

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

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Thanks for the constructive critics in the *Interactive comment on Earth Syst. Dynam. Discuss.*, <https://doi.org/10.5194/esd-2019-35>, 2019. by referee #3. In the following, I will repeat and answer to these comments. Furthermore, a possible action is proposed.

Comment 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

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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.

Answer/Action

The confusion shall be clarified. This manuscript revisits the relationship between the (global mean) surface temperature of the Earth and its radiation budget as is frequently used in Energy balance models (EBMs). The main point is, that the effective heat capacity (and its temporal variation over the daily/seasonal cycle) needs to be taken into account when estimating surface temperature from the energy budget. As a starting

C2

point, a zero-dimensional model of the radiative equilibrium of the Earth is introduced

$$(1 - \alpha)S\pi R^2 = 4\pi R^2\epsilon\sigma T^4 \quad (1)$$

where the left-hand side represents the incoming energy from the Sun while the right-hand side represents the outgoing energy from the Earth. This is used to calculate the temperature

$$T = \sqrt[4]{\frac{(1 - \alpha)S}{4\epsilon\sigma}} \quad (2)$$

The wording “This is clearly not what people usually assume when using EBMs” shows that there are implicit assumptions in the approach. To my point of view, the assumptions can be explicitly spelled out to obtain arguments which steps are necessary to make. I show that the global energy balance should not be calculated from this approach, because it neglects the implicit assumption of a fast rotating Earth with significant heat capacity. I am not aware of a paper which explicitly shows that

$$T = 0.989 \sqrt[4]{\frac{(1 - \alpha)S}{4\epsilon\sigma}} \quad (3)$$

The author knows the fundamental work of Budyko (1969) and Sellers (1969) where the EBM could be geographically explicit, but their result has not been used to calculate the mean temperature (3).

Your statement that the author comes back to the geographically explicit EBM as a compensation of the incoherent assumption of a local radiative equilibrium cannot be found in the manuscript. The calculation of the global mean temperature from the energy balance is not irrelevant.

Your final point is that the first equation on the top of page 4 is clearly invalid unless strong hypotheses are imposed, which are not specified in the text. If you see the text

C3

above this equation, it is clearly written that it is about the diurnal variation. In a revised version, I can explicitly spell out that this approximation is exactly the point of the low diurnal cycle due to the heat capacity.

Comment 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).

Answer/Action

I am not aware of a study analyzing the effect of the heat capacity on global climate. Indeed, the manuscript shows this effect in Fig. 3.

Comment 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.

Answer/Action

The effective heat capacity is time-scale dependent. For the day and night cycle values in the order of the atmospheric heat capacity are realistic for our Earth with 24 h rotation. Indeed, 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. In the revised version, I will be more explicit here and could include a figure to show how the temperature would change in a slowly rotating planet. For longer time scales, the mixing in

C4

the ocean interior plays the major role as shown in the manuscript.

Comment 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.

Answer/Action

The purpose of equation (15) and Fig. 4 is to show the influences of the meridional heat transport and the seasonal cycle. They do not change the global mean temperature, but the temperature gradient. The statement "global mean temperature is not affected by the transport because of the boundary condition" was written to show the reader that the heat transport does not play a role (unless other feedbacks are included). In the revised version, I could be more explicit in writing down the formula.

Comment 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.

Answer/Action

At the bottom of page 5, it is not the first point where equation (4) comes from. Equation

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(4) comes directly from the basic incoming radiation (Fig. 1) which goes with the cosine of latitude and longitude:

$$(1 - \alpha) S \cos \varphi \cos \Theta .$$

I see now the point that the motivation for equation (4) in the manuscript needs a better introduction. The beauty of (4) is that we ignore the Earth orbital parameters first (no obliquity and no precession) which makes an analytical calculation possible. The global mean temperatures are not affected by the tilt and the values are identical to the one calculated in (12) of the paper. Indeed this shall be stated earlier in the manuscript.

Comment 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.

Answer/Action

Energy balance models have been used to diagnose the temperatures on the Earth when applying complex circulation models. The outcome of the new approach is that

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effective heat capacity matters for the climate system. This cannot be seen in

$$T = \sqrt[4]{\frac{(1 - \alpha)S}{4\epsilon\sigma}} \quad (4)$$

The motivation is that we may think of a climate system having a higher net heat capacity producing flat temperature gradients. The effect of the non-linearity is similar that a weaker variation in the seasonal cycle (Fig. 6) causes warming at mid and high latitudes. I agree that the step from the diurnal to seasonal cycle is not well elaborated. The aim was to show one of the potential consequences of the effective heat capacity to explore the full range of solutions. Indeed, the increase in the mixing is admittedly ad hoc. On long time scales, the effective heat capacity is not an intrinsic property of the climate system but is reflective of the rate of penetration of heat energy into the ocean. For the main point of the paper, the GCM experiments are not essential and could be dropped.

Comment 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.

Answer/Action

Thanks. I realized now that this point shall be more elaborated in the revised version.

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