

## Response to review by M. Heimann

We thank the reviewer for their considered and constructive comments. Please find below our responses to the reviewer's comments. We have uploaded our proposed revised manuscript that addresses this and the other reviewer's comments in a separate Author Comment on the article's Discussion page.

### *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.*

We thank the reviewer for prompting us to make explicit what our work “adds to our understanding of the global carbon cycle - climate system”. As we state in the revised version of the abstract, three specific results of our work are:

- that different feedback formalisms measure fundamentally the same climate-carbon cycle processes;
- that temperature dependence of the solubility pump, biological pump, and CO<sub>2</sub> solubility all contribute approximately equally to the ocean climate-carbon feedback; and
- that concentration-carbon feedbacks may be more sensitive to future climate change than climate-carbon feedbacks.

These results would not have been possible without the simple, mechanistically-based model that we develop in this manuscript.

Regarding the previous models that the reviewer cites: There is no explicit representation of biophysical processes in the model of Gregory et al. (2009), which consists of fits to fluxes between ocean, atmosphere and land carbon stocks predicted by C4MIP models, or the ocean carbon cycle component of Meinshausen et al. (2011), which is a parametric fit to predicted impulse response functions. That our model is a mechanistically based representation of the carbon cycle, even if that representation is highly aggregated and simplified, allows us to deliver the insights above. We concede however that our motivation of the model in the Introduction was not particularly detailed on these points. In the revised manuscript we will expand upon the model's motivation in the second paragraph of the Introduction, as well as refining the more detailed description at the start of section 2.

*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<sub>2</sub> fertilisation effect work in reality in a way as parameterized here with a simple  $\beta$ -factor formulation? Or global respiration with a simple Q<sub>10</sub> temperature response?*

We use the term “mechanistic” to convey that we have in our model representations of real-world processes, such as photosynthesis, respiration, ocean-atmosphere diffusion and the solubility and biological pumps. Our model is not a precise, first-principles mechanistic description of these processes at the microscopic scale, but then again all models are simplifications of reality; we merely choose to perform the simplification at a more aggregated level than most Earth System Models. The  $\beta$ -factor (or ‘Keeling formula’) and Q<sub>10</sub> temperature responses are previously used parameterisations of the response to climate change of globally aggregated NPP and respiration, respectively. We will clarify our use of the term ‘mechanistic’ in the revised manuscript (see second paragraph of the introduction).

*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.*

Gregory et al. (2009) and Meinshausen et al. (2011), as well as others, already provide simple models “tuned” to emulate one or several of the more comprehensive models. We believe there is scope for a model such as ours, in which we do not force our model to closely fit historical data (or future projections) but rather parameterise each process with the best available (globally aggregated) knowledge about that process. See our response to the next comment below for further information.

*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<sub>10</sub> 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<sub>2</sub> 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.*

We thank the reviewer for these comments and suggestions on model calibration and evaluation. Following the reviewer's suggestion, in the revised version of the paper we will include historical carbon fluxes and temperature anomalies alongside model predictions (see revised Fig 2).

We have attempted to 'tune' several different combinations of parameters to match current carbon stocks (one example is  $K_C = 0.25$ ,  $Q_R = 2.5$  and  $w_0 = 0.2$ ). However the tuned parameter sets lie well outside the best available independent estimates of those parameters (see references in Table 1). This is not surprising since we do not expect a mechanistically based model of this simplicity to precisely reproduce historical carbon stocks.

Rather than forcing the model to fit historical data, we choose to parameterise each process with the best available knowledge about that process. Gaps between our model and observations then point to what other processes should be included in a more complex model to improve accuracy. This is in line with our stated model purposes of understanding and learning, rather than emulation and prediction. In the revised manuscript, we will clarify our choices taken during the parameterisation of our model (see section 3).

#### *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?*

We concede that in the submitted version of the manuscript, while the land feedback metrics were accurate, the agreement between the numerical and analytical results for ocean feedback metrics was poor. Deriving approximate metrics for ocean feedbacks is challenging, as the deep ocean does not reach equilibrium on the time scale of our simulation. We have taken the opportunity to derive alternative approximations to the ocean feedback metrics (see description in section 4.2). The approximated ocean climate-carbon feedback is now more accurate. The ocean concentration-carbon feedback remains in poor agreement. As we will explain in the revised manuscript (see second paragraph of section 5.2), this is partly due to an approximation made in analytically estimating the deep ocean uptake, but partly also due to numerical concentration-carbon feedback calculations requiring climate-carbon feedbacks to be switched off.

