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Interactive Comment

Interactive comment on "The mechanisms of North Atlantic CO_2 uptake in a large Earth System Model ensemble" *by* P. R. Halloran et al.

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We thank the reviewers for their careful consideration of our work and welcome the fact that they agree with us that this manuscript sets up clearly and addresses an important question; "This is an important question because a decline in ocean uptake of carbon from the atmosphere, as has been hypothesized here and in other regions such as the Southern Ocean, could have important consequences for future changes in climate." (Reviewer 1) and "The introduction to this manuscript is well written and compelling. It is a nice review of the state of understanding with respect to mechanisms of North Atlantic carbon uptake" (Reviewer 2). Below we explain how we will address all of the reviewers' comments.

Reviewer 1:

Comment 1) I am still slightly confused about which "CO_2 flux" is some times being discussed. Perhaps the authors would consider adopting the natural/anthropogenic/contemporary terminology in key places such as p14552, line 9 "we will explore subpolar North Atlantic CO_2 uptake. . ." to be more specific?

Answer 1) Re-reading the manuscript we absolutely agree that following this suggestion will make things much clearer, we will certainly make these changes. Thank you for this suggestion.

Comment 2) I found the result of the absence of a role of the MOC variability in changes in carbon uptake intriguing (neither short- nor long-timescale filtering had any appreciable effect) especially as it has been implicated in recent observed subpolar North Atlantic CO_2 uptake reduction (Perez et al 2013) and is central to the hypothesis for the peak and decline in subpolar North Atlantic CO_2 fluxes as presented in Figure 9. Is this lack of sensitivity because the MOC in the ESM doesn't vary that much? Or is the importance reduced because only ~30% (for case 1, Table 2) actually goes from the low latitude to the high latitude box (even less for case 2 where mixing from below appears more important)? I think perhaps plotting the other box model inputs from the ESM, as in Figure 10 would be useful for the reader.

Answer 2) This is another very sensible suggestion, initially we decided not to present all of this data to keep the number of plots down, but we are very happy to add this to a revised version of the manuscript, allong with associated discussion.

Comment 3) on p14557 about numerous idealized simulations suggests running the emulator with different CO_2 power-law curves, but these experiments are not presented. Why not instead describe forcing the model with filtered input, which you actually do present?

Answer 3) Again a good suggestion. We certainly appreciate how this could be confusing, we will switch the wording to describe forcing the model with filtered input.

Comment 4) Technical corrections: 1.) Typo: p14552, line 17 "Revelle" not "Ravelle" 2.) Figure 1 is never actually referred to. 3.) The abbreviation ESPPE is used on p14577, line 7 but not actually defined until p14558, line 4. 4.) Typo: p14560, line 24 "a time series vary around zero". 5.) Parameter names in Table 1 do not match those in Table 2. For example there is no "flux_north" in Table 2 but there is a "piston(Sp)", which I suppose is "subpolar", but this is not clarified. Similarly "a" and "b" in Table 1 are defined as 7/14 and 2/14 respectively, but then "alpha" and "beta" in Table 2 are given different values. 5.) I found the scatter plots (Figures 5b and 11b) with pastel coloring and transparent dots with dark outlines to be very difficult to read, largely because the color on the legend is not reflected on the plot where all the points are overlying. I appreciate that there is a lot of data on here, but the transparency really doesn't help the reader in this respect. These plots also are missing units.

Answer 4) Thank you for highlighting these minor points. We will make all of these changes in a revised manuscript.

Reviewer 2:

Reviewer 2's major comments are stated and then expanded upon below.

Overarching comment) The conclusion that chemical change is to be the dominant mechanism of future CO_2 uptake change in the North Atlantic appears to be a re-presentation of the results from Volker et al. (2002), but without any additional evidence that the mechanism is occurring in ESMs or in nature. The possibility that this may be just the behavior of this box model needs to be addressed carefully by the authors.

