

Interactive comment on “Impact of an extremely large magnitude volcanic eruption on the global climate and carbon cycle estimated from ensemble Earth System Model simulations” by J. Segschneider et al.

Anonymous Referee #2

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Authors assess the effect of a large volcanic eruption on the climate and the carbon cycle response using the MPI-ESM. This is an ordinary manuscript that does not come out as strong on analysis and new insights, but rather appears to be more of a sensitivity study as also pointed out by the other reviewer. At the very least, the additional analysis suggested by other reviewer will help to improve the paper. The description of models, in particular the terrestrial carbon cycle model, appears insufficient at times which makes it difficult to draw inferences about “why”. The description of the simulation protocols is also weak. I suggest that the authors strongly consider the suggestion

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from the other reviewer and also attempt to make their arguments a bit more coherent in order to link the behaviour of the terrestrial and ocean carbon cycle to grass root processes in these models. In addition, several sentences need rewording and appear as if they have been written in a hurried manner. The basis of these overall comments is explained in my minor comments below.

Page 8694. In the abstract, when mentioning that the land carbon cycle response (in terms of loss of carbon) is distinct compared to smaller eruptions – please mention “why” this is the case.

The last sentence of the abstract appears fairly weak. Ending the abstract on merely a note that 200 years later after the eruption the land and ocean carbon pools are different from their control (pre-industrial states) is not sufficient. It is more useful to note – how different? The answer probably is not that much.

Page 8695, last sentence. “The employed models describe not only the . . .”. Please consider using “include” or “represent” instead of “describe” in this sentence. Also consider replacing “carbon cycle compartments” with “carbon cycle components”.

Page 8696. Line 5. Replace with “An earlier ESM study with the Hadley . . .”.

Page 8696. Line 25. “In particular, they simulate either . . .”. Are you sure there’s an “either” here. Or did Tjiputra and Ottera (2011) investigate the response to both types of volcanic eruptions.

Page 8697. Line 18. “They find that while their precipitation anomalies are comparable to observations, the carbon cycle anomalies . . .”. What is carbon cycle anomalies? Is it the anomalies in atmospheric CO₂ concentration or something else? Please be more explicit. The discussion around precipitation and carbon cycle anomalies here is not clear.

Page 8697. Line 28. Is “ensemble simulation” an accepted phrase?

Page 8698. “that we obtained the forcing from an . . .”. What kind of forcing - aerosol

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optical depth (AoD) or something else? Please make it explicitly clear that an earlier model simulation (which model?) with climate chemistry calculations was used for such and such timescale to get AoD (or whatever is it that you got) for driving your MPI-ESM simulations? If its present form the manuscript text does not clearly documents how the driving data were obtained. Is there a reference to the chemistry model simulation you are mentioning?

Page 8698. Line 8-18 seem redundant and at the very least need some rewording.

Page 8699. Line 10. Remove the words “so-called” from this sentence and its other instances in the same paragraph. The description of the terrestrial carbon cycle component in this paragraph appears insufficient. Given the discussion later in the next section it would be useful to introduce your reader to basic linkages. This is done to some extent in the next section but not well for the terrestrial part. For example, on Page 8700, authors say “The land vegetation carbon pool, as for the ocean, will decrease in response to reduced SWR and temperature, potentially driving a flux from the land into the atmosphere”. Rather than this it would be better to make the explicit linkage that reduced SWR and temperature reduce photosynthesis (note that the manuscript doesn’t show this) which then leads to reduction in vegetation biomass. For soil carbon rather than just saying “cooling reduces soil respiration”, I suggest being a little more thorough by saying that “reduction in temperature decreases the soil decomposition rates, which reduces soil respiration rates and since the litter inputs into the soil carbon pool do not change substantially the soil carbon pool gains carbon”. The current basic descriptions of process linkages are insufficient.

Page 8702. The sentence, “The long integration times are needed to cover the tail of the return to pre-eruption levels in particular for the carbon cycle components” is unclear.

Page 8702. Line 14. Replace “maps” with “plots” here and other places.

Page 8704. It would be really useful to include a plot of time series of sea ice cover

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(and may be volume) even with the annual cycle. Typically, the September and March northern hemisphere sea ice covers are plotted. This will more clearly show how the sea ice evolves in response to the eruption. Gazing through the 2D plots in Figure 7 is not that convenient.

Page 8705. Figure 8 is difficult to interpret because unlike the Brovkin et al. (2010) paper (their Figure 3) it show the anomalies and not the actual pool sizes. At a first glance, it is difficult to believe that such a large eruption dents the land pool by only 10 Pg C, but when plotted as the actual pool – more information is available (like Fig 3 of Brovkin et al. 2010) and the changes can be appreciated. I strongly suggest plotting Figure 8 in terms of pool sizes and not anomalies.

Please don’t start every section with “In this section . . .”

Reword the sentence, “As the CO₂-flux is driven not only by changing wind fields but also by changes in temperature and export production there is, however, no one-to-one relationship in particular in southern summer”.

Page 8707. In the sentence, “In the northern winter of year 2/3, anomalous fluxes into the atmosphere show up in northern high latitudes” is the anomalous flux into or out of the ocean.

Page 8709. Terrestrial carbon community usually does not appreciate units of mol C/m². Please change mol C/m² on line 11 into Kg C/m².

Page 8709. The sentence, “A central point here is to understand how the ocean and land differ in their response to a change in the carbon content of the respective other compartment” needs rewording. Also please consider using “components” rather than “compartments”. The word compartments is somewhat misleading.

Page 8712. Reword this unclear sentence, “In 5 comparison with the simulation of the 1258 AD volcanic eruption (Brovkin et al., 2010), the results for the Yellowstone-like eruption differ in addition to the larger amplitude in the climatic and carbon cycle

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signals also qualitatively”.

Page 8712. What is YTT?

Page 8713. What is Tephra?

Page 8714. The sentence, “. . .that this could explain the observed dip in atmospheric CO2 concentration rise after the 1991 Mt. Pinatubo eruption”. “Dip” and “rise” in the same sentence is confusing and make this sentence unclear.

Page 8716. The concluding sentence is confusing and does not link up to the rest of the manuscript. You did not do uncoupled simulations (e.g. radiatively and biogeochemically) so in principle you cannot comment on the linearity of the response. I am unclear what is the purpose of this concluding sentence.

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