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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 #1

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General comments

This manuscript discusses the results of a reduced-form global carbon-cycle model simulating the surface-water carbonate system between 1850 and 2300, and compares the results with medians of CMIP5 Earth System Models. Such reduced-form models are important tools as they are much cheaper and quicker to run than ESMs and thus, if properly validated, can be used to test many more emission scenarios and allow parameter sensitivity studies.

The results are interesting and, as far as I can tell, scientifically sound, although I do have some remarks on the discussion of Figure 7 (see below). However, in an

earlier paper (Hartin et al., 2015) the model has been validated, and thus I expect this manuscript to go beyond the point of model comparison with CMIP5 ESMs only and additionally conduct sensitivity studies and/or explore a range of emission and/or land use change scenarios. Unfortunately, both of these last two points are lacking in the manuscript.

The aim of the manuscript is not clearly stated in the introduction and should thus, in my opinion, involve more than just "highlighting the capability . . . to project changes in the upper ocean carbonate system over the next three centuries", as written in the abstract. The manuscript can generally benefit from typographical editing, as it contains some ambiguous statements and grammatical incorrect formulations. Specific points spotted by me are mentioned below.

I understand that the focus of the paper is not to discuss the setup of Hector, rather than to discuss practical applications. However, some fundamental questions came into mind when reading Sections 2 and 3, which will be discussed below. In general, the model description is somewhat difficult to understand without having read Hartin et al. (2015) and so a revision of these sections, which I would merge into one section with two sub-sections, is recommended. It is important that the reader can understand the basic concepts of the model without having to refer to Hartin et al. (2015).

In summary, I recommend major revisions for this manuscript, whereby the major point is to include additional work on sensitivity analysis and/or explore a range of emission and/or land use change scenarios.

Specific comments

- p. 19270, lines 18-19: "Under a high emissions scenario. . . aragonite saturations". This sentence implies that the relations between warming and acidification / aragonite saturation, i.e. δpH/δT and $\delta\Omega/\delta$T, are linear. However, both from previous work (e.g., Riebesell et al., 2009) as from Figure 7 of this manuscript is becomes clear that this is not the case. In other words, the slopes of δpH/δT and $\delta\Omega/\delta$T are different when there is a warming of 3°C then when there is a warming of 1°C. Please rephrase this sentence.

- p. 19272, line 26 to p. 19273, line 2: This section describes the main advantage of using reduced-form climate models relative to ESMs, focussing mostly on the ability of running arbitrary future climate change scenarios and sensitivity studies. While reading this section, I expected both of these to show up in the manuscript. However, the model is only run with the RCP scenarios, and the discussion focuses mostly on RCP8.5. These aspects are surely missing in the manuscript and should, in my opinion, be added to it.

- p. 19273, lines 3-7: This section only explains why the study is timely and lacks a description of the aim of this study, except maybe for "projecting changes in the surface ocean carbonate system over the next three centuries". As such, the difference between this manuscript and Hartin et al. (2015) is not clear. One could consider the previous section of the manuscript (the advantages of reduced-form models vs. ESMs) as an aim; however, this aim is not met (see previous comment). Please describe a clear research aim and also describe the experiments carried out here or elsewhere in the introduction.

- p. 19274, line 6 (Eq. 1): why are different signs used for FO(t) and FL(t) as opposed to FA(t) and FLC(t)? I understand that the latter two are by definition positive but for the other fluxes this might not necessarily be the case. From the definition of FL(t) (Eq. 2) I understand why FL(t) has a negative sign in front of it (if NPP exceeds RH there is a net uptake of atmospheric CO2), but this way of formulating is, in my opinion, not very intuitive.

- p. 19275, line 15 (Eq. 4): Please state clearly that in the current form Eq. 4 is only valid for the surface boxes, as the latter term ($Fatm \rightarrow i$) is only present for these boxes. More fundamentally, I was a bit confused to see that in the land part of the model NPP and RH are explicitly calculated while they are not in the ocean part of the model (which

is the focus of the model). Please comment on this choice. The current implementation implies that NPP and RH in a single box are in equilibrium, i.e. do not affect $Fatm \rightarrow i$ and the fluxes between the various boxes. Or is this taken into account for by tuning the model such that the steady-state volume transport from the surface high latitude to the deep ocean amount to 100 Pg C?

- p. 19276, line 5: I was very surprised to see that the intermediate and deep ocean carbonate systems are not calculated by Hector. Why did the authors make this choice? Without including these waters, the reference to changes in deep waters in the introduction (p. 19272, line 16) could be removed as these changes are not further discussed.

- p. 19276, lines 3-19: From this section, it does not become clear to me how Pg C of a box relates to the computed DIC concentration. Is all oceanic C assumed to be present as DIC or is there also a Corg component? If not, why not and how is this validated?

