Biogeosciences Discuss., 8, C1765–C1784, 2011
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Biogeosciences Discussions

Interactive comment on "Sensitivity of atmospheric CO_2 and climate to explosive volcanic eruptions" *by* T. L. Frölicher et al.

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Received and published: 1 July 2011

We would like to thank very much reviewer 1 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.

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 occasions 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.
Given the strong interdisciplinary work related to the application of Earth System Models, it is difficult to provide all the information within the page limit of a manuscript to completely justify all statements. We avoid phrases like 'probably due' or 'most likely' and we clarified the points that has been raised by the reviewers (see specific replies below). We included (i) a panel in Fig. 1 showing VOD changes to clearly show the prescribed forcing used in this study, (ii) new panels in Fig. 7 showing the regression of Rh on soil moisture and temperature to discuss mechanisms, (iii) new panels in Fig. 8 showing Alk, $sDIC_{res}$ and $sDIC_{bio}$ changes to discuss in detail the changes in sDIC, and (iv) a new figure (Fig. 9) showing changes in pH as well as the driving mechanisms behind these changes.

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

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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 CO_2 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:

"Different approaches are currently used in literature to estimate the carbon cycle-climate sensitivity γ. The most widely applied are briefly summarized to put the approach adopted here in the context of published studies. Dufresne et al. 2002, Friedlingstein et al. 2006 and Plattner et al. 2008 estimate the climate sensitivity of the land and ocean carbon inventories using a linear feedback approach. Friedlingstein et al. 2006 assume in their equations 2 and 3 that the carbon uptake by land, ΔC_{land}, and ocean, ΔC_{ocean}, can be approximated by a term which is linear in the change of the global mean atmospheric CO_2 concentration, ΔC_{atm}, plus a term which is linear in the change of global mean atmospheric surface temperature, ΔT:

$$\Delta \mathrm{C_{land}} + \Delta \mathrm{C_{ocean}} = (\beta_{\mathrm{land}} + \beta_{\mathrm{ocean}}) \cdot \Delta \mathrm{C_{atm}} + (\gamma_{\mathrm{land}} + \gamma_{\mathrm{ocean}}) \cdot \Delta \mathrm{T}. \tag{1}$$

β_{land} and β_{ocean} denote the sensitivities of land and ocean carbon storage to CO_2 in GtC per ppm. γ_{land} and γ_{ocean} denote the sensitivities to climate change in units of GtC per $^\circ$C. Together with the mass balance of the volcanic only simulations:

$$\Delta \mathrm{C_{atm}} \cdot 2.123\ \mathrm{GtC\ ppm^{-1}} + \Delta \mathrm{C_{land}} + \Delta \mathrm{C_{ocean}} = 0, \tag{2}$$

equation 1 can be combined to read:

$$\gamma_{\text{land}} + \gamma_{\text{ocean}} = -\frac{\Delta C_{\text{atm}}}{\Delta T} \cdot (2.123 \text{ GtC ppm}^{-1} + \beta_{\text{land}} + \beta_{\text{ocean}}). \quad (3)$$

Adapting this approach for our volcano experiments would require a radiatively uncoupled simulation (aerosols and CO_2 do not influence radiation) with CO_2 prescribed from the fully coupled simulation to estimate β's. The approach by Dufresne et al. 2002 and Friedlingstein et al. 2006 is specifically tailored for 21st century simulations with a dominant influence of anthropogenic carbon emissions on the atmospheric CO_2 increase. However, the approach cannot be readily applied to estimate the CO_2-climate sensitivity from observations or proxy reconstructions as the β factors cannot be easily derived from observational data within the required precision.

