Reply to RC2

Review of:
Implementing a coral reef $CaCO_3$ production module in the iLOVECLIM climate model

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

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 $CaCO_3$ 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.

We thank Andy Ridgwell for his comments and suggestions. We have replied point by point below in blue.

Generalizability/applicability of the model

A couple of assumptions are made in the coral reef $CaCO_3$ 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(?)

We have added more information on the used MODIS product in the manuscript and have deposited the regridded file on zenodo where it is available with a doi: 10.5281/zenodo.10776565:
"K_{490} is the diffuse attenuation coefficient at 490 nm taken from the Level-3 binned MODIS-Aqua products in the OceanColor database (available at: http://oceancolor.gsfc.nasa.gov). The MODIS data are taken from the entire mission composite at 9km resolution, encompassing 15 years from 2002 to 2016, and have been regridded on the CLIO grid (3° by 3°).

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 author's 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?

This is indeed important for future applications in climates that strongly differ. However, using a homogeneous constant K_{490} value, or a geographical 2D K_{490} variable that is constant in time should have limited impact in different climate states. The main improvement would be to compute K_{490} using nutrient and productivity, but this is beyond the scope of this study. We plan to try this in the future, in particular with the coral reef module implemented in a higher resolved model.

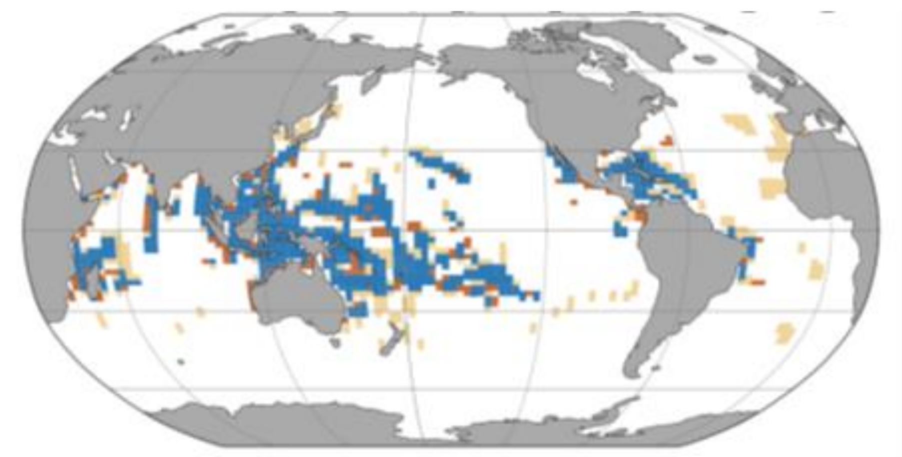
Nevertheless, we have still tested using a homogenous K_{490} value. We have chosen the mean K_{490} value between 30°S and 30°N: $K_{490} = 0.041$. We can compare this simulation with fixed homogenous K_{490} with the 'best case' simulations presented in the paper.

The simulated coral distribution is similar in both cases (see figure below) but the production value is increased with the fixed K_{490}, resulting in higher global production (1.02 Pg $CaCO_3$/year) compared to the 'best case' simulation (0.82 Pg $CaCO_3$/year).

