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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.

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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 Received and published: 11 January 2016

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.

**We thank the reviewer for these insightful comments. Addressing these comments has substantially improved the manuscript and taken it beyond the point of just a simple model comparison. 1. We ran a series of model sensitivity experiments to quantify how influential some of Hector's parameter inputs are on its outputs (in particular, pH and âĎęAr). Sensitivity analyses are important to both to document model characteristics, explore model weaknesses, and to check to what degree the model behavior conforms with what we know of the ocean system. We selected eight land and ocean parameters, varying each by $\pm$10%. 2. We conducted a thorough read through of the manuscript addressing numerous typos and grammatical errors. 3. We have increased our discussion of Hector within the manuscript as well as adding more detail of the model to the Appendix. We hope the reader can now better understand the details and workings of Hector without having to read Hartin et al., 2015 - GMD.

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. _pH/_T and _/_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 _pH/_T and _/_T are different when there is a warming of 3_C then when there is a warming of 1_C. Please rephrase this sentence.

**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. 19272, line 26 to p. 19273, line 2: This section describes the main advantage of using reduced-form climate models relative to ESMs, focusing 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.

**The authors thank the reviewer for these insightful comments. We have substantially

refocused the paper and included an experiment on the parametric sensitives within Hector. Please see the general comments section above for a more thorough description.

- 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.

**We thank the reviewer for this comment. We have substantially changed the manuscript and included a section on quantifying the model's sensitivity to parametric inputs. For example, in the abstract, "In this study we examine the ability of Hector v1.1, a reduced-form global model, to project changes in the upper ocean carbonate system over the next three centuries, and quantify the model's sensitivity to parametric inputs." And the introduction has also been updated: "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."

- 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.

**While we agree with the reviewer that this equation may not be very intuitive, we decided to leave the equation as is to be in agreement with Hartin et al., 2015 and Meinshausen et al., 2011. The total change in atmospheric carbon is from the anthropogenic emissions plus any emissions from land-use changes minus the uptake from the ocean and land systems. Within Hector uptake by both the ocean and land is positive. We agree that these signs can be easily switched but within Hector they are positive.

- 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!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!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?

1. **The manuscript has been updated to reflect those changes in equation 4. 2. It would be great to include NPP and RH within the ocean component. However, in order to keep it simple we do not model the organic carbon cycle within the ocean. "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 and Carlson, 2006." Being that Hector is a global model, we felt that it was more important to accurately calculate the inorganic carbon system first. 3. NPP and RH represent the carbon balance with the terrestrial component and they do not necessarily have to be in equilibrium with each other. NPP and RH indirectly effect the oceanic uptake of CO2 by changes in the atmospheric CO2 levels.

- 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.

**In order to keep Hector as computationally efficient and simple, we decided to initially develop Hector with only the carbonate system in the surface ocean in mind. We note that there may be large value in simulating the carbonate system in intermediate and deep waters and hope future releases of Hector will include these changes. We have also removed the text referring to deep water changes as we agree it is not needed.

- 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?

**The text of the manuscript is updated to reflect the questions above. We assume all carbon to be inorganic carbon within Hector. We acknowledge that we are missing a portion of the total carbon system, but due to small fraction of organic carbon compared to inorganic carbon we have choosen to simplify Hector and leave this portion out. Future versions of Hector may include calculations of the organic carbon pool. "There are four measurable parameters of the carbonate system in seawater: DIC, alkalinity (TA), pCO2 and pH, and any pair can be used to describe the entire carbonate system. DIC (μmol kg-1) is calculated as a function of the total carbon in the box (PgC), the mass of carbon, the density of seawater, and the volume of the box. 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."

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

**References are added and the section expanded: "We assume negligible carbonate precipitation/dissolution and assume no alkalinity runoff from the 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). On glacial-interglacial time scales alkalinity and the dissolution of CaCO3 sediments is an important factor in controlling atmospheric [CO2] (Sarmiento and Gruber, 2006). Therefore, on these scales Hector will underestimate the oceanic CO2 uptake. For purposes of our studies we are interested in 100-300 year timeframe."

- 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 over determination of the carbonate system been dealt with?

**When DIC and TA were given we used those parameters to calculate the rest of the carbonate system to be consistent with the calculations in Hector, for example, BATS. When the entire carbonate system was available online, as in HOT, we used the values directly from the site. And for those like ESTOC, Iceland and Irminger Sea, we used those variables supplied to calculate the remaining parameters of the carbonate system.

