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Biogeosciences Discussions

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.

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General

We thank Referee #2 for her/his efforts to review our manuscript. We disagree, however, with Ref #2's remark that our manuscript "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". There is no other study that dealt with the effect of extremely large volcanic eruption on the carbon cycle using the full-scale ESM, simulating an

ensemble of eruptions with a carefully constructed forcing from volcanic aerosols over a period of 200yrs. Moreover, this is the first study that describes the carbon cycle response to Yellowstone-size eruption with realistic forcing in terms of AOD, investigation of long term response, ensemble approach to provide an estimate of uncertainty (25% of signal compared to single realization experiments), impact on land vegetation (pronounced signal compared to smaller scale eruption), and the marine biosphere (retardation of marine plankton bloom in high latitudes after eruption). Also the finding that eruptions of different scale have an opposite effect on atmospheric CO2 during the first years after the eruption is certainly novel. This will now be stated more explicitly in the ms. (Sec. 4, p. 8712, insert new sentence at ln 14: The response of the marine and terrestrial biosphere result in an increase of atmospheric pCO2 1-2 years after the eruption (Fig. 9) which is then followed by the decline driven by the soil carbon pool (Fig. 8), whereas for the 1258 A.D. eruption the atmospheric pCO2 decreases immediately after the eruption (Fig. 3 in Brovkin et al., 2010).

With regard to the analysis of carbon cycle response on eruption strength and carbon cycle-climate feedback we refer to the detailed reply to Ref #1 (points (1) and (2)).

Response to specific comments

Ref#2 -1

-(p8694...) 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.

We changed this to "The land vegetation pool shows a decrease by 4 GtC due to reduced short wave radiation that has not been present in a smaller scale eruption".

Ref#2 -2

-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.

We now quantify the final perturbations in the last sentence of the abstract: After 200 years, the ocean and the land carbon pools are still different from the pre-eruption state by 3 GtC and 4GtC, respectively, and the land carbon pools (vegetation and soil) show some long lasting local anomalies that are only partly visible in the global signal.

Ref#2 -3

-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".

We changed "describe" to "represent" and "compartments" to "components".

Ref#2 -4

-Page 8696. Line 5. Replace with "An earlier ESM study with the Hadley . . .".

We changed 'early' to 'earlier'

Ref#2 -5

-Page 8696. Like 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.

We changed the sentence to "In particular, they do simulate Pinatubo-like eruptions (Volcanic Explosivity Index VEI 6) every 5 years in one model run and Tambora like eruptions (VEI 7) every 25 years in another model run for the period 2000 to 2100 where CO2 emissions follow the IPCC-A2 scenario."

Ref#2 -6

-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.

We changed "carbon cycle anomalies" to "biogeochemical response" as in the original ms. of Rothenberg et al. 2012 but cannot make the discussion more precise than it is in the ms. of Rothenberg et al., 2011

Ref#2 -7

-Page 8697. Line 28. Is "ensemble simulation" an accepted phrase?

Yes, see e.g. title of BAMS , Vol. 82 (11), 2001

Ref#2 -8

-Page 8698 "that we obtained the forcing from an . . .". What kind of forcing – aerosol 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[n] 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?

The forcing is described in the original ms. in section 2.3, including the references to the model MAECHAM5/HAM and the papers by Timmreck et al. 2010 and Niemeier et al. 2009 where the forcing and the models to derive it are described in detail. We added "volcanic" before forcing in line 5 to make the sentence more clear.

Ref#2 -9

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

To emphasize better the novelty of our work we changed the text to:

We are thus the first to give an estimate of the behaviour of the Earth System for an explosive eruption at Yellowstone as it may occur in the foreseeable future (Wicks, 2006) with more realistic short wave radiation perturbations than the 100 times Pinatubo AOD perturbation assumption. The study also gives a first estimate of the range of the responses of the climate system and carbon cycle to a large volcanic eruption depending on the state of the climate system at the time of explosion. As a further novelty we investigate the long term response of the system beyond 20 years.