Scheffer et al. 2006, Frank et al., 2010 and Joos and Prentice 2004 used an alternative approach to exploit information from the last millennium paleo-records of atmospheric CO_2 and temperature. They estimate the sensitivity as regression or ratio between changes in atmospheric CO_2 and temperature (termed α in Scheffer et al. 2006 and γ in Frank et al., 2010 as:

$$\gamma_{\text{Frank}} = \alpha_{\text{Scheffer}} = \frac{\Delta C_{\text{atm}}}{\Delta T}. \quad (4)$$

This approach is suitable to estimate the sensitivity directly from the CO_2 and temperature records. Here, we followed this second, observation-based approach. However,

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a regression as applied in the studies by Scheffer et al. 2006 and Frank et al., 2010 is not meaningful in the context of our volcanic simulations as the evolution of CO_2 and temperature is different on the multi-annual time scales of the simulations. Instead, we average the signals over time and estimate γ as the ratio between the temporal global mean atmospheric CO_2 change and the temporal global mean atmospheric surface temperature change:

$$\gamma_{\text{this study}} = \frac{\Delta\overline{C_{\text{atm}}}}{\Delta\overline{T}}. \tag{5}$$

The above equation is evaluated for different averaging periods: 5, 10, 15, 20 yr after the eruption. We also computed sensitivities estimates using peak values ($\gamma = \Delta C_{\text{atm,peak}}/\Delta T_{\text{peak}}$), but this does not alter our conclusions (not shown).

γ is also computed from our CSM1.4 simulations over the industrial period and the 21st century consistent with the above approach. In detail, γ are obtained by calculating temperature and CO_2 differences between a simulation with warming and a simulation without warming and estimating the regression of residual time-series. The temperature and CO_2 differences have first been smoothed with a 10-yr running mean before calculating regression coefficients (Roy et al., 2011), as we are interested in isolating the long-term trends in CO_2 storage for this century scale simulations. "

For further details, please see answers to specific points below.

Specific comments

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1. **p. 2964, lines 27-28: Please state clearly how the conversion factor is used. Also, what are its units.**
   We deleted this sentence as it causes confusion. Additionally, we completely changed the description of the volcanic forcing in the method section (see comments to point 1 of Reviewer 2).

2. **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.**
   We agree with the reviewer and we deleted the references as requested. However, as shown in Fig. 5b, the NCAR CSM1.4-carbon model shows increased temperatures over Eurasia even when averaged over the first five years in the Pin.10x case. When averaged over the first winter (DJF) in the Pin.10x case, the warming pattern over Eurasia is statistically significant (not shown).

3. **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.

We agree with the reviewer that the explanation of the analysis was confusing. We addressed the issues raised by the reviewers by reformulating the text and pointed out the difference between our approach (which follow the approach used by Frank et al. 2010 and Scheffer et al. 2006) and the approach used by Friedlingstein et al. 2006. Please see specific reply above. Due to limiting computational resource, we are not able to run an additional experiment with fixed BGC CO2. We further changed 'cumulative' to 'temporal mean' to avoid confusion.

4. **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.**

   Freshwater fluxes in the NCAR CSM1.4-carbon model are only applied to alkalinity and DIC. We changed the calculation of $sDIC_{bio}$ and $sDIC_{res}$, and replaced all figures accordingly. The conclusions are not affected by this.

5. **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.**

   This is a very interesting and important point. We like the idea of an additional uncoupled simulation with CO_2 prescribed from the coupled volcanic simulation. However, we are currently not able to run this simulation due to computing resource limits. Instead, we recalculated the radiative forcing due to the atmospheric CO_2 decrease by taking the changes in atmospheric CO_2 at the end of the simulations (2 ppm for the Pin.10x case after 20 years) and clearly stated that an additional simulation would be needed:

