Reply to RC1: 'Comment on egusphere-2023-1162', Anonymous Referee #1, 03 Nov 2023

Review: Implementing a coral reef CaCO3 production module in the iLOVECLIM climate model

Bouttes et al.

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GENERAL COMMENTS

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This paper presents a new model of coral reef carbonate production, designed to be coupled to earth system model frameworks and allowing for the simulation of long-term feedbacks between coral calcification and the larger carbon cycle. I am not aware of any other models of coral calcification that are designed to be run in a fully-coupled manner with ESMs, and as such agree that this model fills a previously unfilled niche in climate system modeling. While the impacts of changing carbonate system properties on biology is often simulated, the feedback effects of that biology on the oceanic and atmospheric carbon are much less often addressed, and may become increasingly important as research into novel fields like carbon dioxide removal increases. Overall, this was a clearly-written paper. While the results are somewhat preliminary, covering the initial setup and some short simulations to verify its ability to replicate current conditions, I think that even as a proof of concept this will be a valuable contribution to the field. There are a few places where the model details and the thought processes leading to those details could be expanded and clarified, which I include in my specific comments below.

We thank the reviewer for their comments on this paper and respond point by point below.

My one broader comment for this paper is in regard to how iCORAL is alternatively framed as either 1) a coral module specific to the iLOVECLIM framework vs 2) a module more generically available for use in ESMs as a whole. Several of the model implementation choices (e.g. subgridding the bathymetry to compensate for low horizontal resolution, adding temperature variability in the tropics) are clearly tailored to iLOVECLIM. Also, the source code is not written in a standalone, modular manner (it loads many of its variables through external fortran modules from the wider iLOVECLIM code*, many parameters are hard-coded, etc.); porting to a new ESM would be a non-trivial task. I suggest revising the text where relevant to clarify that the algorithms and concepts behind the model may be applicable more widely, but significant work would be necessary to port, test, and validate the model under a new ESM coupling.

We have modified the text to make it clearer that this is a module specifically adapted to the iLOVECLIM model, but which is largely reusable in other ESMs.

*Regarding the source code, the author comments (CEC1) imply that they made iLOVECLIM source code available to referees, but I did not receive any documents apart from those available for public review. Therefore, I have not been able to check whether the coral_mod_paper.f90 code (as linked in the Code availability section) compiles and runs in that context. However, the coral_mod module itself is clearly written -- well-commented, cleanly organized and formatted -- and seems mathematically sound.

We have made a repository with the iLOVECLIM code available to the reviewer. The link to this repository has been given to the topical editor who should be able to provide it. As discussed in CEC1 we hope to be able to have the entire model code publicly available in the future but this is not the case at present due to unresolved licensing issues for several parts of the code.

In addition, we have created an offline version of the iCORAL module which is avalaible on zenodo (doi: 10.5281/zenodo.10932293).

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SPECIFIC COMMENTS

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Section 2.1: The iLOVECLIM setup description could use a bit more detail. First, I wasn't entirely clear what the relationship between iLOVECLIM and LOVECLIM was. Based on the iLOVECLIM GMD special issue text (https://gmd.copernicus.org/articles/special_issue30.html), I gather that iLOVECLIM derived from LOVECLIM, minus ice (and possibly minus ocean carbon/bio?), then developed independently? A quick summary of this history in the paper would be useful to orient readers. Also, is the ocean carbon cycle model described in this paper a standard part of the iLOVECLIM configuration? I believe you're using the HAMOCC model for ocean biogeochemistry; can you specify which version? Are there any other parts of your iLOVECLIM configuration (other than the addition of iCORAL) that differs from other published implementations?

As noted by the reviewer, the only difference between iLOVECLIM and LOVECLIM in the version used is the carbon cycle module. The ice sheet is indeed different but it is not used in this version. We have made this clearer in the text:

“The ice sheet module (not used in this version) and the ocean carbon cycle module (used in this version) differ from LOVECLIM (Bouttes et al., 2015).”

The rest of the model, apart from the new coral module, is standard. The carbon cycle module used is the standard version of iLOVECLIM coming from HAMOCC3.1. We have made this clearer:

“The ocean carbon cycle, which is the standard carbon cycle module of iLOVECLIM and described in Bouttes et al. (2015), is based on a Nutrient-Phytoplankton-Zooplankton-Detritus (NPZD) model (HAMOCC3.1, Six and Maier-Reimer, 1996; Brovkin et al., 2002).”

Line 91-92: "Photosynthesis takes place in the euphotic zone in the upper 100 meters." Can you clarify whether this depth is prescribed or an emergent property of the model?

