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

by Nathaelle Bouttes and co-authors / egusphere-2023-1162

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Coral reefs and associated controls on carbonate precipitation and burial and a potentially key, but to date overlooked, interactive carbon cycle component in Earth system models. To my knowledge the authors are right in that previously, only prescribed, rather than interactive, carbon and alkalinity sinks associated with shallow carbonate production have been implemented in (3D ocean-based) global models. As such, the current work represents a very useful modeling advance and highly appropriate for the timescale capability of the 'iLOVECLIM' climate (Earth system?) model. The paper is well-written and the model parameterizations generally well described and justified. I do have a number of minor comments (listed later). However, I do also have some questions about whether some of the assumptions made in the construction of the coral reef CaCO3 production module tie carbon and alkalinity feedback too closely to the modern marine environment and observations, preventing direct past (geological) applicability and potentially also somewhat limiting future capabilities of the new coupled model.

- **Generalizability/applicability of the model**

  A couple of assumptions are made in the coral reef CaCO3 production module have implications for its applicability to non-modern, and particularly paleo situations.

1. Diffusive attenuation coefficient

   As an initial note – I think URLs are not allowed these days(?) I did go to the page and try and retrieve the data, but either I was being incompetent, or the details given in the text are insufficient to retrieve the specific data in question. Ideally, the retrieved data would be placed somewhere with a DOI. I did check the DOI given for iCORAL, but it is only the FORTRAN file and does not include any boundary conditions. A DOI is in any case needed for the current version of iLOVECLIM, and that could then include relevant boundary conditions such as for the K490 field(?)

   So my question is: how important is the diffusive attenuation coefficient field? If a mean global value was applied uniformly, or representative open ocean value applied uniformly, how different does the projected distribution of reefs and global carbonate production become?

   Using the present-day satellite-retried spatial pattern potentially strongly pins the modelled distribution to 'now'. In the future with changing river flow, sediment loads, etc., the pattern may change, introducing a bias in future simulations. Much more problematic would be paleo applications, particularly when the land-sea mask is different and one can no long map present-day satellite retrievals onto past oceans. What are the authors plans for applying iCORAL-iLOVECLIM to the geological past and what are they planning to assume re. K490?

   My guess, given that the baseline model (Figure 5a) struggles with e.g. correctly projecting the absence of reefs in the NW Atlantic anyway, is that high sediment loads and the absence of hard substrates may be more important than getting light attenuation 'right'. Hence I wonder whether one could apply a mean or representative value globally, accept a small degradation in model fidelity, but remove this tie to the present-day?

2. Sea-floor bathymetry

I understand exactly why the authors have imposed a much higher resolution sea-floor bathymetry on the reef module. However, while for far future simulations one could simply take into account a mean sea-level change, things become (isostatically) more complicated if you go back to e.g. the last glacial maximum, and I am sure that (and the glacial-interglacial cycles in general) will be a scientific target of the authors.

If the paleo questions were restricted to the last glacial cycle, then relatively high resolution (10 minute) reconstructions are available, e.g. ICE-6G-C, GLAC-1D, as per PMIP4. What would the coral reef coverage and carbonate production look like if the 0 ka dataset from ICE-6G-C was used? If the authors plan deglacial (ICE-6G-C) or penultimate deglaciation (GLAC-1D) applications, if would be worth-while in the current paper calibrating and evaluating a slightly lower-resolution pale-enabled version of iCORAL/iLOVECLIME using e.g. ICE-6G-C bathymetry data.

Moreover, I cannot help but wonder what the results of simply using the iLOVECLIM ocean grid would be. Sure, reef locations would be very patchy, but as long as there was some sort of distribution of reef occurrence between Pacific, Indian, and Atlantic Ocean basins, I see no reason why the feedback between climate and carbonate removal should not be equally plausible (given a tuning resulting in a plausible initial global carbonate burial flux). This would make iCORAL-iLOVECLIM generically (and equally) applicable past global carbon cycle/climate questions.

As an aside – I did not see the bathymetric resolution stated. The authors state that they bathymetry comes from '*GEBCO 2014*' and cite '*GEBCO Compilation Group, 2022*'. Going to the GEBCO Compilation Group website, the current data-set is 2023 and at a resolution of 15 arc-seconds. No dataset further back than 2019 is available that I could see and so I am unsure what '*GEBCO 2014*' refers to. So a little more detail on the dataset used is needed.

3. Temperature variability in iLOVECLIM

   The 3rd assumption that ties iCORAL to the present-day is the imposition of enhanced sea-surface temperature variance in the tropics. Again, I can see the reasoning behind this, but some details are missing. In particular, the text says: '*for details, please see supplementary information*' but I could not find the SI anywhere.

   How big an effect is this? Is is a relatively small effect, or is it fundamental to getting the distribution of reefs and global carbonate sink anywhere near correct (a comparison would be helpful to see and I suspect informative to readers)? If the former – could not the bias imposed by adopting un-adjusted iLOVECLIME climatology be 'tuned away'? If the latter – what confidence do the authors have in future and past applications? I was under the impression that variance may change in the future. If only a little, then this may not matter. But what about the last glacial, or the Eemian? Would SST varience be expected to be more, less, or about the same? i.e. how safe is the assumption of observationally-derived SST variance in the past?

   Lastly, why only restrict the modification to the tropics? Why not globally? I guess one answer is that there are very few reefs outside of ±30° (Figure 2). However, rather more model-projected reef locations occur outside of the tropics (Figure 5a,b), and there will potentially be a very different (and spurious) bleaching response either side of the boundary.