Ref#2 -10

-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.

We followed the suggestion of Ref#2 and inserted “reduction in temperature decreases the soil decomposition rates, which reduces soil respiration rates. in Sec. 2.2 and removed the ‘so-called’ in Sec. 2.1. We also replaced ‘gross primary production’ by ‘photosynthesis’ in Sec. 2.1. We did not insert “and since the litter inputs into the soil carbon pool do not change substantially the soil carbon pool gains carbon” as the litter

input to the soil does change in our experiment.

Ref#2 -11

-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.

We changed “are needed to cover the tail of the return to pre-eruption levels" to “are needed to investigate the long term response of the carbon cycle”.

Ref#2 -12

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

we still consider "maps" more adequate than "plots" for the horizontal distributions that are shown we changed “maps”to “plots of horizontal distribution”

Ref#2 -13

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

We will include time series of sea ice extent for March (upper) and September (lower panel) of each year (see figure below) in a new figure and keep panels b,c of the original Fig. 7 to give an indication of the spatial distribution of the sea ice cover anomalies.

New text: Changes in sea ice cover also have a potential to effect carbon fluxes: Any increase of sea ice cover as a result of the cooling will strongly inhibit carbon fluxes between ocean and atmosphere and suppress oceanic biological production. To estimate if this is important, we show time series of global sea ice cover in March (typical maximum ice extent on the northern hemisphere) and September (typical minimum ice extent on the northern hemisphere) and horizontal distributions of sea ice fraction anomalies in September of Year 2 and March of Year 3 (Fig. 7 Sea ice cover increases quickly from 15.5x 106 km2 to a maximum of 20x 106 km2 for individual ensemble members in response to the cooling in March and from 6.0x 106 km2 to 11x106 km2 in September (Fig. 7a,b). At the time of maximum ice extension in the respective hemisphere anomalies are largest in months 21 and 27 (Fig. 8a,b). Interestingly, larger than normal ice cover prevails for more than 10 years.

Ref#2 -14

-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.

The data for the carbon pools of the 1258 A.D. volcanic eruption in Brovkin et al. 2010 were taken from the Millennium run. In this run, numerous subsequent volcanic eruptions are simulated, and thus a history of the volcanic signals is carried forward. It was therefore not possible to compute anomalies with regard to identical years of a control run, and therefore totals of the carbon pools had to be shown. These include any model response to previous volcanic eruptions and internal model variability, so are less precise in terms of showing the exact signal of the 1258 A.D. eruption than our anomalies with respect to corresponding years of the control run from which the individual ensemble members are initialized. As the ensemble members are initialized from slightly different states of the climate and carbon pool sizes they would follow different trajectories even without volcanic perturbation. It could therefore be confusing to show absolute values of the carbon pools. An example for this is shown above in the requested time series of total sea ice cover. Are the anomalies around year 35 of some ensemble members a result of the volcanic forcing? This is unlikely but can not

be rejected from the figure. We, therefore, argue that the provided anomalies represent the model response to the volcanic eruption more precise than the absolute pool sizes given in Brovkin et al. 2010. However, we now provide the mean pool sizes of the control run in the figure caption.

Ref#2 -15

-Please don't start every section with "In this section . . ."

Only 2 out of 10 sections are started with "In this section". We changed the second occurrence (Sec. 3.2.3.) to: The terrestrial carbon cycle is further analyzed by investigating the. . ...

Ref#2 -16

-[p8706 ln 27] 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".

we changed "there is, however, no one-to-one relationship" to "it is not possible to identify wind stress as the dominant driving force in particular in southern summer"

Ref#2 – 17

-Page 8707.[ln3] 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.

We changed this to "..anomalous fluxes from the ocean into the atmosphere "

Ref#2 -18

-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.

We chose to keep molC/m2 to be consistent with Brovkin et al. 2010 but added in the text the perturbation size in KgC/m2

Ref#2 -19

-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.

We changed the sentence to:

A central point here is to understand how the ocean reacts to a change in the carbon content of the land component and vice versa.

