**Geoscientific
Model Development
Discussions**

Interactive comment on "Presentation, calibration and validation of the low-order, DCESS Earth System Model" *by* G. Shaffer et al.

G. Munhoven (Referee)

Guy.Munhoven@ulg.ac.be

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1   General comments

Shaffer, Malskær Olsen and Pedersen describe a new Earth System Model, built around the well-known HILDA model (Shaffer and Sarmiento, 1995; Shaffer, 1996; Siegenthaler and Joos, 1992). HILDA was further developed (it now includes a vertically resolved instead of the single-box high-latitude deep ocean) and extended: it has been coupled to a zonal mean atmospheric energy balance model, land biosphere and a sediment model. I entirely agree with the authors arguments about the usefulness of such a model as it will allow to explore the evolution of the coupled climate-carbon cycle over timescales that only just start to come within reach of intermediate complexity

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models.

The paper is well within the scope of *Geoscientific Model Development*. The comprehensive model description makes the paper rather long; most of the given details are nevertheless necessary and useful to comprehend the foundations of the model. Because of the length of the paper and the large number of parameters and variables used, readers would certainly appreciate if the authors could provide a comprehensive list of symbols, acronyms and abbreviations for reference.

According to the *Geoscientific Model Development* guidelines (`http://www. geoscientific-model-development.net/submission/manuscript_ types.html`), the version number of the model must be given in the title. Furthermore, the model webpage URL should be given, and hard- and software requirements and the license information provided. Please complete this information.

In order to improve readability, the authors should refine the sectioning of their paper. Some sections are overly long (e.g., section 2.2 runs over nearly six pages; the sediment model description runs over almost thirteen pages without a single section or subsection heading). There are sections that can and should be significantly shortened. The section on the implementation of weathering fluxes will benefit from a partial rewrite; the description of the sediment model ought to reflect the actually adopted model version. In their current form, these two sections are with regard to some aspects lengthy, overly detailed if not confusing, with respect to others, incomplete. Details are given in the "Specific comments" below.

I am confident that the authors can address these shortcomings. I found the model approach taken by Shaffer et al. highly interesting and promising, and I am looking forward to the revised version of this paper.

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2 Specific comments

Specific comments mainly concern the profile of the vertical diffusion coefficient, the weathering flux implementation and the sediment module, which are addressed in separate sections. Additional minor points are exposed in a fourth section.

2.1 Vertical diffusion coefficient profile

The parameterisation of the vertical diffusion coefficient in the low-mid latitude deep ocean, $K_v^l(z)$, is presented on pages 51–52. It is completely different from that used in previous versions of HILDA:

- Shaffer and Sarmiento (1995): $3.2 \times 10^{-5}\,\mathrm{m^2 s^{-1}}$ (constant)

- Siegenthaler and Joos (1992):
  $K(z) = 465 + 7096 \times \exp(-(z - 75\mathrm{m})/253\mathrm{m})$ (in $\mathrm{m^2 yr^{-1}}$), or equivalently,
  $K(z) = 31.74 \times 10^{-5} \times (1 - 0.95(1 - \exp(-z/253\mathrm{m})))$ (in $\mathrm{m^2 s^{-1}}$)

- Here (eq. (16) and Table 1):
  $K_v^l(z) = 2 \times 10^{-5} \times (1 + 5.5(1 - \exp(-z/4000\mathrm{m})))$ (in $\mathrm{m^2 s^{-1}}$).

The two parameterisations for which a depth dependency is considered lead to completely different profiles: decreasing with depth with the Siegenthaler and Joos (1992) formulation (from $24 \times 10^{-5}\,\mathrm{m^2 s^{-1}}$ at 100 m depth to about $1.5 \times 10^{-5}\,\mathrm{m^2 s^{-1}}$ at 4000 m); increasing with depth in this study (from $2.3 \times 10^{-5}\,\mathrm{m^2 s^{-1}}$ at 100 m depth to about $9 \times 10^{-5}\,\mathrm{m^2 s^{-1}}$ at 4000 m). And yet both have been calibrated on observational data. These are fundamentally different characteristics that deserve some critical discussion: what are the reasons, what are the implications? Would the constant diffusion parameter formulation of Shaffer and Sarmiento (1995) not be sufficient?

