Responses in Italic

First of all, the authors thank Prof. Christoph Heinze very much for his thoughtful and constructive comments and advice.

The manuscript investigates the effect of direct oceanic water column CO_2 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 CO_2 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 CO_2 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: CO_2 injection does not change the control run result for 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 CO_2. The manuscript confirms previous studies: A part of the injected CO_2 will outgas at a certain point in time, leading to less than 100% efficiency of the injection with respect to keeping anthropogenic excess CO_2 isolated from the atmosphere.

Yes, we agree with the reviewer that the universality of the last concluding sentence is not completely backed up by our study. This would have required the comparison of the injection simulations with and without the land module. We will rephrase the last sentence of the conclusion, accordingly.

The comment related to the CMIP5 inter-model spread in land carbon storage change is discussed below.

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 CO_2 is small in view if the CO_2 emissions in the RCP8.5 emission driven case. A more modest emission scenario would have been maybe more appropriate in view of the amount of injected CO_2 as used here.

Our choice of the experimental design is motivated by the current trend of CO_2 emissions, which continues to follow largely the trajectory of the RCP 8.5 emission scenario [Peters et al., 2013] and also by our choice of the objective of our study, i.e., to investigate the response of the global carbon cycle during and after the direct CO_2 injections, considering a strong perturbation of the climate system. This has helped to investigate the effect of climate-induced changes on the fraction retained by comparing our 'Complete Mitigation runs' with the 'With Emissions simulations' (section 3.3). The justification of the small injection rate is that we wanted to compare and validate the fraction retained as well as the changes in seawater chemistry to the results of Orr et al. [2001; Orr, 2004].

The terrestrial carbon cycle model used here is originally based on TRIFFID. This model has at times shown a more sensitive behavior 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 CO_2 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 CO_2 fertilization effect on land as large source of uncertainty.

Yes, the authors agree that it is necessary to address and discuss the uncertainties related to the response of the terrestrial carbon cycle model to the direct CO_2 injections.

The process of CO_2 fertilization, which is here one of the dominant terrestrial carbon cycle feedbacks after CO_2 is injected, has direct relevance for the future trajectory of atmospheric CO_2 [IPCC, 2013] and thus for our targeted atmospheric carbon reduction of 70 GtC by the year 2120. The future strength of CO_2 fertilization in response to continued carbon emissions as in the 'With Emissions runs' is subject to the choice of the CO_2-fertilization parameterization and hence uncertain. In the new manuscript version we will provide a quantification of this uncertainty concerning the targeted atmospheric carbon reduction through direct CO_2 injections based on additional model runs, following the approach of Matthews [2007]. For these runs, we scaled the CO_2 sensitivity of the terrestrial photosynthesis model and have performed simulations of the RCP 8.5 control run, I-800 and I-3000, in which we have varied the strength of the CO_2 fertilization effect by increasing and decreasing it by 50% (CO_2 fert. =+50% / -50%) relative to the default model. We add a description of these simulations to the experimental design section.

In the results and discussion section (section 3.1), we describe carbon budgets of the perturbed control runs (RCP 8.5 CO_2 fert.=+50% and RCP 8.5 CO_2 fert.=-50%) and how these differ from the unperturbed control run. In addition, we illustrate the results in a new Figure 2, in which, in addition to time series of all control runs, we also show bar diagrams of the absolute changes in the carbon reservoirs and fluxes between the perturbed control simulations and the unperturbed control run for the years 2120 and 3020.

