Earth Syst. Dynam. Discuss.,
doi:10.5194/esd-2016-20-RC2, 2016

Earth System Dynamics Discussions

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

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Received and published: 30 May 2016

The manuscript investigates the effect of direct oceanic water column CO2 injection on the redistribution of carbon under a high emission scenario following RCP8.5 its extension to 2300/2500 according to Meinshausen et al. (2011) and keeping emissions at a constant value until year 3020. The authors employ an Earth system model of intermediate complexity (UVic EMIC) and a standard protocol for prescribing the CO2 injections. The study goes beyond the state-of-the-art by confronting not only an ocean biogeochemical model (with atmospheric reservoir) but a coupled Earth system model including also a terrestrial biosphere component (and a simple atmosphere representation) with ocean CO2 injections. The model runs are carried out in a technically correct way as far as one can judge from the description. If I am not mistaken, the main result of the study is the following: CO2 injection does not change the control run result for

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Discussion paper

land carbon storage in a significant way for the forcing and injection protocol as applied. The last sentence in the conclusions (l. 348-350) maybe true in general but is hardly backed up by this particular study. The CMIP5 inter-model spread in land carbon storage change is much larger at year 2100 (Jones et al., J.Clim., 2013) than the amount discussed here as caused by ocean injection of CO2. The manuscript confirms previous studies: A part of the injected CO2 will outgas at a certain point in time, leading to less than 100% efficiency of the injection with respect to keeping anthropogenic excess CO2 isolated from the atmosphere.

The authors correctly motivate their study with the current discussion on feasible mitigation targets to limit radiative warming to 2deg or 1.5deg C with respect to the pre-industrial. Respective emission scenarios would require at some point negative emissions. Why did the authors choose the business as usual strong warming scenario for their study? The amount of injected CO2 is small in view if the CO2 emissions in the RCP8.5 emission driven case. A more modest emission scenario would have been may be more appropriate in view of the amount of injected CO2 as used here.

The terrestrial carbon cycle model used here is originally based on TRIFFID. This model has at times shown a more sensitive behaviour to forcing than other models (see e.g. Friedlingstein et al., J. Clim., 2006/C4MIP, where both the Hadley Centre model and the UVIC model show significant outgassing after 2050). Would results with other terrestrial modules potentially show an even smaller deviation from the control run for the injection scenarios? The spread among different terrestrial carbon cycle modules concerning CO2 uptake in Earth system models is large, also in view of the effect of nitrogen cycle perturbations. The fluxes as presented in the paper should have been discussed in view of also these uncertainties. The authors correctly mention the as yet difficult to quantify CO2 fertilisation effect on land as large source of uncertainty.

The authors say that “direct injection of CO2 is presently in conflict with . . .” international protocols/conventions. This is correct but may also be an understatement. Direct CO2 injection has been abandoned as a mitigation option because its environmental risks are potentially large (see WBGU report, 2006, for a summary of related risks, http://www.wbgu.de/en/special-reports/sr-2006-the-future-oceans/). The injection protocol of OCMIP/GOSAC as applied in the study does not account for the potential of fast rising bubbles after CO2 injection (e.g., Bigalke et al., Environ. Sci. Technol., 2008). Deeper ocean environments are sensitive to small pH variations (e.g., Gehlen et al., Biogeosciences, 2014). These aspects should be discussed in order to avoid misunderstandings by non-expert readers.

The authors discuss a transient Southern Ocean fluctuation of their model on one hand, and the lack of realistic internal variability in the EMIC employed on the other hand. The strength of EMICs is their low demand for computational resources. They would be suited to carry out ensemble simulations with large numbers of members. This advantage could have been used to assess the robustness of the results. Maybe these would have become more significant or different for slightly perturbed initial conditions in an ensemble simulation?

Deep injection of CO2 could potentially accelerate neutralising fossil fuel CO2 by dissolution of CaCO3 from the sea-floor. Usually, on a 1000-years-time scale, the negative carbon cycle feedback through CaCO3 sediment dissolution is not important but rather on a several 10,000 year time scale (Archer, J.Geophys.Res., 2005). Water column injection potentially could change this, though injection in the deep Pacific, where injection would be most effective, CaCO3 sediment is scarce. Nevertheless this aspect would warrant discussion. Is the (presumably small) CaCO3 effect larger than the land biosphere effect as discussed here?

A successful revision of this honest study would hopefully make the results more significant in quantitative terms.

Small details:

Abstract, l. 17: An ... feature are effects (conflict singular/plural)

I find the introduction of the acronyms CM, WE, DAC, and GIC not helpful. One can spell the terms out (maybe in italics).

l. 136: misplaced comma

l. 183: comma after simulations required

Figure 1: The small rectangles with injection sites are difficult to identify.

Figure S2 should be placed in the main section. It shows the small effects.

I do not want to stay anonymous.

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