

Interactive comment on “Albedo and heat transport in 3-dimensional model simulations of the early Archean climate” by H. Kienert et al.

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Overview: This paper rigorously investigates the CLIMBER climate model in Archean configuration. The paper is fairly well-written, and has relatively few grammatical and typographical errors. I recommend that the paper be published, but only after the limitations of CLIMBER are made more clear to the reader. When simulating a climate vastly different from our own, we should try to use models with as much basic physics and as few empirical parameterizations as possible. Empirical parameterizations are less likely to be valid when used to extrapolate to a very different climate, whereas basic physics should still hold. We know what the equations for atmospheric dynamics are, and can get a fairly good picture of atmospheric behavior when we use them even in a fairly coarse atmospheric GCM. But CLIMBER instead employs empirical re-

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relationships for atmospheric dynamics that are unlikely to be valid in different climates. The total number of simulations done here is not excessive (there is no real parameter sweep across uncertain parameters), so I don't understand why a coupled GCM couldn't have been used. We would still have plenty of uncertainty from the parameterized cloud scheme of a GCM, but at least we'd have a more realistic picture of the effect of changes in rotation rate and atmospheric pressure on things like the atmospheric circulation and vertical temperature structure. This is critical because the vertical temperature structure determines the radiative forcing you get from adding CO₂ and because the surface winds force the ocean circulation. Resolving these dynamical effects is the main reason you would do a 3D study, as opposed to the old 1D radiative convective studies. I am very sympathetic to the idea of using simple models to gain a better qualitative understanding, but it's not clear to me that CLIMBER really delivers more qualitative understanding than a coupled GCM does. These limitations of the model are not made clear enough in the current draft of the paper. This is important since many people may read this paper who are not climate dynamicists and might not immediately recognize that CLIMBER is very different from a modern coupled GCM.

Comments:

1. Issues with Model: As outlined above, CLIMBER is simply not up to the task of accurately calculating changes in atmospheric circulation and lapse rate (and therefore the essential question of radiative transfer for this project) in a vastly different climate from modern. The atmospheric component of CLIMBER is essentially a sophisticated energy balance model that approximates atmospheric heat and moisture transport by eddies as a diffusive process (*Petoukhov et al.*, 2000). An empirical parameterization (Eq. (3) of the paper) must be used to calculate the lapse rate. Since we expect the lapse rate to be driven by convection to the moist adiabat in the tropics, this parameterization can presumably do a reasonable job there. But in the extratropics eddies

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are critical for determining the lapse rate (e.g., *Schneider*, 2006), and large inversions will develop over ice in the winter hemisphere (e.g., *Pierrehumbert*, 2005). The empirical parameterization used here is certain to simulate lapse rates incorrectly in these regimes. This is absolutely critical for the present study because the radiative forcing you get from adding CO₂ to the atmosphere is highly dependant on the lapse rate. Another issue is the atmospheric circulation pattern. As described in section 2.2.3, rough parameterizations must be used to calculate atmospheric cell positions and strength. This is important because it will lead to surface winds, which drive the much more sophisticated ocean model. If the atmosphere is doing something screwy, the ocean will be too and can't be trusted.

Why are these issues particularly relevant for the present study? First, because the rotation rate is changed, which will clearly affect the dynamics. As the authors note in section 2.2.4, they simply move the cell boundaries to where they think they should be based on some previous work. Given this, it's hard to claim that the model has really calculated the effect of changing rotation rate (since a big part of it was really imposed). Furthermore, the effect that changing rotation rate would have on eddy behavior and therefore extratropical lapse rate appears to be completely neglected. A second issue is that the authors change the atmospheric pressure. This will not only affect Raleigh scattering, which the authors do include, but should also tend to increase atmospheric heat transport, all else being equal, and counteract the effects of increased rotation rate on the meridional temperature profile. Such effects cannot be calculated using an empirically based model like CLIMBER.

The path to an acceptable publication that I see is to discuss these issues more clearly and frankly, to make sure the reader has no misconceptions about what's been done. Specifically, I think you need to:

1. Revise the abstract so that you describe CLIMBER a bit, rather than just saying you used a 3D model. I would write something like: "We use CLIMBER, an

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- intermediate complexity climate model with a sophisticated energy balance atmosphere and some dynamics based on empirical parameterizations coupled to an ocean GCM.”
2. Stop talking about 3D models like they’re all the same thing. For example, on the last line of page 527 you allude to ECHAM/MPI-OM as if it’s just another 3D model, when actually it is a coupled ocean-atmosphere global climate model that is in a completely different class from CLIMBER. I think you need to go through your paper critically and remove misleading statements like this.
 3. Discuss openly the issues I’ve raised above about the atmospheric dynamics and lapse rate in CLIMBER. I think you need to add this to the relevant subsections of section 2 and you need to emphasize this in the conclusions. You particularly need to note in the conclusions that the model requires empirical parameterizations to calculate the lapse rate, and that the radiative forcing associated with an increase in CO₂ will depend strongly on these assumptions. I also suggest reiterating the call made by (*Feulner*, 2012) for the application of “state-of-the-art” climate models to this problem in the conclusions.

2. Low-latitude Ice States: I have a couple comments about the first paragraph of page 540, since I’ve worked on this issue. First, low-latitude ice states do not “become unstable when sea-ice dynamics are taken into account” in all models. As the authors note, CCSM includes sea-ice dynamics and can simulate low-latitude ice states (*Yang et al.*, 2012). Sea-ice dynamics do tend to destabilize low-latitude ice states, but how destabilizing this is depends on the model used. Second, I suspect you would find low-latitude ice states in CLIMBER as well if you decreased the bare sea ice albedo. Notice that CLIMBER only uses a relatively low value for bare sea ice albedo if the ice is melting, but it’s 0.72 (fairly high) otherwise.

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