

Interactive comment on “Evaluation of coral reef carbonate production models at a global scale” by N. S. Jones et al.

Anonymous Referee #1

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I like the manuscript, and I think it deserves to be published.

Nonetheless, firstly, there is some physiological literature that must be taken into account in the discussion and, probably, in the model construction (e.g., Marshall & Clode. 2004. *Coral Reefs*, 23: 218-224; Colombo-Pallota. 2010. *Coral Reefs*, 29: 899-907).

Secondly, I'm concerned about the so-called Lough SST model. Basically, the authors are using the same line of reasoning of McNeil et al. (2004. *Geophys Res Lett*, 31: L22309), which was widely criticized by Kleypas et al. (2005. *Geophys Res Lett*, 32: L08601). Loug & Barnes (2000. *J Exp Mar Biol Ecol*, 245: 225-243) are Porites growth data in an environmental gradient, not in a time-line gradient. For this, take a look to Worum et al. (2007. *Limnol Oceanogr*, 52: 2317-2323), Cooper et al. (2008. *Global Change Biology*, 14: 529-538), Tanzil et al. (2009. *Coral Reefs*, 28:519–528), and

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Carricart-Ganivet et al. (2013. *PLoS ONE*, 7: e32859), between others.

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