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Comment

Interactive comment on “Sensitivity of atmospheric CO₂ and climate to explosive volcanic eruptions” by T. L. Frölicher et al.

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We would like to thank very much reviewer 2 for his careful reading of our manuscript and we appreciate the constructive comments that helped to clarify and improve the manuscript. We have addressed all comments as described below. The reviewer comments are in bold text, our responses are in plain/italic. We included a revised manuscript as a supplement to the responses to referee 1.

Main point

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I am a bit worried about the use of gamma here (carbon cycle-climate sensitivity) which has been mainly introduced in the context of coupled carbon cycle-climate simulations forced by CO₂ emissions - where any climate change was caused by changes in the carbon cycle. Here, the climate is driven by AOD changes that represent volcanic eruptions. The mechanisms are fundamentally different. In particular a large quasi instantaneous perturbation in the soil carbon pool is triggered by temperature decrease. This perturbation returns back to normal values with the time scale of the heterotrophic respiration of the carbon pool. This dominates the response of atmospheric CO₂, somewhat modulated/ damped by the ocean. In climate change scenarios, the perturbation is a positive temperature anomaly and the response of the system is more or less in balance with the forcing. As such the analysis of gamma in dependence of time scale in the present paper does not make too much sense to me (the integrations are much shorter than the potential life time of the perturbation in the soil carbon pool, anyhow.) The authors do not seem to fully be aware of this difference, as they argue the response of their model would be stronger if it had a larger climate sensitivity (warming/atmospheric CO₂ doubling), whereas in the model carbon-to-T response (caused by AOD increase) is computed. In particular the sentences on p1981 In 3-8 I find irritating.

We think our analysis of the carbon cycle-climate sensitivity is important and should be included in this manuscript as it shows the limitation of the approach and also the limitation when trying to derive sensitivity from last millennium CO₂ and temperature reconstructions. However, we agree that we failed to explain this in detail in the manuscript. Therefore, we entirely changed the description of the carbon cycle-climate sensitivity in section 2.3 and in section 3.2. For further details, please see answers to specific points below and to the points of reviewer 1.

We deleted the sentences of lines 1-8 on page 2981. Additionally, we clearly state in the discussion section that the simulated sensitivity of the atmospheric CO₂ concentration to temperature depends on the forcing type and that a clear comparison of γ

derived from proxy data with model estimates is somewhat difficult. Furthermore, we changed the discussion on p. 2984 to:

“In brief, the simulated sensitivity of the atmospheric CO₂ concentration to temperature depends on the forcing type, the forcing strength and the time scales of interests. The mechanisms in play might be different for a volcanic eruption than for a exponentially increasing perturbation. This makes a clear comparison of γ derived from proxy data and with model estimates using different forcing types somewhat difficult, although the analysis of the proxy data likely reveals the right order of magnitude of γ .”

Other general issues:

- 1. Several decades is often used in the ms., but only two are simulated. I would think "several" are at least 3-4. Of course this points to the relatively short integration time of only 20 years for the experiments.**

We used the term several decades when describing land and ocean carbon inventory changes. It is appropriate to use several decades as the simulated carbon inventory changes of the land (as shown in Fig. 1e, Fig. 6a) as well as the ocean (as shown in Fig. 1f) are still significant different from internal variability at the end of the simulation (after 20 years) in the Pin.5x - Pin.100x cases. The simulated global mean changes in the land and ocean carbon inventory for the Pin.1x and Pin.2x case are also significant different from the control simulation after 20 years. However, a series of ensemble simulations starting from different initial conditions should be performed to properly account for internal variability for these cases. As the focus of this paper is not only on the Pin.1x and Pin.2x case, we keep the term several decades.

- 2. Some figures are way too small and it is therefore often difficult to see how they support the statements made in the text.**

We increased the size of Fig. 1, Fig. 2, Fig. 6, Fig. 8, Fig. 9 and Fig. 10 to avoid unnecessary surrounding white space (see supplementary to referee 1).