"The decadal-scale negative perturbation in atmospheric CO_2 cause radiative forcing anomalies which influences the temperature evolution. This is superimposed to the short-lived radiative perturbation by sulphate aerosols. To accurately quantify the additional radiative forcing, additional uncoupled simulations with fixed CO_2 (CO_2 has no radiative effect) prescribed from the coupled volcanic simulation, would be necessary. The temperature difference between the new experiments and the performed experiments would yield the climate impact of the CO_2 perturbation. However, as a first guess, a 2 ppm decrease as simulated at the end of the simulation in the Pin.10x case gives a radiative forcing of -0.15 W m^{-2} (5.35 W m^{-2} $\cdot$ ln(284 ppm/282 ppm)) and an equilibrium global mean surface temperature change of -0.02°C (0.04 W m^{-2} $\cdot$ 2°C $\cdot$ (3.7 W $m^{-2})^{-1}$) when applying the climate sensitivity of the NCAR model. This is very small, but consecutive eruptions have the potential to leave an integrated signal in the ocean as found in earlier studies (Church et al. 2005; Frolicher et al. 2009; Stenchikov et al. 2009) and further discussed below."

The discussion has changed accordingly: *"Carbon uptake by the land and ocean causes a reduction in atmospheric CO_2 and a small negative radiative forcing by CO_2. This amplification is very small for the NCAR CSM1.4-carbon model, but could potentially be larger in other Earth System Models or in reality. "*

6. **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.**

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We agree with the reviewer and we changed the sentence accordingly:
"Interestingly, atmospheric CO_2 increases and the land carbon inventory decreases in the first couple of years in the Pin.100x."

7. **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.**
   As discussed in point 5, the possible feedback is indeed very small. We changed the sentence to:
   "Thus, very large volcanic eruptions produce small additional radiative forcing through lowered CO_2, which perturb the Earth System on longer time scales than the pure atmospheric residence time of the volcanic aerosols."
   Furthermore, we changed the abstract to:
   "The multi-decadal decrease in atmospheric CO_2 yields a small additional radiative forcing that amplifies the cooling and perturbs the Earth System on longer time scales than the atmospheric residence time of volcanic aerosols."
   We also reduced the size of the sign showing the small feedback in Fig. 13 (now Fig. 14) and we replaced the arrow with a dashed arrow to highlight that it is a small feedback. 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."

8. **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.**

We changed the paragraph to make clear that there is a non-linear relationship between atmospheric CO_2 concentration and net surface solar flux:

"In a first attempt, the scaling of the peak perturbations of the climate and carbon cycle is assessed with respect to the volcanic strength (Fig. 2). There is a non-linear relationship between the peak perturbations in global mean surface temperature and atmospheric CO_2 and the perturbation in global mean net surface solar forcing (solid lines in Fig. 2a). The temperature perturbation per unit change in solar flux increases with the magnitude of the eruption, whereas the sensitivity of atmospheric CO_2 to changes in solar flux becomes smaller with increasing eruption strength (Fig. 2a). This difference in sensitivity between temperature and CO_2 has implications for γ as discussed below."

9. **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.**
   We have changed to sentence accordingly:
   "As expected from the Beer-Lambert law, global mean shortwave surface radiation does not linearly scale with visible optical depth and the global perturbations in surface temperature and CO_2 scale with the log of the VOD perturbation (dashed lines in Fig. 2a)."

10. **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 and 3d? For readers not used to seeing cumulative changes in state variables, like this reviewer, it would be useful to make this connection.**
    We added this connection to the caption of Fig. 3a:
    "(a) Simulated cumulative temporal mean changes in atmospheric CO_2 vs. temporal mean changes in atmospheric surface temperature for different Pinatubo scalings. The point values leading to the curves are time integrals of the data in Fig. 1c and Fig. 1d. Note that the ratio of the cumulative values equals the ratio

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of time averaged values and thus climate-carbon sensitivity γ defined by equation 5 and shown in panel b; the larger the slope of the curve the larger γ. (b) Time-series of estimates of the climate-carbon cycle sensitivity γ derived from the Pinatubo scaling experiments."

11. **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.**
    See comments above.

12. **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.**
    As we focus on the atmospheric CO_2 response after volcanic eruption in this paper, the changes in cloud cover are not investigated in detail. The investigation of cloud cover changes and associated solar flux changes is beyond the scope of this paper. Therefore, we skipped this sentence.