Answer to overarching comment) We feel that the work we present here moves significantly beyond Volker et al. (2002), rather than simply delivering a 're-presentation of the results from Volker et al. (2002)'. Specifically, by applying the box model to a state-of-the-art ensemble of Earth System Model simulations we have (1) identified two distinct timescales and mechanisms of variability in high latitude N. Atlantic CO_2 uptake, (2) identified 'peak and decline' behaviour in ESMs (previously described in

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a theoretical study by Volker et al. (2002)), (3) applied the methodology presented in Volker et al. (2002) to an ESM ensemble and by doing so explained the drivers of differences between the Earth System Model ensemble members on both short and long timescales. By doing this, we show (for example) that by using single set of box model parameters we are able to explain the vast majority of the ESM's ocean carbon flux variability, including inter-model differences, based on the ESM's alkalinity, salinity, AMOC and temperature variability alone. We then take this further, identifying the relative importance of these factors in driving the ensemble's behaviour. Whilst we build on the findings of Volker et al. (2002), Volker et al. (2002) only examines the longer-timescale mechanism, does not identify these changes within in Earth System Models (let along a state-of-the-art perturbed parameter ensemble of such models), does not (and given their approach can not) touch on the uncertainty in the timing of this CO_2 uptake 'peak and decline', and does not explore the role of time-varying alkalinity, temperature or salinity under climate variability and/or climate change (i.e. they only explore changing atmospheric CO_2 and AMOC strength).

The major suggestions for change by reviewer 2 are:

Comment 1) Describe the box model in greater detail.

Answer 1) The reviewer expands upon this with the comment "Pg 14559, Box model equations need to be presented. It is not possible to understand Table 1 or to begin to understand the parameter-setting process otherwise". This is a very useful point. Whilst most of the equations are presented in figure 3, the air-sea flux equation is not presented (the equation in which the piston velocity parameter is used). We will add this equation. We feel that with this addition, the description already in the manuscript, and references to the paper that initially presented the box model, enough detail about the box-model will be given, but we are very happy to expand upon this further if required.

Comment 2) "prov[e] more convincingly that their method of emulation with this model is effective to capture the mechanisms.

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Answer 2) This comment is built on by comment 3, so these will be taken together below, but first briefly we want to emphasise that our primary reason for considering the box model to be useful is that using only a single set of parameters, the box-model is able to explain the vast majority of the model differences across the ESM ensemble (which exhibits diverse carbon flux responses), driven only with change in the ESM's bulk properties. It is very hard to imagine how this would occur unless the box model captured the mechanisms at play in the ESM simulations.

Comment 3) "They need also to more carefully describe and justify their analysis via comparison back to the mechanisms occurring in the ESMs (not just the CO_2 fluxes)."

Answer 3) We would initially like to emphasise that the box model was used precisely because it is extremely difficult to robustly identify the operation of detailed mechanisms within complex ESMs. This issue and our solution is appreciated by reviewer 1 "To investigate the causes of this behavior, due to the complex nature of the coupled system and the emergent character of the potential drivers of this CO_2 flux variability, Halloran et al use a box model to "emulate" the larger system. Use of simplifying frameworks such as this is becoming increasingly necessary as the ESMs become more intricate, allowing specific processes of interest (in this case temperature, salinity, alkalinity, Atlantic overturning strength and atmospheric CO_2) to be considered in isolation."

Reviewer 2 however helpfully provides some specific suggestions about what could be done:

Comment 3a) "They might use the latitudinal distribution of CO_2 flux as compared to the ESMs, as opposed to just the integrated flux, which can vary widely in space"

Answer 3a) The reason for using the box model is that it simplifies the system to a level that we can understand and interrogate. The box model therefore does not represent the spatial nature of the CO_2 flux, other than that going into the separate boxes. The "integrated flux" we use is integrated precisely to allow it to be comparable with the areas of the box model boxes (i.e. we've already integrated it at the smallest possible scale). No smaller-scale information is available from the box model for comparison. However to address this suggestion as far as is possible, we propose to present information from the low and high latitude boxes (see below).

Comment 3b) "Physical comparisons indicating that the model is reasonable would also be of use"

Answer 3b) The box model only represents physical circulation as prescribed from the Earth System Model simulations, so a physical comparison will unfortunately not tells us anything about the operation of the box model. We apologise if we have misunderstood this point, and are very happy to consider further comparison if suggested.