3. **State 'gamma' of CCSM (from Frank et al. 2010 it seems to be 6ppmv/degC)**

γ of 6.3 in Frank et al. 2010 for the CSM1.4-carbon model (and all other models) was calculated for variations in temperature for the Little Ice Age - modern amplitude of ΔT bigger than 0.70°C . This corresponds to the period 1820 to 2036 for the NCAR CSM1.4-carbon model. The γ of 6.3 lies between (and is thus consistent with) the γ value in Fig. 4 of 4.3 for the period 1820 to 1999 and of 9.9 for the period 2000 to 2099.

We have added the following text:

"The estimate of $\gamma = 6.3 \text{ ppm}^\circ\text{C}$ for the same model in Frank et al. 2010 is consistent with our estimates. They calculated γ for variations in temperature of 0.7°C , typical for the Little Ice Age - modern amplitude. This would correspond to the period 1820 to 2036 in the CSM1.4-carbon model (see Table S3 in Frank et al. 2010; note that the period is not correctly given in Frank et al. 2010: 1861 to 2077 instead 1820 to 2036) and lies between our estimate for the period 1820 to 1999 and the period 2000 to 2099."

Specific comments

1. **p2958 l 4 it is claimed that the model is forced by "pulse-like stratospheric sulfur release" but the model is forced by VOD perturbations (why is VOD used instead of AOD?)**

The model is forced by aerosol optical depth changes, but aerosol optical depth changes in the mid-visible wavelength (VOD) are shown in the figures. We replaced Fig 1b (now Fig. 1a) with a figure showing the prescribed VOD perturbation. We changed the sentence in the abstract to:

“Impacts of low-latitude, explosive volcanic eruptions on climate and the carbon cycle are quantified by forcing a comprehensive, fully coupled carbon cycle-climate model with pulse-like stratospheric aerosol optical depth changes.”

and:

“Results from a suite of sensitivity simulations with different magnitudes of stratospheric aerosol optical depth changes and from global warming simulations show that the carbon cycle-climate sensitivity γ , expressed as change in atmospheric CO₂ per unit change in global mean surface temperature, depends on the magnitude and temporal evolution of the perturbation, and time scale of interest.”

We also changed the description of the forcing in the method section:

“The radiative volcanic forcing caused by the release of sulphate aerosol into the stratosphere is calculated in the model by prescribing zonal-mean time series of aerosol optical depth (Ammann et al. 2003). The aerosol optical depth was scaled linearly with the aerosol loading assuming a single aerosol distribution of 0.5 μm for volcanic aerosols. Details are described in Ammann et al. 2003.”

We replaced visible optical depth changes with aerosol optical depth changes throughout the manuscript, except for the description of Fig. 1 and Fig. 2, where we show the changes in volcanic aerosol optical depth in the mid-visible wavelength. We also changed the caption of Fig. 2:

“Aerosol optical depth changes in the mid-visible wavelength (VOD) for the different Pinatubo scalings are also given in brackets (top abscissa).”

The caption of Fig. 12 has been changed accordingly:

“Volcanic forcing expressed as aerosol optical depth changes in the mid-visible wavelength (VOD).”

2. I 11 correct "initially" weak ... to "initial weak" (the pert. is not initially a weak sink, then a strong sink. It is first a sink, then a source)

Done.

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3. **I 22 "depends on the perturbation" is unclear. size of? kind of? also see above comments on gamma**

We changed the sentence to:

"Results from a suite of sensitivity simulations with different amounts of sulfur released and from global warming simulations show that the carbon cycle-climate sensitivity γ , expressed as change in atmospheric CO₂ per unit change in global mean surface temperature, depends on the magnitude and temporal evolution of the perturbation, and time scale of interest.

4. **p 2962 I 27 change "and" to "or"**

Done.

5. **p 2964 I 18/19 the sentence as it stands makes no sense**

We have changed the sentence to:

"The land model does not include other land surface processes that affect atmosphere-biosphere interactions such as an explicit nitrogen cycle, fires and other disturbances, herbivory, dynamic vegetation cover, or anthropogenic land cover change."