13. **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.**
    We agree with the reviewer and highlight in the text only regions where precipitation changes due to the Pin.10x eruption are significant different from natural variability.
    "On regional scale, precipitation reduction exceeds natural variability in many tropical and mid-latitude regions, such as Indonesia, the northern part of South

America, the western part of North America and central Europe, and over the tropical Pacific Ocean in the latitudinal bands around 20°N to 5°N and 5°S to 20°S (Fig. 5c), and parts of the Southern Ocean around 40°S to 50°S.”.
Furthermore, we changed following sentence:
“Global mean precipitation is reduced by up to 0.5 mm d^{-1} (17%) in the first 5 years parallel to global mean atmospheric surface temperature in the Pin.10x case compared to the control simulation.”.

14. **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.**
We skipped the sentence. However, we stated on p.2975 on l.1-2 that the simulated precipitation anomalies are highly uncertain and that the simulated changes in precipitation strongly depend on the model. Furthermore, we highlighted the precipitation biases in the NCAR CSM1.4-carbon on p.2965 on l. 19-23. Therefore, it should be clear that the simulated precipitation changes of the NCAR CSM1.4-carbon model are uncertain and model-dependent.

15. **p. 2974, lines 14-20: Given the low resolution of the model, how credible is this regional difference?**
We agree with the reviewer that the simulated precipitation changes are uncertain and that the model may miss some important aspects due to the low resolution to successfully simulated precipitation pattern and changes. However, we clearly stated these possible caveats on p.2965 on l. 19-23 and on p.2975 on l.1-2. Robock et al. (2008) also used a low resolution GCM model (the National Aeronautics and Space Administration Goddard Institute for Space Studies ModelE) with a 4° latitude by 5° longitude horizontal resolution. Therefore, it is worthwhile to indicate that different models could lead to different results.

16. **p. 2974, line 28: This claim appears to be unfounded, since there is no**

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apparent metric of success.
We changed this sentence to:
"In conclusion, the difference patterns show that the temperature changes on the Earth surface as a consequence of volcanic eruptions has been reasonably simulated in comparison with data-based reconstructions (Robock et al. 1992)"

17. **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.**
We do not agree with the overall assessment of reviewer 1. We clearly state on p. 2975 that "Volcanic-induced cooling causes soil temperature and soil overturning rates to decrease. The decrease in soil overturning and associated increase in soil carbon overcompensates the decrease in litter flux (carbon flux from vegetation pools to soil pools) due to the NPP decrease, resulting in higher soil carbon inventories." We also state, that an interpretation of R_h changes is complicated due to different influencing factors and processes with very different time scales. However, we included, as suggested, the regression of respiration against soil moisture and temperature in Fig. 7, and changed the time period of the regression from 240 months to 60 months (Fig. 7). A discussion of the new figures is included in the description of the terrestrial biosphere changes (section 3.4):
"On regional scales, the increase in NEP occurs mainly in the tropics as illustrated for the Pin.10x case in Fig. 6b. The total carbon storage in soil and vegetation is increased in most of the tropical regions; changes elsewhere are small. Vegeta-

tion carbon changes in the tropics (Fig. 6c,f) are opposite in regions with negative soil moisture changes (and negative precipitation anomalies) and in regions with small and positive soil moisture changes (Fig. 5c,d). Drying and soil moisture decrease lead to a decrease in NPP in northwestern South America, in parts of East Africa and in Indonesia, and thus to a decrease in vegetation carbon pools (Fig.6c,f). A significant connection between soil moisture and NPP is simulated in these regions as the regression pattern in Fig. 7a shows. Besides soil moisture decrease (Fig. 7b), volcanic-induced cooling causes soil temperature and soil overturning rates to decrease (Fig. 7d). The decrease in soil overturning and associated increase in soil carbon (Fig. 6e) overcompensates the decrease in litter flux (carbon flux from vegetation pools to soil pools) due to the NPP decrease, resulting in higher soil carbon inventories.