It is prescribed: “Photosynthesis is prescribed in the euphotic zone, set as the upper 100 meters.”

Line 95: Given that iCORAL is an extension of ReefHab, a quick description of that model would be useful. It sounds like ReefHab is a classic coral production model, and iCORAL uses the same basic equations but adapted to use ESM-derived input rather than just observational datasets (and with some expanded constraints like the bleaching addition.) Saying this explicitly would help orient readers unfamiliar with ReefHab.

Our model description includes the description of ReefHab, so that the reader does not have to be familiar with ReebHab. We have specified the parts of the code that are derived from ReefHab and those that are new developments and have added more information as follows:

"As in ReefHab, iCORAL first computes…"

"The nutrient and salinity thresholds utilised in the coral module are similar to those of ReefHab. The thermal limits however use the temperature in each grid cell at each depth unlike ReefHab which only uses sea surface temperatures."

"This equation expands on that used in ReefHab, which was similar but without $f_T(T)$, $f_O(\Omega)$ and $f_B(bleach)$."

Figure 1. I didn't find this figure particularly useful; the graphical elements don't provide any new information. I suggest either turning this into a more information-dense schematic (perhaps diagraming the input from coupled model components vs. offline datasets, and the additional pre-preprocessing steps applied before feeding that data into iCORAL, etc.) or eliminating it.

As suggested we have removed this figure.

Line 110: Perhaps clarify that the production depth is defined as the depth at which light is at the Imin level? Readers unfamiliar with the typical light attenuation equation may not immediately parse this detail.

As suggested we have added this clarification:

"The production depth is defined as the depth at which light is at the I_{min} level."

Line 115: Please provide more detail about how the K490 coefficients were chosen and calculated. What specific MODIS data was used to construct the binned data? Did you use the entire mission composite, and if so, through what dates? At what horizontal resolution (4km or 9km)? How were these data matched to the iLOVECLIM horizontal grid? Other satellite-based algorithms are available to calculate this attenuation coefficient near turbid water (for example, NOAA's version based on Tomlinson et al., 2019: https://doi.org/10.1080/2150704X.2018.1536301); were these considered?

We have used the entire mission composite at 9 km resolution, encompassing 15 years from 2002 to 2016. This has been regridded on the CLIO grid (3° by 3°). We have not considered other satellite-based algorithms. Although it would be interesting to study the differences obtained using other products, with the resolution of the iLOVECLIM model we expect this would be second order. Were the coral reef module implemented in a higher resolution ocean model, it would likely be interesting to evaluate the effect of different satellite-based algorithms.

We have added the information about the MODIS data in the text:

"The MODIS data are taken from the entire mission composite at 9km resolution, encompassing 15 years from 2002 to 2016, and has been regridded on the CLIO grid (3° by 3°)."

Also, I'm curious about the decision to use a satellite product to derive these coefficients, rather than deriving the attenuation directly from the model itself (i.e. as a function of chlorophyll + clear-water attenuation, with the latter tied to the simulated phytoplankton group). Given the intent to use this model for paleo-scale simulations, I'm concerned that this choice may decouple the model's simulated chl from the prescribed attenuation coefficients that are at least in part a function of satellite-era chl concentration. I recognize that using a prognostic base for this calculation comes with its own set of difficulties (requiring skillful phytoplankton simulation and some estimate of the CDOM/sediment/etc. distribution), but I think this choice warrants some additional discussion in the paper. You do later mention this as a caveat in the Discussion section 4.1 (Model caveats), but I'd like to see it addressed more fully, either in the Methods or in that caveats section.

We agree it would be better to compute it in the model, but with the low resolution of iLOVECLIM and the use of a simple NPZD, it is likely that the computation will bring biases and probably not improve the module overall for the present day. We however think that this should be tested, in particular in a higher resolution model. We have added discussion in the "model caveats" section with respect to this:

"As iLOVECLIM has low resolution and includes a simple NPZD model, computing the attenuation would likely add biases to the model results for the present-day climate. It should nonetheless be tested in future studies, and in particular if the module was included in a higher resolution ocean model and for use in different climates and land configurations. This will be tested in future work."

In addition, following the second reviewer's advice (Andy Ridgwell), we have tested the use of a homogeneous K_{490}, further details are given in the response to this comment.

On a related note, is the iLOVECLIM configuration capable of responding to long-term changes in wetting/drying? I.e. if sea level rises, can grid cells previously designated as land become inundated, or vice versa? If so, how does the iCORAL model handle parameterization of grid cells without satellite-era data to use? Given that an inundation-related scenario is presented as a driving theory for the potential importance of coral reefs in the carbon cycle, this is a key technical point in the potential usefulness of this model setup.