*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.*

We thank the reviewer for prompting us to clarify what we judge as 'agreement' between the results of our simple model and the results of previous simulations. First, we note that the results of complex models display considerable spread (as also noted by the reviewer in the following point below). While some of our results differ by nearly a factor of 2 from the mean results of Friedlingstein et al, all our feedback metric results are within their reported spread

(Table 3). Second, we consider it remarkable that such a simple model can reproduce the results of highly complex models so closely, and would not consider a discrepancy of a factor of 2 an invalidation of the simple model. We will discuss these discrepancies more carefully in the revised manuscript (see first paragraph of section 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.*

This is an interesting idea, but beyond the scope of our study. As discussed above, our goal is not to emulate or evaluate ESMs, but rather to develop process-based understanding.

*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.*

We thank the reviewer for raising this concern. It is correct that our model can only anticipate changes in the carbon cycle arising from those processes that it has modelled -- and may therefore neglect other important future changes in the carbon cycle. We will acknowledge this caveat in the manuscript (see second-last paragraph of section 5.2).

*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 BO in Figure A1a is missing. Obviously there is some error in the listed formulas or the chosen approximation is very poor.*

We respectfully disagree with the reviewer's calculations. By our calculations, under the conditions the reviewer indicated the approximation gives 0.0398 PgC/(ppm yr). While this is not as severe as the 20-30 times the reviewer suggested, it is still a significant difference at 2 to 3 times the exact expression. We took the opportunity to derive a more precise approximation (see Table 3 and the last sentence of section 4.3) that gives a value 0.0240 PgC/(ppm yr).

We thank the reviewer for noticing the omitted curve in Figure A1; this will be rectified in the revised manuscript (see our proposed revision).

#### *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 QR the symbol  $T$  should be replaced by  $\Delta T$ .*

Thank you, in the revised manuscript we will correct this mistake.

• p.5, line 13: *The way the Revelle factor is used here is weird: Formally, using the notation here, it is defined as:*

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

*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.*

We respectfully disagree with the reviewer's general definition of Revelle factor. According to Sabine et al. (2004) [see citation in our manuscript] and the AR4 [see [https://www.ipcc.ch/publications\\_and\\_data/ar4/wg1/en/ch7s7-3-4-2.html](https://www.ipcc.ch/publications_and_data/ar4/wg1/en/ch7s7-3-4-2.html)], the general definition of Revelle factor is in our notation

$$R = \frac{\partial p(c_m, 0)}{\partial c_m} \frac{c_m}{p(c_m, 0)}$$

that is, the mixed-layer ocean carbon stock in the right-hand quotient should not be fixed at pre-industrial CO<sub>2</sub> levels. Substituting our model's expression for partial pressure of CO<sub>2</sub> [equation 5 in our manuscript] gives  $R = r$  for all  $c_m > c_{m0}$  as expected.

• 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?*

<equation omitted>

*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).*

We agree that the form of the atmosphere equation is unusual! In line with the suggestion of the comments provided by Heitzig (see above), we will rewrite equation 8 as an algebraic equation for conservation of carbon amongst our stocks, alongside a new differential equation to account for aggregate carbon flows into or out of our three stocks. This formulation will remove all integral equations.

We thank the reviewer for identifying that  $E(t)$  is incorrectly defined. We will correct this mistake in the revised manuscript (see definition preceding the new equation 9 and section 3).

• p. 7, eq 9: *For consistency with the text the symbol  $T$  in the differential quotient on the left should be replaced by  $\Delta T$ .*

Thank you, in the revised manuscript we will make this change to improve the clarity of the manuscript (see equation 10 in the revised manuscript).

• *Table 3, 4th and 3rd line from bottom: The references to the Figures A1a and A1b are not correct.*

Thank you for noting this mistake. We will correct the figure numbering in the revised manuscript.

- *Table A1: What is the meaning of  $w$  (without subscript)? Presumably it should be  $w_0$ ?*

We thank the reviewer for noting this mistake. We confirm the  $w$  in Table A1 should be  $w_0$ . We will correct this mistake in the revised manuscript.