

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.

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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 #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 CO₂ 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 CaCO₃ forma-

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tion/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 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, pCO₂, 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 CaCO₃ 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.

**Thank you for this comment. There were multiple assumptions within Hector that are not well described. Throughout the text we have added in the needed discussion on these assumptions. 1. For example, “TA is calculated at the end of spinup and held constant in time, resulting in 2311.0 $\mu\text{mol kg}^{-1}$ high latitude box and 2435.0 $\mu\text{mol kg}^{-1}$ for the low latitude box. These values are within the range of open ocean observations, 2250.0 – 2450.0 $\mu\text{mol kg}^{-1}$ of solution (Key et al., 2004; Fry et al., 2015). We assume negligible carbonate precipitation/dissolution and assume no alkalinity runoff from the

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land surface to the open ocean. Most studies hold alkalinity constant with time and this is a reasonable assumption over several thousand years (Lenton, 2000; Zeebe and Wolf-Gladrow, 2001; Glotter et al., 2014; Archer et al., 2009). After thousands of years the dissolution of CaCO₃ will restore ocean pH, thereby increasing the uptake of more CO₂. On ~10,000 year scales Hector will underestimate the CO₂ uptake, however, we are interested in 100-300 year timeframe.” 2. “All carbon within the ocean component is assumed to be inorganic carbon. Dissolved organic matter is less than 2% of the total inorganic carbon pool, of which a small fraction is dissolved organic carbon (Hansell et al., 2001). Therefore, for simplicity we chose not to include organic carbon within Hector.” 3. “The dynamics of ocean uptake of CO₂ is strongly dependent on the rate of downward transport of CO₂ laden waters from the surface ocean to depth. We neglect any climate feedbacks on the carbon cycle resulting from changes in ocean circulation and hold ocean circulation constant in time. CMIP5 models show up to a 60% decrease in the Atlantic meridional overturning circulation (AMOC) by 2100 (Cheng et al., 2100). We use our sensitivity analyses to change the circulation thereby changing the downward transport of carbon. A 10% change in ocean circulation (Tt) results in a <4% change in air-sea fluxes and moderate effects on surface pH and $\delta^{13}C_{org}$. Therefore, a 60% decline in the overturning circulation may result in roughly a 20% change in the air-sea fluxes of carbon according to this sensitivity analyses.”

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.

**While we are not able to get to the root cause of some of these biases, we have included a table of validation metrics for both the high and low latitude ocean carbonate system comparing Hector to the CMIP5 median. The bias in DIC is most likely from

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our carbon pool values initialized higher the CMIP5 median. There is bias in pCO₂ particularly in the high latitude when compared to CMIP5, but we find Hector to be in closer agreement with the observational record. The text has been updated to reflect these findings.

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).

**The authors thank this reviewer for their very helpful comments. The manuscript has been substantially improved in both the structure and readability.

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).

**We thank the reviewer for this comment. We agree that the abstract and introduction lacked a clear description of the purpose of this study. We have since made substantial changes. For example, this is the last paragraph of the introduction: “Our goal of this study is to quantify how well Hector, a reduced-form model, that explicitly treats surface ocean chemistry, emulates the marine carbonate system of both observations and the CMIP5 archive, and to explore the parametric sensitivities of Hector's ocean outputs. We run Hector under different emissions pathways, comparing the marine carbonate system to the observational record and CMIP5 archive. The remainder of the paper is organized as follows; section 2, a detailed description of Hector's ocean component, the data sources and simulations run, section 3, results of the model comparison and sensitivity experiments and lastly, section 4, a discussion of the results.”

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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.

**The Methods sections was combined into multiple subsections 2.0 Model Description, 2.1 The Ocean Component, 2.2 Simulation and experiments, 2.3 Data Sources. A significant portion of the model description was moved to the Appendix and expanded on.

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.

**Thank you for this comment, we have since divided the results section into 3.1 Model and Observation Comparisons and 3.2 Model Parameter Sensitivity.

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).

**The authors thank the reviewer for this great comment. The Discussion section stands as a discussion of the model comparison and sensitivity analysis. It is here we address some of the assumptions made within Hector. The conclusions now restate the significant findings of the paper instead of discussing these assumptions. See detailed comments above for more information on these assumptions.

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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 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 ($AR^*1.8CA$). I suggest that you simply remove Figure 6.

