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Interactive comment on "The Bern Simple Climate Model (BernSCM) v1.0: an extensible and fully documented open source reimplementation of the Bern reduced form model for global carbon cycle-climate simulations" *by* Kuno Strassmann and Fortunat Joos

Anonymous Referee #2

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This paper by Strassmann and Joos presents the reimplementation of the Bern Simple Climate Model (BernSCM), a reduced form model of the anthropogenic perturbation of the carbon-climate system. This is a historic model for the community, since it and its offspring have been used since the IPCC SAR. This new implementation is useful for the community, especially as this paper focuses on transparency and the model's code is provided in an open-source format.

Being an old model, the BernSCM ignores some relatively recent developments in climate sciences and modeling. In itself, it is not so much of a problem, as the authors leave the door open to further development of the model, both in the manuscript and in the model's code. However, mention and discussion of these caveats is required, especially regarding some specific points I develop below.

I also believe that the paper could benefit from a more careful rewriting, especially for some sections that I had to read several times – and I am still not 100% sure of what is done in some parts of the paper! In all honesty, some parts give the impression that the authors were in a rush for writing the paper.

So I fear publication can only be recommended if the few scientific issues I raise below are answered/discussed, and if the text itself is improved.

Major points:

1. My first point concerns the use of the same IRF parameters for the ocean carbon cycle and the climate system. If I understand it well, the function r_O is the same for determining the ocean C sink and the temperature change, e.g. in equations (15) and (16). Although it would seem intuitive to use the same function, because – obviously – we are talking about the (same) world's ocean in both cases, I see several issues in doing so.

First, I am not quite sure one can assume that the diffusion process is the same for heat and for actual material such as carbon. (The assumption seems more reasonable for convection.) But more importantly, the biological pump does not affect heat transport, while it does for carbon. (Although, I am not sure whether there was a biological pump at all in the models used to calibrate the r_O function – another thing worth being mentioned.)

Second, global patterns of heat uptake vs. carbon uptake are different. This means that one unit of incoming f_O is dispatched differently than one of f_O^H, at the scale of the global surface ocean. Therefore, it is likely that each of them is affected differently by the oceanic circulation. For the climate response, it is also known that this pattern affects an internal feedback (the ocean heat uptake feedback) in a way that changes the apparent time-scales of the climate response, see e.g. Geoffroy et al. (2013b) and references therein.

Third, the typical climate IRF only has two time-scales (e.g. Geoffroy et al., 2013a), and these are quite different from the time-scales from Joos et al. (1996). And, maybe more importantly, the typical two-box climate model implied by the typical climate IRF (Geoffroy et al., 2013a) includes a bidirectional exchange of energy between the surface and deep oceans. This is not the case in the assumed formulation presented here. There are some fundamental reasons for not having this bidirectional exchange for the carbon cycle: the so-called 'ocean invasion' is a slow process, and ultimately there is a sink of C in the deep ocean that involves geological chemical reactions (and time-scales). But can this be also applied to the climate system and heat transport?

Therefore, I believe this is an assumption made by the authors that r_O can be applied to the climate system as well. Despite all of the above, it may still be acceptable. But it should be presented as such, and it also warrants a discussion in the text. Additionally, the response to a step of radiative forcing (typically 4x CO_2) of this climate model has to be compared to that of more complex models. I strongly suggest adding a (sub)figure in which the BernSCM climate response is compared to that of CMIP5 models, taken e.g. from Geoffroy et al. (2013b). This would complement figure 3.

2. I was very troubled by section 3 and how the carbon-climate feedbacks are represented/investigated with BernSCM, in relation with C4MIP. At first, I thought BernSCM was trying to emulate the C4MIP models' sensitivities (which would have been a new feature).

In the end, my understanding is that the uncertainty range provided e.g. in table 4 is obtained by combining variations of: (i) the ocean model, 2 options; (ii) the land model, 2 options; (iii) the experimental setup, i.e. coupled/uncoupled/Tonly/Conly, 4 options. That is a total of 2×2×4=16 configurations. But my concern, here, is that I think that turning a process on or off can hardly be considered a new configuration of the model. Therefore, although the results shown e.g. in figure 3 or 4 are interesting, the ranges provided in table 4 are artificial and misleading.