In tropical regions (e.g. Northeast South America) where small or positive soil moisture changes are simulated after the volcanic eruption, the NPP and the vegetation carbon pool increase due to the volcanic-induced cooling. In contrast to the drying regions discussed above, cooling leads to an increase in NPP at these locations. These are regions where NPP and temperature changes are strongly related in the tropics (Fig. 7c). Soil overturning rates are also reduced, which leads to an increase in soil carbon content. Thus, soil and vegetation carbon pools increase in regions with a positive soil moisture perturbation.

In mid-latitudes of North America and Europe, cooling and soil moisture changes lead to a decrease in NPP (Fig. 7a,c) and vegetation carbon pools, and thus to a decrease in litter flux. The cooling also reduces the soil overturning rates (Fig. 7d) and increases soil carbon content. This effect dominates over reduced litter input. Totally, NEP changes are about zero, as vegetation carbon changes and soil carbon changes compensate each other. "

The time period of the regression has been included in the caption of Fig. 7:
"The regression has been calculated over the first 5 years."

We also included in section 3.4:
"A full detailed analysis of changes in respiration is beyond the scope of the manuscript."

18. **p. 2977, line 4: Please state in what way the ocean carbon cycle may play an important role.**
We have changed the sentence to:
"Besides the terrestrial changes the ocean carbon cycle plays an important role in regulating atmospheric CO_2 after explosive volcanic eruptions."

19. **p. 2977, lines 8-9: The pH map in Fig 8d does not match the text. The increase in pH is nearly global, and the largest increases in pH occur in regions where sDIC doesn't change, or decreases.**
This important point has been raised by both reviewers. We included a separate paragraph to analyze changes in surface pH and to discuss the mechanisms behind these changes. Furthermore, we included a panel showing alkalinity changes in Fig. 8 and we included a new Fig. 9 showing pH changes and pH changes caused by temperature changes only.

"Interestingly, significant increases in pH are simulated almost everywhere (Fig. 9a), although sDIC and SST do not change uniformly (Fig. 8a,g). In order to quantify the driving mechanisms of the pH changes, we used the unperturbed SST from the control simulation when calculating the pH changes to distinguish the pH changes caused by SST changes alone ($\Delta \mathrm{pH}_{\mathrm{SST}}$) and pH changes due to other drivers (DIC, ALK, SSS). The pH changes in low latitudes are mainly driven by the decrease in SST (Fig. 9b), somewhat damped by the increase in sDIC (Fig. 9c, Fig. 8a). A reduction of SST leads to changes in the dissociation constants, which change the partitioning of total DIC among the various carbon species. In high latitudes (e.g. between 40°S and 60°S) changes in pH are

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mainly driven by changes in DIC and/or ALK (Fig. 9c, Fig. 8a,f). SST changes there are small. "

20. **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 nDelta $\mathrm{sDIC_{bio}}$ and nDelta $\mathrm{sDIC_{res}}$. The patterns of change in air-sea carbon flux differ considerably from the change in SST. Please explain why the difference occurs.**
We included patterns showing changes in $\mathrm{sDIC_{bio}}$ and $\mathrm{sDIC_{res}}$ and we discussed them accordingly:
"In the first 5 years after the volcanic eruption, the largest increases in sDIC are simulated in low-latitude shallow waters between 40° N and 40° S (Fig. 8a, 10a,b). The increase in sDIC there is mainly caused by an increase in $\mathrm{sDIC_{res}}$ (Fig. 8c) due to volcanic-induced cooling (Fig. 5b, 8g, 10g,h) and an associated increase in the solubility of CO_2 which enhances air-sea carbon flux (Fig. 8d). Less grid cells are statistically significant for air-sea carbon fluxes than for $\mathrm{sDIC_{res}}$ changes due to the high variability of air-sea carbon fluxes in the control simulation. The enhanced carbon storage by the ocean in the first years after the eruption are also shown by the positive anomalies in $\mathrm{sDIC_{res}}$ in low-latitude shallow waters in the Atlantic as well as the Indo-Pacific Ocean (Fig. 10e,f). In southern high latitudes between 40° S and 60° S, decreases in $\mathrm{sDIC_{bio}}$ lead to a decrease in sDIC there (Fig. 8b,10c,d)."

