Reply to the reviewer's comments: TransEBM v 1.0: Description, tuning and validation of a transient model of the Earth's energy balance in two dimensions (gmd-2020-237)

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Summary of changes

We thank Shaun Lovejoy for his detailed feedback on our manuscript, which raises many highly interesting aspects. In response, we plan to

- provide an estimate of the relaxation times and discuss them in the revised manuscript and
- extend the manuscript to discuss in particular the heat transport, restarts, linearity, and tuning further, as well as the other many interesting points that the referee raised.

A detailed response to the valuable comments is given below.

1 Detailed response

(Original report cited in italics)

1.1 General comments

A). The authors are commended for pursuing the development of energy balance models, making them more realistic. This is a welcome counter to the increasing trend of using GCMs to answer all climate questions. The authors justify their approach by invoking the flexibility of such "low complexity models" with respect to GCMs (they allow for "fast and repeated" simulations). But there are other advantages to their approach and there is no reason that high model complexity is a sine qua non for realism. In actual fact, the development of GCM alternatives is very timely. This is because it is increasingly clear that each (increasingly complex) GCM has its own climate - presumably none have the same

climates as the real world.
By exploiting large amounts of historical data, EBMs have the potential of reproducing the real world climate, thus providing results with both lighter computations but that are also more reliable. The authors might mention that it has been proposed that models of EBM type can be thought of as high level models that attempt to account statistically for huge numbers of interactions, of details (e.g. [Lovejoy, 2019]). EBMs and kindred approaches are therefore not just "poor man's" GCMs.
While I have a number of technical questions that I would like the authors to address, overall the paper is well written and the public availability of the code makes it especially appealing. If the authors can answer the questions below, I recommend it for publication, it will be welcome addition to the literature.

We thank Shaun Lovejoy again for this favorable assessment of our work. We agree that EBMs can provide insights different to those provided from GCMs. We will update the introduction with the mentioned reference to introduce the framing of EBMs it proposes and emphasize the value of EBM-type models further.

B). My main disappointment is that the authors didn't provide much theoretical guidance to understanding their results (nor indeed for justifying the numerical constraints such as choice of time step and "spin-up" time). Indeed (ignoring the "restarts") their EBM is linear so that standard linear analysis could be made. This is facilitated by the excellent North and Kim monograph that develops the theory for the (admittedly simpler) 1-D case with constant coefficients.
Specifically, equation 1 could be Fourier (or – depending on the application – Laplace) transformed in time to reveal the key time scales. For example for deviations from the mean, we take A = 0 and obtain:

$$\tilde{T}(\omega,\underline{r}) = \frac{\tilde{F}(\omega,\underline{r})}{(i\omega\tau)+1-(\nabla\cdot D\nabla\tilde{T})/B}; \tau(\underline{r}) = C(\underline{r})/B(\underline{r}); \tilde{F}(\omega,\underline{r}) = S_0\tilde{S}_F(\omega,\underline{r})a(\underline{r})/B(\underline{r})$$

Where the tilde indicates Fourier transform in time and F is the effective forcing (incidentally, using the notation S 0 and S F for quantities with different units is not good practice). North and Kim develop essentially this equation for the case where B, C, D are constants and then expand the temperature in Legendre polynomials. This leads to:

$$\tilde{T}_n(\omega) = \frac{\tilde{F}_n(\omega)\tau_n^{-1}}{(i\omega\tau_n)+1}; \tau_n = \frac{\tau}{1+(D/B)n(n+1)}$$

Where the subscript "n" is for the n th polynomial. In the 2-D case discussed here, the case with homogeneous coefficients can instead be dealt with either full spherical harmonics (or make the flat earth approximation and perform spatial Fourier transforms). The result is a typical relaxation time scale τ_n than depends on the spatial scale ($\approx 1/n$).