More generally speaking, the text should make it clear that there are not many parameterizations available for the model, and so it does not cover the full range of existing multi-model uncertainty (and therefore, it cannot be used in a probabilistic fashion). Again, it is not so much of a problem in itself, but this has to be made very clear.

3. I have some trouble with the way the solving of the differential system is presented, but more importantly I believe there is a mistake with how the temperature-dependent parameters are implemented.

I am not convinced by the lengthy demonstration of appendix A1. Equations (A2) and (A3) are the 'results' of this section, and I believe the following demonstration is not needed. Equations (A2) and (A3) can simply be obtained by using the 'exponential integrator' method to solving a first-order differential system. Although not everyone may know this method, it could be summed up in one or two equations (and appropriate references) rather than be re-demonstrated from scratch.

Equations (A2) and (A3) are simply obtained by 'reminding the reader' that the solution to the differential system:

$$\frac{\mathrm{d}m}{\mathrm{d}t} = -\frac{m(t)}{\tau} + a\,F(t) \tag{1}$$

can be discretized by multiplying by $\exp(\frac{\delta t}{\tau})$ and integrating between t_n and $t_{n+1} = t_n + \delta t$:

$$m_{n+1} = \exp -\frac{\delta t}{\tau} m_n + \int_0^{\delta t} \exp(-\frac{\delta t - s}{\tau})\, a\, F(t_n + s)\, \mathrm{d}s \tag{2}$$

where $m_{n+1} = m(t_{n+1}) = m(t_n + \delta t)$, and δt is the time step.

The above equation is exact, but can hardly be solved. It is usual to assume that F is constant over the small time period of δt, which leads to the solution:

$$m_{n+1} = \exp(-\frac{\delta t}{\tau})\, m_n + \tau\, (1 - \exp(-\frac{\delta t}{\tau}))\, a\, F(\tilde{t}) \tag{3}$$

which is basically equation (A2) and (A3) combined. $\tilde{t}$ remains to be chosen, e.g. to be t_n (forward method), t_{n+1} (backward), or any other fancier method possible. When assuming δt = 10 yr and a $F(t)$ is linear between t_n and t_{n+1}, one immediately finds the δt^2 equations.

So far, no fundamental problem with the authors' equations and text. I just believe it could be written in a more efficient and straightforward way. But a problem arises when one assumes that the time-scale τ varies with time (through e.g. temperature) so that we have in fact $\tau = \tau_0 + \Delta\tau(t)$. The exponential integrator method can still be applied, albeit by using τ_0 and not τ in the exponential function.

To do so, it is easier to rewrite the differential equation as:

$$\begin{aligned} \frac{\mathrm{d}m}{\mathrm{d}t} &= -\frac{m(t)}{\tau_0 + \Delta\tau(t)} + a\, F(t) && (4) \\ &= -\frac{m(t)}{\tau_0} + \frac{m(t)\, \Delta\tau(t)}{\tau_0\, (\tau_0 + \Delta\tau(t))} + a\, F(t) && (5) \end{aligned}$$

which completely changes the exponential integrator form:

$$\begin{aligned} m_{n+1} = \exp(-\frac{\delta t}{\tau_0})\, m_n \quad &+ \int_0^{\delta t} \exp\,(-\frac{\delta t - s}{\tau_0})\, a\, F(t_n + s)\, \mathrm{d}s \\ &+ \int_0^{\delta t} \exp(-\frac{\delta t - s}{\tau_0})\, \frac{m(t_n + s)\, \Delta\tau(t_n + s)}{\tau_0\, (\tau_0 + \Delta\tau(t_n + s))}\, \mathrm{d}s \end{aligned} \tag{6}$$

leading to:

$$m_{n+1} = \exp(-\frac{\delta t}{\tau_0})\, m_n + \tau\, (1 - \exp(-\frac{\delta t}{\tau_0}))\, a\, F(\tilde{t}) + (1 - \exp(-\frac{\delta t}{\tau_0}))\, m(\tilde{t})\, \frac{\Delta\tau(\tilde{t})}{\tau_0 + \Delta\tau(\tilde{t})} \tag{7}$$

The latter equation raises the issue that it is virtually impossible to use with a backward approach since $\Delta\tau(t_{n+1})$ is not known. But a bigger issue is that, if I understand it correctly, the authors do not use this equation nor any equivalent. I believe they simply apply the equation of the case with constant τ but with a value of τ that changes through time. That is, they use the following equation:

$$m_{n+1} = \exp(-\frac{\delta t}{\tau_0 + \Delta\tau(t)})\, m_n + (\tau_0 + \Delta\tau(t))\, (1 - \exp(-\frac{\delta t}{\tau_0 + \Delta\tau(t)}))\, a\, F(\tilde{t}) \qquad (8)$$

instead of the one above.