21. **p. 2977, line 12: Is an increase of 4% in POC export statistically significant?**
Yes. The increase of 4% averaged over the first 5 years is statistically significant. For your information, one standard deviation of deseasonalized monthly POC export production in the control simulation is 0.16 Gt C/yr. We changed the sentence to:

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"Global integrated export of particulate organic carbon increases significantly by 4% (0.37 Gt C yr^{-1}) averaged over the first 5 years after the Pin.10x eruption."

22. **p. 2977, line 23: How do you define 'North Atlantic'?**
    We modified the sentence to *"The largest increases in export production are simulated in the eastern North Atlantic between 30°N and 60°N where the export production increases locally by up to 30 to 40 g C m^{-2} yr^{-1}"*

23. **p. 2977, lines 24-25: If POC export didn't change much elsewhere, what causes the increase in sDIC$_{bio}$ between 10N and 20N in the Atlantic and the decrease between 30S and 10S in the Atlantic and between 60S and 40S in the Indo-Pacific?**
    In other regions, changes in the export production can also be significant. Therefore, we changed the sentence to:
    "In other regions, smaller changes are simulated as is also the case in global warming simulations (Steinacher et al. 2010)."

24. **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.**
    The panel for the Atlantic Ocean shows the mean over the band of latitudes from 90°N-78°S and the Indo-Pacific shows the mean over the band of latitudes from 65°N-78°S. We have added the band of latitudes to the labels of the plot to clearly state the latitudinal band over which we have calculated the average. Furthermore, we changed the caption to:
    "Hovmöller diagram of monthly mean sDIC, $\mathrm{sDIC_{bio}}$, $\mathrm{sDIC_{res}}$ and potential temperature differences between the Pin.10x case and the control simulation in the Atlantic (including the Arctic Ocean) and the Indo-Pacific Ocean at different depths.

The volcanic eruption starts in year 0.5."
We added the same text to the caption of Fig. 9.

25. **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.**
See comments above.

26. **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.**
We agree with the reviewer and deleted the sentence. As the atmospheric CO_2 decrease for a Pinatubo-like perturbation also strongly depends on the initial conditions, and not only on the pCO_2 in the atmosphere, a suite of ensemble simulations would be needed to quantify the response. Therefore, the sentence has been deleted.

27. **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.**
We do not change the color-bar. Using red color for dryer conditions and blue color for wetter conditions is a common practice and the readers are normally used to read precipitation and soil moisture changes in this way.

28. **Figures 9 and 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.**
Done.

29. **Figure 10: The caption initially states that this is a global mean, which**

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appears to be an incorrect statement.
We changed the caption to:
"Hovmöller diagram of monthly mean sDIC, $\mathrm{sDIC_{bio}}$, $\mathrm{sDIC_{res}}$ *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 at time 0.5."*
Furthermore, we indicate the latitudes for the Atlantic and the Indo-Pacific in the figure.

Technical corrections

1. **p. 2967, line 12: should 'land and carbon storage' be 'land and ocean carbon storage'**
   Done.
2. **Figure 1: Enlarge text**
   Done.
3. **Figure 2: Enlarge text**
   Done.
4. **Figure 3: Enlarge text in legends, add units to panel a**
   Done.
5. **references: The lead author's name of Le Quéré et al. 2009 is listed incorrectly.**
   Changed.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/8/C1765/2011/bgd-8-C1765-2011-supplement.pdf

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