We have recently changed the iLOVECLIM code to modify the bathymetry during a simulation (Bouttes et al., Climate of the Past, 2023). When a grid cell previously considered as land becomes ocean, its ocean variables are initialised with the closest neighbours' values. This will be adapted for the coral reef module in future work, and we will also likely test computing the attenuation coefficient so that it can evolve in very different configurations and climates.

Line 155-160: The description of the 1-m vertical subgridding could be clearer; it took me a few readings to figure out what was being done. Using terminology like "vertical resolution" and "depth interval" was a little misleading to me, since initially I interpreted this as a sub-gridding of the water column, rather than a pairing of each 3-deg grid cell with its subgrid-scale bathymetry distribution.

We have added more details to make this clearer:

"Because the vertical resolution in the model is relatively coarse (increasing from 10 meters at the surface to 28 meters at 100 m depth), coral production is computed on a sublevel vertical grid every meter. This allows us to account for the fine vertical changes in light attenuation, surface availability and bathymetry. The other variables, taken from the ocean model, are homogenous in an ocean grid cell (temperature and aragonite saturation state). The carbonate production at 1-meter vertical resolution is then aggregated in each ocean cell."

In addition, following the comments from Reviewer2 we have added an additional figure with the vertical grids.

Line 243: Criv and Ariv are never explicitly defined; I assume those are the river input flux of DIC and Alkalinity, respectively?

Yes, we have added this in the text.

Line 259: "AR(p)" may be too jargon-y for a non-statistical-modeling audience; please provide a quick definition of this model type and what the model order (p) refers to. Also, you mention supplementary material here, but I could not locate any supplementary material associated with this manuscript.

We inadvertently forgot to include the relevant memo in the supplementary material when preparing the submission. That document (now uploaded in comment AC4: https://doi.org/10.5194/egusphere-2023-1162-AC4) will be included with the revised manuscript's Supplement. It provides references to the exact dataset used and also provides a detailed description of the processing, as well as the results for each of the six AR models fitted. We nevertheless provide a (very) short outline about the essence of AR(p) models. The text was rewritten as follows:

"We have thus generated additional temperature variability, based upon the analysis of the daily sea surface temperature anomalies in a tropical region with extended coral reef cover (19–16°S, 148–154°E). We fitted a series of autoregressive models of order p, denoted AR(p) models (p = 1, ..., 6) to the daily time series in each grid point in this area. An AR(p) model predicts the value of a variable at time t as a linear combination of the p previous values plus random noise. The fitting procedure provides the parameter constants for the linear combination (i.e., autocorrelation parameters – for details about the dataset used and the processing steps, please see the "*Autoregressive Model to Parametrise Temperature Variability*" memo in the Supplement). Here, we selected the AR(1) model, as the RMSEs of the higher order models were not statistically different. Accordingly, we generate an AR(1) variate with an auto-correlation parameter of 0.90 and a Gaussian distributed random noise with a standard deviation of 0.28 to add daily variability to the otherwise anomalously smooth temperature evolution in iLOVECLIM."

Line 263: Where did the values of 0.90 and 0.28 come from? Are they standard values or were they derived from the fitting process?

These values were derived during the fitting process. This is now more clearly stated in text (see reply to previous comment).

Figure 4: Anomaly plots would be helpful to allow quicker direct comparison between column 1 and 2.

As suggested we have added a column with the anomaly to facilitate the comparison between data and model.

