Earth Syst. Dynam. Discuss., 2, C308–C313, 2011
www.earth-syst-dynam-discuss.net/2/C308/2011/

Earth System Dynamics Discussions

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

A. Jarvis

a.jarvis@lancs.ac.uk

Received and published: 31 October 2011

"- The abstract should reflect that the issue of transient negative feedbacks is already published (Baker and Roe), but that discrete phases emerge in the present study due to ocean circulation being included."

Happy to include this point and reference.

"- It would be worth bringing Baker and Roe greater prominence in the introduction (perhaps start of second paragraph), as this is a key building block for the current study."

Again, happy to do this although the current manuscript reads (P3, L15-19):

“For example, Baker and Roe (2009) investigated the effect of the oceanic heat uptake feedback on equilibrium climate sensitivity and transient climate change using a simple 1d upwelling-diffusion Global Energy Balance Model (GEBM) and showed the importance of characterising feedbacks with different timescales when inferring climate sensitivity.”

In addition, it is important to realise that, unlike all previous work on this subject other than Hanson et al (1985; Science, 229, 857–859, 1985), in the current paper timescale is not defined simply in the time domain, but the time constant(s) are used as a linear dynamic measure of feedback response. That said, I’m more than happy expand discussions within the context of the Baker and Roe (2009) work given it is very relevant.

"- and in section 5 (results), explicit discussion on the contrast between your Fig 4 and Baker and Roe Fig 6. would be useful (rather than just saying, cf. Fig 4b with their Fig 6)."

I agree, this needs more explanation than I have given in the original manuscript. I will expand on the similarities and differences, not least to say how their Figure 6 is of the time evolution of the feedback strength whilst my Figure 4 is of the (stationary) feedback strength with increasing timescale (time constant(s)).

"- Also, could mention that Gregory and Forster 08 kind of include a transient ocean feedback (albeit focussing on a narrow timescale range). Indeed, the current paper mentions later on that Gregory and Forster 08 home in on _70/e year timescale (i.e. not equilibrium as implied in the introduction)."

I will also mention this in the Introduction as suggested, although again Gregory and Forster (2008) did not appreciate the 70/e timescale they are working around and I think its important to spell out the uniqueness of the approach I am advocating.

"- another relevant paper: Williams, K.D., W.J. Ingram and J.M. Gregory (2008): Time variation of effective climate sensitivity in GCMs. J. Climate 21(19) 5076-5090

ESDD
2, C308–C313, 2011

doi:10.1175/2008JCLI2371.1."

Thanks for pointing this out, again it is relevant and I will include it, although again note they are discussing “Time variations” rather than timescales.

"Second, accessibility could be improved with more effort to separate technical details and language from more physical discussion (e.g. ’feedforward dynamics’). Subheadings could help. Section 7 could be more clearly separated this way. Perhaps in the Results section it would be worth re-iterating briefly in words up front what G and g are. I found use of g and G confusing, as G is often used to mean ’gain’."

I agree that both use of sub-headings and further clarification of g and G would be valuable for improving clarity and I will address this.

"The use of ’three climate models’ is overstated. There is one GCM, and two simpler models. It is encouraging that the simpler models give similar profiles to the GCM, rather than implying that adding the simpler models makes the results much more robust."

Will modify text accordingly

"Move the sentence ’Rather than split...’ up to be the second sentence, as this is the key method point."

Will do

"In the description of the results, I would use the phrase ’can be viewed as’ or similar, when talking about a negative feedback eroded by spectrum of positive feedbacks. Personally I find Baker and Roe’s alternative view of a transient negative feedback more intuitive, but perhaps the progressively-eroded negative view makes sense in the framework of the current analysis."

It does make more sense as we are specifying what is causing the negative feedback to be transient rather than leaving this hanging. However, I agree the sentence in

ESDD

2, C308–C313, 2011

question would benefit from reading “A transient negative feedback can be viewed as a negative feedback that is eroded by a series of lagged positive feedbacks, as shown in Fig. 4.”

"It would help to introduce more clearly what you mean by a reference system (with ref to Roe 2009 right at start). If the reader is familiar with reference systems, the first two sentences are redundant."

My reading of the climate literature on feedbacks is that many researchers in this area are not familiar with the critical need to define the reference system before defining the feedback(s) which operate on it. For me this is the primary reason for the confusion that is often propagated in this area. For example, most climate scientists believe feedbacks are physical things. However, this cannot be so because if we simply change the definition (bounds) of the reference system the definition of the feedback(s) has to change accordingly. As a result, feedbacks are systems traits, not physical entities, and feedback analysis is simply a construct within which to explore the dynamic behaviour of something. I included this in an earlier version of this manuscript (submitted elsewhere), but it proved inflammatory so I removed it. If it helps, I would very much like to re-include it within the context of a more careful definition/explanation of what reference system I am using.

"Also, clarify what exactly you use as a reference system here. Currently it sounds a bit like everything except the deep ocean is in the reference system."

It is the atmosphere up to the tropopause (because rapid stratospheric adjustment is included in the input forcing term) in addition to the ocean mixed layer. I will expand to the text to make this clear.

"The paragraph starting: ’the physical processes giving rise’ can be removed, as this has been stated twice already (abstract and introduction)."

More than happy to do that although I’ll need to retain the Aires and Rossow (2003)

ESDD

2, C308–C313, 2011

reference by modifying the following paragraph slightly as I assume linearity at this stage and need to underline the possibility of the nonlinear case (particularly given Professor Speranza' comments).

"The final paragraph tends to exaggerate the problem: - 'G only corresponds... as tau->infinity'. Alternatively, if g peaks below the timescale of interest. - 'highly context specific'. Your results, with the plateaus in G, suggest that it's not _that_ bad."

I would agree if the norms were to define climate sensitivities for timescales that corresponded to these plateaus. Unfortunately, effective climate sensitivities are not defined in this way. That said, I will remove 'highly' if that helps.

"Perhaps this could be called, 'quantifying the relationship...', since it is a method section"

Good idea and will do.

"It would help the reader to identify exactly what your aiming at if you move to the top of the section the part from 'A finite difference approximation...', to 'The objective is to find the values of'. This last sentence is a key one which should be highlighted, in advance of details about annealing and choosing tau."

That makes sense. I will reorganise this section along these lines.

"A question: in specifying tau, did you consider the tau intervals growing with timescale? I.e. I would have thought your method could separate timescales 10 and 20 much better than 1010 and 1020."

This is a good question and yes I spent a significant amount of time trying to do this. However, you need to make some strong prior assumptions about the distribution of g(tau) to do this. Also, the only advantage of doing this is that it would lead to radically faster code, not a different result, because the fixed 10 year increment is sufficient to capture the 'fast' feedbacks.

ESDD

2, C308–C313, 2011

"About the method's tendency to smooth peaks: is this because you are averaging over 100 anneals? If different anneals tend to centre the peak at different tau, this would smooth the peak. This may be improved I guess. e.g. you could look at the mean magnitude of the peak across the 100 anneals."

It is mainly due to the inefficiency of the annealing itself, not the averaging. The annealing is not an efficient estimator and so struggles to resolve some detail, hence the smoothing. This inefficiency is also the reason I am using annealing (see P8 :1 -).

"Some discussion on the nature of the three models would be worthwhile. In particular the ocean treatment, which is central to these results."

Good point. I will expand this section accordingly.

"The first sentence could be clarified - somewhat obscures the clear point you're trying to make."

I see what you mean! I will clarify/simplify this statement.

"'BaRker and Roe'..... 'so UNpredicatable'"

Noted and will be corrected.

"for GFDL, tau_d and tau_c are reversed I think."

You are right, and I will correct this.

Many thanks for this helpful review.

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