Unless the authors can prove the difference between the two is negligible, I am afraid there is a fundamental mistake in the solving of the model.

4. I believe the model should be completely described in the paper. I mean: formulations for e.g. functions $p_S^{CO_2}$, ψ, χ, as well as all the parameter values should be given. The model is relatively simple, and there are not that many parameters. Even if the values can be accessed in the code, the fact that this paper is a model description makes it necessary to be as exhaustive as possible.

Minor points:

p. 1 (sec. 1): SCMs have many more usages than what is given here. Generally speaking, I find that the citations of this paper are too self-centered. I think everyone acknowledges the importance of the original Joos et al. (1996) paper, but much has been done since then regarding IRFs.

p. 3, l.13: The "essentially linear behavior" is an assumption of the model.

p.3, l. 16: IRFs are indeed equivalent to box-models, albeit with <u>constant</u> parameters!

p.3, l.25: The non-inclusion of LULCC could be discussed a little.

p.6 l.10: It is probably better to give all the equations, even if very similar.

p.6 l.20: "conversation" => "conservation" (probably many typos I missed...)

p.6 l.22: I don't think it is 10^4 or 10^5 kyr. Unit is probably yr.

p.7 l.2: At this stage, it is very unclear whether the response based on HRBM is a usual linear IRF calibrated with climate-carbon feedbacks on, so that those are linearized within the IRF, or if the time-scales of the response are indeed interactively changed by temperature during the simulation. Note also that I don't think the name "IRF" can be given to a model with time-varying parameters. I believe an IRF is the integrated form of the differential equation, which can be obtained only when the parameters do not vary with time. When they do, there is no integrated form, and the model is just a box model.

p.7 l. 13: Similarly, I would question the fact that the equation shows that IRF and box model are equivalent. I think they are per definition. The only difference being that one is the integrated form of the other.

p.7 l. 20: Can cite Li et al. (2009) who provide a nice discussion on the (over)interpretation of those parameters.

p.7 l.25: It is more than 'they can be viewed'. Per construction, IRFs show the exponential eigenmodes of the system they are calibrated upon. Raupach (2014) or Enting (2007) provide some insights on this.

p.8 (sec. 3): I really find this section difficult to apprehend. It would benefit from some re-organizing, e.g. with a subsection on the beta/gamma framework, and then one on what it gives when applied to BernSCM. This is also the part that made me wonder whether C4MIP models were emulated or simply used for comparison.

p. 8, l.16: Table 3 does not provide any parameter value...

p.8, l. 28-29: Please, name those simulations "T-only" and "C-only". The dash makes a lot of difference when reading the text that follows!

p.9, l.9: I don't think alpha is the "transient climate sensitivity" in the usual sense. Find another name.

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p.9, l.10: Which original paper?

p.9, l.31: The "combinations" remain quite unclear.

p.10, l.1: Inconsistent temperature units (this is in the whole paper).

p.10, l.2: More important comment related to my first major points. The choice of a climate sensitivity does not affect the time-scales of the climate response. However, it is know that a higher climate sensitivity implies a slower climate system (e.g. Baker Roe, 2011).

p.10, l.9-10: The last bit of this sentence is very uninformative.

p.10, l.12: 3.2<u>K</u>

p.10, l.15-17: I believe the fundamental reasons exposed in my major point number 1 also explain a lot, here. Hence the need to compare the climate response alone, and not coupled to the carbon cycle as in figure 3.

p.10, l.23: Those sensitivities are not defined. . .

p.10, l.29-32: I don't see the point of those sentences. Yes, the obtained sensitivities are zero. But this is per construction, since the uncoupled cases are used to investigate the sensitivity. This relates to my major point 2.