6. **p 2966 I 1-6 what is the reason for the additional 280-yr control simulation? (control, or spin-up?) I presume the 1000-yr run did not include volcanic forcing?**

We performed all simulations (control and sensitivity simulations) on an IBM SP5 at the Swiss National Supercomputing Centre (CSCS). However, the 1000-yr preindustrial control simulation has been performed by the National Center for Atmospheric Research (NCAR) on a IBM SP4. Although the results obtained at the CSCS are virtually identical to those obtained by NCAR within the limits of internal variability (Frölicher et al., 2009), we performed all simulations on the same supercomputer and with the same compiler to be fully consistent. Both, the CSCS 280-yr control simulation as well as the NCAR 1000-yr control simulation,

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do not include volcanic forcing. We have changed the text accordingly:

“The variability of a 280-yr control simulation, an extension of the 1000-yr control simulation and performed on the same supercomputer as the sensitivity simulations, is used to determine the statistical significance of the responses.”

7. **I 13 it should be made clearer here what is perturbation and what is AOD (it is finally revealed in the results section that the background AOD is only 0.01 and therefore delta AOD is about the same as AOD, but this is difficult to guess here) also, VEI is based on erupted volume - does this scale linearly with AOD? I doubt it....**

We modified the description of the applied volcanic perturbation (see comments above). We deleted the description of the background level as it confuses:

“Aerosol optical depth increases to a maximum within the first 5 months after the eruption and gradually returns to its background level over the next several years (Fig. 1a).”

Further, we considered the Pin.100x case as a first-order approximation for a super-eruption (e.g. Toba eruption). As many processes may be different for a super-eruption in comparison with a Pinatubo-like eruption (e.g. non-linear chemical processes, etc.), we did not focus on the Pin.100x case in detail in our manuscript. We modified it to: *“The largest simulated eruption (Pin.100x) may be comparable to the Toba eruption (~74 thousand years before present). The Toba eruption is the largest known eruption of the past 100'000 years and could potentially have contributed to glacial cold conditions.”*

8. **p 2967 I 1 has Pin.10x a higher signal to noise ratio than 20x, 50x, 100x?**

Certainly not. We have changed the text to:

“The focus in the entire manuscript is on the Pin.10x case. It should be viewed as an illustrative example with a reasonably high signal-to-noise ratio.”

9. **I 15 what is “the overall concept in carbon balance”?**

This paragraph has been entirely rewritten (see answer to comments above and to comments of reviewer 1).

10. **I 24-28 this should go into section 2.2**

We moved the sentences to section 2.2 by modifying them to:

“To calculate the carbon cycle-climate sensitivity for the industrial period and for the 21st century, we use a simulation with warming and a simulation without warming conducted with the same NCAR CSM1.4-carbon model. Both simulations are forced with historical emissions over the industrial period followed by the SRES A2 IPCC emission scenario. In the simulation without warming, greenhouse gases and other radiative agents were kept at their preindustrial values in the radiation module of the model. For further details see Frölicher et al., 2009 and Frölicher et al., 2010.”

11. **p 2969 I 5 ...the first 5 months "after the eruption" (months 6-11 in the figures)**

As already indicated in the caption of Fig. 1, the volcanic eruption starts in month 6 of the simulation and the net surface solar flux is decreasing mainly in month 6 to 11. We added 'after the eruption' to the sentence. To further clarify this fact, we included following sentence to the section 2.2:

“The volcanic eruption starts in month six for all cases.”

12. **I 24 add "pCO₂" before "is still..."**

We included: *“the atmospheric CO₂.”*

13. **p 2970 I 6 for a statement "as typical for" you would need ensemble runs for Pin.10x**

We have changed it to: *“as simulated in”.*

14. **I 9 "this is small.... yes, but Pin.10x is not, so is this really relevant?"**

We agree with the reviewer that the additional feedback is small (see also reply

to reviewer 1). Therefore we down-toned this in the abstract and the conclusions and stated that the additional feedback is very small. We changed the description and usage of Fig. 13 (now Fig. 14). This figure is intended to show the processes causing atmospheric CO₂ decrease after a volcanic eruption and should not highlight the very small feedback. Therefore, we reduced the size of the sign showing the small feedback and we made the arrow showing the feedback dashed. We changed the figure caption accordingly: *“Flowchart of changes after an explosive volcanic eruption. The small impact of enhanced mixing and export production, and decreased stratification on air-sea gas exchange as well as the impact of precipitation and diffuse radiation on NPP are neglected here. The dashed arrow shows the small amplifying feedback for climate change.”*

15. I 16 what is meant by ...land uptake "relies" on regional changes?

We have deleted the sentence, as we are discussing the regional changes in the land carbon uptake in a separate chapter.