Although the above simple analytic result is strictly valid for constant coefficients, in fact the model constants are homogeneous (or slowly varying in the case of D) over wide swathes so that - except for region boundaries - we should be able to use the above estimates to obtain the basic time scales for adjustment. The basic relaxation times (= C/B) are for ocean and land where (using values from table 2) we obtain $\tau \approx 10$ days (land), ≈ 6 years (ocean). These are fundamental model time scales should help justify the time step (≈ 1 week) and (should) help explain the "spin-up" time (this is really a nonlinear model concept, in this linear model it is more accurately, the time for the classical, exponentially decaying transients).

Another useful consequence of the above is that it explains rather naturally the annual cycle phase shifts: at critical frequencies $\omega = \omega_n = 1/\tau_n$, the phase shift is $-\pi/4$ indicating that the temperature lags the forcing by $365/8 \approx 46$ days. At high frequencies, the lag is $\approx -\pi/2$ whereas at low frequencies it is ≈ 0. Putting in numbers, for annual forcing, we therefore anticipate for ocean regions, the temperature lags by $\approx 60-80$ days, whereas over land, $\approx 10-20$ days, numbers that compare reasonably with the simulations (fig. 11).

The same equations should (presumably) explain the amplitudes of the annual cycles.

The referee raises many interesting points here that we will be happy to take into account in the discussion of the revised manuscript. With respect to an analytical solution of the model at hand, we do not see how this would be possible in light of the non-homogeneous, spatially-resolved boundary conditions that represent the surface types.
We agree that an estimation of the relaxation time scales (as provided previously for the model in Zhuang et al. (2017)) would strengthen the manuscript and provide valuable insight into the time scales at work and how they relate to the numerical constraints and the simulated seasonal cycle. We will incorporate this in the manuscript upon revision.

1.2 Detailed comments

1. In the paper, the transport equation (eq.1) has a diffusion term $\nabla \cdot D\nabla T$ where the 2-D gradient operator is used, D is the thermal conductivity. In the North and Kim book (and earlier papers going back to Sellers 1969), the diffusive term is $\frac{\partial}{\partial\mu}D(1-\mu^2)\frac{\partial T}{\partial\mu}$ where μ is the (dimensionless) cosine of the colatitude. A consequence is that in the North and Kim treatment, D has the same dimensions as the sensitivity: W/(m^2 C), it is a conduction coefficient per radian, the value is the same as that given in the 1981 review: D = 0.67 W/(m^2 C). In the paper (table 3), values of the order of 1 are given for D and the units are in W/C. The obvious explanation for the discrepancy is that the model is 2D so that the model uses values multiplied by the average grid area which is 6.2x10 10 m^2. While this would have the correct units, it is very far from the values given. Therefore, please express

D in standard units of thermal conduction W/(m C). (I checked the Zhuang paper and it doesn't give the values either, I suppose they were somewhere in the Fortran code?).

Indeed, as the referee assumed, both the values and computation of D (different from that used in other references from North et al. we consulted, including North and Kim (2017)) reflect those in the Fortran code accompanying Zhuang et al. (2017). We will investigate the possibility of expressing the thermal conductivity parameters in terms of the suggested units.

2. P11. Explain restart a little better. It seems to essentially be a way of introducing nonlinearity, but this is not clearly expressed. Can the mathematics not be indicated (explicitly with an equation) with forcing that depends on the temperature?

Since they are a central part of our modifications to the model, we will gladly improve the explanations of the restarts. Restarts allow the changing of the boundary conditions, relating these adaptations of the boundary conditions (i.e. changing ice sheets and sea level) to the GMT at the end of the previous run would be possible, but has not been implemented at this point.

3. P17, phases, amplitudes: It would be useful to estimate these for homogeneous regions (see the above).

There are latitudinal dependencies which break the homogeneity leading to differing amplitudes and phases in nominally homogeneous regions. Therefore, we find it hard to determine regions, for which these calculations can be done.

4. Tuning: this paper follows the tradition of guessing, then tuning model parameters to attempt to fit the data. There are so many parameters that this is hazardous. Is there no way to try to estimate the parameters directly? (Using standard units for thermal conductivity would make this more transparent!). Also I'm surprised (table 8) at how little tuning affects the initial guess parameters. I'm not sure a lack of sensitivity is a good thing? Please comment.

