Biogeosciences Discussions
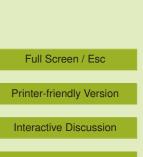
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Interactive comment on "Controls on the spatial distribution of oceanic delta;¹³C<sub>DIC</sup>" *by* P. B. Holden et al.

Anonymous Referee #2

This paper takes an interesting approach to mapping out the controls on the ocean carbon cycle. Rather than trying to produce a well-tuned model, and then test its sensitivity to environmental change, the sensitivity of the model to its tunable parameters is varied across a huge range that is simultaneously meant to represent environmental sensitivities. This approach has the drawback that many of the solutions are likely to be unreasonable, but the strength that it is less reliant on arbitrary tuning. (I say less, rather than completely unreliant, since both the choice of parameters to vary and their ranges remain arbitrary.)

I think this is a useful contribution, and worthy of publication. My comments are aimed

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at improving the paper.

General comments

- I find it a bit confusing that the paper initially states its motivation to study the glacial-interglacial change, but the conclusions are not presented in a way that bears on the glacial-interglacial change. I would suggest that the authors focus this paper just on the pre-industrial and industrial time periods, even if their ultimate motivation is to explore the glacial in future.

- The list of 'Parameters' in 3.1 includes items that are not parameters under investigation. For example, CO2 solubility, Carbonate compensation, and Sea ice cover were not directly changed; they change as a result of other parameter changes. I find this confusing. I think it would be better to discuss only the actual parameter changes here, with a proper description of the parameters. For example, what exactly is the air-sea gas exchange parameter? Furthermore, the results are strongly dependent on the chosen range. Yet, little or no explanation is given for the choice of range for each parameter. I think the ensemble would be much more useful if both the high and low values were justified, and an attempt made to select the ranges for different parameters based on the same logic.

- I think it would be best to reserve use of the word 'uncertainty' for parameters that are uncertain in the modern world. Many of the ranges here are meant to span Quaternary climate variability, and as such, are significantly larger than modern uncertainties. Maybe it would be better to use a term like 'sensitivity range', rather than 'uncertainty', when discussing these.

- Finally, I feel that the paper comes very close to offering some valuable mechanistic insight on carbon uptake, by identifying the contrast between d13C EOF 1 and d13C EOF2 / DIC EOF 1. But it seems to stop short of carrying this on to provide deeper insight, in terms of what this means for physical mechanisms. For example, do the temperature-dependence of fractionation and the long equilibration timescale for iso-

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topes dominate d13C, whereas the thermocline ventilation rates dominate the transfer of DIC into the interior?

Other specific comments - p 11846, line 4: I am personally not a fan of the moniker EMIC, since it seems imprecise, and is actually applied to quite a heterogenous mix of models. I would prefer to see this phrased as something like 'an Earth system model of low resolution with a non-dynamical energy balance atmosphere'. However, this is just an opinion.

- p 11846, starting line 18: "The motivation. . ." is a run-on sentence. Please rephrase.

- p 11854, lines 1-3: The importance of the input parameter range is key, as nicely explained here, and should be highlighted elsewhere in the paper, including the introduction and conclusion. This has a large bearing on the interpretation of the emulator coefficients.

- In my experience, the terms PC and EOF are often conflated, which can lead to confusion. Perhaps the best way to describe their use here would be to include an equation of the SVD, explicitly stating which matrix/elements you refer to with each term.

- The Ridgwell and Death, 2012 reference is missing.

- It would be great to plot the d13C observations directly on the model output figures, as done by Tagliabue and Bopp (2008).

- p11866, line 5: This error seems too large - should it be +/- 0.014?

- p11868, line 6: 'unsurprisingly'? Is it unsurprising that air-sea gas exchange does not contribute to uncertainy in the ocean carbon sink?

- Many of the figure labels are too small, and not legible. It would also be nice to include the emulator coefficient names on figures 6 and 7, perhaps as a labeled axis beneath the colour scale? At least, it would be helpful to draw vertical lines on the coefficient

plots in order to separate the atmosphere, ocean, land and marine biogeochemistry parameters.

Interactive comment on Biogeosciences Discuss., 9, 11843, 2012.