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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

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Report on

Kuno Strassmann and Fortunat Joos

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

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General comments

This very interesting paper describes the Bern Simple Climate Model (BernSCM) v1.0. BernSCM simulates relations between CO_2 emissions, atmospheric CO_2, radiative forcing (RF), global mean surface air temperature (SAT), as well as carbon and heat

fluxes between atmosphere, ocean, and land biosphere. It is a reduced form coupled carbon-climate model that emulates more complex coupled models by replacing complicated components with nearly linear behaviour by impulse response functions (IRFs). This (to the best of my knowledge, novel) approach leads to a coupled carbon-climate model which is easy to understand and needs only low computational cost to be run. Comparisons with results from two multi-model intercomparison studies (C^4MIP; IRFMIP from Joos et al., 2013) show that BernSCM simulations give representative results with respect to current knowledge about carbon-climate interactions.

I am convinced that this manuscript can be scientifically important in two ways: 1) The practical application of the model itself or extended versions in its own right or as part of bigger models can lead to advances in multiple directions. 2) The theoretical foundations of the manuscript based on IRFs provide an interesting perspective on the theory of ecological modelling. Very appealing is the interpretation of the IRFs as representing parallel systems with multiple boxes, for example. Apart from some minor exceptions, the manuscript and in particular the appendix and the provided Fortran code of the model are carefully prepared. The authors took care that the BernSCM model and its implementation can be reproduced. Furthermore, the manuscript is well organized and the results are nicely presented.

There are some technical problems with the equations that describe the model and I suspect an inherent theoretical problem as soon as the IRFs become time-dependent or depend on other states of the system (e.g., temperature, CO_2). While the technical problems can be solved easily, I am not sure about the theoretical issue, as I will explain in more detail below.

Even if the theoretical issues cannot be completely resolved, I consider this manuscript worth for publication in Geoscientific Model Development, if the authors make the readers aware of the situation.

Specific comments

General explanation

The theoretical idea of this manuscript is very appealing. The IRFs used to substitute complex model components are provided by earlier simulations of highly complex models and just plugged into BernSCM. This makes the model structure pretty simple and the model can be used to understand ongoing processes on a global level without getting lost in distracting details, for example. Furthermore, the computation is very fast due to the use of the IRFs. This speed is even improved by disassembling the IRFs into their most important time scales, which allows an interpretation of the substituted IRFs as describing an underlying parallel multi-box model. This approach allows a very fast recursive computation which is carefully explained in great detail in the appendix and implemented in the provided Fortran package of BernSCM v1.0.

The unit issue

In some equations the units do not fit. The main reason is that ε has been given the wrong explanation and the wrong unit in Table 2. The correct unit is GtC/ppm and a better description could be “mass of C per atmospheric concentration”. This solves the unit problem in equations (5) and (25). Equation (8) should then be

$$p_A^{CO_2} = m_A \cdot \varepsilon^{-1}. \tag{1}$$

As far as I could see from the code, it is implemented correctly.

In equation (7) the units do not give the desired (Table 1) μmol/kg. To that end the unit of $M_{\mu mol}$ needs to be changed to gC/μmol.

Very confusing is also the use of different time units like in equation (9). Carbon fluxes are measured per year and heat fluxes per second (W=J/s). Nevertheless the integral limits are in both cases t_0 and t. I could not find any correction term in the manuscript. In the code this correction seems to be made.

In equation (10), the unit results in W, not in PetaW as stated in Table 1. Also here a correction term is necessary. Again, this seems not to be an issue in the code.

Linear equations and IRFs

Equation (14) is only true if $m(t_0) = 0$. The general equation for the state m at time t is

$$m(t) = r(t - t_0)\, m(t_0) + \int_{t_0}^{t} f(t')\, r(t - t')\, dt'. \tag{2}$$

Since the authors use equation (14) to compute perturbations with an equilibrium value $m(t_0) = 0$, this does not lead to problems, but the way equation (14) is described is mathematically not correct. I have the feeling this happened, because the authors from the beginning had a perturbation with equilibrium equal to zero in mind, but started the section then with a slightly more general set up. Line 17 on page 5 does not mention perturbations.

