AMET/3852317.v1 Review Report

Subject Appropriateness of the Manuscript

The topic of this manuscript falls within the scope of Advances in Meteorology

Recommendation

Reject (Paper is seriously flawed; do not encourage resubmission.)

Comments

The paper is seriously flawed and poorly structured. I suggest to reject it. The latter is the reason why below only the major comments are listed.

- The most serious flaw in the paper is a formal application of the error propagation theory to the problem at hand. Eq.(2), which is a foundation of the author's approach, is likely unable to estimate the true error accurately (ironically, author put the definitions of precision and accuracy shortly before this formula). Eq.(2) does not observe the equations of the Earth system dynamics, and formally increases the error each time step. As a result, it overestimates the true uncertainty, at least for sufficiently large time instants. Given the numbers, listed by the author (p.36: 'every calculational step imposes another $\pm 4 \text{ W/m2 TCF}$ average error onto the modeled climate response'), this 'sufficiently large time' should be at small as 10 years. My own experience of the Earth system modelling shows that such large imbalance of radiative fluxes would crash the model within several years at best. To be more specific, I would say that the formal application of the error propagation theory violates the fundamental conservation laws: it always increase uncertainty, while energetic constraints inhibit such increase for sufficiently large anomalies from initial value. This issue is well known for people dealing with numerical weather prediction: prediction error increases for a couple of weeks and then saturates.

- By the way, despite the statement made by the author and in some references cited by the manuscript, the statement that error analysis and propagation is ignored in climate studies is untrue. This error analysis is frequently employed for a calculation of Bayesian uncertainty, which now is a routine task for simplified climate models (e.g., for the Earth system models of intermediate complexity, EMICs; if interested, author could find the proper references himself).

- Concerning the poor structure of the paper, I would mention Sects. 2.1.1 and 2.1.2. Which information from these Sections is used thereafter?

- The definition of FCO2 and Fwv (p.8), while correct for the Manabe and Wetherald (1967) model, is not applicable for the SRES and RCP simulations. The reasons are i) Manabe and Wetherald prescribed cloud amounts and cloud water+ice paths, ii) they neglected changes of other atmospheric constituents. Cloud amounts and cloud optical properties are now interactive in climate models, and both SRES and RCP scenarios include aerosol emissions into the atmosphere. As a result, FCO2 can not be calculated as a residue of unity and Fvw. Aerosol forcing is the most uncertain among all forcing agents (IPCC RA5, chap.7) and can not be neglected in the definition of FCO2.

- Eq. (6) (and its slightly extended version included in the Supplementary Information) is seriously flawed: i) it neglects the forcing from agents other than CO2 and water vapour, ii) it ignores climate inertia (according to the discussion in the Supplementary information, this was already mentioned by one the previous referees), iii) it extrapolates the CO2+WV forcing as calculated for the difference between the preindustrial climate (PI) and the climate of the "Earth without atmosphere' to the forced climate change starting from PI. Because sensitivity (infinitesimal) of the forced response around the PI state is likely to differ from the mean (finite-response) sensitivity between PI-'Earth without atmosphere', items i and ii cast doubts on the value 0.42 which is used in Eq.(6). Item ii can only be accounted for by adding finite heat capacity term C dT/dt to Eq.(6) (C is the climate heat capacity 10**9 J/m2, (Andreae et al., 2005, doi 10.1038/nature03671)), but the latter is not a remedy for the major uncertainty associated with the value 0.42 for FCO2.

Author does not make a difference between the model bias for the initial state and uncertainty associated with projection (p.26, first paragraph). I note that this comments was already raised by one of the previous referees. I do not understand author's reasoning that 'climate is propagated through time as state magnitudes, not as anomalies' (p.32). The approach to make a Taylor expansion around some prechosen state is quite common in physics, and it is unclear why this approach is not suitable for climate studies.
Finally, Supplementary Information contains a rather lengthy discussion of the author with previous referees. At first, I agree with all listed comments made by previous referees and disagree with replies on

these comments. Two examples of this are mentioned above. I would say that paper is based on lack of knowledge. This lack of knowledge is reflected, in particular, in rather tendentious list of references. I would suggest not to bother further referees for reviewing this flawed manuscript and just to reject it.