16. I 22 "As soon as....." It is more likely that the flux from the atmosphere to the land and hence decreasing pCO₂ in the atmosphere causes the ocean-atmosphere flux (look at the numbers in Fig.1, or plot them with equal scales)

We agree with the reviewer and we have changed the sentence accordingly: *“The initial increase in ocean carbon uptake can be explained by an increased solubility of CO₂ in cooler ocean water (see section 3e). The ocean changes from a carbon sink to a carbon source after a couple of years because of the large terrestrial carbon uptake and decreasing pCO₂ in the atmosphere.”*

17. I 29 large eruptions, not large volcanoes

Done.

18. p 2970 I 4 radiative forcing "anomalies"....

We have changed it to *“causes radiative forcing anomalies”*.

19. **p 2972 | 19 why does the carbon cycle anomaly of the land go not back to zero in Pin.1x?**

We assume that the long residence timescale of some soil carbon pools are responsible for that. However, only one single simulation for the Pin.1x case has been performed. This is probably not enough to detect significant changes due to the low signal to noise ratio of the Pin.1x case.

20. **p 2972 | 19 different from the 'standard deviation' of the control simulation?**

The statistical significance of the changes are calculated using two-sided Student's t-test ($p \text{ value} < 0.05$), as described in the 'Analysis Methods' section. We have changed the sentence to:

"In our model and for a pulse-like sulfur injection, the decrease in precipitation is not significant, although the simulated precipitation changes are in general similar to those of Robock et al. (2008)."

21. **I 25 delete Ocean or replace by Sea**

Done.

22. **p 2973 | 7 define R_h**

R_h has been defined already in the section 'Methods'. We reformulated the sentence for clarity: *"The time-integrated response of the land carbon inventory to the Pin.10x eruption is divided into soil and vegetation carbon pools as well as into Net Ecosystem Production (NEP), net primary production and heterotrophic respiration."*

23. **I 14 Tropics to tropics**

Done.

24. **p 2977 | 4 role... for what?**

We have changed the sentence to *"Besides the terrestrial changes the ocean*

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carbon cycle may play an important role in regulating atmospheric CO₂ after explosive volcanic eruptions.”

25. I 9 why does an increase in DIC lead to an increase in pH?

See reply to comment 19 of reviewer 1.

26. I 23 looks more like 15 gC/m²/yr in Fig. 8c

We only show changes in Fig. 8 that are significant to avoid cluttering the plots. Furthermore, we rescaled the colors in Fig. 8c to show small changes as well. Contour lines have been modified to clearly show the large local changes in the North Atlantic. Furthermore, we changed the sentence to:

“The largest increases in export production are simulated in the North Atlantic region where it increases locally by up to 30 to 40 g C m⁻² yr⁻¹ (Fig. 8c). In other regions, smaller changes are simulated as is also the case in global warming simulations (Steinacher et al. 2010).”

27. p 2978 I 10 replace surface-to-deep by deep-to-surface

Done.

28. p2980 I 2 delete 'due' (sensitivityto)

Done.

29. I 20 If I calculate this for Pinatubo x1, 0.08x2.5x0.1 = 0.02 deg C I would argue this amplification is not relevant

See reply to point 14.

30. p 2981 I 1 if the model would be started from present pCO₂ also oceanic pCO₂ would be higher, so it is not clear that the ocean uptake would be higher.

We deleted the sentence.

31. **p 2991 | 24 correct to Le Quéré, C.**

Done.

32. **Fig 4 check 2yr,5yr... in caption against legend 5yr,10yr...**

We changed the caption description accordingly.

33. **Fig 10 check 'global mean' vs. Atlantic, Indo-Pacific**

We have changed the caption to:

"Hovmöller diagram of monthly mean sDIC, sDIC_{bio}, sDIC_{res} and potential temperature differences between the Pin.10x case and the control simulation in the Atlantic and the Indo-Pacific Ocean at different depths. The volcanic eruption starts in year 0.5. "

Furthermore, we indicate the latitudes for the Atlantic and the Indo-Pacific in the figure.

References:

Frölicher, T. L. Ensemble modeling of the coupled carbon cycle-climate system. PhD thesis, 180 pp., Climate and Environmental Physics, University of Bern.

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