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Although I understand that the number of free parameters in the model should be kept as low as possible (page 67, lines 21–26), I think it would justified to have different diffusion coefficient profiles for heat and for solutes. As explained by the authors, the physics behind the respective exchange processes is not the same. The papers by Harvey and Huang (2001) and Harvey (2001) describing a model broadly similar to the DCESS Earth system Model (ESM) might be useful to consider for discussion.

2.2 Weathering fluxes

Weathering fluxes are described in section 2.6. While reading the text, one does not really comprehend why all of the lengthy developments are actually required. A posteriori, it appears that large parts are related to model calibration (deduction of various steady-state flux values). Sectioning would help readers to better find their way; a clear separation of model description and calibration would also be helpful. This whole section could possibly be shortened, focussing on the essentials. If the authors judge that all of the information presented here is necessary, they ought to provide readers with a clearly developed and exposed chain of logic.

The treatment of silicate weathering, with the intermediate step where the silicate mineral is converted to carbonate first, is unnecessarily complicated. Is there any compelling reason not to use the classical formulation based upon the reaction

$$CaSiO_3 + 2\,CO_2 + H_2O \Leftrightarrow Ca^{2+} + 2\,HCO_3^- + SiO_2$$

straight away and express W_{Sil} either in terms of Ca dissolved, HCO_3^- produced or CO_2 consumed, and relate the other fluxes (DIC and alkalinity) to that one? In the current description, units for W_{Cal} and W_{Sil} are missing. (The same holds for W_P, W_{OrgC} and *Vol* although there is little ambiguity regarding their units.)

On page 62, at lines 15ff, the authors present discussion related to the pre-industrial state of selected aspects fo the weathering fluxes. However, nothing is said about how the flux terms are treated in general: atmospheric CO_2 consumed by weathering processes is taken out of the atmosphere? river bicarbonate produced is added to the surface ocean, affecting both DIC and alkalinity balances? sediments take the rest of the balance into account through the DIC and alkalinity fluxes that they return to the ocean? I sincerely hope that the statement saying that "[...] half of $R_{C,PI}$ goes to the part of this biogenic carbonate formation that is buried while the other half goes to CO_2 production in the ocean surface layer" does not reflect the way weathering fluxes are actually taken into account in the model. Please clarify this, the more since the carbonate formation has already been accounted for (page 54). If this statement is only a comment, I would suggest to either discard it or carefully rewrite the text to make this clear. In its current form, I find the text potentially misleading.

Equation (36) looks like the global ocean alkalinity balance at steady state, where the terms related to organic matter have already been simplified (taking the pre-industrial steady state of those into account). Or is it the steady-state Ca balance? At time-scales of 100,000 yr and less, the Ca balance is not of much relevance, because of the long residence time of Ca in the ocean (of the order of 10^6 yr). On those time scales, DIC and alkalinity are the important factors to consider.

Finally, since the guidelines of *Geoscientific Model Development* ask to put empha-sis on the discussion of the applicability of the described models, it would be worth mentioning that the chosen Q_{10} formulation for the weathering rate laws needs to be completed by additional factors, such as continental surface area and types of ex-posed bedrock that are subject to weathering as sea-level rises and falls (see, e.g., Munhoven, 2002), if the model is to be applied in a realistic way to the study of glacial-interglacial cycles, which would obviously be a typical application.

