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Earth System Dynamics Discussions

Interactive comment on "The magnitude-timescale relationship of surface temperature feedbacks in climate models" *by* A. Jarvis

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General considerations

In this paper the author proposes a linear approach to the characterization of "...feedbacks operating to modulate the global mean surface temperature response of climate models." (470, 6-7).

The analysis is performed under the condition that "...the climate system is stable and fully damped..." (472, 1) as time progresses in the context of "...fully equilibrated climate model perturbation runs." (475, 8).

The "...equilibrated climate model data." (483,10-11) the author looks at in the paper are three "…. which span a range of complexity …..." (475,12).

Analysis of the time scale of different feedbacks allows reduction to a "third order" reduced models accounting for "...more than 99% of the variance of their higher order parents." (480, 16-17).

The objective of identifying feedbacks operating at different timescales in determining average global surface temperature is shareable and some results proposed in the paper are interesting. However some attention should be given, in my opinion, to the general frame in which the analysis is proposed.

In the course of the numerical "perturbation runs" (doubling CO2, etc.) the model climate system after some time (hopefully!) relaxes towards its attractor set in configuration space and its dynamics (hopefully...) converges to the "steady climate" dynamics (the one operating on the attractor) . But such a convergence is typically non uniform and can be extremely slow on parts of the attractor set (as we often experience, for example, in numerical analysis of extremes). As a consequence the convergence to a steady value of average global surface temperature (AGST), or some other moment of the statistical distribution (measure) on the attractor, does not guarantee in any way that the general statistics (i.e. "climate state") of the system is converging (if convergent at all!) to its steady configuration (the new "climate state") at the same rate as average temperature. As a consequence, even if average surface temperature converges to a fixed value, reference to climate "equilibrium" or "equilibration" in such connection is, in my opinion, inappropriate, where not misleading: the only "equilibration" we have under control is that of average global surface temperature itself.

Looking at the same problem at another angle, average temperature is not a "climate state parameter". Also use in the paper of some thermodynamic concepts and terminology may be somewhat misleading: temperature is a thermodynamic state variable, but not a "climate state" one.

Once the above concepts are clarified and the limitations of average surface global temperature as a climate state indicator are taken into consideration, what are we left with? From a dynamical point of view, we are analysing some heat exchanges taking place in the process of relaxation of the model climate system towards its attractor set. These processes are dependent on where in phase space the system is evolving and there is no a priori guarantee that the "processes" we identify (in the case in question by time-scale analysis) are significant in the final climate state (on the attractor). But, most of all, since we are not dealing with (climate) state parameters, much may depend on how the modification of "climate forcing" is performed and not only on the initial and final forcing.

I think the analysis proposed in the paper should be brought in the context of a more orthodox view, eliminating from the text all the references (or allusions) to "climate" as represented by the average global surface temperature: it should be stated clearly that we are just looking at a single, specific time evolution of such a variable.

Within the above indicated limitations, I think the analysis of heat exchanges as a function of time scale is interesting and the paper is sufficiently clear and legible.

Specific comments

470, eq. (1) When we write this type of equation in Thermodynamics temperature T (integrating factor of the "non exact" differential of heat dq) is a thermodynamic state variable (with all the known implications). But, as remarked in the general comments above, "thermodynamic state" is not "climate state"……..

471, 6 "…..equilibration process of climate models": I would prefer the explicit mention of "…average surface global temperature equilibration".

472, 1 "… the climate system is stable and fully damped…..": this has to be proved; anyhow reference should be made, here too, to average surface global temperature that is the only variable under control. 21 "……... the state equations of each model…..": why the "state" equations? I presume these are the "equations of motion" or "model equations" or something similar. Reference to the "state" of anything is here

really misleading! The equations of motion could describe no steady "climate" at all!

473, 13 "….the temperature response is equilibrating in < 5000 years….."; question: are we sure that the initial model climate is fully "equilibrated"? 15 "….because the 5000 year time constant will only have expressed 63% of its response by then.": what does this mean? 20 "A 10 year increment for τ was found to be an adequate trade off between, on the one hand capturing the subtleties of the g(τ) relationship, whilst on the other being coarse enough to make the estimation of g(τ) manageable.". Given the statistical inference following in the analysis proposed in the paper the sampling step should be decided on the basis of the stability of the estimators.

475, 10 "….not impossible" is "….not possible…" ?

477, 20 "Well mixed systems ….. ": the necessity of the consequence that "…poorly mixed systems such as the global deep oceans, which have a blend of both diffusive and circulatory distribution mechanisms, produce a peaked distribution of feedback amplitudes with timescale." is not clear to me as, a priori, I could expect any distribution.

480, 16 "… the reduced order models are able to explain more than 99.9% of the variance of their higher order parents.". This type of claim is typical of a posteriori analysis of complex processes: for decades we have been exposed to similar claims in the context, for example, of principal component analysis: ok, it is possible to "capture" (not "explain"!) virtually all of the process variance with a reduced number of components, but this does not mean that the dynamics of the process can be correctly projected onto the statistically identified process: there is no intrinsic predictive skill into this operation. Since Lorenz 1980 (at least) we know that the time modulations that have more statistical weight are not necessarily the ones on which nonlinear system dynamics "projects" its dynamics (time derivatives). Otherwise, we would have very drastically reduced the number of components of model equations a long time ago!

Conclusion

I think the paper proposes interesting results concerning time scale analysis of heat exchanges in climate models during relaxation experiments. However such results should be proposed in a correct context.

A couple of suggestions for further analysis along the way followed in the paper : - the existence of “peaks” in the statistical distribution in Fig. 4 (a) could be consequence of some specific “balancing” of terms in the equations of motion; this, where possible, could be worth investigating; - time scale analysis can be also performed in a nonlinear context.

But the central problem remains the analysis of real “climate state” (and not of any “surrogate”).

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