p.11, l.4: I believe it is 0.5K, according to figure 4. Also these values are for a fixed climate sensitivity. So I wonder how informative they are.

p.11 (sec. 5): I don't find all the discussion about BernSCM/C4MIP very convincing, for the reason already exposed above.

p.12, l.1: Yes, but that requires building EOFs on more complex models. Mention and citations needed here.

p.12, l.3: Note that regarding precipitation (and likely cloud cover as well), we now know that the response is forcing dependent (e.g. Shine et al., 2015; and references therein).

p.12, l.10: Yes. But simple models usable in a probabilistic fashion already exist out there.

p.12, l.23: GWPs and other metrics require inclusion of non-CO2 species. So I'm not sure the sentence here is relevant.

p. 12, l.26: I don't like the use of "fixed", here. It is e.g. not influenced by external factors such as climate change.

p. 13 (sec. A1): As I wrote in my major points 3, I believe this section could be more straightforward.

p. 13 (sec. A2): This section is awfully complicated! It makes me wonder about several things, and I could not find the answer... Couldn't a solver be used for the backward method? Is the backward method solved with an exact solution, or is the method proposed an approximation? Does it have to be that complicated?

Also, I find the equations extremely difficult to follow. There are four (!!) levels of notation: U,V,W refer to p_{fk} which refer to A_k which refer to the original parameters τ_k and a_k. I am convinced this part could be written (and implemented in the code?) in a much simpler way

p.12 (sec. A3): Again, not completely clear how the climate-carbon feedback is implemented. See major point 3.

p.25 (fig. 3): A representation of the land and ocean fractions could be provided. Also, see major point 1: the climate response alone should be shown somewhere (be it within figure 3 or separately).

p.26 (fig. 4): Maybe show ranges from C4MIP?

p.29 (tab. 3): I don't find this table very informative. Parameter values and functional forms should be provided instead.

p.30 (tab. 4): Using the words “parameters” is one of the things that made me wonder whether C4MIP models were used as input to BernSCM or just to compare outputs. I would call that e.g. “metrics”.

References:

Baker, M.B. and G.H. Roe. The Shape of Things to Come: Why Is Climate Change So Predictable? Journal of Climate 2009 22:17, 4574-4589

Enting, I.G. Laplace transform analysis of the carbon cycle, In Environmental Modelling Software, Volume 22, Issue 10, 2007, Pages 1488-1497, ISSN 1364-8152, https://doi.org/10.1016/j.envsoft.2006.06.018.

Geoffroy, O., Saint-Martin, D., Olivié, D. J. L., Voldoire, A., Bellon, G., and Tytéca, S.: Transient Climate Response in a Two-Layer Energy-Balance Model. Part I: Analytical Solution and Parameter Calibration Using CMIP5 AOGCM Experiments, J. Climate, 26, 1841–1857, 2013a.

Geoffroy, O., D. Saint-Martin, G. Bellon, A. Voldoire, D.J. Olivié, and S. Tytéca. Transient Climate Response in a Two-Layer Energy-Balance Model. Part II: Representation of the Efficacy of Deep-Ocean Heat Uptake and Validation for CMIP5 AOGCMs. Journal of Climate 2013b 26:6, 1859-1876

Joos, F., Bruno, M., Fink, R., and Siegenthaler, U.: An efficient and accurate representation of complex oceanic and biospheric models of anthropogenic carbon uptake, Tellus B, 48, 397–417, http://onlinelibrary.wiley.com/doi/10.1034/j.1600-0889.1996.t01-2-00006.x/abstract, 1996.

Li, S., Jarvis, A. J., and Leedal, D. T.: Are response function representations of the global carbon cycle ever interpretable?, Tellus B, 61, 361–371, 2009.

Raupach, M. R.: The exponential eigenmodes of the carbon-climate system, and their implications for ratios of responses to forcings, Earth Syst. Dynam., 4, 31-49, https://doi.org/10.5194/esd-4-31-2013, 2013.

Shine, K. P., Allan, R. P., Collins, W. J., and Fuglestvedt, J. S.: Metrics for linking emissions of gases and aerosols to global precipitation changes, Earth Syst. Dynam., 6, 525-540, https://doi.org/10.5194/esd-6-525-2015, 2015.

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