- p. 19276, lines 16-18: I would like the authors to comment on the validity of these assumptions, thereby providing references.

- p. 19278, lines 10-12: This statement makes me wondering how time series were treated where more than 2 carbonate system parameters were measured. If I recall correctly, this is the case for parts of these time series. How have possible inconsistencies related to overdetermination of the carbonate system been dealt with?

- p. 19278, lines 12-14: It should be mentioned here which proxy these data are based on (δ11B for pH and assuming constant TA for calculating ΩAr). Moreover, since ΩAr is also calculated in the Pelejero et al. (2005) paper, why didn't the authors also calculate other carbonate system parameters here?

- p. 19278, lines 14-16: In my opinion, it would be much better if historical rates of change for the various locations were compared with, and calculated on the same time interval, as the values published by Bates et al. (2014), rather than this rather arbitrarily chosen 20-year period. Such a direct comparison would make it much easier to assess the performance of the model.

- p. 19278, lines 25-26: Even though a comparison might not be statistically robust, it would still be very interesting if the authors commented on the performance of Hector relative to CMIP5 models run under prescribed emissions.

- Results and discussion: What I miss here is a discussion of the reasons behind the consistent offset of Hector and the median of the CMIP5 models, most notably in pH, ΩAr and DIC, where Hector consistently calculates higher DIC, pH and ΩAr and lower pCO2. The bias after 2100 for pCO2 is mentioned, but this offset is consistent throughout the whole simulation period.

- p. 19281, lines 14-19: I miss a short discussion on the impacts of seasonality in ΩAr (e.g. Sasse et al., 2015) and possible changes therein.

- p. 19281, line 20 to p. 19282, line 2: as said before, the sensivities δpH/δT and $\delta\Omega/\delta$T are not constant with time and thus these trends are not linear. The authors must provide here which ΔT is used to calculate the ΔpH/ΔdT and $\Delta\Omega/\Delta$dT. Moreover, it would be very interesting to discuss the ΔT at which ΔpH is maximal.

- p. 19282, lines 3-6: this figure discussion is somewhat meagre. Discuss by how much these parameters have changed / will change and when changes will slow down and/or revert direction. Also show the high latitude projections for comparison, or, if they are very similar, discuss them. It's somewhat strange that they are mentioned everywhere except for this figure.

- p. 19282, lines 14-20: Move this section to the end of Section 5 (where Fig. 8 is discussed) as it fits much better there.

- p. 19282, lines 21-22: This information is of vital importance for the understanding of the setup of Hector and thus must be included in the method section. It partly answers my previous question (p. 19276, lines 3-19) on whether there is a Corg component in

Hector, but I'd still like to see how this choice is validated.

- p. 19282, line 21 to p. 19283, line 5: this paragraph belongs to the Discussion, not the Conclusions section.

- Table 2: Wouldn't it be useful to (additionally) give the values after spin-up, as they are used as historical background values, rather than the initial values?

- Table 4: In its current discussion in the manuscript, the table is redundant and a reference to Taylor et al. (2012) on p. 19278 would be sufficient instead. However, I'd rather see the authors leaving the Table in the manuscript and indicating which models are used for which median and RMSE calculations. Currently, for each parameter it is only indicated how many ESMs are used for its calculation, but not which ones, while this could be important information. If they decide not to do so, they should remove Table 4.

- Table 5: Why are Δ values not calculated for 2100?

- Figure 1: Figure 2 of Hartin et al. (2015) is much clearer than Figure 1 of this manuscript. I would advise the authors to use the former figure, or an adapted version thereof, instead of the current Fig. 1. To improve the current Fig. 1, "surface" should be added to "high latitude". Moreover, the 'earth pool' needs to be added as FA(t) and FL(t) seem now to be represented by the same arrow. Additionally, the units of the diagram are conceptually incorrect. The represented fluxes (TT, TH , EIL and EID) have units of m3 s-1, while the reservoirs (Ca, CHL, CLL, CIO and CDO) have units of Pg C. This should be adapted. Finally, in the caption it is stated that the initial carbon pools have units of Pg C yr-1, which should obviously be Pg C.

Technical corrections

- p. 19270, line 6: remove "the", and capitalise Earth System Models. Line 15: shouldn't 0.4 units be 0.40 units? Line 17: I know it must result from rounding but to read that 2.21 – 0.80 equals 1.42 is a bit strange. Perhaps rephrase and leave out the 0.80. Lines 19-21: “Hector reproduces . . . compared to observations and CMIP5 models”. This sentence is somewhat unclear. Add ‘respectively’ at the end to make the distinction between historical (trends vs. observations) and future (projections vs. models) comparisons clearer, or fully rewrite this sentence.