The tuning process did indeed not use a regression minimizing the tuning metrics across the whole multi-dimensional parameter space. We agree that automating and improving this procedure is desirable and have therefore since submitting this manuscript worked on a procedure to do just that. As it stands, the tuning did sample the parameters spaces of the individual parameters. In conjunction with the, by comparison, limited amount of parameters, the tuning result is not pure guess work, and, in our opinion sufficient for the applications presented here. The sensitivity of the model to changes to the parameters varies as shown in Figures 8 and 9: Most parameters can produce large responses in the simulated temperature profile, in a number of cases even small changes to the parameters have a drastic effect, e.g. any parameter related to the ocean or outgoing radiation. Table 8, on the other hand, reflects the fact that the initial parameterization provided by Zhuang et al. (2017) was already quite quite good with respect to our tuning goals. Overall, the model does not, in our assessment, seem to suffer from a lack of sensitivity.

5. P18: You mention nonlinearity, the equation is linear so that presumably, the origin is in temperature dependence of parameters (e.g. albedo), and this occurs via restarts but this is not mentioned. Intuitively, this means that the model has a slow nonlinearity, but it would be important to state this mathematically more clearly. Please explain (see point 2 above).

As suggested, we will expand on the (limited) non-linearity in the model.

6. P19: You mention "spin-up". This is a term normally used for nonlinear models such as GCMs. Your model is linear so that you are discussing transients, and these can be analyzed by classical methods (see above). I think it would clarify how the model really works.

We thank the referee for pointing out this imprecision in our wording and will improve this upon revision.

7. Section 3.3: Here and as earlier with spin-up the key is the effective global scale relaxation time: the parameters are no so different from North and Kim so that the relaxation times should not be very different. I expect the Last Millenium simulation to thus be a low pass filter of the forcings at time scales longer than the longest (ocean) relaxation time, hence that their long term statistics for example will be the same. This is of course not true if the restarts introduce enough nonlinearity. The only complication is due to the diffusion term that is variable in space. But it's magnitude is not in fact so variable.

We will provide an estimate of the relaxation times and discuss them in the revised manuscript.

8. Table 1. – The caption of the table says the heat capacity values are taken from Zhuang. et al. (2017b) while in the table it suggests they are from Zhuang. et al. (2017a), but they do not correspond to the values given in Zhuang. et al. (2017a).

We thank the referee for spotting this error, indeed the values are taken from the model code (Zhuang et al., 2017b in the manuscript) and we will change the references accordingly.

9. Line 231 – Why was minimizing the RMSE prioritized over agreement with GMT?

In our assessment, for a two-dimensional model to provide added value to zero- or one-dimensional EBMs, it is important that the spatially resolved temperature field produces realistic features, which is why the latitudinal profile was prioritized over the absolute value of the GMT. The latitudinal profile furthermore showed considerable deviations from observation in regions of interest (e.g. the polar regions), whereas the GMT was already very close to that found in the ERA climatology. Similar arguments hold for the seasonal profile. There are, of course, applications where the GMT, or more so the development of the GMT anomaly would take precedence. Depending on the applications the relative importance of the tuning metrics would change.

10. Table 7. – Clarification on why what the bracketed (x) represents as opposed to just x?

We are grateful that the referee noticed this omission in the manuscript. The bracketed x is meant to represent a notably lesser, but not negligible, influence than the other parameters. We will update the description accordingly.

11. Line 246, Figure 10a, could the choice of 1960-1989 climatology – a period with strong aerosols which mask the CO2 warming – be the reason for underestimate of warming? What are the consequences of changing other climatologies, say per-industrial or closer to present (less aerosols)?

We agree that the choice of reference period for the tuning will have an influence on our results. The underestimation is also definitely related to the forcing considering that a similar underestimation is not an issue in the past millennium simulations. So far, we had not considered the aerosol forcing as a possible source, but it is certainly worth looking into and examining further, we thank the referee for raising this interesting point.

