**Biogeosciences
Discussions**

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

Anonymous Referee #1

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general comments ——————-

This manuscript presents an analysis of the impact of volcanic aerosols on a coupled carbon-climate model, examining the response of annual mean surface climate, the carbon cycle, and feedbacks between them to large perturbations of stratospheric visible optical depth (VOD). The authors also explore how the response varies with the strength of the VOD perturbation.

In its present state, I do not think that the manuscript should be published in Biogeosciences. I think that significant revisions are necessary. My principle concerns can be categorized as:

1) Some assertions in the manuscript are not backed up with analysis. There are a number of ocassions in the manuscript where the authors have phrases like "likely due to" and "is mainly caused by", but the claim is not backed up. The causal relationship has not been established. The authors have not ruled out alternative mechanisms.

2) The definition of gamma, the carbon cycle-climate sensitivity omits a key feedback. The author's definition of gamma differs from the motivating definition from Friedlingstein et al. 2006, but there is not justification given for the difference.

specific comments ——————

p. 2964, lines 27-28: Please state clearly how the conversion factor is used. Also, what are its units.

p. 2965, lines 11-14: The citations given don't support this claim very well. Frölicher et al. makes the claim but with the caveat that "the changes are not significantly distinct from zero". Regarding Stenchikov et al, 2006, the present study is using CSM1.4-carbon with atmospheric resolution of T31. Stenchikov et al. 2006 gives results for CCSM3 with atmospheric resolution of T85. I don't think it is reasonable to cite their work as evidence of good behavior for the model used in this study.

p. 2967, lines 11-21: I find the explanation of the analysis confusing. You start by stating that changes in land and ocean C storage are linearly related to global TS and global CO2. But then you state that you neglect this reservoir separation as well as the CO2 feedback. Please justify the simplification of neglecting Friedlingstein's beta terms. Also, please explain how to physically interpret the expression you are evaluating. Friedlingstein's gamma is the ratio of the change in reservoir inventory, equivalent to cumulative surface flux, to the change in TS. But you are looking at cumulative change in inventory divided by cumulative change in TS. Please explain why you are putting an additional indefinite integral in the numerator and denominator of this quotient. If I'm understanding the text, I think that this is a significant flaw in the analysis. If you were to do experiments with fixed BGC CO2, you could compute a clean gamma,

and then back out beta from the existing experiments.

p. 2968, line 18: Does the analysis really use salinity normalized PO4, which is presumably what sPO4 denotes? This only makes sense if the model applies freshwater fluxes to PO4, which I am fairly certain CSM1.4-carbon does not do.

p. 2970, lines 3-11: The first sentence has not really been demonstrated. The followup analysis mistakenly applies equilibrium response sensitivities, which have multidecadal timescales, to multiyear transient CO2 perturbations. The TS response to a transient CO2 perturbation that begin to decay within a few years will be much less than the equilibrium TS response to the peak CO2 perturbation. The first sentence could instead be quantified by doing an additional experiment with volcanic perturbations, but with fixed radiative CO2. The TS difference between this new experiment and the ones described in the manuscript would yield the climate impact of the CO2 perturbation.

p. 2970, lines 19: You haven't justified the claim "most likely in response to cooler temperatures." While it may be true, you haven't demonstrated why this cause is any more likely than other potential causes.

p. 2971, lines 3-5: Again, this has not been demonstrated. Even for the largest perturbation, atmospheric CO2 is only down by 5 ppmv after 20 years.

p. 2971, line 18-26: In my opinion, the CO2-net surface solar relationship is not 'nearly ... linear'. The statement about the change in sensitivity as net surface solar increases contradicts a linear relationship. This paragraph would be easier to follow if it stated that there is a non-linear relationship, and then proceeded to explain it.

p. 2972, line 4: I think 'decrease nearly exponentially with increasing VOD' is easily misunderstood. Perhaps 'scale with the log of the VOD perturbation' would be better.

p. 2972, lines 14-16: Is it correct to state that the point values leading to the curves in Fig. 3a are indefinite integrals of the data in Figs 3c & 3d? For readers not used to seeing cumulative changes in state variables, like this reviewer, it would be useful to

make this connection.