-19271, line 11: “there is some concern. . .” Is it a bad thing per se that the oceanic sink will be less efficient? Please phrase more neutrally. Line 17: change “the preindustrial” to “preindustrial times” Line 20: change” forming H2CO3, dissociating. . .” to “thereby forming H2CO3, and dissociating. . .” Line 24: CO2(aq) has not yet been defined here; additionally, H2CO3 has been used before and its difference with CO2(aq) is not explained. I feel it’s better to use CO2* as the sum of H2CO3 and CO2(aq) here. Lines 25: “A doubling of CO2”. What is meant here, atmospheric pCO2? Please phrase clearer. Line 26: add a reference to where this percentage of ca. 10 comes from (see also previous comment).

- p. 19272, line 3: change “biogenic carbonate” to “biogenic calcium carbonate” Lines 8-9: It is a bit unnecessary to give this many references here. Please make a selection. Line 15: the IS92a scenario hasn’t been used for a while in global predictions. Please provide a reference using either of the RCP scenarios (e.g. Bopp et al., 2013). Line 21: Capitalise Earth System Models. Line 22: replace “prescribed emission pathways” by “Representative Concentration Pathways” or, in case the authors would like to keep the statement more general, define RCPs here.

- p. 19273, line 16: “. . .are typically parameterized”. Shouldn’t it read “. . .are typically not parameterized”? Lines 21-24: the sections mentioned here do not match the sections in the manuscript. I would however advice the authors to apply the sections as described here, i.e. to merge the current sections 2 and 3 (see general comment).

- p. 19274, lines 19-22: Replace “consisting” by “and consists”, “deep box” by “a deep box” and “simulated” by “simulating”. Line 23: “15 % of the ocean”. Change into “15 % of the surface ocean by volume” (or at least I assume that this is meant here).

- p. 19275, lines 3-4: “in the high latitude. . .” versus “for the low latitude” is inconsistent. Line 6: Shouldn’t “Fi=2” be “n=2”?

- p. 19276, line 7: change “A” to “Appendix A”. Line 9: provide definitions of LL and HL here. Lines 10-11: “that when. . .global ocean”. Please rephrase, this is not very clear. Line 23: “A1”. I assume Appendix A is meant here, not equation A1. Please clarify. The same applies to p. 19277, lines 2 and 8.

-p. 19277, line 1: provide the units of this unit conversion factor. Lines 22-23: Move the definition of RCP to the introduction (see an earlier comment).

- p. 19278, lines 8-9: The references are also given in Table 3 and can thus be removed here for readability.

- p. 19280, lines 20-22: This doesn’t really fit here and has already been mentioned before (Introduction / Methods). Line 23: add “compared” between “RCP8.5” and “to”. Lines 25-27: which 14-year period is meant here? Where do the numbers for CMIP5 and HOT come from? Please provide references. Also, are the CMIP5 and Hector values for the whole surface ocean or for the low latitude box? (so that it can be compared to the HOT site) Finally, these numbers are different than those presented in Table 5, which is quite confusing. As mentioned above, I’d recommend sticking to a single comparison, i.e. the values published in Bates et al. (2014). Line 27: change “Repeat” to “Repeated”

- p. 19281, line 2: shouldn’t the percentages of 19 and 25 % be 0.19 % and 0.25 % yr-1, as opposed to the 0.34 % per year mentioned before? Line 3: change “Of” to “of” Line 4: change “latitude” to “latitudes” Line 7: replace the second “low latitude ΩCa” by “a” Line 14: change “Century” to “century”

- Appendix: Don’t start the appendix with an equation without any introduction. Add 1-2 lines before Eq. A1.

- Table 1: Remove the first column, as these parameters do not come back anywhere else in the manuscript. Change the name of the last column to “Reference” or “Notes”. Finally, add a reference to the average wind speed (e.g. Liss and Merlivat, 1986; or Sarmiento and Gruber, 2006)

- Table 3: The column “Ocean Carbon Measurements” needs to be renamed as not all of these parameters were actually measured.

- Table 5: The font size is somewhat small. Moreover, the distinction between high and low latitudes is currently not very clear. I would advise the authors to use different colours instead of brackets.

- Figures 2-6: units on the y-axes are lacking. For pH, add the scale. The model abbreviations at the right hand side should be replaced by proper descriptions (e.g. “High latitude” and “low latitude” within the plot area). The legend should be split into “Model” (CMIP5 and Hector) and “Observations” (the plotted time series). In most of the plots, the observations are invisible. Make sure that the observations are plotted on top of the model results, like has been done for DIC at low latitudes.

- Figure 7: increase the size of the plot. Also the differently coloured dots in the upper part of the legend (“Scenario”) are rather confusing as the plot consists of both dots (Hector) and crosses (CMIP5). It would be better to use lines here instead, and save the dot and cross for the lower part of the legend (“Model”).

- Figure 8: again, replace the model abbreviations on the right hand side of the plots by a proper description and add the pH scale.

References not mentioned in manuscript

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