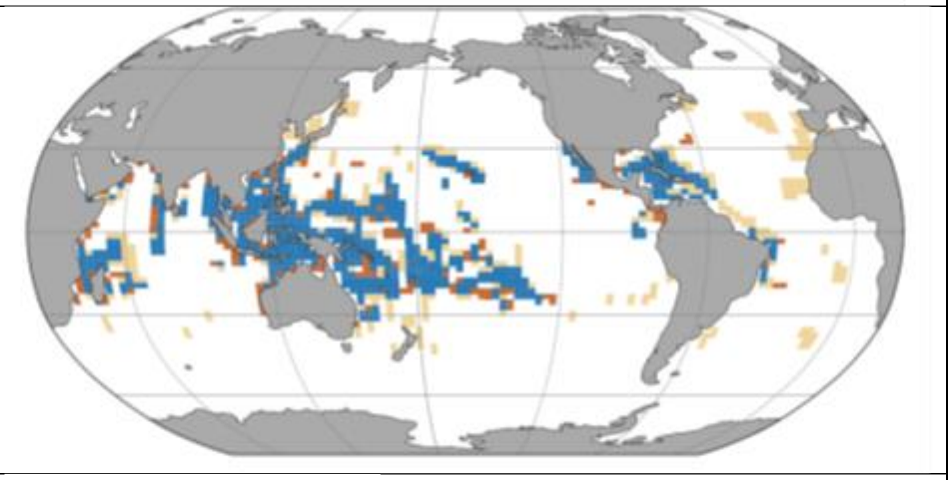
While using a scalar or a 2D matrix for K_{490} will not improve simulations for different climates, using a 2D field improves comparisons with local modern data. We thus prefer to keep a 2D K_{490} field as it permits a better representation of observations and allows us to better identify alternative reasons for possible mismatches.

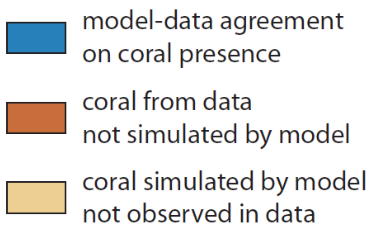
	Area (1e3 km3)	CaCO3 production (Pg CaCO3/year)
Reference simulation (best case)	390	0.82
With fixed K_{490}	490	1.02

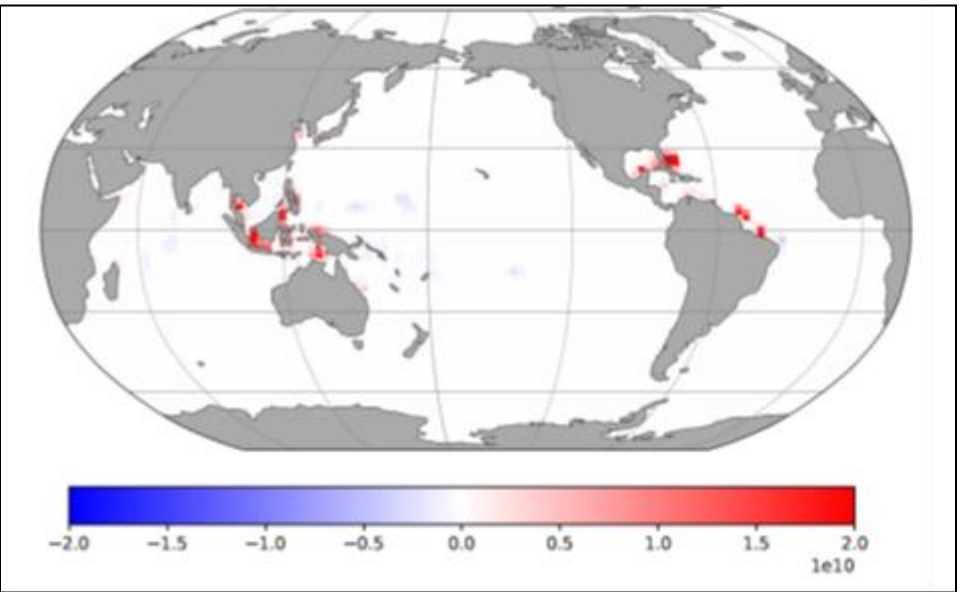
Coral location in the 'best case' simulation



Coral location with fixed K_{490}





Production anomaly (g/year) between the Kd fixed simulations and the ‘best case’ simulation	

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-6GC, 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. ICE6G-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.