I think in general and across all this points above – firstly, knowing the importance (or not) of making the various assumptions and imposing boundary conditions derived from modern observations would be informative and helpful. Secondly, the more that iCORAL can utilized internal iLOVECLIM fields and boundary conditions, the more generally applicable it will be to the future and particularly the geological past. If the authors do not want to make the choice between more 'realistic' and modern-tied vs. a poorer fidelity simulations of present-day reef distributions and global carbonate productions, then why not calibrated, evaluate, and present, two (or more) alternative setups and calibrations that could be used with iLOVECLIM applied to different questions? Overall, many of the choices and assumptions made in developing iCORAL seem to be orientated towards reproducing observations rather than enabling carbon-climate feedback and the stated aim of '*past and future* *coral-climate coupling*'.

– **Model fields and coral reef location evaluation**

I think missing is a sufficiently critical discussion of the model fields driving iCORAL (Figure 4). To me, the surface ocean saturation is rather lower in the tropics in iLOVECLIM vs. observations, while nutrients – which are assumed to prevent reef formation above a threshold – are higher. (Note that the depth of the 'surface' layer is not given in the figure and needs to be.) There are more localized mismatches in temperature and salinity which may or may not also play a role.

I am not at all concerned about the existence of model-data mismatches, which is par for the course, but rather that their potential implications are not sufficiently discussed. 3 parameters are tuned and I wonder to what degree they are countering errors in the simulated environmental fields. In all biogeochemical modeling of this sort, the risk is always that you correct for a deficiency by distorting something else, with the potential that e.g. the strength of carbonate-climate feedback could end up very different.

I think that at the very least, more discussion about how biases in certain simulated environmental parameters and regions might impact projected reef locations. Further evaluating iCORAL by feeding it observed fields (Figure 4a) in place of simulated fields (Figure 4b) would be interesting. Replacing fields one-by-one might be further instructive. One could do this comprehensively, potentially even retuning iCORAL for each combination of simulated or observed environmental fields. Or it might be sufficient in the paper simply to take iCORAL as it is (and its current tuning), and test swapping out the simulated for observed fields.

In addition, there may be better ways of comparing simulated and observed fields (Figure 5). For instance, for each observed reef location, one could pull out the simulated and observed values at those locations and cross-plot, perhaps color-coding for basin. Or color-code as per in Figure 6 and pulling out both 'real' and simulated locations. This would be a way to try and identify whether there are any specific model environmental biases which tended to generate false positives or negatives in reef location.

The more you can pull out specifically why – in terms of simulated environmental conditions or model parameters or structure – false positives or negatives occur, the more we'll learn.

- **Minor comments**[1]

– **18** – 'feedback' would be a better (much more commmon) word than '*retroaction*'.

– **24-25** – '*The model enables assessment of past and future coral-climate coupling on seasonal to millennial timescales*' – just noting the aim in the context of the present-day assumptions and my comments above.

– **52-53** – You don't have to add them, but just pointing out some empirical / machine learning papers: Couce et al., Future habitat suitability for coral reef ecosystems under global warming and ocean acidification, Global Change Biology DOI: 10.1111/gcb.12335 (2013); Couce et al., Tropical coral reef habitat in a geoengineered, high-CO2 world, GRL 40, doi:10.1002/grl.50340 (2013).

– **87-88** – Without digging out Millero (1995), I can't remember whether it included anything about solving the carbonate system or not. If not, missing are details of the numerics. Millero (1995) is also full of typos in various dissociation constant coefficients, so there must be a better reference for what the authors have adopted in terms of e.g. dissociation constants.

– **89** – '*nitrates*'? Do you mean: nitrate and ammonia. Or nitrate and ammonia and dissolved N2? Or just NO3, which would be singular 'nitrate'?

– **107-109** – Please add a brief justification for the limits. e.g. I think the northern Red Sea reaches 41 PSU around the Gulf of Suez (Google further tells me that there are 35 coral taxa in the Gulf of Suez). For phosphate – is this a real-world threshold, or chosen in light of the iLOVECLIM surface nutrient simulation? Looking at the WOA annual mean surface PO4, locations incorrectly simulated in the model in Figure 5a lie in surface waters with PO4 above 0.2 – here I am looking at the NW Atlantic and SE Pacific. Is plankton productivity (and turbidity) not the more proximal factor

[1]Suggested text changes indicated with → and suggested inserted words underlined. **x** represents line number.

influencing the presence/absence of corals (with nutrient availability influencing productivity)?

I noticed that only later down the text does it state: '*The nutrient and salinity thresholds utilized in the coral module are similar to those of ReefHab.*' It would still be helpful to know a little more on the justification, and how important these assumptions are in leading to e.g. Figure 5a.

- **Section 2.2.2** – A schematic of the gridding and grid relationships would be helpful. Maybe pick a single illustrative region and show of he grid relate, both horizontally and vertically. This could be combined with Figure 1 as a second panel (or thrown in SI).
- **150** – K_{arag} could be confused with K_{sp} (of aragonite) to a sloppy reader like myself. If it is a saturation value (reference value or threshold), why not Ω_{ref} or something?
- **164-165** – Text describing the relationship between grids, gradients, etc. would be much clearer with a figure (see earlier comment).
- **196** – It is a shame there there is not a DOI or anything less nebulous than a webpage ('*https://www.coralreefwatch.noaa.gov/ product/5km/methodology.php*').
- **Section 2.2.4** – I don't know why this doesn't come across clearer. It is correct (in terms of DIC and ALK relationships and flux balance), but a little round-about.
- **Section 2.2.5** – See comment on present-day observationally-tied temperature variance.
- **269-270** – Maybe make this clearer earlier in the text (see earlier comment).
- **452** Given iCORAL is embedded within iLOVECLIM, we need a DOI for the version of iLOVECLIM used (indeed, the code for iCORAL utilizes a number of iLOVECLIM modules and the iCORAL code is insufficient in isolation).