**The authors agree with the reviewer and we removed Figure 6 from the manuscript.

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

**We agree with the reviewer and we removed this figure from the manuscript as it didn't add anything substantial to the study.

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.

**The significant figures issue has been addressed and the manuscript is updated to reflect these changes.

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.

**The text changed to reflect these 2 comments: "ESMs are computationally expen-

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sive and typically run using stylized experiments or a few Representative Concentration Pathways (RCPs), greenhouse gas concentration trajectories used in the Intergovernmental Panel on Climate Change 5th Assessment Report. This generally limits the ESM-based analyses to those scenarios. The occurrence of ocean warming and acidification is consistent across the Coupled Model Intercomparison Project (CMIP5) ESMs, but their rates and magnitudes are strongly dependent upon the scenario (Bopp et al., 2013)."

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"?

**Corrected.

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:

**TA is now defined within the text. "There are four measurable parameters of the carbonate system in seawater: DIC, total alkalinity (TA), pCO₂ and pH, and any pair can be used to describe the entire carbonate system."

p. 19271, ln. 24-25: The DIC concentration should be defined as the sum of [HCO₃⁻], [CO₃²⁻], and [CO₂^{*}], where [CO₂^{*}] = [CO₂ (aq)] + [H₂CO₃].

**The text has been updated: "Dissolved inorganic carbon (DIC) is the sum of [HCO₃⁻], [CO₃²⁻] and [CO₂^{*}], where [CO₂^{*}] = [CO₂(aq)] + [H₂CO₃]."

p. 19272, ln. 2: Again, use CO₂^{*} instead of CO₂ (aq). [CO₂^{*}] is used later in the manuscript (p. 19276 and Appendix) but without an explanation of what the definition signifies.

**Corrected. "Dissolved inorganic carbon (DIC) is the sum of [HCO₃⁻], [CO₃²⁻] and [CO₂^{*}], where [CO₂^{*}] = [CO₂(aq)] + [H₂CO₃]."

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p. 19273, ln. 19: Bicarbonate and carbonate should be written HCO₃⁻ and CO₃²⁻ respectively.

**Corrected.

p. 19276, ln. 9, 14, Here and throughout the manuscript the unit for TA should be in $\mu\text{mol kg}^{-1}$ (not mol kg^{-1}).

**Corrected.

p. 19276, ln. 27: Surely, pCO₂ is the difference in pCO₂ between atmosphere and ocean (not the difference in [CO₂]).

**Yes, it is the difference between the atmosphere and ocean and the manuscript has been updated.

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.

**We agree with the reviewer that the discussion of Figure 7 was initially unclear. After rewriting we determined that this figure did not add anything substantial to the study and we decided to remove it from the manuscript.

p. 19282, ln. 16: Calcium carbonate should be written CaCO₃ (not CaCO₃⁻).

**Corrected.

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

**Corrected.

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

**Significant figures have been addressed and are more consistent throughout the text.

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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.

**Saturations throughout the manuscript were changed to saturation levels.

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.19271-19272 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.

**Thank you for the comment on this. We agree that this is a little confusing. We have since rewritten many of the sections where absolute values are involved and deleted the DIC composition sentence.

p. 19272, ln. 6: I would write “saturation levels” instead of “saturations”. **All saturations were changed to saturation levels throughout the manuscript.

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 (.). **This sentence was deleted as it did not contribute to the manuscript.

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

p. 19274, ln. 9-12: A couple of strange sentences here. This paragraph should be clarified. **A more detailed model description was moved to the Appendix and was updated to reflect this comment.

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

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references.

**This sentence has been corrected. “It consists of four boxes; two surface boxes (high and low latitude), one intermediate, and one deep box.” “Once carbon enters the high latitude surface box it is circulated between the boxes via advection and water mass exchange, simulating a simple thermohaline circulation.”

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

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

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.

**Corrected to preindustrial.

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

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

**The authors correctly all significant digit issues throughout the manuscript.

p. 19281, ln. 2: I think “19 and 25 %” should instead be “0.19 and 0.25 % yr⁻¹”, correct? **Corrected.

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

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

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

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Tables: Table 2, row 6: Change "Carbon DO" to "Deep ocean carbon".

**After significant reorganization of the manuscript we deleted table 2 and included more text about Hector's parameters within the manuscript.

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.

**Table 5 was separated out into two tables, one for high latitude and one for low latitude.

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

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