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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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"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

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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."

The equilibration of the global mean behaviour of climate models is an area that has received much attention and the norms of this field are to consider climate models as fully damped systems, hence the ubiquitous use of global metrics such as 'equilibrium climate sensitivity' (ECS). In addition, it is also a norm to analyse the global mean surface temperature (GMST) response of these models and to consider this as an appropriate means of characterising the global aggregate response of these models. Indeed, this is how ECS is defined. Therefore, although I agree that the global mean behaviour of A-OGCMs does not provide a full picture of the underlying dynamic behaviour, it can provide valuable insights into the processes that dominate the global aggregate response. Furthermore, the analysis presented in this manuscript includes that of a Global Energy Balance Model (GEBM) where the complexity alluded to by the reviewer is absent and GMST is most definitely a state variable, not an average.

"Looking at the same problem at another angle, average temperature is not a "climate state parameter"."

Here and elsewhere I'm not sure who is being quoted. I've searched my text and I do not refer to GMST as a "climate state parameter". However, if through the type of analysis that is presented the reviewer believes this is implied I'm more than happy to include a written caveat to cover this when discussing the A-OGCM and EMIC responses.

"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."

Again, the manuscript does not refer to GMST as a "climate state".

"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."

This is highlighted in the Discussion and Conclusion section which currently reads (p15, L24 onwards):

"To see if the results presented here are more general obviously requires more full equilibrium run data sets becoming available. Because ocean circulation can depend on the nature of the forcing applied to A-OGCM's (Manabe et al., 1991; Stouffer, 2004) g(tau) can be a function of the forcing disturbance, making any generalisation more difficult."

However, I agree that this important point needs to also be raised in the Introduction too so that the reader is aware of this up front. The revised manuscript will include this.

"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."

The 'orthodox' view in the climate literature would be that climate can, in part, be characterised by global average metrics. However, I think it would be valuable to include some further discussion of the restrictions of considering particular time evolutions of GMST in A-OGCMs as suggested, and the revised manuscript will include this. However, I cannot sign up for "eliminating from the text all the reference (or allusions) to "climate" as represented by average global surface temperature", not least because, in the GEBM case, the analysis as presented is an accurate representation of the climate model dynamics. Although it would be wrong to conclude the A-OGCM and EMIC are similarly linear, it is clear that the linear feedback analysis provides a picture that is consistent across all three model types. Finally, as raised earlier, the analysis of global mean behaviours of climate models has a long, established history and the case for rejecting all such analyses needs to be made (elsewhere). Until such a time, it is entirely appropriate to contribute further to the analysis of the global mean response of climate models, as is done by this manuscript.

"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”"

Equation (1) is ubiquitous in the climate literature and used in exactly this way (i.e. in the analysis of GMST data). If it helps, I will add a list of the assumptions that are being made when applying this to GMST data.

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

Will do.

"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."

Again, the GMST context will be explained in the revision. With regard proof, it is clear from the data presented in Figure 3 that these three models each move from one equilibrium to another without oscillations. Furthermore, these are the norms of this field of research and underpin the ubiquitous use of EQS.

"“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!"

Happy to replace “state equations” with “model equations”.

"473, 13 “: : :.the temperature response is equilibrating in < 5000 years: : :.”; question: are we sure that the initial model climate is fully “equilibrated”?"

Inspection of the ‘control runs’ of the A-OGCM would suggest that the answer is ‘yes’. For the EMIC and the GEBM this is guaranteed.

"15 “: : :.because the 5000 year time constant will only have expressed 63% of its response by then.”: what does this mean?"

The intention is to point out that the 5000 year time constant feedback was more than sufficient to account for the longest timescale dynamics in the GMST responses. I will revise the text to make sure this is clear.

"20 “A 10 year increment for was found to be an adequate trade off between, on the one hand capturing the subtleties of the g(tau) relationship, whilst on the other being coarse enough to make the estimation of g(tau) 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."

I’m not sure what exactly is meant by “the sampling step should be decided on the basis of the stability of the estimators”. The the sample interval for the numerical approximation is annual, in keeping with the GMST data (P8, L14-). This is 10 times faster than the fastest feedback being estimated (because the instantaneous feedback is factored into the reference system). The selection of the values of tau against which g(tau) is estimated does not affect the stability of the numerical approximation. Although I’m sure a more sophisticated approach could be used in future to identify the ‘optimum’ distribution of timescales to use, the results are sufficiently robust to suggest

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that formalising the optimisation of the timescale distribution is unlikely to change the results substantially. However, a more sophisticated approach could conceivably make the annealing code run much faster!

"475, 10 “not impossible” is “not possible” ?"

Will correct

"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."

If diffusive systems have an inverse timescale-amplitude distributions and well mixed systems have infinitely peaked distributions (with a single timescale and amplitude) then partially mixed systems have to be intermediate between the two. I will expand the discussion of this in the revised manuscript to make this clearer.

"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!"

I agree “capture” is better than “explain” and I’ll change this. As for the arguments

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over whether the reduced order model is useful; I agree that often they are nothing more than fits and explain nothing about the parent system. However, in this case this is not true. The dynamics of a system described by a near symmetric or low variance distribution of timescales-amplitudes is fully described by its first moment. As a result, the reduced order model captures the dominant dynamics of the parent system in a meaningful way. Furthermore, these simple models are commonplace in climate studies and this manuscript shows, for the first time, why they provide such useful summaries of the parent model they seek to emulate.

"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."

Thank you for this suggestion. I will look into this. However, it must be kept in mind that the GEBM, and to a lesser extent the EMIC, are not governed by these equations.

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

See previous responses

Many thanks for these helpful comments

Interactive comment on Earth Syst. Dynam. Discuss., 2, 467, 2011.