The infinite time scale issue

When equation (20) is inserted in (14) to obtain equation (21), a_∞ somehow disappears. As soon as $a_\infty \neq 0$ (ocean IRF), a term is missing in equation (21). The

equation should then look like

$$m(t) = \int_{t_0}^{t} f(t')\, a_\infty\, dt' + \sum_k \int_{t_0}^{t} f(t') a_k\, e^{-(t-t')/\tau_k}\, dt'. \tag{3}$$

If $f(t') \geq c > 0$, then the first part of the equation goes to infinity as $t \to \infty$ and the perturbation grows indefinitely. If some constant share from a constant input is never going to be decayed, this share accumulates forever. Also the carbon coming from carbon conversion in the ocean model (page 7, line 20), is going to decay at some point. A constant share of remaining carbon should not result from a multiplication with an input flux coming from the atmosphere. The same explosion effect can be seen in equation (A1). Also in Equation (A14) the to a_∞ associated term $\frac{1}{2}\, a_\infty\, (\Delta t)^2$ is missing. It cannot already be included in the present sum, because $B_\infty = \frac{1}{2}\, a_\infty$ is never going to be multiplied with an exponential.

I do not know how this problem is handled in the implementation of the model.

The theoretical issue

In my opinion, the theoretical foundations of this model are sound as long as the substitute IRFs are time-independent and also independent of other state variables. However, the great power of BernSCM emerges when temperature or CO_2 dependencies are explicitly allowed in the IRFs. I am not perfectly sure, if the theory behind the IRF approach is still valid in this case, even though the simulations show reasonable results.

I think it is important to stress the fact, that equation (14) works for time-dependent forcings, but for time-independent processes only. The impulse response function r here depends only on the difference $t - t'$ of the time t at which we are interested in the perturbation $m(t)$ and the time t' at which the input $f(t')$ came into the system.

The absolute time t' is not used by the impulse response function r. Consequently, the underlying black-boxed process which can be modelled by this approach is assumed to be time-independent (has constant coefficients). If now the IRF depends additionally on temperature, the impulse response function needs to "know" the current time t' and becomes $\tilde{r}(t', t - t')$. A different forcing function f leads to a different system state which then results in a different IRF. The system is inherently non-linear, even though it looks linear. From the code (and unfortunately only from there) I could see that the IRFs are provided as a set of coefficients a_k and a set of time scales τ_k. Probably these numbers result from an analysis of complex simulations (e.g. HRBM). If so, the numbers come from a non-linear model and this very IRF is representative only for this very model run. In a non-linear setting, a different model run (initial value, temperature, sensitivities) could theoretically lead to a very different IRF which is then going to be ported to BernSCM. The analysis in section 4 shows that this does not have drastic influence here, probably in part because the external emission forcing was chosen to be the same (SRES A2).

Additional to this possible dilemma, the IRF comes with additional numbers for temperature sensitivity. These numbers are used in each time step to adapt the IRF in dependence of current mean surface air temperature. As mentioned above, $r(t - t')$ becomes $\tilde{r}(t', t-t')$. In the derivation of equation (A7), which is crucial for the numerical implementation, this leads to a problem. The term

$$R_i = 1/\Delta t \int_{t_{i-1}}^{t_i} r(x)\, dx \tag{4}$$

becomes

$$\tilde{R}_i = 1/\Delta t \int_{t_{i-1}}^{t_i} \tilde{r}(t - x, x)\, dx \tag{5}$$

and the integration becomes much more difficult, in particular if both a_k and τ_k are temperature dependent.

I am not sure whether this problem can be discussed away by numerical means or even by purely theoretical considerations, but at the moment I have a strange gut feeling about this issue.

Technical corrections

- in general: Punctuation around equations is missing very often, in particular full stops or commas after the equations when necessary.
- in general: Some abbreviations are never introduced, e.g., HRBM, HILDA. Sometimes the explanation of the abbreviation comes late in the text. This happens in particular when reading the figure captions and figures are referred to in more places. Maybe this is hard to circumvent without destroying the text flow.
- There is a mix of British (analyse) and American ("behavior") English.
- in general: It is difficult to find out which constant means what since tables 1 and 2 are not complete. Some terms are explained in the text, some in the tables.
- page 1, line 14: "in an spatially"
- page 2, line 34: "of BernSCM a an IAM component"
- page 2, line 35: "managment"
- page 3, line 1: "cycle assessments(Levasseur et al., 2016)":
  space before parenthesis

- page 3, line 26: What does “LULUC” stand for?
- page 4, line 4: “by” or colon missing at the end?
- page 4, line 7, equation (4): How is φ_{NPP} defined?
- page 4, line 11: “Ao” instead of A_O, “eps” is probably meant to be “ε”
- page 4, line 14, equation 8: How are the functions ψ and χ defined?
- page 5, line 1: “and the separation of SAT from radiative equilibrium”:
  Does “separation” here refer to the difference between 1 and the ratio $\frac{\Delta T}{\Delta T^{eq}}$? To me the word “separation” is rather confusing in this context.
- page 5, line 9, equation (12): I could not find a description of $p_{A0}^{CO_2}$ anywhere. Typo?
- page 7, line 5, equation (21): The lower limit of the integral should be t_0 instead of 0.
- page 7, line 15: “a_{O_k}” is called a_k in Figure 1 (blue box). Also in the red box the constants are called a_k. Only in the green box they are called a_{Lk}. Similar problems with τ.
- page 7, line 20: Which model from Table 3 is here referred to? The Bern2.5D or the 4-box Siegenthaler and Joos?
- page 8, lines 9-10: “here, the IRF substitutes for the HILDA ocean model, and the HRBM land biosphere model are used for the standard setup”:
  It took me a while to understand this phrase. Maybe an additional “for” in front of “the HRBM” and omitting the comma are helpful?

- page 8, lines 13-14: “and the dependency of land C on temperature (f_{decay}) increases with warming, eq. (2))”:
  From equation (2) I cannot see what happens with warming. Going to equation (19), I can see that it depends very much on f_{NPP}. This is defined in equation (4) and depends heavily on φ_{NPP}, which is not explained at all.
- page 9, line 4, equation 25: An interchange of β_O and β_L on the right hand side makes it better comparable with the left hand side and the following text.
- page 9, line 7-8: “β is the change in carbon stored (in GtC)”:
  Following Table 3, β has the unit GtC/ppm and in Friedlingstein et al. (2006) it is referred to as ”sensitivity of land carbon storage to atmospheric CO_2“. If I understood equation (25) correctly, this is a more precise and less confusing description. The same holds for γ.
- page 9, line 26: I did not immediately recognise “airborne fraction” as a technical term. Maybe a short explanation could avoid confusing non expert readers.
- page 10, line 1: “(Figure. 3)”
- page 10, line 1-2: Why different units?
- page 10, lines 3-5: Looking at Figure 3 (upper panel), after 100 years all simulated values are greater than 0.3. Where does the value 0.3 from the text come from?
- page 10, lines 5-7: “For AF simulated with BernSCM, the standard coupled setup is close to the IRFMIP multimodel median, but the BernSCM uncertainty range is asymmetric. The IRFMIP multi-model range is similarly asymmetric.:”
  The word “but” confuses me, because the “IRFMIP multi-model range is similarly asymmetric”.

- page 12, line 21: “structural simplicitly”
- page 13, line 15, equation (A4): The integral limits are interchanged. They do not change with the parameter transformation, because $dt' = -dx$ makes for a second sign change.
- page 15, line 2, equation (A14):
  Maybe it is better to write $(\Delta t)^2$.
- page 15, line 13: “explicite”
- page 15, line 20: “Equations (A1,A2)”
  Space between A1 and A2?
- page 16, line 13, equation (A20):
  Maybe $(m_{S_n} - m_{S_{n-1}})$ is correct? I am not sure.
- page 17, line 12: “explicite”
- page 17, line 19: “Equations (11,10,A28)”
  Space between equation numbers?
- page 17, line 24, equation (A30): Is it correct that f_{O_n} appears twice in this formula?
- Figure 4: For me it is impossible to differentiate between dashed and dashed-dotted lines here. Maybe a different colour/line-style scheme could help here. Since the differences resulting from the use of different numerical schemes are almost invisible anyway, one could even go without trying to make them visible and simply mention that the differences are small. On the other hand, the point the authors want to emphasise here, is that due to the very small differences, the fastest scheme can be implemented. This leads to the entire appendix and the Fortran implementation. So it is rather an important point.

- Table 1: “f_A: net flux to atmosphere flux”
- Table 1: “f_{deep}: Flux mixed layer to deep”:
  Why does “Flux” start with capital F? Missing “ocean” at the end?
- Table 2: Capitalization of first word in second column inconsistent?
- Table 2: From the units I think c_p should be called “specific heat capacity”.
- Code: Why is in the file parLandHRBM.inc the first weight negative? If I understood correctly, those weights are the a_{L_k} values which here nicely sum to one, but how do you distribute a negative share of incoming carbon?