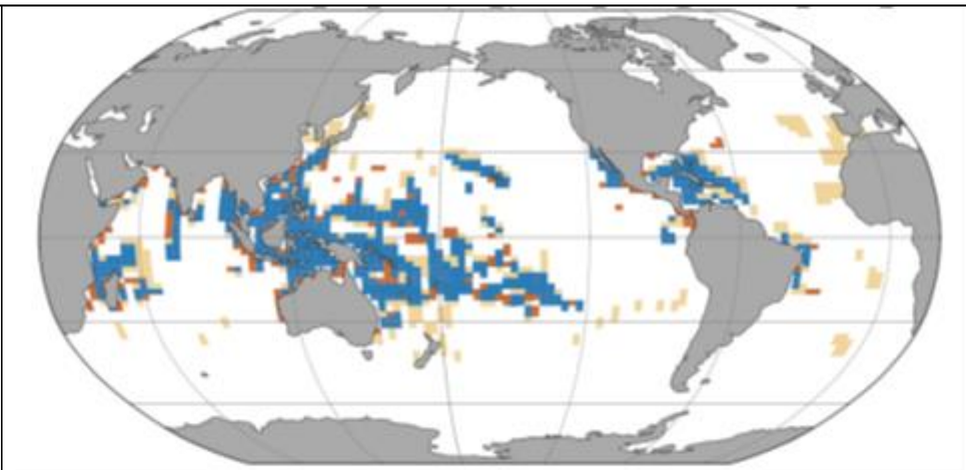
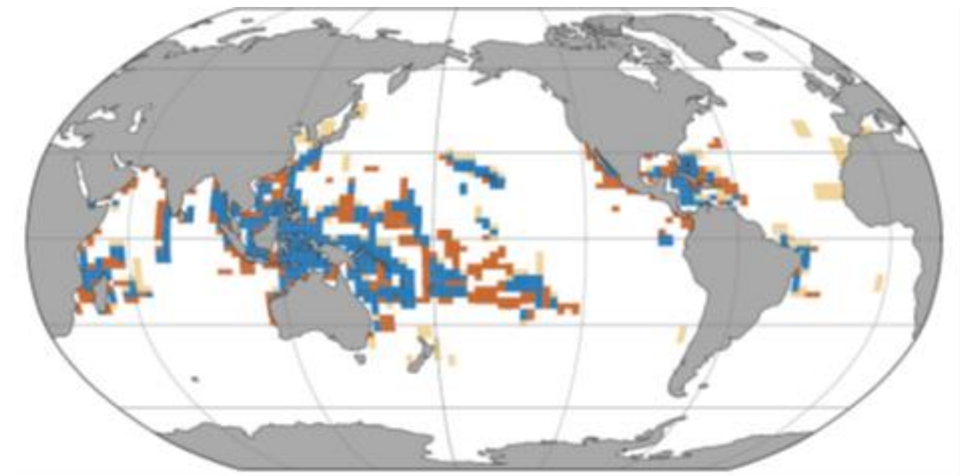
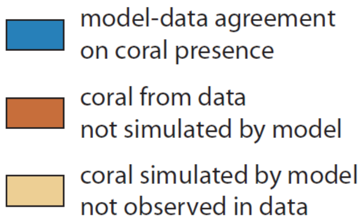
We used the GEBCO_2014 data released in 2015 and available here: https://www.gebco.net/data_and_products/historical_data_sets/. We have added the website reference in the manuscript to make it easier to find.

