Review – Earth System Dynamics Discussion

Title: Climate change, in the framework of the constructal law

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Review

This manuscript describes a simplified model of key elements of the climate system based on the constructal law. The model is used to study the sensitivity and response of the earth to changes in (shortwave) albedo and the (longwave) emissivity of the atmosphere. The model builds on earlier work by Reis and Bejan (2005).

The model contains a number of simplifying assumptions. Basically, it has an 2750 m deep ocean (i.e., aqua-planet). The surface is divided into two zones called the high (equatorial) and low (polar) temperature zones. The model includes no evaporation in the energy balance (e.g. energy balance in Eqn 3) so the radiative terms for the solar current and heat loss to space are implicitly defined at the top of the atmosphere. On that interpretation, there is no material atmosphere and the heat advected from equator to pole must be through the material ocean as noted in the manuscript just after Eqn 17. Note that to include evaporation, the model would need to add two atmospheric boxes for the high and low temperature regimes. Those boxes could receive the evaporation and condense the water at height.

The model is first solved for steady state conditions by selecting a value for x that subdivides the total surface area into high and low temperature zones such that the heat flux is a maximum from the high to low temperature zone (Eqn 27). That solution is perturbed to examine the dynamics. The results of the perturbation are compared with observations (p. 252, para. after Eqn 31). A key part of the paper is comparison to observations. In that respect I did not understand the basis of the comparisons in Section 4.1, since the changes experienced by the real earth would be better approximated by the ramp change discussed in S4.2.

When examining the ramp change (section 4.2) and the modified ramp change (section 4.3) I also did not understand the basis of the comparison with observations at the bottom of p. 252 and top of p. 253. You have perturbed the system (starting in 1880) with a ramp change lasting 120 years. You have then examined the transient response. But to compare with Hansen et al 2005 you have to compare the same thing. The most straightforward comparison is the time integrated enthalpy change.

For example, the inference from the Hansen et al 2005 paper, when the volcanoes are ignored is that heat began to accumulate in the earth system (and mostly in the oceans) from the 1950s onwards (See Fig. 1c in Hansen et al 2005). Hence you really need a comparison over 50 years.

My comparison is as follows.

The Hansen et al 2005 result is that heat storage into the earth system (and more or less into the ocean) was ~ 0 W m-2 in ~ 1960 increasing to ~ 0.8 W m-2 by 2003. That is equivalent to an average rate of heat storage of around 0.4 W m-2 and let us assume that this occurred from 1950 until 2000.

The net effect is that we add ~ 3 x 10^23 J (of enthalpy) to the earth system and most of this would go into the global oceans.

In your case, with reference to your Fig. 4b, you have an imbalance in 1950 (year 70 using the case 3 time scale) of around 0.9 W m-2 rising to ~ 1.1 Wm-2 in 2000. The average is around 1 W m-2. Over 50 years, for an area 5.1 x 10^14 m2, this equates to added heat of around 8 x 10^23 J. In your model, the slab is heated uniformly with depth (Eqns 3 and 7). You have a slab 2750 m thick, but you assumed only 1/3 of that is heated equating to a slab some 917 m thick. So if 8 x 10^23 J were used to heat a slab of sea water (specific heat ~ 4 KJ/kg/K) that is 917 m thick and covers 5.1 x 10^14 m2, the uniform increase in T of the slab would be ~ 0.4 K. That is consistent with Fig. 4a (dT from year 70 to year 120) in your manuscript.

In summary, you have added heat that is 3 times larger than Hansen et al. 2005 but a T change of 0.42 K that is similar to Hansen et al. 2005.

You could object and note that if Hansen et al 2005 showed ~ 0 Wm-2 in 1950, then for a fair comparison, we should start your model with 0 Wm-2 as well. In that case, from your Fig 4b, starting at time 0, after 50 years, T has increased ~ 0.1 K. The energy imbalance is 0 Wm-2 at time 0 and at time = 50 years, the energy imbalance is ~ 0.7 W m-2. So with those assumptions, you will more or less get same amount of integrated enthalpy change in the oceans (~ 0.35 Wm-2 for 50 years) but the corresponding T change is a factor of 3 too small. As far as I can see, no matter which way you go, you always have a factor of 3 difference (in either accumulated heat or T change). According to my analysis, this arises because your model requires the whole slab to be heated uniformly. This difference was not made obvious in the manuscript because you compared the instantaneous energy balance and T but the key comparison is the T and integrated enthalpy change. Can you please confirm that this is correct?

The differences noted above imply a difference in the vertical distribution of heat through the ocean and suggest that the in-built assumption of uniformly heating the whole slab (Eqn 3 and 7) has not happened over the last 50 to 100 years. The data of Domingues et al 2008 (their Fig. 1b) show that virtually all the heat was held in the top 100 m until ~ 1980 when heat started to progressively penetrate deeper. That means that the T change cannot be uniform over the entire (assumed 917 m thick) slab of sea water.

In summary, I do not see agreement with the results of Hansen et al 2005 that is claimed by the authors. I calculate disagreement by a factor of ~ 3.

I also note that in the model solutions, e.g. Figs 3,4,5, that the increase in T_H is generally greater than T_L in both transient and most equilibrium solutions. However, in most IPCC models, the poles (the low T part) warm faster than the equator. This is well documented in the last IPCC report (AR4) and has also occurred in observations. This is another interesting difference that needs to be further explored. The reason for this difference was not immediately obvious to me. Perhaps it was caused by the model assumption that the albedo was the same in both the high and low temperature parts and perhaps because the perturbation to albedo was also the same.

Perhaps the authors could check my calculations and modify the manuscript accordingly.