- p. 19278, lines 12-14: It should be mentioned here which proxy these data are based on (_11B for pH and assuming constant TA for calculating Ar). Moreover, since Ar is also calculated in the Pelejero et al. (2005) paper, why didn't the authors also calculate other carbonate system parameters here?

**The text is updated to reflect these comments. Pelejero et al, 2005 used 11Boron for the analyses of the carbonate system.

- 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.

**The authors agree and have since changed the comparison to more in line with Bates et al., 2014.

- 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.

**This is an interesting and valuable suggestion and something that we may pursue in the future. It is outside of the scope of our current study, however.

- 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, Ar and DIC, where Hector consistently calculates higher DIC, pH and Ar and lower pCO2. The bias after 2100 for pCO2 is mentioned, but this offset is consistent throughout the whole simulation period.

**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 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.

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

**The reviewer makes a good point about seasonality. We included some text to reflect this: "Accounting for seasonal variations in the ΩAr saturation levels may move this time of undersaturation forward by 17 $\pm$ 10 years (Sasse et al., 2015). Due to Hector's time step of 1 year, we may be overestimating the time when ocean acidification reaches a critical threshold."

- p. 19281, line 20 to p. 19282, line 2: as said before, the sensivities _pH/_T and _/_T are not constant with time and thus these trends are not linear. The authors must provide here which _T is used to calculate the _pH/_dT and _/_dT. Moreover, it would be very interesting to discuss the _T at which _pH is maximal.

**In agreement with other reviewers after rewriting the discussion for this figure we determined that figure 7 did not add anything substantial to the study and we decided to remove it from the manuscript.

- 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.

**We agree that the discussion of old Figure 8 and new Figure 6 was lacking. We have updated the manuscript in the following way, "Lastly, Figure 6 highlights pH and âĎęAr projections under all four RCPs from 1850 to 2300. Over the last 20 years both pH and âĎęAr have declined sharply and will continue to rapidly decline under RCP 4.5, 6.0 and 8.5 outside of their preindustrial and present day values. These RCPs represent a range of possible future scenarios, with ocean pH varying between 8.15 and 7.46 for the high latitude and âĎęAr varying between 1.94 and 0.60. High latitude âĎęAr saturation levels presently are much lower than the low latitude and reach under saturation before the end of the century. Even under a best case scenario, RCP 2.6, low latitude pH will drop to 7.73 by 2100 and to 7.43 by 2300 and âĎęAr saturations will remain outside of present day values."

- 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.

**These lines are now within the results section where the figure is explained. "Even under a best case scenario, RCP 2.6, low latitude pH will drop to 7.73 by 2100 and to 7.43 by 2300." Along with most of the line within the Discussion section: "pCO2 and DIC are increasing rapidly as atmospheric [CO2] continues to rise under RCP 4.5, 6.0 and 8.5. pH, and âĎęAr are decreasing rapidly outside of observations and are projected to continue to decrease under all scenarios (Figure 6). These changes may results in drastic changes to marine ecosystems in particular the CaCO3 secreting organisms. For example, the rate of coral reef building decreases, calcification rates of planktonic cocolithophores and foraminifera decreases, changes in trophic level interactions and ecosystems, have all been proposed to be potential consequences of ocean acidification…"

- 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.

**We agree with the reviewer that these assumptions were not properly discussed. We updated the manuscript to discuss organic carbon, TA changes and ocean circulation. "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 and Carlson, 2001). Therefore, for simplicity we chose not to include organic carbon within Hector." "We assume negligible carbonate precipitation/dissolution and assume no alkalinity runoff from the 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). On glacial-interglacial time scales alkalinity and the dissolution of CaCO3 sediments is an important factor in controlling atmospheric [CO2] (Sarmiento and Gruber, 2006). Therefore, on these scales Hector will underestimate the oceanic CO2 uptake. For purposes of our studies we are interested in 100-300 year timeframe." "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 âĎęAr. 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."

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

**This has been moved out of the conclusions section and separated into the model description section and the discussion section. Please see the comment above for details.

- 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?

**We agree that values after spinup are important. However, after significant reorganization of the manuscript we deleted table 2 and included more text about Hector's parameters.

- 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.

**We agree with the reviewer that more information is needed to make this table more useful. The table is updated to reflect these comments. We added a column of carbonate parameters that were used in this manuscript.

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

**The formatting in Table 5 was not correct. Since then, I have reformatted Table 5.

- 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.

