TransEBM v. 1.0: Description, tuning, and validation of a transient model of the Earth's energy balance in two dimensions

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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.

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:

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

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:

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

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.

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 $\frac{\partial}{\partial\mu}D\left(1-\mu^2\right)\frac{\partial T}{\partial\mu}$ 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 $6.2\mathrm{x}10^{10}\mathrm{m}^2$. 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?).

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?

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

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.

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).

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.

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.

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).

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

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

11. Line 246, Figure 10a, could the choice of 1960-1989 climatology – a period with strong aerosols which mask the CO_2 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)?

Figure 10. – It appears the tuning gives little improvement (GMT anomaly and average latitudinal temperature) or actually hinders emulation of the reanalysis (seasonal

amplitude)? What is the reason for the much lower amplitude of the seasonal cycle in both hemispheres?

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.

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?

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

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?

16. As mentioned above, in table 3 please use correct units for thermal conductivity.

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

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?).

-Shaun Lovejoy

References:

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