Comment 3c) "They need also to more carefully describe and justify their analysis via comparison back to the mechanisms occurring in the ESMs (not just the CO_2 fluxes)"

Answer 3c) These points relate to a number of more detailed comments. We present these comments below before responding:

Comment 3ci) "By using a single box model that replicates the behaviour of a wide range of Earth System Model formulations using only a single set of parameters (i.e. not retuning the simple model to emulate each different version of the more comprehensive model), one can be confident that the box model contains (and therefore that one has identified) the key processes important to the change of interest within those Earth System Model formulations." COMMENT: This statement is not adequately justified. It is not clear WHY a single box model emulator leads to confidence that all key processes are captured. If the CO_2 flux is an emergent behavior of a complex ESM, how can one be so sure that a box model will capture it? Similarly, points 2 and 3 here need justification. The rest of the analysis hinges on these statements being carefully justified.

Comment 3cii) "This gives us confidence that the box model represents all of the 1st order processes involved in the ESM simulation of North Atlantic CO_2 uptake, and provides us with a diagnostic tool to identify what drives CO_2 uptake variability in the ESPPE". The reviewer makes the same point about this statement as about the statement above. Answer 3ci-ii) All of these points can be summarised by saying that the reviewer is not convinced that the same mechanism is occurring in the box model as is occurring in the Earth System Model simulations, and consequently that if they are not, we are simply re-presenting the findings from the original box modelling work. Putting aside the fact that this paper does a lot more than look just at the mechanism driving 'peak and decline' behaviour examined theoretically in Volker et al. (2002), as discussed above, we do appreciate where the reviewer is coming from - it would be fantastic to be able to definitively demonstrate that this mechanism was operating in the ESM by looking just at the EMS. However, because the surface ocean CO_2 concentration is a response to the movement of DIC within the ocean, and to the air-sea CO_2 flux, and both the air-sea flux and DIC concentration influence each other, we need to think of clever ways to disentangle these. This is why we use the box model. However, we thank the reviewer for pushing us on this, because we think we can do more – see below. Our prime reason for believing that the mechanisms operating in the box model are the same as those occurring in the Earth System Model is that we have 27 ESM simulations with a wide range of behaviours, but, given the inputs (AMOC strength, T, S, total alkalinity and atmospheric CO_2) from the ESM, and without retuning for different simulations, the box model does a good job at replicating the ESMs air-sea flux. One can visualise the box model as being a single equation containing 5 variables (the input timeseries), and the ESM results being answers to those equations. We have only 5 unknown variables, so 5 correctly resolved answers would be enough to tell us that the equation was correct (e.g. 5 simultaneous equations), but instead we have 27 correctly resolved answers and therefore an extremely well constrained set of simultaneous equations. We will clarify this argument in a revised manuscript to justify the statements picked out by the reviewer. Further, we feel that we can present additional 'evidence that this is not just a behavior of this very coarse model', demonstrating that the ESM behaviour is consistent with the box model behaviour. To do this we propose to:

- Display low-latitude as well as high-latitude ESM air sea flux, as the box model understanding would predict, the low-latitude flux follows atm. CO_2, rather than the high-latitude peak and decline behaviour.
- Present figures showing:
  - High-pass filtered ESM high-latitude pCO_2 plotted against high-pass filtered ESM high-latitude pCO_2 calculated with Dissolve Inorganic Carbon (DIC), total alkalinity, temperature and salinity sequentially held constant. We hypothesise that the high-frequency variability occurs largely in response to temperature and alkalinity variability, not salinity (or atm. CO_2) variability, so we would expect to see that the relationship broke down when alkalinity and temperature were held constant, but not when salinity and DIC are held constant (as demonstrated in the box model and presented in figure 11 of the manuscript).
  - Low-pass filtered ESM high-latitude pCO_2 plotted against low-pass filtered ESM high-latitude pCO_2 calculated with DIC, total alkalinity, temperature and salinity sequentially held constant. Based on the box model results we would expect to see that temperature and alkalinity start to break the relationship down (but that relationship remain essentially linear), then with DIC held constant, that linearity to break down, and with salinity held constant, no significant change is seen in the relationship (as demonstrated in the box model and presented in figure 5 of the manuscript).