2.3   Sediment module (Sections 2.4, 3.2.2 and Annex)

Shaffer et al. propose an interesting and promising numerical approach for solving the advection-diffusion-reaction equations that describe the early diagenesis of carbonate and organic material in the surface sediment. For clarity, the fundamental hypotheses of the chosen formulation (homogeneous calcite distribution, steady-state profile for %OrgC, steady-state solute profiles ...) should be recalled in a few sentences at the beginning of the description. Else, one has to return back thirty pages to find that information. The description is also extremely long and a subdivision into shorter sections will certainly help to improve its readability.

Shaffer et al. include in their model a dependency of porosity on the calcite dry weight fraction %$CaCO_3$. The adopted formulation from Archer (1996) is, unfortunately, rather particular. It derives from a regression equation obtained by deMenocal et al. (1993), based upon data from a single site (ODP 663) in the eastern Atlantic. That regression equation was modified by Archer (1996), who dropped the contribution from %Opal and the dependency on depth below the seafloor-interface. More universal formulations are available and probably more appropriate in this context (see, e.g., Herbert and Mayer (1991) or Zeebe and Zachos (2007)). It is, however, not clear whether this dependency of porosity on %$CaCO_3$ has any significant influence on the global model results. The authors show how different %$CaCO_3$ values affect porosity and the sediment formation factor and they present a short qualitative comments regarding potential consequences in the appendix. The actual influence on model results (e.g., the distribution and evolution of $CaCO_3$ dissolution fluxes, OrgC remineralisation rates,...) has, however, not been quantitatively assessed. Since this variable porosity characteristic of the model is singled out (Conclusions, page 85, lines 3–6), such an analysis would be of order.

While the description of the OrgC and O_2 equations in the appendix is adequate, that related to the carbonate system ($CaCO_3$ and CO_2-HCO_3^--CO_3^{2-} or DIC-ALK) will benefit

from a clearer focus on the actually implemented model (versions). The authors lay the theoretical foundations for a complete and lean early diagenesis model with organic matter respiration effects on calcite dissolution included ... to drop these effects at the end. They explain how the detailed porewater carbonate chemistry may be solved ... to finally select a standard version where CO_3^{2-} is the only carbon carrying solute considered. It should be noticed that the standard version of the sediment model proposed here is essentially an extension of the classical $CaCO_3$-CO_3^{2-}-Clay model (described, i.a., in Broecker and Peng (1982) and also used by Keir and Berger (1983) and others with non-linear calcite dissolution kinetics). Here,

- the single mixed-layer has been subdivided into a stack of seven sublayers, each of which has its own porosity and all of which share the same %$CaCO_3$;

- a parallel OrgC-O_2 model has been attached to the $CaCO_3$-CO_3^{2-}-Clay model;

- the organic and inorganic sub-models are only coupled via ρ_{sm} and w_s (but not via carbonate chemistry).

Since none of the model versions used in this paper includes the effect of organic matter respiration on calcite dissolution, the description could gain considerable clarity if the terms related to organic matter respiration would be omitted from the equations (starting with equations (A18) and (A20), where these terms contain errors anyway—see "Technical comments" below). The differences between the results obtained with the three model versions tested in section 3.2.2 are, as far as one can see from the presented results, negligibly small. Why not limit the model description to the sole version with simplified chemistry, and only briefly mention the results of the tests carried out (without actually showing the results)? This would allow substantial shortening of the appendix and also of section 3.2.2. Else, it would be useful to clearly state the two complete sets of equations used in the two model versions.

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On pages 97 and 98, it is explained that the advection velocity can reverse if calcite dissolution becomes sufficiently intense. In that case previously buried material from below can be returned into the bioturbated layer (a process that the authors call "mining", which is more commonly known as "chemical erosion" — see, e.g., Keir (1984) or Archer (1991)). How are the characteristics of the "mined" material determined? Does the model produce so-called synthetic cores that can be used for this purpose?

Despite the long and detailed description of the sediment model, two important questions regarding the conservation of carbon and alkalinity remain open:

1. If the organic matter respiration terms are neglected in equations (A18) and (A20), how can the total mass of carbon in the system be conserved during organic matter remineralisation in the sediment?