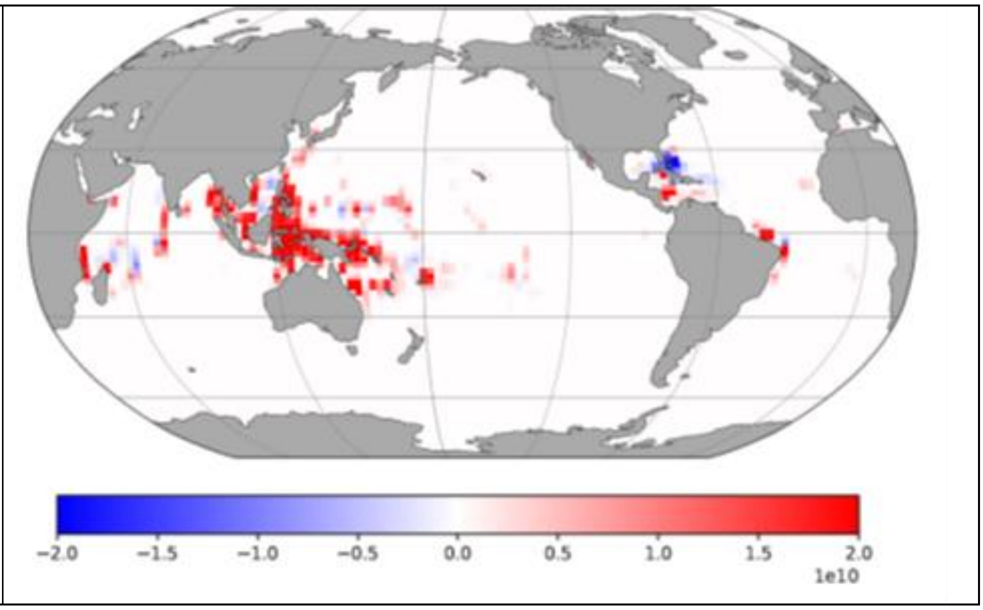
In addition, we have tested using the ICE-6G_C bathymetry (we expect the results would be similar with the other LGM reconstruction GLAC-1D) instead of GEBCO to compute the subgrid available area at each vertical level (see figures below). Using the ICE-6G_C bathymetry results in a larger area covered by coral reefs (908×10^3 km^2 compared to 390×10^3 km^2 with the GEBCO bathymetry). The global production is also higher with 2.26 Pg $CaCO_3$/year. Tuning the model to obtain results within the range of observed data should be doable. However, the location of coral reefs is degraded, with more grid cells where the model does not simulate coral reefs contrary to observations. Because the bathymetry resolution is degraded, we miss areas where coral reefs should develop.

Hence with ICE-6G_C the coral location is still first order accurate, but there are more mismatches with the observed data. As discussed by the reviewer, our anticipated work will indeed focus on the future Anthropocene and glacial interglacial changes. We intend to first use a passive submersion version, where only the eustatic sea level change is accounted for. We also envision testing the ICE-6G_C bathymetry, but as the reviewer points out, this only covers the last 26 kyr. We plan to use both methods for the last deglaciation (passive submersion with the GEBCO bathymetry and evolving ICE-6G_C bathymetry) and compare approaches.

We do not plan to consider deeper time periods. For such deep past periods the bathymetry changes more and it might be better to use the reconstructions, despite their low resolution. As we will confine our studies to glacial-interglacial periods, we think it is better to prioritize resolved bathymetry with passive submersion (and the ICE-6G_C or GLAC-1D for the last deglaciation).

	Area (1e3 km^3)	$CaCO_3$ production (Pg $CaCO_3$/yr)
Reference simulation ('best case')	390	0.82
With ICE-6G_C bathymetry	908	2.26

Coral location in the 'best case' simulation	
Coral location with ICE-6G_C bathymetry	
	
Production anomaly (g/year) between the simulations	



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.

Infeed, we forgot to add the SI. As also replied to reviewer 1 we have added a specific comment for this, see comment AC4 (https://doi.org/10.5194/egusphere-2023-1162-AC4).

How big an effect is this? Is it 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 iLOVECLIM 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 variance 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'.

We realize that this was not correctly formulated in the manuscript: on line 257, "in the tropics" is incorrect and must be deleted. Temperature variability was actually added everywhere, but the AR model *parameters* were calibrated on data from tropical region with extended coral reef cover (as correctly stated on lines 258-259. This paragraph was partly rewritten, also in response to a comment by Referee #1 (see reply to their comments)

For the pre-industrial steady-state distribution, temperature variability affects neither location nor productivity, as bleaching is never triggered. This will play a role when temperature will increase, as is the case in future simulations following the SSP scenarios. This will be the subject of future work and is beyond the point of this paper.

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.)
As suggested, we have added the depth of the model surface layer in the figures (5 m).

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.

The results of the coral module will indeed depend on the environmental fields simulated by the climate model. As in every coupling, the parameter tuning is strongly dependent on the input variables, here temperature, salinity, phosphates, light, aragonite saturation state. As suggested we have added more details on the model biases and their possible impact on coral reef development:

“While the coral reef model could be best calibrated and compared to observations using present-day environmental conditions, we aim for iLOVECLIM applications to climates far beyond the current state. Therefore, we use a dual approach. We test the model using best observational drivers but make sure that we could link these drivers to internal model variables or use simplified approaches applicable for wide range of climates. However, the coupled model application to other climates is beyond the scope of this paper.”

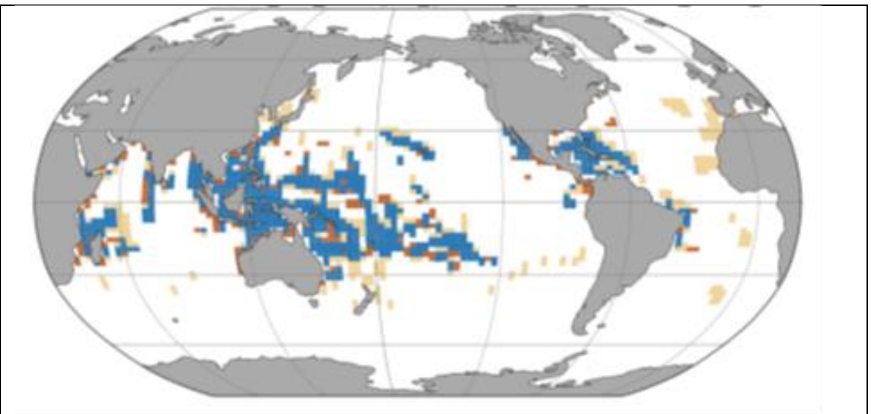
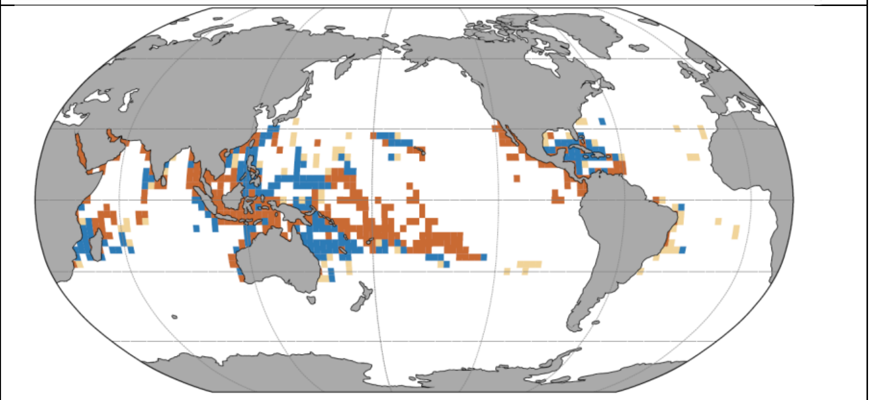
“The sea surface temperature in the model is generally slightly higher than in the observations, especially in the tropics where it can be 2°C higher than in the observations. The coral reef development is limited by a maximum temperature, which could be reached quicker than in observations due to the high temperature bias. The distribution of simulated nutrients exhibits greater biases. The concentrations simulated by the model are generally low compared to observations, especially in eastern equatorial upwelling regions. The resulting effect is the opposite as the one due to the temperature bias: the coral reef development will be less affected by phosphate changes as the maximum limit is further away due to the lower phosphate bias. The saturation state is also in generally good agreement with data, despite some differences locally. In particular it is slightly higher than the observed values in the tropics.”

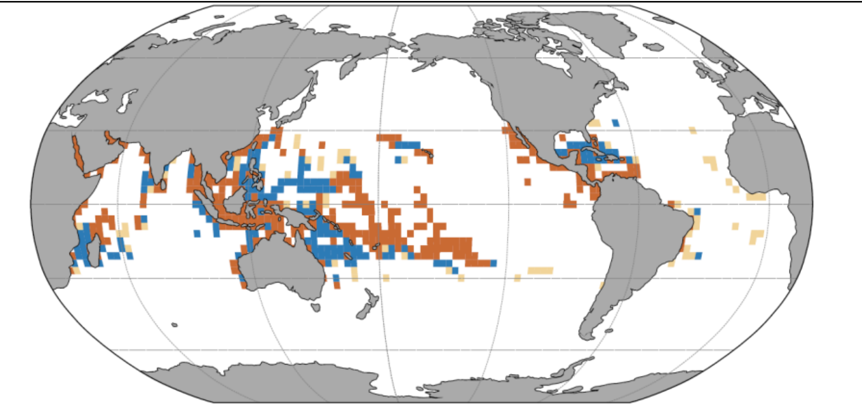
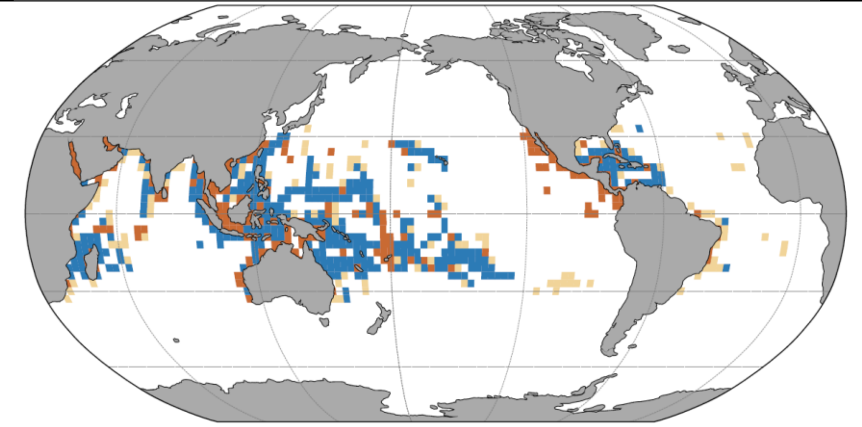
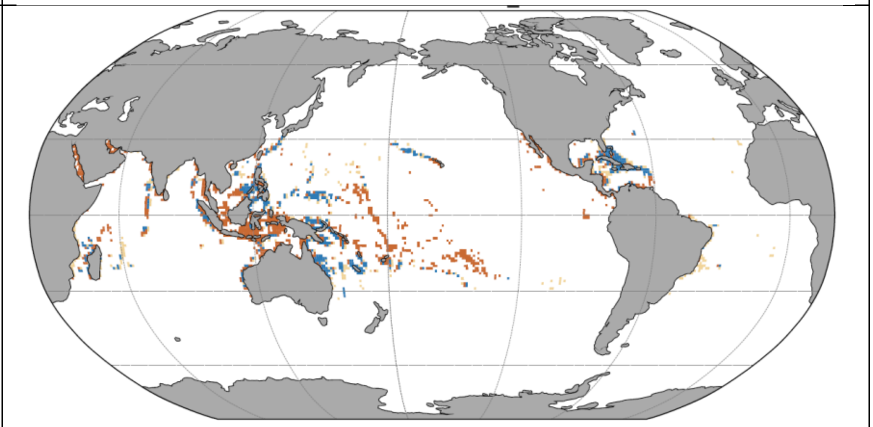
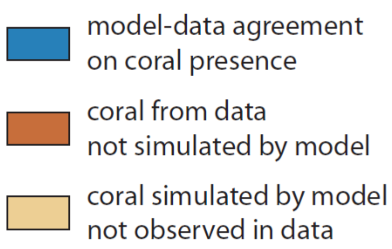
The tuning will necessarily compensate for any model bias. It also depends on the model resolution. To give an idea of how important this is we use an offline version of the coral module forced by observed annual fields of the input variables (as presented on Figure3). We have added the offline module in the data section of the manuscript with a doi: 10.5281/zenodo.10932293.

We have run a simulation of coral production with a 1degree resolution grid, and a set of simulations with a 3degree resolution grid. The horizontal iLOVECLIM resolution is also 3degrees. With the 3 degree grid we first use observed fields for the inputs, then use the SSTs from iLOVECLIM only, then the PO4 from iLOVECLIM only (the other inputs fields are still from observed data).

The simulated surface area is of the same magnitude with iLOVECLIM and the 3-degree grid offline model. The production is halved when using the offline version. Changing from 3-degrees to 1-degree horizontal resolution results in a decrease of both surface area total production. When swapping the observed temperature field by the iLOVECLIM temperature, the area and production are decreased, as the iLOVECLIM SSTs are higher, hence more limiting. It is the opposite when swapping the PO4 field for the iLOVECLIM one, as the PO4 in iLOVECLIM is lower than in the observations.

	iCORAL in iLOVECLIM	iCORAL offline			
		With 3deg observed fields	With 3deg observed fields + SST from iLOVECLIM	With 3deg observed fields + PO4 from iLOVECLIM	With 1deg observed fields
Area (1e3 km^2)	390	420	240	540	300
annual CaCO3 production (PgCaCO3/year)	0.82	0.40	0.21	0.51	0.28

iCORAL in iLOVECLIM	
iCORAL offline, 3 degrees with observed fields	

iCORAL offline, 3 degrees with SST from iLOVECLIM	
iCORAL offline, 3 degrees with PO4 from iLOVECLIM	
iCORAL offline, 1 degree with observed fields	
	

Minor comments[1]

1 Suggested text changes indicated with → and suggested inserted words <u>underlined</u>. **x** represents line number.

{Suggested text changes indicated with → and suggested inserted words underlined. x represents line number.}

- **18** – 'feedback' would be a better (much more common) word than 'retroaction'.
  Changed as recommended
- **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.
  We have added a reference to the model limitations:
  "The tuned model simulates the presence of coral reefs and regional-to-global carbonate production values in good agreement with data-based estimates, despite some limitations due to the imperfect simulation of climatic and biogeochemical fields driving the simulation of coral reef development. The model enables assessment of past and future coral-climate coupling on seasonal to millennial timescales, highlighting how climatic trends and variability may affect reef development and the resulting climate-carbon feedback."
- **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).
  This has been added as suggested.
- **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.
  The reference to Millero (1995) was removed (it does indeed not include anything about solving the carbonate system) and the text modified as follows:
  "Surface ocean pCO_2 is computed from temperature, salinity, DIC and ALK using the polynomial ACBW solver from SolveSAPHE (Munhoven, 2013), updated to revision 1.0.3 (Munhoven, 2020), with the pH_{SWS} configuration."
- **89** – '*nitrates*'? Do you mean: nitrate and ammonia. Or nitrate and ammonia and dissolved N2? Or just NO3, which would be singular 'nitrate'?
  This was a typo and has been corrected ('s' removed).
- **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 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.
  The limits are the same as in ReefHab, which allows us to compare our new results with their previous results. To have more information on the justification of the limits we refer to Kleypas et al. (1999) and references therein, which we have added in the manuscript:
  "The habitability is based on modern observations of coral presence and environmental conditions (Kleypas et al., 1999b and reference therein)."
  Kleypas, J. A., McManus, J. W.C. and Menez, L. A. B.: Environmental Limits to Coral Reef Development: Where Do We Draw the Line?, Am. Zool. 39 (1), 146-159, doi:10.1093/icb/39.1.46, 1999.
  In the future, the coral reef module could be improved by changing the habitability limits, but this is beyond the scope of this paper.
- **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).

  As suggested we have created a new figure to detail the vertical model grid cell and the subgridding. This has been added as supplementary figure 1.

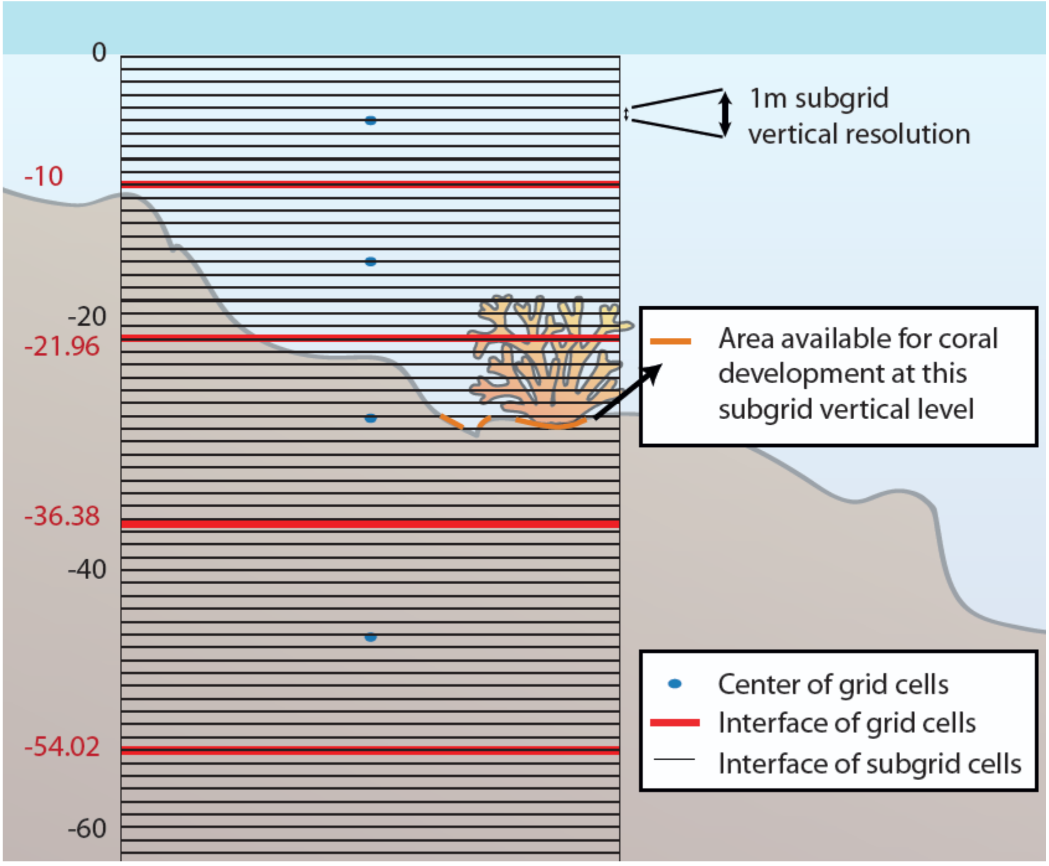


Figure S1. Schematic of the model vertical grid (m) with centre in blue dots and interface in red lines, and the 1m vertical subgrid (black lines) used for computing coral reef development.

- **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?
  K_{arag} has been replaced by K_{omega} in the revised manuscript to avoid the kind of confusion mentioned.
- **164-165** – Text describing the relationship between grids, gradients, etc. would be much clearer with a figure (see earlier comment).
  As suggested we have created a new figure (see response to related comments earlier)
- **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).
  This has been amended in response to the earlier comment (see above).
- **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).
  We do not have the rights to publish a version of iLOVECLIM, but in addition to the added coral module files that are available on zenodo, we also provide an offline version, also on zenodo (doi: 10.5281/zenodo.10932293) , of the coral reef module. This has been added in the manuscript.