Ref#2 -20

-Page 8712. [ln 5] 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 signals also qualitatively”.

We changed this to "In comparison with the simulation of the 1258 A.D. volcanic eruption (Brovkin et al., 2010), the Yellowstone-like eruption not only differs in the larger amplitude in the climatic and carbon cycle signals. They also show a different response of the marine and terrestrial ecosystems”.

Ref#2 -21

-Page 8712. What is YTT?

Younger Toba Tuff, as had been defined previously in the ms. on p8701 ln 7

Ref#2 -22

-Page 8713. What is Tephra?

We now state explicitly that tephra is a volcanic ash layer.

Ref#2 -23

-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.

A dip in the rise of atmospheric pCO2 is exactly what was observed after the 1991 Mt. Pinatubo eruption. Formulations such as “ a sharp decline in the growth-rate” as in Angert et al. 2004 do not really sound better. In principal it is less precise, as a decline could be steady, while “dip” signifies a short term reduction

We tried to improve the sentence by changing it to: . . . and that this could explain the dip in the long term rise of atmospheric pCO2 from fossil fuel burning that was observed after the Mt. Pinatubo eruption

Ref #2 -24

-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.

The reference to the uncoupled simulation as in the C4MIP framework is not applicable here. The temperature decrease after the volcanic eruption simulated here does not arise from atmospheric pCO2 changes, but from the aerosol forcing. As the gain of the soil carbon pool results in a decrease of the atmospheric carbon content, and thus drives a flux from the ocean into the atmosphere that is contrary to the inherent oceanic response resulting from cooling (see discussion section4, p8709-8710), it is then obvious that any model without an interactive carbon cycle would miss the flux from the ocean to the the atmosphere driven by the land gain. So one does not require “uncoupled” simulations to infer that one needs a coupled model, where there is an interaction between the land and the ocean via the atmosphere, to properly simulate the effects of the volcanic eruption on the carbon cycle

We tried to make this point more clear by changing the paragraph to:

Moreover, the response of the carbon cycle system is different from the sum of the responses of the individual carbon cycle components. This emphasizes the need to employ Earth System Models with an interactive carbon cycle to investigate the impact of volcanic eruptions on the carbon cycle.

Interactive comment on Biogeosciences Discuss., 9, 8693, 2012.

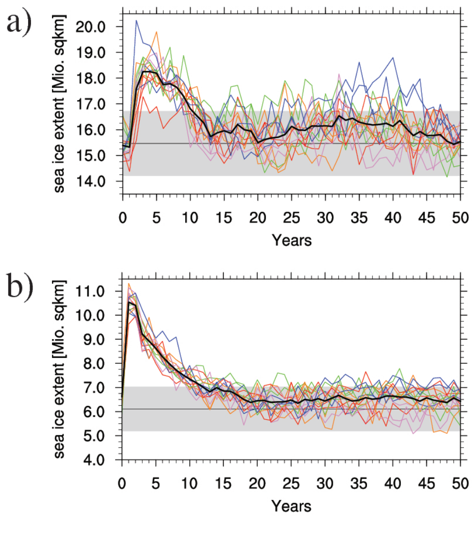

Figure 7: Time series of global sea ice extent in a) March and b) September in 10^6 km^2.

Fig. 1. New Fig. 7

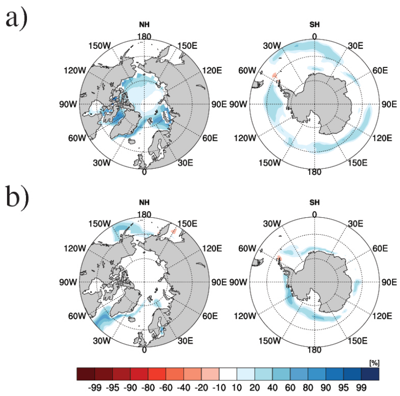

Figure 8: Maps of ensemble mean anomalies of sea ice fraction [%] for a) September of year 2 and b) March of year 3 of the model integration.

Fig. 2. New Fig. 8