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Earth System Dynamics Discussions

Interactive comment on "Revisiting ocean carbon sequestration by direct injection: A global carbon budget perspective" *by* F. Reith et al.

Anonymous Referee #1

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The authors investigate the impacts of ocean carbon injection (and of direct carbon capture and storage with no leakage) on the carbon inventories of the atmosphere, the ocean, and the land biosphere using the UVIC model. This is a solid study that should be published after taking into account the following comments:

1) The authors evaluate the impact of climate change on the fraction retained by comparing their complete mitigation (CM) simulations without emission forcing after 2020 and the RCP8.5 simulations with continued emissions (WE) (Line 181). They conclude (line 182) that larger climate change in RCP8.5 leads to a higher fraction of injected carbon retained in the ocean (FR).

I doubt that the difference between the CM and RCP85-WE simulations is indicative of climate change. I suspect that the higher fraction retained in the CM compared to the

WE simulation is largely the result of differences in the Revelle factor/carbonate chemistry. The higher carbon emissions under RCP8.5 lead to a higher atmospheric and oceanic CO2 and a higher Revelle factor. In turn a smaller fraction of anthropogenic carbon ends up in the ocean in the RCP8.5 case compared to the zero emission CM case. As in the long run, both simulations with and without ocean injection tend to achieve the same carbon partitioning between the ocean and the atmosphere (when neglecting ocean-sediment and weathering fluxes as done here) this mechanisms also affects the fraction retained. More injected carbon remains in the ocean for the low than for the high emission case.

A proper evaluation of the climatic impacts would require an RCP8.5 simulations with carbon emissions, but with radiative forcing from anthropogenic agents set to zero. Then, climate would remain at equilibrium while atm. CO2 and carbonate chemistry would still change.

(Alternatively, I may misunderstand the experimental protocol. This would then require a clarification in the method section.)

2) A caveat of this study is that ocean sediments and the effect of calcium carbonate dissolution (also known as calcium carbonate compensation) is not considered. This caveat should be addressed in the introduction and conclusion section. This mechanisms could be relatively important as ocean carbon injection may bring the excess carbon close to deposits of calcium carbonate and thus would permit carbonate dissolution to occur on much faster time scale than for emissions into the atmosphere.

3) The marker tracer used to compute the fraction retained should be explained in detail in the method section. As the fraction retained (FR) is a central metric in this study, it is not enough to refer to the literature.

Further comments

Line 44: "reach a chemical equilibrium (mainly an equilibrium between the ocean and

atmospheric carbon reservoirs)." This statement is not completely true as carbonate compensation and weathering feedbacks are important for time scales longer than $\sim$5000 years.

L 93: What about non-CO2 forcings?

Line 127: could you please say a few more words about the diagnostic marker tracer. How is carbonate chemistry and air-sea and air-land flux computed for this tracer?

Line 183: I doubt that the FR remains higher with than without climate change. I also doubt that this statement applies to all time scales (longer than the simulations).

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