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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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General comments

The authors introduce a new variant of a simple analytical, highly parameterised global carbon cycle - climate model, which is used to formally analyse the four major feedback loops in the system, i.e. the land and ocean concentration carbon feedbacks and the land and ocean climate carbon feedbacks. The simplicity of the approach allows the authors to derive analytical approximations to the definitions of various feedbacks metrics at play in the global carbon cycle - climate system.

Simple analytical global carbon cycle models and simple climate models have been used many times in the past. Also the literature contains several simple coupled carbon cycle - climate models (e.g. Gregory et al., 2009 or Meinshausen et al., 2011). It is not clear however, what this particular new variant adds to our understanding of the global carbon cycle - climate system. The motivation outlined in the introduction is not very convincing. The dynamic characteristics of the chosen "mechanistic" model formulation clearly is determined by the simple model structure and the chosen parametrisations of the exchange fluxes. Also the stated "biophysical or biogeochemical interpretation of the model parameters", given that these represent global averages is plausible but not very compelling. E.g. why should the global CO_2 fertilisation effect work in reality in a way as parameterized here with a simple β-factor formulation? Or global respiration with a simple Q_{10} temperature response? Perhaps the main value of the simple model is educational, as it can easily be programmed by students and one can show in this simple model system how the feedback metrics are computed. But as a tool for policymakers nor for generating new carbon cycle science, this model does not provide added value to the already existing simple models. A simple model "tuned" to emulate one or several of the more complex models would be more useful.

Perhaps a missed opportunity for demonstrating the validity of the model is a more careful calibration and evaluation. Clearly the "mechanistic" model parameter values are not based on first principles, but contain large uncertainties. E.g. the Q_{10} value used here (1.72) is highly uncertain (see e.g. Mahecha et al., 2010). Why not tune the model parameters so that the current global carbon budget is properly matched? The model substantially underestimates the historical ocean carbon uptake (Table 2), and, when driven with the historical emissions from the Global Carbon Project (Le Quere et al., 2017), the numerical version of the model underestimates the current ocean uptake. In addition, a graph showing the model performance against the atmospheric CO_2 record from ice cores and direct observations could demonstrate that at least on multi-decadal time scales the model performs reasonably. Figure 2 clearly is not sufficient as it does not show any observations. Another useful model evaluation would be to follow the impulse response simulation protocol defined by Joos et al. (2013) and compare the dynamics of this model with the impulse response simulations of more comprehensive models as shown in that paper.

Specific comments

1. As shown in Table 3, the results of the analytical approximations of the feedback metrics compared to the numerical simulations is pretty poor. Does this not invalidate the simplifications made in deriving the analytical approximations?
2. The comparison of the feedback metrics with the results of Zickfeld et al. (2011) and Friedlingstein et al. (2006) in Table 3 shows that the simple model with the chosen parameter values responds substantially different - the discrepancies range up to a factor of 2. This is clearly at odds to what is claimed in section 5.1 and 5.2.
3. On the other hand, also the comprehensive models show a large spread in the feedback metrics. A more useful analysis/comparison would be possible if the model parameters were tuned to emulate the various comprehensive models.
4. The statements in section 5.2 and 5.3 about the behaviour of the carbon cycle - climate system and the feedback metrics under increasing emissions clearly refer to this particular simple model. While plausible, the real world may behave differently.
5. The direct ocean concentration-carbon feedback given as exact in Table A1 and approximated in Table 3 (5th line from bottom) differ very much: Evaluated with the standard model parameters at a value of ca corresponding to 800 ppm the exact formula gives 0.0152 PgC/(ppm yr) while the approximation gives 0.396 PgC/(ppm yr). (I assumed in the exact formula that the symbol w is actually w_0).

Also the solid red curve showing B_O in Figure A1a is missing. Obviously there is some error in the listed formulas or the chosen approximation is very poor.

Technical corrections

Technically, the formulas in the manuscript contain a some inconsistencies and not correctly defined symbols.

- p. 4, line 25: In the exponent of Q_R the symbol T should be replaced by ΔT.
- p.5, line 13: The way the Revelle factor is used here is weird: Formally, using the notation here, it is defined as:

$$R = \frac{\partial p(c_m, 0)}{\partial c_m} \cdot \frac{c_{m0}}{p(c_{m0}, 0)} \tag{1}$$

  Inserting the definition $p(c_m, \Delta T)$ given here (eq (5)) this expression does not evaluate to the constant r as it should according to the text.

- p. 6, line 25: The atmosphere equation, written as an integral equation is weird. Why not write it similar to the biosphere and ocean mixed layer equation as normal first order differential equation?

$$\frac{dc_a}{dt} = e(t) + \frac{Dc_{m0}}{rp(c_{m0}, 0)}(p(c_m, \Delta T) - c_a) + \frac{\mathsf{NPP}_0}{c_{t0}} Q_R^{\Delta T/10}(c_t - K(c_a, T)) + \mathsf{LUC}(t) \tag{2}$$

  where $e(t)$ are the emissions (in PgC/yr); E(t) in equation (8) are the integrated emissions (this is nowhere defined in the text, and wrongly described on p.5 line 7).

- p. 7, eq 9: For consistency with the text the symbol T in the differential quotient on the left should be replaced by ΔT.

- Table 3, 4th and 3rd line from bottom: The references to the Figures A1a and A1b are not correct.
- Table A1: What is the meaning of w (without subscript)? Presumably it should be w_0?

Literature

- Gregory, J. M., Jones, C. D., Cadule, P., and Friedlingstein, P. (2009), Quantifying Carbon Cycle Feedbacks, J Climate, 22(19), 5232-5250.
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