2. How is the alkalinity sink related to organic matter remineralisation in the sediment accounted for? The model considers the alkalinity source related to organic matter production in the surface ocean, as witnessed by the $r_{AlkP}NP^{l,h}$ term (page 54, line 1) and the sink related to remineralisation in the water column (equation 21). Equations (A18) and (A20) do not include the relevant terms for this alkalinity sink.

If either one of these two budgets is not closed, the model will inevitably drift away.

2.4   Miscellaneous minor points

Section 2.3 and later: the symbol q is used here to denote the deep overturning circulation. In the historical HILDA papers (Shaffer and Sarmiento, 1995; Shaffer, 1996;

Siegenthaler and Joos, 1992), q was always use to denote the interior exchange parameter, which has become K_h here. The change of meaning of q may lead to confusion for readers familiar with HILDA. Please chose some other symbol than q, not previously used for denoting something else in HILDA before.

Page 69, main paragraph: the production figures lead to a global mean export rain ratio of 0.18, which is rather high, compared to recent estimates (e.g., Lee, 2001; Jin et al., 2006).

Page 73, line 17: the commonly accepted definition of the term "lysocline" (see, e.g., Broecker and Peng, 1982) is that this is the depth in the ocean where the calcite content of the sediment begins to decline (where dissolution effects start to be significant). As such, it does not have a thickness. What you mean here is more correctly called the "transition zone" (sublysocline transition zone, to be complete). Please correct throughout.

3 Technical corrections

Page 47, equation (12): HCO_3^{-1} should read HCO_3^{-}

Page 50, line 17: start sentence with "In the low-mid ..."

Page 54, line 20: delete comma at the end of the line

Page 59, lines 11–13: please attribute each of the two references to the respective information.

Page 61, line 24: please specify units for W_{Cal} and W_{Sil}

Page 62, equation (35): β_R in the equation has not been defined; it is not used anywhere else and can be safely discarded

Page 63, line 3: "W_{OrgCl}" should read "W_{OrgC}"

Page 63, line 15: would it not be more correct to say that W_{Sil} and W_{Cal} are in a constant ratio — I do not see how one can be a fraction of the other, as they relate to different processes

Page 69, line 7: ". . . than the 4.6 . . . "

Page 72, line 3: "below 1000 m" is ambiguous

Page 74, line 20: delete comma after "pre-industrial"

Page 78, line 23: "Caldiera" should read "Caldeira"

Page 81, line 19: correct to "There is a decrease in the net CO_2 flux . . ."

Page 89, line 13: correct to ". . . , the above solution for . . . "

Page 88, equations (A8), (A9) and text below: earlier in the text, W denotes weathering rates. Please use some other symbol for W_{Org}, in order to avoid possible confusion (W_{OrgC} denotes the weathering rate of old organic matter).

Page 91, lines 3–12: I had expected to find this information already on page 86 (around line 15, shortly after equation (A2), where D_b appears for the first time) and would recommend to move it there.

Page 91, line 12: please provide references for these numbers

Page 92: equations (A18) and (A20) are not correct. The parts involving $\mathrm{OrgC}_{\mathrm{ox},i}$ must not be set within brackets; they are individual terms, not factors. In equation (A18), that term must furthermore be divided by D_{CO_3}; in equation (A20) it must be multiplied by $F_{s,i}$ and divided by D_{HCO_3}. Or, were the brackets possibly meant to indicate that those terms were neglected later on (see specific comments above)? In this case, the terms

better had to be dropped altogether (see "Specific comments" above).

Page 95, line 5: delete comma after "steady state"

Page 98, line 17: "Fortunate" should read "Fortunat"

Page 99, line 29: "Caldiera" should read "Caldeira"

Page 103, line 15: "Caldiera" should read "Caldeira"

Table 1: K_H should read K_h

Table 2: I suggest to further add $CaCO_3$ and organic carbon burial to complete the balance

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