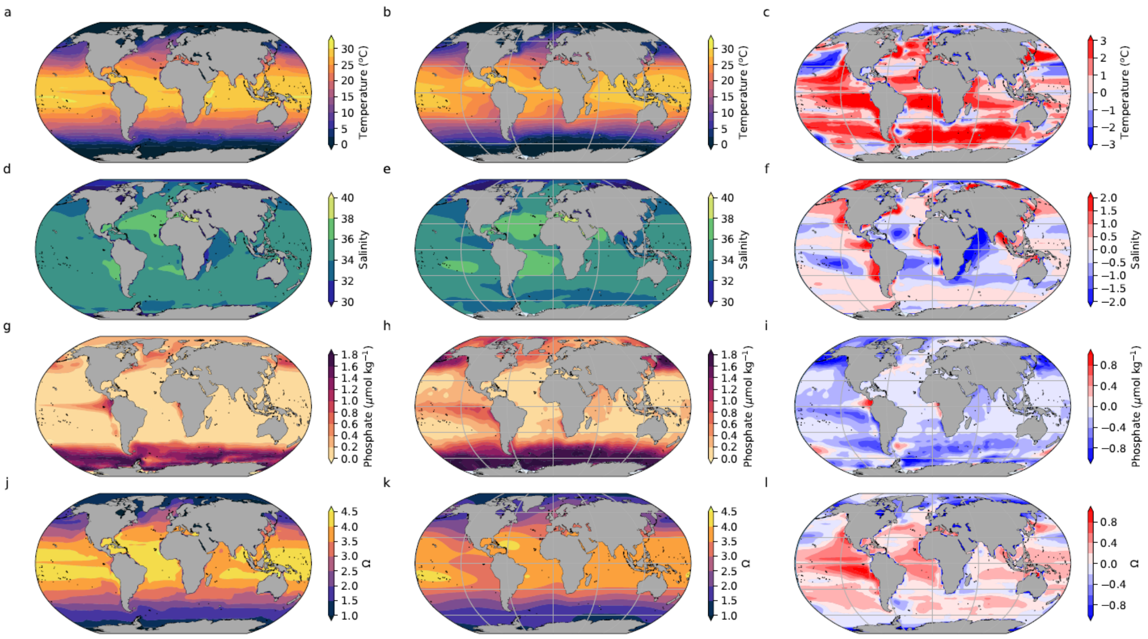

Figure 3. Model (left), observational data (middle) and model-data difference (right) surface maps of (a, b, c) temperature (°C), (d, e, f) salinity, (g, h, i) phosphate (µmol kg^{-1}) and (j, k, l) aragonite saturation state (Ω). The model outputs are averaged over 2000-2010. The data are from Locarnini et al. (2018), Zweng et al. (2018), Garcia et al. (2018) and Jiang (2015). The model outputs have been regridded on the data grid to compute the anomaly.

Lines 319-322: You mention that coral overproduction in the model might be due to lack of riverine nutrient input leading to lack of competition from macroalgae. While that is one possible source, lower-resolution models (even 1-deg) tend to underestimate coastal pelagic production due to poor resolution of shelf dynamics, which could alter the conditions in these regions. And it doesn't appear that this model simulates macroalgae at all, so even with improved riverine input that competitive pressure would be missing in the simulations. A bit more discussion of the possible drivers of oversimulation of coral could be useful here.

This model does not simulate macroalgae, but the indirect effect of abiotic conditions on the competitive advantage of macroalgae is partially accounted for by limiting coral habitability to grid cells where the phosphate concentration is below 0.2 µmol L^{-1}, as done previously in ReefHab.

Line 323: "might not be present in the observed data." Are you saying that these types of features might exist in the real world but not be captured by this particular dataset? Or that the model is simulating a type of isolated coral reef that probably doesn't exist in the real world?

Indeed, we are saying that such isolated small coral reefs might not be captured in observational products. But it is equally possible that other limiting factors may prevent coral reef development in these regions and that the model is over-predicting coral reefs there. We have made this clearer in the text:

"In addition, the model also simulated small isolated coral reefs with small areas (in purple) that might not be captured in the observed data. Alternatively, other limiting factors, not represented in iCORAL, might prevent coral reefs to develop in such areas."

Lines 432-434: This statement implies that perhaps the answer to my inundation question above is no, the model cannot handle wetting/drying. It's not clear to me how one would reparameterize coral growth parameters to be dependent on sea level without explicitly allowing for this; can you expand on this idea?

As previously discussed, we have modified the code to allow for bathymetry changes. However, this is constrained to periods when we have bathymetry reconstructions. As we intend to use this model for periods when such reconstructions do not exist, we also plan to test changing the bathymetry by simply modifying the sea level. We will then compare the results using both methods for the periods with bathymetry reconstructions (Last Deglaciation) to evaluate differences between the methods. This will be performed in future work beyond the scope of this paper, and is also discussed as a response to reviewer 2.

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TECHNICAL COMMENTS

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Equations: Equation numbers would be nice (possibly not a requirement for this journal?).

We have added equation numbers.

Line 266, 277, etc: Use of present perfect rather than past tense read a little strangely to me.

We have changed the text here to the past tense.

Figure 2: Is this just presence/absence data? Please clarify in the caption or add an appropriate legend.

Yes, it is just the presence of coral reefs, we have modified the caption:

"Coral location from UNEP-WCMC (2018) dataset. Brown cells indicate the presence of coral reefs in these cells. In white grid cells no coral reef has been detected."

Fig. 7a: I suggest adding the blue/purple highlights to this panel for consistency with Fig 7b, 7c, 8, and 9.

This has been added.