

## ***Interactive comment on “Projections of ocean acidification over the next three centuries using a simple global climate carbon-cycle model” by C. A. Hartin et al.***

### **Anonymous Referee #2**

Received and published: 25 January 2016

#### General comments:

This study presents a reduced-form model (Hector) that can be used for global-scale, long-term studies on e.g. surface ocean acidification related to CO<sub>2</sub> emissions and climate change. The model includes carbon exchange between the terrestrial, oceanic, and atmospheric carbon reservoirs, although with highly simplified parameterizations for many processes (e.g. constant TA, no CaCO<sub>3</sub> formation/dissolution and possible feedbacks over the several-century long model period, no seasonality or long-term change in oceanic productivity, etc.).

Within its limitations, the Hector model appears to be a tool that in comparison with

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



other more complex – and much slower – models is highly useful and well suited for e.g. different future climate long-term sensitivity experiments. The model is calibrated/validated by comparing model output to both measured data and output from other models. Simulated surface water pH, pCO<sub>2</sub>, etc., are generally in good agreement with output from the much more complex (and computationally costly) CMIP5 models – although with a considerable offset that changes over time. Finally, a sensitivity study demonstrates the model sensitivity (in terms of PH and aragonite saturation) to a couple of different RCP scenarios.

It is made clear by the authors that coastal dynamics as well as short- and long-term trends in biogeochemical processes are outside the scope of the present study. This is fine; I don't mind this type of really large-scale model set-up. However, my main issue with the paper is that we see so little discussion concerning possible problems with the approach. There are a couple of lines in the Conclusions section mentioning e.g. eutrophication and changing river loads. But first of all, these comments should be in the Discussion section, and further, they really need to be elaborated a bit. For example, what does it mean that you have no TA change, no CaCO<sub>3</sub> formation/dissolution, no deep water dynamics (?) and so on? I find it difficult to judge what effects these assumptions might have on the reliability of long-term model runs.

Are the above mentioned processes (or lack of processes) the main reason for the bias between Hector and CMIP5 output, or is the bias rather related to some systematic difference between emission/concentration forced models? I understand that you don't expect a perfect match (and by the way, I don't suggest that the CMIP5 output is necessarily better than Hector output), but it would be interesting to know a bit more of why they differ.

I recommend a major revision of this paper before publication – partly because of a need (in my opinion) to improve the structure and readability of the paper (see below), and further due to the reason explained above as well as numerous minor comments (see Specific comments and Technical corrections).

Structure of the paper:

First of all, I think you need to describe the purpose of this study much more clearly. As a reader you learn from the Introduction that the model is fast, powerful, robust, and timely (which I'm sure it is). I would however like to see (preferably as a final paragraph of the Introduction) a clear description of what it is that you're going to do with your model and why (without a subsequent discussion of what other models can or cannot do).

I would further prefer to see the Material and Methods section in one chapter divided into sub-sections (e.g. 2.1 Model description, 2.2 Ocean component, etc.) instead of three separate chapters – but this is of course a matter of personal preference.

In order to increase readability, the Results/Discussion section could also be divided in subsections; e.g. one part that covers the model calibration/validation, and a second part focused on the model sensitivity to different RCP scenarios.

The Discussion section tends to spill over a bit into the Conclusions section. I'd like to see a Conclusions section that merely summarizes your main findings - without too much discussion (the discussion should be in the Discussions section). In my view the Conclusions section shouldn't include results/statements that have not been described earlier in the manuscript. For example, you mention for the first time in the manuscript how Atlantic meridional overturning circulation may decrease according to model studies. You "also note" that eutrophication, upwelling, etc. will affect acidification in coastal seas. However, these factors have not really been addressed in any detail earlier in the manuscript (which I think they should).

In summary I would definitely recommend quite a bit of an effort to improve the structure of the paper.

Language:

I have some issues with punctuation and sentence construction here and there in the

**BGD**

12, C9388–C9394, 2016

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



manuscript (some examples are mentioned below in “Technical corrections”).

#### Artwork:

Figure 6 doesn't add anything that isn't already shown in Figure 5. Unless I'm mistaken, there is a more or less linear relationship between the saturation states for aragonite and calcite ( $\Omega_{AR} \sim 1.8\Omega_{CA}$ ). I suggest that you simply remove Figure 6.

Figure 7: Very difficult to see anything in this figure! In its present form, Figure 7 doesn't contribute much to the manuscript.

#### Specific comments:

Throughout the manuscript, you use anything from 1 and up to 5 significant digits when presenting results and data. I would like to see a bit more consistency.

There are a number of acronyms in the manuscript. These should (in general) be explained the first time they appear:

p. 19270, ln. 7: Here, the CMIP5 acronym appears for the first time. Although the meaning might be evident to most people in the field, I think you should at least explain the acronym (and maybe somewhere in the Introduction even write a sentence explaining what this project is).

p. 19270, ln. 11-12: Here you should also define the RCP acronym that is used throughout the manuscript.

p. 19272, ln. 22: You mention “earth system models” already in the abstract, so the ESM abbreviation should be defined there. Perhaps also use capital letters; “Earth System Models”?

p. 19276, ln. 3: You don't explain that TA is short for total alkalinity.

