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Earth System Dynamics Discussions

Interactive comment on "Analytically tractable climate-carbon cycle feedbacks under 21st century anthropogenic forcing" *by* Steven J. Lade et al.

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This is a nicely designed study, and well presented manuscript which attempts to develop and document a simple ("stylized") model of the global climate-carbon cycle system, but in a way which enables analytical analysis of its behaviour and feedback mechanisms. The intended aim is to facilitate improved understanding of the system dynamics and more readily quantify which processes contribute to certain feedbacks and long-term responses.

Overall, I very much like the approach and the intention – such modelling studies can develop improved insight and can strip back confounding and complicating issues of

model complexity to reveal more fundamental underlying behaviour.

I have a few comments below and a few queries about the extent to which the intentions have been realised – I think these can be readily addressed with revised text and some more context/explanation.

Chris Jones.

Overarching comments:

1. There are numerous simple/stylized carbon cycle models in the literature. You cite Raupach (2013) which I like very much. There is also one I developed and have published with several times, including as recently as last year (Jones et al., Tellus, 2003; Jones et al., Tellus, 2006; Jones et al., ERL, 2016). Then there is the MAGICC model often used in IAMs, the Joos IRF, and also the Oscar model which some of the current authors know very well. So I wonder if a bit more explanation is needed for why a new model is required – could you start from one of the existing ones and achieve the same thing? I guess your main driver is the ability to analytically derive the feedback functions – but it's not clear to me the same is not possible from these previous models (I haven't tried it with the Jones et al simple model, but may do!) – Mike Raupach derived eigenmodes, so I would imagine feedback metrics could follow also, but again I haven't tried.

2. On a similar line – I was looking forward to seeing the analytical derivation of feedback factors, but then realised this was not as "tractable" as your title suggests, and you have to make a lot of assumptions in your sections 4.2 and 4.3. This seems a shame – if this is the case, have you not lost your unique and attractive feature? The resulting expressions are still useful, but not as analytical as you suggest. I also wondered, on seeing the expressions in table 3, if you had compared these to the expressions of Ric Williams et al – who have done a similar based assessment of terms controlling ocean heat/carbon uptake and TCRE. (See e.g., Richard G Williams et al 2016 Environ. Res. Lett. 11 015003)

3. You discuss (page 18, line 16) that you might want to develop this model further to include other mechanisms and forcings. Modellers have attempted a two-box approach for the ocean – by splitting upper and deep ocean before. But I would suggest maybe a two-box approach for the land also – not splitting veg vs soil, but maybe by tropics and extra-tropics as these can have very different responses (even by sign) to changes in climate. IPCC AR5 fig 6.22 shows the latitudinal distribution of "gamma" – there is a clear change of sign towards high latitudes, and so a tropics/high-latitude 2-box approach might be a nice (and novel) development

4. In a few places, you discuss non-linearities – this is good, and important to bring out. It's not necessarily true that land dominates the non-linearity, but this does show up in your rapid-forcing transient runs. If you ran longer, or with slower changing forcing, the ocean would have more chance to exhibit these too – see, e.g. Schwinger et al (J. Climate, 2014). Hence both land and ocean can have pronounced non-linearities and for this reason, C4MIP took the decision to move back towards the Friedlingstein definition of feedbacks as the difference between COUPLED and BGC runs (see Jones et al., GMD, 2016 – documentation of C4MIP). Hence this differs from the Arora et al definition of using the RAD forced run.

5. My final request would be to ask if you can more directly or relevantly bring this back to complex models – how does this approach help us develop/evaluate/constrain them further? For example you claim in the discussion that the carbon-climate feedbacks are "less sensitive" than the carbon-concentration ones. And that this is due to "the shape of K(Ca,DT)". So how does that help with my ESM? What controls the shape of this in ESMs? And can we measure and constrain it from obs? If so, then your analysis brings a way in which we might narrow the spread in model projections, or at least evaluate a very relevant aspect of the models. If not, then all it does is leave us with a better feel of why the models continue to diverge – if you have any ideas how to make this jump that would be great to see.

Minor comments:

1. Page 4. Your NPP function of CO2 claims to include the effects of climate change – but surely these also depend on the climate sensitivity. For models with high/low climate sensitivity, there is a different trade-off of the effects of CO2 and climate. So I don't follow how the impact on NPP can be made without reference back to the temperature as well as the CO2

2. Page 7. I couldn't quite see if you had a link between ocean heat and ocean carbon uptake – I don't think so. Should these be related? There might be a false extra degree of freedom in your model – I would expect for example rapidly mixed oceans to have high rates of both heat and carbon uptake – and vice versa for poorly mixing oceans. But if you have independent mechanisms of carbon uptake and transient response to climate do you miss this link?

3. Table 1: please be careful to stress "climate sensitivity" as "transient climate response" – you do say so, but using the wrong name makes it look like a very low value (1.8K)

4. Page 14, line 23 – just to check here you mean "1% increase up to double CO2" and not a step-change to double CO2.

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