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Earth System Dynamics
Discussions

Interactive comment on "Temperatures from Energy Balance Models: the effective heat capacity matters" *by* Gerrit Lohmann

Anonymous Referee #3

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The manuscript presented by Gerrit Lohmann aims at investigating the role of the effective heat capcity in an EBM model (Energy Balance Model) when put in the context of the day / night succession. Starting from the classical very simplified global EBM without diurnal cycle, the author progresses into showing that when adding the the diurnal cycle, the effective heat capacity of the planet cannot be neglected. The effect obtained is however quite small on the result. In a subsequent section, the author discusses how temperature meridional gradients may be affected by the oceanic vertical diffusivity and therefore the corresponding "effective" heat capacity. Though I am in principle a big supporter of simplified modelling approaches, **I do not recommend publication of the manuscript in its present form**. Indeed, the first sections of the manuscript concentrate on very obvious results that are ill-presented while the second

part do not provide enough supporting evidence on the claims laid.

Main comments

1. Neglecting the diurnal cycle in EBMs is a rather standard procedure. This assumes that the Earth receives a mean daily incoming solar energy equally distributed over each latitude bands. This is indeed most of the time quite a reasonable hypothesis for such simplified models, since the ocean surface temperature diurnal changes are small (at most a few degrees). This paper confirms this usual assumption, with the red and dotted brownish curves of Figure 2 being almost indistinguishable.
   The presentation on this section is however extremely confusing. The author starts with the classical 0-dimensional time average EBM. He then presents the 1-dimensional case with a daily cycle as an extension, just introducing it as a local extension of the 0-dimensional case. However, considering that there is a local energy balance is not a valid assumption in general, contrary to the one at the global scale. Obviously, it is clearly entirely irrelevant to consider that there can be an instantaneous radiative equilibrium, with temperatures dropping to zero Kelvin as soon as the Sun sets. This is clearly not what people usually assume when using EBMs !
   The usual starting point corresponds to equations (11-12) where the solar forcing is averaged over one Earth rotation. This is more or less what people have been using in geographically explicit EBMs, including the very first ones. Budyko and Sellers 1969 where indeed geographically explicit, without a diurnal cycle, as in equation (11). The authors comes back to it as a compensation of the incoherent assumption of a local radiative equilibrium. So part 2 is just showing that an irrelevant hypothesis produces irrelevant results. It brings nothing interesting, but only confusion.

To add to the confusion some assumptions are clearly not explained. On the top of page 4, the first equation is clearly invalid unless strong hypotheses are imposed, which are not specified in the text.

2. The second part of the paper discusses the role of heat capacity in the "diurnal averaging" of temperatures. Results are summarized on Fig.3. As discussed above, the fact that temperatures are much lower for small heat capacities is rather obvious (with Earth losing most of, or all its thermal energy during the night).

3. Using the typical oceanic vertical diffusivities for estimating a heat capacity is not very relevant. The diurnal cycle is buffered by the very top layers of the ocean that are usually almost well-mixed by winds and also by the diurnal cycle itself. The interior ocean vertical diffusivity has no role.

4. I do not see what is the purpose of solving equation (15) and showing Figure 4. This does not relate to the diurnal cycle, nor to heat capacity, nor to vertical mixing. What is the point ? The statement "global mean temperature is not affected by the transport because of the boundary condition. . .." is a bit strange. I would write more simply that here, global mean temperature is a measure of global heat content (uniform heat capacity) which depends only of global net radiative fluxes, not internal redistribution.

5. Bottom of page 5 *"Until now we assumed that the Earth's axis ..."*: It is quite awkward to explain only now where equation (4) page 2 comes from. Indeed equation (4) is certainly not standard for the Earth in particular in the context of EBMs and climate modeling. A planet with no tilt has no seasonal cycle. Many

EBMs have an explicit seasonal cycle. Again, the starting point of the paper is very awkward.

6. In the last part, the author presents some experiments with the COSMOS coupled model, to investigate the role of vertical mixing on the meridional temperature gradient. Unfortunately, it is not clear at all that these results are linked to the diurnal cycle or heat capacity. The author sets an experiment with an 25-fold increase in the background diffusivity. The logical outcome of this experiment should be to increase dramatically the oceanic circulation (not shown in the manuscript) and thus to increase massively the heat transport and the vertical mixing in the ocean. How does this relate to the heat capacity or the diurnal cycle is a mystery for me and how conclusions can be drawn from there is likewise impossible to understand. The only clear result is a weakening of the equator-to-pole gradient (likely due to increase heat transport by the ocean); however there is no physical basis to link this to past climates as the author is doing, since no probable mechanism can be suggested to increase the diffusivity by a factor 25 globally.

7. In the conclusion, there are some mentions of possible linearization of the long wave radiation. Since this is critical to the whole paper (averaging T is not the same as averaging T^4), I am surprised not to see a much more detailed discussion of this point much earlier in the paper.

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