt-Clio 3 be more reliable than ECBilt-Clio 2 ? The reference given at <http://www.knmi.nl/onderzk/CKO/differences.html> does not list so clearly the differences between versions 2 and 3.

4. Page 321: The authors say that : "if we assume that our inferences about the indiscernible influence of initial conditions are reasonable, it would imply that the PMIP2 protocol for 6ka experiments is valid". The inferences are certainly reasonable, but are they correct ? Can they be generalised to higher-resolution 3-D? Fully answering this question is probably not possible, but the authors are expected to better discuss the elements that could potentially invalidate their conclusions: Influence of the Laurentide Ice Sheet (freshwater flux, impact on monsoon, etc.) ? Model resolution ? Processes not taken into account ? Influence of multi-centennial variability etc.

5. 600 years are needed to reach a quasi equilibrium. What is the error made by only doing 100 or 200 years of spin-up, as most GCMs do ?

Note a misprint in the abstract: "intial" instead of initial. Note sure "bistability" is standard English.

Interactive comment on Clim. Past Discuss., 2, 315, 2006.