Figure 10. – It appears the tuning gives little improvement (GMT anomaly and average latitudinal temperature) or actually hinders emulation of the reanalysis (seasonalamplitude)? What is the reason for the much lower amplitude of the seasonal cycle in both hemispheres?

Due to the prioritization of the latitudinal profile (and reducing its deviation from observations) in the tuning, this is the metric with the most notable improvement, whereas the GMT was already in quite good agreement beforehand and therefore only improved in decimals. The amplitudes of the seasonal cycle are mostly affected by the changes to the heat capacities of ocean and atmospheric column, which also promoted the slight shift and the better agreement in the southern hemisphere, but came at the expense of the agreement in amplitudes.

12. Line 83: The refence to [Rypdal et al., 2015] for scaling and EBMs is not accurate. When [Rypdal et al., 2015] made their fractional modification of [North et al., 2011]'s heat diffusion model they obtained it precisely by removing the critical energy balance term! The [Rypdal et al., 2015] model neither deals in energy, nor does it allow for balance. It is unstable with respect to infinitesimal step forcings, it's ECS is infinite. In the expression "EBM", one thus must eliminate the "E" and the "B". To correct this fundamental fault, one must reinsert the linear balance term that was removed and that corresponds physically to black body emission, and in this case one obtains the Fractional Energy Balance Equation ([Lovejoy, 2019], see the discussion of this point in [Lovejoy et al., 2021] http://www.physics.mcgill.ca/ gang/eprints/eprintLovejoy/esubmissions/QJRMS.FEBE.revised.3.11.20.pdf). However the introduction of the necessary balance terms comes at the *cost of having two different high and low frequency scaling regimes. Incidentally, the authors could also mention that energy balance models have been used to make global scale climate projections to 2100 [Procyk et al., 2020]. http://www.physics.mcgill.ca/ gang/eprints/eprintLovejoy/neweprint/esd-Procyk.discussion.2020-48.pdf.*

We will modify the discussion of the reference in response to the referee's comment. Adding a discussion on the use of EBMs for projections is indeed valuable and we will make the requested improvement to the manuscript.

13. In the abstract it is stated that the EBM lacks internal variability. I find the statement a little odd since a linear model cannot generate internal variability: none would be expected?

We will correct this statement and emphasize that, while the EBM is not fully deterministic, in particular when considering randomized forcing, it does not represent internal variability of the atmosphere-ocean system.

14. Line 95: The issue of energy storage was not mentioned. Please discuss where the energy is stored in this model.

The model does not have an explicit storage of energy, the effect that storage has on the climate is approximated using the heat capacity parameters.

15. In eq. 1, a is the co-albedo, not the albedo, although eq. 4 uses it as an albedo. Please fix this. Also, as indicated above, the notation S_0 and S_F with different units is confusing. Why not use the North and Kim notation?

The notation with respect to the albedo is indeed inconsistent between the equations and we thank the referee for raising this point and will correct this. With respect to insolation, the notation in the manuscript follows the implementation of the model as it is provided in Zhuang et al. so as to ensure that the manuscript and model code match.

16. As mentioned above, in table 3 please use correct units for thermal conductivity.
Please refer to the reply to point 1.

17. Eq. 2: where does this form for D come from? It seems a bit weird?

This is in accordance with the model code provided for and documented in Zhuang et al. (2017). The computation of D was not changed in our extension of the model.

18. Given that the longest time scale in the model is the ocean relaxatjon time of about 6 years (see above), presumably, the last Millenium simulation is just a low pass filter of the forcing? (or is nonlinearity somehow important?).

These are very interesting points, which we will investigate further.

References

North, G. R. and Kim, K.-Y.: Energy Balance Climate Models, Wiley-VCH, Weinheim, 2017.

Zhuang, K., North, G. R., and Stevens, M. J.: Model code: A NetCDF version of two-dimensional energy balance climate model based on the full multigrid method in FORTRAN, URL https://github.com/ElsevierSoftwareX/SOFTX-D-16-00023.

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