There are numerous typos/mistakes related to descriptions of the carbonate system:

p. 19271, ln. 24-25: The DIC concentration should be defined as the sum of  $[HCO_3^-]$ ,

**BGD**

12, C9388–C9394, 2016

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



[CO<sub>3</sub><sup>2-</sup>], and [CO<sub>2</sub><sup>\*</sup>], where [CO<sub>2</sub><sup>\*</sup>] = [CO<sub>2</sub> (aq)] + [H<sub>2</sub>CO<sub>3</sub>].

p. 19272, ln. 2: Again, use CO<sub>2</sub><sup>\*</sup> instead of CO<sub>2</sub> (aq). [CO<sub>2</sub><sup>\*</sup>] is used later in the manuscript (p. 19276 and Appendix) but without an explanation of what the definition signifies.

p. 19273, ln. 19: Bicarbonate and carbonate should be written HCO<sub>3</sub><sup>-</sup> and CO<sub>3</sub><sup>2-</sup> respectively.

p. 19276, ln. 9, 14, . . .: Here and throughout the manuscript the unit for TA should be in μmol kg<sup>-1</sup> (not mol kg<sup>-1</sup>).

p. 19276, ln. 27: Surely, ΔpCO<sub>2</sub> is the difference in pCO<sub>2</sub> between atmosphere and ocean (not the difference in [CO<sub>2</sub>]).

p. 19281, ln. 20-28: Are temperature dependent changes in pH and aragonite/calcite saturation linear? I don't think so, but maybe it's a good approximation within the current range of temperature change (?). This should be clarified.

p. 19282, ln. 16: Calcium carbonate should be written CaCO<sub>3</sub> (not CaCO<sub>3</sub><sup>-</sup>).

Technical corrections:

p. 19270, ln. 14-15: “low latitude (>55o)” should be “low latitude (<55o)”?

p. 19270, ln. 15-19: You switch between 1-3 significant digits when presenting results here.

p. 19271, ln. 8: I would write “surface and deep water warming”. Further, “calcium carbonate saturations” sounds a bit strange to me, maybe you could write “calcite and aragonite saturation levels” instead.

p. 19271, ln. 17: I think you should define the preindustrial pH level (~8.2) here. From the abstract the reader can of course calculate the level to be 8.17 in low latitudes, and later on in table 5 find values for both low and high latitudes. In addition, on p.19271-

**BGD**

12, C9388–C9394, 2016

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



19272 you present the DIC composition at a pH level of 8.2 without mentioning why you choose this value. I get what you mean but it's nonetheless a bit annoying.

p. 19272, ln. 6: I would write “saturation levels” instead of “saturations”.

p. 19272, ln. 11-12: Which model studies? I think of course that you refer to the ones in the following sentences, but just to be clear you could for example finish the “Modeling studies” sentence by a colon (: ) instead of period ( . ).

p. 19273, ln. 4: “capability” instead of “capabilities”.

p. 19274, ln. 9-12: A couple of strange sentences here. This paragraph should be clarified.

p. 19274, ln. 20-21: Strange sentence: “two surface boxes (high and low latitude), an intermediate and deep box, simulated a simple. . .”. Maybe you could instead write: “two surface boxes (high and low latitude), one intermediate and one deep box, simulating a simple. . .”. The same description by the way appears on line 1-2 (same page), but without references.

p. 19274, ln. 22: “15% of the ocean” – 15% of the ocean surface area/volume, right?

p. 19275, ln. 16: “change in change in” – remove one of the “change in”.

p. 19276, ln. 20: “. . .based on salinity, temperature, and pressure” – according to ln. 7-8, same page, you have neglected the effects of pressure.

p. 19279, ln. 23 and 26: Here and throughout the manuscript you switch between preindustrial and pre-industrial. Choose one form and stick to it.

p. 19280, ln. 10: Change “More observations in the. . .” to “Moreover, observations in the. . .”

p. 19280, ln. 14-17: Again, try to be consistent with the number of significant digits when presenting results.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

p. 19281, ln. 2: I think “19 and 25 %” should instead be “0.19 and 0.25 % yr<sup>-1</sup>”, correct?

p. 19281, ln. 3: “of” instead of “Of”.

p. 19282, ln. 13: Correct the sentence here – “. . .unprecedented in the potentially the last. . .”.

p. 19282, ln. 16: “Organisms” instead of “organism”.

Tables:

Table 2, row 6: Change “Carbon DO” to “Deep ocean carbon”.

Table 5: Very difficult to read this table. The columns appear to have shifted one step to the right in relation to the title row.

---

Interactive comment on Biogeosciences Discuss., 12, 19269, 2015.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper