Response to Reviewer Bradley Opdyke:

Reviewer comments (in italics): *"Reviewing this paper has been an interesting exercise to go through. The authors have written the paper well, however, what nags me about the whole process is the question 'How worthwhile and valid are the comparisons the authors are making?'. . ..because in many ways this is an apples and oranges comparison."*

Our response (in plain text): The skill of a model should always be assessed to establish what level of confidence can placed in its ability to capture what it intends to model. By attempting to do this for the first time for carbonate production models we have also highlighted that modern rates of carbonate production are seriously under-monitored and yet this is a critical parameter for understanding and predicting both reef-environment responses and the carbon cycle. As a consequence we argue for standardised and long-term measurements (e.g. lines 544-546); if this is done, the rigor of any model comparison test will naturally also improve. It is clearly worthwhile to establish the need for more and better data, and it is valid to use what is currently available as a demonstration.

"The TA method, for example, offers a snapshot of G values over a certain area on that particular day, whereas looking at G values derived from Porites offers up the possibility of integrating over a longer time scale."

This is an important point and one that we also raised in the Discussion (lines 463-474) when describing the stochastic nature of coral calcification and the inherent variability of data collected by this method.

"It doesn't approach the time scales I like looking at, but it goes in that direction. In many ways I don't think any of the scenarios really come close to touching what really is going on in coral reef communities around the globe. One of the strongest controls on the G value of a given reef goes back to Maxwell's concept of a juvenile to a mature to a senile reef. This explains the big differences between what a scientist will observe between a healthy reef in the Indo-Pacific and the Caribbean. Yes, it has a lot to do with accommodation space and circulation, and it is not really part of the model."

Timescale is fundamental. Our study focuses on models developed for the short timescales over which carbonate production has been quantified - the reality is that this is limited to the recent decades based on census and core data and very short 'snapshots'

(ΔTA methods). The longer, geological timescale of reef growth is quite another question. At first glance, it may seem logical that the response of a system across all timescales should be controlled by the same factor(s), and so involve the same component(s) to model. However, the established control of a process at one timescale does not extrapolate to mean it plays a role at the shorter interval. For example, consider atmospheric CO_2 concentrations; on geological timescales the silicate weathering rate is the dominant process controlling variability, but it would be foolish to call on this to model seasonal, annual, decadal, or century-scale variability (instead on these short timescales the biosphere is the key player)! In fact this subject is reviewed by Hatcher (1997) and Perry et al. (2008); both describing a hierarchy of processes and the scales/methods which are appropriate to them. We refer to this in the Discussion (lines 472-474).

It really is pretty cool that porites does so well when everything else is dying around them, but is it really a good basis for a global production model? I doubt it.

Massive *Porites* have been shown to be more robust compared to many coral species and capable of continued calcification under extreme environmental conditions (e.g. Fabricius et al., 2011), but they are clearly not doing "so well when everything else is dying around them" as seen in the recent records of dramatic calcification declines (e.g. Cooper et al., 2008) and described in lines 519-522. We do demonstrate that a global production model based on the *Porites* calcification-temperature response cannot provide a reliable estimate of global reefal carbonate production, but intriguingly it does have skill estimating calcification rates in other massive coral genera from the Caribbean (lines 405 and 451) hinting that the relative calcification response could be consistent between species.

"I can't help reading this sort of thing without wanting to put the story into a longer context. If you are talking about reef areas you really have to cite Steve Smith's 1978 paper in Nature. That is where the 600,000 square kilometer estimate of reefs originally came from, and Steve is no slouch. Milliman used it in his 1993 paper and I used it in my 'Return of the Coral Reef Hypothesis' paper. Now this reef area has a bit of a geologic component to it. . . in other words in includes shallow carbonate accumulations that are Holocene in age but no longer actively producing carbonate. In that context the ReefHab number of close to 200,000 square kilometers is probably more appropriate to approximate areas with higher G values today."

We cite Smith (1978) in the Discussion (line 352) regarding reef area and we do discuss in detail how this estimate affects both empirical and model estimates of reefal carbonate budgets (lines 349-377). We have, however, added Opdyke and Walker (1992) to this part of the Discussion. We also recognise that Opdyke and Walker's (1992)'s global budget estimate should be included in the Introduction with the other examples (lines 67 and 68).

"It is interesting that the Silverman estimate of 1.1 Pg is close to the 1.4 Pg that I included in the range of possible neritic accumulation. But we have remember the 1.1 Pg value was measured under high modern pCO_2 conditions and saturation states were higher not that long ago."

While this is correct, the poor fit of the Silverman$^{SST\Omega}$ model to observed reef scale calcification rates means that it may be achieving an estimate of global reef carbonate production (G_{global}) close to the Opdyke and Walker (1992) values by coincidence. The ReefHab^{Irr} estimate is actually a better fit (getting the same 1.4 Pg yr^{-1} value as Opdyke and Walker; 1992), although the same caveat applies here as for Silverman$^{SST\Omega}$. We have expanded on the ability to fit global verses local values in the Discussion (lines 342-346).

Manuscript changes in response to Reviewer #1's comments:

Reviewer #1's comments concerned two main points: (1) that insufficient physiological literature had been taken in to account in the discussion and model construction; and (2) that the Lough^{SST} model (Lough and Barnes, 2000; Lough, 2008) is not suitable for time-line (future) simulations as it is derived from data across an environmental gradient. We are in full agreement with both statements.

To address the first comment, we have now included a detailed paragraph in the Discussion (lines 485-515) on the physiological mechanisms of calcification in relation to the evaluated models. In addition, further reference to physiological literature regarding the temperature response of calcification has now been added to the Discussion where we describe why the Lough^{SST} model in isolation is not suitable for future (time-line) simulations (lines 516-536). This description draws on the evidence for reduced calcification rates observed in the last few decades coinciding with increasing temperatures.

We have also added the model description (section 2.1, lines 138-140) to emphasis that this study evaluates published models of reef calcification. Plus the addition of a sentence highlighting the different value of E_k used in Kleypas$^{Irr\Omega}$ then in ReefHab^{Irr} (lines 179-181).

References

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