p. 2972, line 14 - p. 2973, line 16: As stated above, I think that this analysis is flawed because it is omitting C release by the land and ocean that is due purely to the decrease in atmospheric CO2. It is misleading to cite Friedlingstein et al. 2006 and call this gamma, as Friedlingstein does, when in fact it is quite different from the gamma of Friedlingstein.

p. 2973, lines 25: You haven't justified the claim "likely due to the relatively small cloud cover these regions." While it may be true, you haven't demonstrated why this cause is any more likely than other potential causes. For instance, you haven't shown the response of cloud cover to the VOD perturbation.

p. 2974, line 8: The phrase 'reduced by up to 2 mm/d' conveys little information. It would be more meaningful to state a threshold that the precip anomaly actually exceeds in these regions.

p. 2974, lines 12-13: Including the sentence that the precip increases 'could be traced back to model biases' with nothing to back it up is pointless. No attempt to make a connection has been presented.

p. 2974, lines 14-20: Given the low resolution of the model, how credible is this regional difference?

p. 2974, line 28: This claim appears to be unfounded, since there is no apparent metric of success.

p. 2975-2976, section 3.4: Panels b-f of Figure 6 are averaged over the first five years of the perturbation. Over this period, the change in respiration dominates the change in NPP, and the change in soil inventory dominates the change in vegetation inventory. Despite this, the majority of this section deals with NPP and vegetation. There is no quantitative analysis of what is the leading factor causing the change in respiration. It would be useful to have a companion to Fig. 7 that shows regressions of respiration

against soil moisture and temperature. Please state the time period of the regression in Fig. 7.

p. 2977, line 4: Please state in what way the ocean carbon cycle may play an important role.

p. 2977, lines 8-9: The pH map in Fig 8d does not match the text. The The increase in pH is nearly global, and the largest increases in pH occur in regions where sDIC doesn't change, or decreases.

p. 2977, lines 9-12: Please back up the statement that the sDIC increase is caused by the cooling, remembering that correlation does not imply causation. If cooling were the only effect, then the increase in sDIC would show the same patterns as the change in SST. It would be useful to show maps of $\Delta sDIC_{bio}$ and $\Delta sDIC_{res}$. The patterns of change in air-sea carbon flux differ considerably from the change in SST. Please explain why the difference occurs.

p. 2977, line 12: Is an increase of 4% in POC export statistically significant?

p. 2977, line 23: How do you define 'North Atlantic'?

p. 2977, lines 24-25: If POC export didn't change much elsewhere, what causes the increase in $sDIC_{bio}$ between 10N & 20N in the Atlantic and the decrease between 30S and 10S in the Atlantic and between 60S and 40S in the Indo-Pacific?

p. 2977, lines 26-28: Is Fig. 10 just a zonal mean, and if so, at what latitude? If the mean is over a band of latitudes, please state define the band. This is particularly relevant for the Atlantic because the zonal mean plots of Fig 9 shows regions of increasing and decreasing $sDIC_{bio}$ that will cancel each other in a full basin average.

p. 2978, lines 17-19: This statement reinforces my point above that computation of gamma should not omit ocean release of C which is solely due to decreased atmospheric CO2.

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p. 2981, line 28 - p. 2982, line 2: Fig. 12f shows the response to Pinatubo on top of a transient CO2 response. From an eyeball view, there doesn't appear to be much amplification of the ocean response. Please quantify this from your experiment.

Figure 4: I find it distracting that red corresponds to negative anomalies in panels c) and d). In every other figure of the manuscript, it corresponds to positive anomalies. Please apply this convention consistently.

Figures 9 & 10: I find it distracting that the panel labeling convention for these figures differs from every other figure in the manuscript. Please be consistent in your panel labeling convention.

Figure 10: The caption initially states that this is a global mean, which appears to be an incorrect statement.

technical corrections ——————

p. 2967, line 12: should 'land and carbon storage' be 'land and ocean carbon storage'

Figure 1: Enlarge text

Figure 2: Enlarge text

Figure 3: Enlarge text in legends, add units to panel a

references: The lead author's name of Le Quéré et al. 2009 is listed incorrectly.

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