**The authors agree that this figure was inconsistent. We have since adapted the figure from Hartin et al., 2015 – GMD.

Technical corrections - p. 19270, line 6: remove "the", and capitalize 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.

**We thank the reviewer for these comments. The authors have updated these comments in the manuscript.

-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).

**We thank the reviewer for these comments. The authors have updated these comments in the manuscript.

- 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.

**All of the above have been changed within the manuscript. The references are shorten and I updated the sentences to reflect RCPs and not IS92a scenarios.

- 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).

**The sections have been updated. Please see the comments under general comments.

- 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).

**All typos have been corrected.

- 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”?

**The reviewer is correct, Fi=2 was corrected to 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.

**All of the ‘A’s have been changed to Appendix. HL and LL are defined. And the sentence has been updated.

-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).

**The authors have updated the manuscript to reflect these suggestions.

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

**The reviewer makes a good point and the references have been removed from the text.

- 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"

**We compare Hector to the observational record from Bates et al., 2014 (table 6) and we also compare Hector across the CMIP record, 1850-2300 (table5). This section of the results has been cleaned up to reflect these comparisons. "Hector accurately simulates the change in ΩAr (-0.0085 yr-1) compared to observations (Table 6). As with pH Hector is slightly higher than the CMIP5 median but closer to the observational record. We only highlight ΩAr, as ΩCa is similar to that of ΩAr. Repeat oceanographic surveys in the Pacific Ocean observed an average 0.34% yr-1 decrease in the saturation state of surface seawater with respect to aragonite and calcite over a 14-year period (1991-2005) (Feely et al., 2012); the average decrease in Hector is between 0.19% and 0.25% yr-1. Saturation levels of ΩAr decrease rapidly over the next 100 years in both the high and low latitude. Hector accurately captures the decline in saturations with low RMSE values for ΩAr (0.027). Under RCP8.5 Hector projects that low latitude ΩAr will decrease to 2.2 by 2100 and down to 1.4 by 2300. The high latitude oceans will be understaturated with respect to aragonite by 2100 and will drop down to 0.7 by 2300."

- 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 Ca" by "a" Line 14: change "Century" to "century"

**All of these typos have been corrected.

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

**The Appendix is now updated with a more thorough model description.

- 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) **The column name was changed to notes and references were added.

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

** "Ocean Carbon Measurements" was replaced with "Ocean Carbon Parameters"

- 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.

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

- 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 2-6 have been updated with scales, better color schemes to see the observations, and better descriptions ("High Latitude" and "Low Latitude"). We also deleted the "Model" legend from the plot.

- 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").

**We decided to remove Figure 7 from the manuscript as it did not add much to the study.

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

**Figure 8 is updated with better descriptions, as in the other figures. Also, we include figures of both the high and low latitude under all 4 RCPs.

References not mentioned in manuscript Bates N. R., Astor Y. M., Church M. J., Currie K., Dore J. E., González-Dávila M., Lorenzoni L., Muller-Karger F., Olafsson J. and Santana-Casiano J. M. (2014) A timeseries view of changing ocean chemistry due to ocean uptake of anthropogenic CO2 and ocean acidification. Oceanography 27, 126–141. Bopp L., Resplandy L., Orr J. C., Doney S. C., Dunne J. P., Gehlen M., Halloran P., Heinze C., Ilyina T., Séférian R., Tjiputra J. and Vichi M. (2013) Multiple stressors of ocean ecosystems in the 21st century: projections with CMIP5 models. Biogeosciences 10, 6225–6245. Liss P. S. and Merlivat L. (1986) Air-sea gas exchange rates: introduction and synthesis. In The Role of Air-Sea Exchange in Geochemical Cycling (ed. P. Buat-Ménard). NATO ASI Series, vol 185. Springer Netherlands. pp. 113–127. Riebesell U., Körtzinger A. and Oschlies A. (2009) Sensitivities of marine carbon fluxes to ocean change. Proc. Natl. Acad. Sci. U. S. A. 106, 20602–9. Sarmiento J. L. and Gruber N. (2006) Ocean Biogeochemical Dynamics., Princeton University Press, Princeton, NJ. Sasse T. P., McNeil B. I., Matear R. J. and Lenton A. (2015) Quantifying the influence of CO2 seasonality on future aragonite undersaturation onset. Biogeosciences 12, 6017–6031.

**Thank you for these references. They are now included in the manuscript.

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