

## ***Interactive comment on “Sensitivity of atmospheric CO<sub>2</sub> and climate to explosive volcanic eruptions” by T. L. Frölicher et al.***

### **Anonymous Referee #2**

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#### Main point:

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 CO<sub>2</sub> 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 CO<sub>2</sub>,

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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 CO<sub>2</sub> 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.

Other general issues:

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.

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

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

specific comments

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?)

l 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)

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

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p 2962 | 27 change "and" to "or"

p 2964 | 18/19 the sentence as it stands makes no sense

p 2966 | 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?

l 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....

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

l 15 what is "the overall concept in carbon balance"?

l 24-28 this should go into section 2.2

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

l 24 add "pCO<sub>2</sub>" before "is still..."

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

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

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

l 22 "As soon as....." It is more likely that the flux from the atmosphere to the land and hence decreasing pCO<sub>2</sub> in the atmosphere causes the ocean-atmosphere flux (look at the numbers in Fig.1, or plot them with equal scales)

l 29 large eruptions, not large volcanoes

p 2970 | 4 radiative forcing "anomalies"....

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

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

| 25 delete Ocean or replace by Sea

p 2973 | 7 define Rh

| 14 Tropics -> tropics

p 2977 | 4 role... for what?

| 9 why does an increase in DIC lead to an increase in pH?

| 23 looks more like 15 gC/m<sup>2</sup>/yr in Fig. 8c

p 2978 | 10 replace surface-to-deep by deep-to-surface

p2980 | 2 delete 'due' (sensitivity ....to)

| 20 If I calculate this for Pinatubo x1,  $0.08 \times 2.5 \times 0.1 = 0.02$  deg C I would argue this amplification is not relevant

p 2981 | 1 if the model would be started from present pCO<sub>2</sub> also oceanic pCO<sub>2</sub> would be higher, so it is not clear that the ocean uptake would be higher.

p 2991 | 24 correct to Le Quere, C.

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

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

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**BGD**

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