Further, in a new section (3.4.3), we show how the carbon budgets of the perturbed injections runs (I-800 CO_2 fert.=+50% / -50%, I-3000 CO_2 fert.=+50% / -50%), when compared to the respective control runs, differ from the anomalies of the injection runs of our original 'With Emissions simulations'. We further present the range of uncertainty for each carbon reservoir and flux at the year 2120 and 3020 in a new figure (new Fig. 6). For that purpose, we define the range of uncertainty as the difference of the absolute changes in atmospheric carbon between I-800 CO_2 fert.=+50% / -50% and the respective *control runs and the absolute change in atmospheric carbon between I-800 and the control run of the 'With Emissions simulations'.*

Finally, we discuss the terrestrial response to injections in the un- and perturbed runs in the context of the large uncertainty range related to the inter-model spread in future land carbon storage change [e.g., Friedlingstein et al., 2006; Arora et al., 2013; Jones et al., 2013; Zickfeldt et al., 2013; Hajima et al., 2014]. We particularly discuss this in relation to the issue of nutrient limitation of photosynthesis currently missing in many terrestrial carbon cycle modules. There is high confidence that low nitrogen availability will limit land carbon uptake. Models that combine nitrogen limitation with rising CO_2 *as well as changes in temperature and precipitation, predict a larger increase in projected future atmospheric* CO_2 *for a given* CO_2 *emission scenario [IPCC, 2013]. Models including terrestrial nutrient limitation are likely subject to a smaller terrestrial response to direct* CO_2 *injections into the deep ocean.*

The authors say that direct injection of CO_2 is presently in conflict with . . . international protocols/conventions. This is correct but may also be an understatement. Direct CO_2 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 CO_2 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.

This is a very good point. We did not intend to trivialize the potential ecological risks of direct CO_2 *injection into the deep ocean. We add a paragraph in the revised introduction section that addresses this issue. Further, we add the neglection of fast rising* CO_2 *bubbles [IPCC, 2005; Bigalke et al. 2008] in the experimental design section.*

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?

We have discussed possibilities to discriminate the impact of the natural variability (the deep convection) from the impact of CO_2 injections, for instance, during the injection phase before the onset of deep convection, or based on curve fitting of results from the other experiments, which show no deep convection events in the Southern Ocean. We came to the conclusion that no correct answer can be given without an ensemble simulation. Although the authors agree that it would be interesting and useful to perform an ensemble simulation with different initial conditions in order to assess the robustness of the ocean deep convection events, we feel that further analysis of it is beyond the scope of this study, which focuses on the response of the global carbon cycle during and after the CO_2 injections. In the manuscript we thus prefer to address this issue as done in line 420, but add a short discussion on the advantage of an ensemble simulation with respect to the reviewers comment.

Deep injection of CO_2 could potentially accelerate neutralizing fossil fuel CO_2 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 discussed here?

Yes, this is a very important point that we will discuss in the new results and discussion section (3.4.3). Dissolution of $CaCO_3$ sediments near or downstream of an injection site is expected to reduce *outgassing and increase the residence time of the injected CO_2. We expect a larger impact of this process in the Atlantic due to the low abundance of $CaCO_3$ sediments in the Pacific and Indian Ocean [Archer et al. 1998]. Model simulations by Archer et al. [1998] have shown that CaCO3 dissolution is sensitive to direct CO_2 injections throughout the Atlantic, but has led to only a slight impact on atmospheric pCO_2. A slightly modified trajectory of atmospheric CO_2 may further impact the terrestrial carbon pool and fluxes, resulting in a different terrestrial response as discussed in our manuscript. A quantitative answer, however, on how the marine CaCO3 sediments feedback or that of the land biosphere to direct CO_2 injections compare to each other can only be given by running the model with and without a sediment sub-model. This has not been done yet, mainly due to the several month long model runtime of the required spin-up of several 10,000 yrs. (Note that the code of UVic runs only on one processor, which typically simulates 200 model yrs. / day run time). We clarify in the experimental design and conclusion sections that we do not consider the effect of calcium carbonate sediments feedbacks in our direct CO_2 injection experiments.*

With respect to small details:

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

Thank you for your careful reading. We correct this mistake.

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

Yes, we agree that this could be confusing. We spell these acronyms out in italics.

l. 136: misplaced comma

Thank you, we correct this mistake.

l. 183: comma after simulations required

Thank you, we correct this mistake.

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

Yes, we thicken the black rectangles in Figure 1 to make them easier to identify.

Figure S2 should be placed in the main section. It shows the small effects. I do not want to stay anonymous.

Yes, we include Figure S2 in the main text as Figure 5.

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