Comment 4) "The "peak and decline" behaviour seen in the low-frequency air–sea CO_2 flux signal is unlike the globally averaged signal (Fig. 2), which under a CO_2 emission

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scenario like RCP8.5 (in which atmospheric concentrations are increasing throughout the 21st Century) would be expected to (and indeed does: Fig. 2) continue increasing, but at a progressively reduced rate. As long as the atmospheric CO_2 concentration is increasing, assuming no dramatic changes in ocean circulation or biology, there will always be an air to sea CO_2 concentration gradient, and therefore air-to-sea CO_2 flux. The decrease in this flux through time reflects the changing speciation of carbon in seawater in response to the increase in carbonic acid concentrations – which partitions carbon progressively in the direction of CO_2, elevating surface ocean CO_2 concentrations, and reducing the air–sea CO_2 concentration gradient (Zeebe and Wolf- Gladrow, 2001; Revelle and Suess, 1957)." COMMENT: Is there evidence that this is happening in the ESMs? Otherwise, this is simply re-presenting the work of Volker et al. (2002) from the same box model. Is there any evidence that this is not just a behavior of this very coarse model?

Answer 4) What we are describing here is simply the well understood chemical buffering of anthropogenic CO_2 as quantified by the Revelle Factor, our description presents neither the findings of our work or those of Volker et al. (2002). Regarding the comment ‘Is there evidence that this is happening in the ESMs’, we have not presented any such evidence because this is the bread and butter of an Earth System Model, but we will of course add a comment and references to make this point clear. In citing Zeebe and Wolf- Gladrow (2001) and Revelle and Suess (1957) we intended to acknowledge that we were summarising previous work, but we will also make this fact clearer in a revised manuscript.

Reviewer 2’s minor comments:

Minor Comment 1) This box model is very simple. For example, the tropics extend 30S-48N. The authors should not be discussing "subtropical processes" in results. They should use the term "Tropical" to be consistent with their model setup and to clarify the very simple nature of their box model system to the reader.

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Answer 1) This is very good point, thank you, we will make this change.

Minor comment 2) Pg 14554, line 1 "Here we attempt to develop our understanding of the possible mechanisms controlling future subpolar North Atlantic CO_2 uptake within Earth System Models." This sentence is overly caveated, remove "attempt to" and "possible".

Answer 2) Thank you, we will change this.

Minor comment 3) Pg 14557, line 7 "ESPPE" Acronym has not been defined yet .

Answer 3) A good point, we will change this.

Minor comment 4) Methods section Pg 14559 Why don't the parameters listed in Table 1 correspond to the parameter values listed in Table 2?

Answer 4) We are sorry about this mistake, we will change the names to match between the tables.

Minor comment 5) Pg 14559 "Indeed the ability of the box model is relatively insensitive to the box model parameters (Fig. 4 and Table 1), suggesting that conclusions drawn on the drivers of the box model CO_2 flux are unlikely to be strongly dependent on the exact choice of box model parameters." It is not clear how this conclusion is to be reached when given Table 1 that lists parameter names and whether they vary or not; and Figure 4 which does not offer any indication of the parameter values in the box model for each timeseries of flux. If the authors mean Table 2, they need to indicate how "Ranking" relates to the panels in Figure 4 more clearly. Are there only 6 in this ranking, or is it 1000 as indicated in the text? If 1000, Table 2 and Figure 4 do actually not correspond.

Answer 5) Thank you for pointing this out, we should have referred to Table 2 not Table 1, we will change this, and more clearly explain the link to figure 4. We ran the box model with 1000 different parameter sets to allow us to identify the best parameter set, then present the top 3 parameter sets to highlight that they have quite different

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parameters but perform similarly well. We do this to demonstrate that the model is not particularly sensitive to the choice of parameter values.

Minor comments 6) Pg 14560, line 18 and on – This methodological discussion belongs in Methods. Pg 14560, line 21 "we high-pass" FILTER Pg 14560. Line 24 "in a time-series THAT varIES"

Answer 6) Thank you for highlighting these points. We will make these corrections.

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- Zeebe, R. and Wolf-Gladrow, D.: CO2 in Seawater: Equilibrium, Kinetics, Isotopes, Elsevier Oceanography Book Series, Amsterdam, 2001. 14554, 14561
- Revelle, R. and Suess, H. E.: Carbon dioxide exchange between atmosphere and ocean and the question of an increase of atmospheric CO2 during the past decades, Tellus, 9, 18–27, doi:10.1111/j.2153-3490.1957.tb01849.x, 1957.

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