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Earth System Dynamics Discussions

Interactive comment on "Volcano impacts on climate and biogeochemistry in a coupled carbon-climate model" *by* D. Rothenberg et al.

D. Rothenberg et al.

darothen@mit.edu

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Please find in this document responses to issues/commentary raised in the two referee comments and one short comment to our manuscript. The responses are grouped by each review, and the referee's comment appears in bold, followed by a statement of how we address this comment in the revised manuscript.

Before addressing the reviews, we would like to thank all the reviewers for their constructive feedback during the open discussion period. This feedback has helped highlight inconsistencies that crept into the manuscript during the revision and editing process, and has greatly improved its clarity and quality.

Short Comment, Thomas Frölicher

We recently published a paper in Biogeosciences (2011) entitled "Sensitivity of atmospheric CO2 and climate to explosive volcanic eruptions", where we describe the sensitivity (magnitude and timescale) of the land and ocean carbon cycle response to the size of volcanic eruptions. For this study we used the precursor NCAR model CSM1.4-carbon. The authors may not be aware of this paper. However, it would be very interesting to know why the NCAR CCSM3 shows a very small terrestrial carbon cycle response after the Pinatubo, whereas the CSM1.4-carbon model shows a larger terrestrial response, despite the fact that both models show similar (at least in sign) temperature and precipitation responses over South America, and both models seem to have a relative small climate feedback onto carbon.

We thank Dr. Frölicher for bringing to our attention Frölicher et al (2011). We previously touched on the topic of that paper on page 291 (lines 8-12) of our Discussion paper, where we note the modeled atmospheric CO2 response following Pinatubo was very weak. The result in Frölicher et al corroborates our suspicion that the modeled response is too weak and potentially represents a serious error in our model. The larger terrestrial response in the CSM1.4-carbon compared to our model's response is a potential explanation for why the atmospheric CO2 response in our model is much weaker, but as we acknowledge in Section 3.4 of the Discussion paper on pages 296-297, we have not entirely studied the role of the oceanic carbon cycle in this response, which may also be important (Watson, 1997). The CCSM3 model has a close to zero carbon response to climate (Thornton et al., 2009), while the CSM1.4 carbon model has a response more similar to other models: strong and positive (Friedlingstein et al., 2006), so the difference in carbon responses is consistent with that.

An acknowledgment of Frölicher et al (2011) was added to Section 3.2, clarifying that our modeled atmospheric CO2 response and terrestrial response is weaker than other models, probably due to the inclusion of very strong nitrogen colimitation, as previously shown (e.g. Thornton et al., 2009).

Review, Anonymous Referee

It should be emphasized here that the similarity between the results from 'volcanic-control' and 'no control' only holds for global-mean results. As the authors presented in the main text, the results between these two methods differ substantially on regional scales.

The sentence on lines 19-22 in the abstract was changed to clarify that, ". . .the method is found to produce similar results in the global average".

"The strength of land and ocean sinks of CO2 are not increasing along with rising anthropogenic emissions (Le Quere et al., 2009; Sarmiento et al., 2010) as evidenced by an increase in atmospheric CO2 levels." The meaning of this sentence is not clear. Please rephrase.

Sentence re-written to clarify that the land and ocean sinks are not increasing in capacity at the same rate as which the source of anthropogenic CO2 is increasing.

"Ammann et al. (2003) scaled the peak aerosol depth for 20th century eruptions by looking at previous estimates of peak aerosol loading . . .." This paragraph does not seem belong to Model description

The volcanic aerosols in the model are the major forcing agent whose response is studied in this work, and we feel it is important to highlight how they are handled in the model. We feel this is an important detail, and because we did not alter the time varying volcanic aerosol forcing dataset, we believe that this information is best suited for the model description.

"This mean anomaly between volcanic runs and control runs was compared to the set of anomaly time series for each individual eruption, averaged over the three pairs (volcano-control) of ensemble members." Please explain what is the purpose of comparing the mean anomaly with the anomaly of each individual eruption? If I understand correctly, the mean anomaly is calculated from the

four-year anomaly mean starting from the month of each eruption. Is there any particular reason to choose four years as the averaging periods?

To our knowledge, this is the first paper that attempts to study a range of volcanic eruptions and their impacts on climate and biogeochemistry, whereas previous papers have focused on a single eruption (Jones and Cox, 2001; Frölicher et al, 2011). The range of volcanic eruptions spanned different explosivities and latitudes, and comparing the mean anomaly to the anomaly of each eruption is intended to study the range of modeled responses. This point was clarified in Section 2.3.1 of the Discussion paper.

We believe that the "four year average" is a miscommunication on our part. The "no-control" anomalies used the two years prior to the eruption merely to identify the seasonal cycle and remove it to facilitate comparison between eruptions which occurred at different times of the year. The four-year figure seems to come from the compositing we used to produce Figures 8a-i; only four-year timeseries are illustrated here because the response is no longer statistically significant after this duration of time following any individual eruption.

"For each month in the years following the eruption, an anomaly is computed based on the two years previous to the eruption to compute the deviation from the average seasonal cycle". Is there any particular reason to choose the previous two years as the periods to calculate anomaly? How will the results change if the previous one or three years were used?

We used the previous two years in order to minimize influences from the transient 20th century warming in the simulations and to extract an "average seasonal cycle." It is difficult to extend to much more than two years due to potentially seeing influence from closely-spaced (in time) volcanic eruptions; on the other hand, we are hesitant to use a single year since inter-annual variability might influence the precise seasonal cycle and contaminate the signal from the volcanic eruption. However, repeating the analysis with these different amounts of years does not alter the results previously obtained; this has been indicated in Section 2.3.1 of the manuscript.

The authors used the study of Shindell et al. (2004) as a benchmark to compare modeled temperature with observations. Is it possible to reproduce and show the results of Shindell et al. (2004) in the temperature figure? Otherwise, the readers will have no clue of how 'observed' temperature anomaly looks like. Likewise, it would be useful to reproduce the precipitation results of Trenberth and Dai (2007), which is used as a benchmark for the comparison of modeled precipitation.

It is unfortunate that it is difficult for the reviewer to find these papers, however they are widely available, and it is difficult to reproduce the figures themselves. We have requested the data in the figure from the authors in order to reproduce alongside our own results, but were unable to obtain it in time for this resubmission.

"These changes in uptake of atmospheric CO2 motivate an analysis of the modeled carbon cycle and terrestrial biosphere" What is the role of the ocean carbon cycle here?

We focused primarily on the terrestrial biosphere in this paper, as it is much larger than the ocean response. To capture the full ocean response, including nutrients added in the ash, will be the focus of future work. This sentence has been clarified to emphasize our focus.

"The decreases in the modeled gross primary production are associated with anomalous decreases in both surface temperature and precipitation, and potentially increases in diffuse radiation (Fig. 5b) in both the global average and in the Amazon." I have two questions here: First, how are the interactions between diffuse radiation, plant growth, and volcanic eruption treated in the model? Second, how does an increase in diffuse radiation lead to a decrease in gross primary production?

An increase in diffuse radiation following the eruption of Pinatubo has been associated with an enhanced terrestrial carbon sink, possibly due to enhanced photosynthesis (Mercado et al, 2009). Our original sentence was corrected to clarify that our modeled response is different than what is expected, possibly due to the suppression of respiration during the cooler and drier period following the eruption (particularly in the Amazon). This is in contrast to the results of Jones and Cox (2001) whose modeled response was to increase gross primary production along with an increase in precipitation, although this is at odds with the observed precipitation response in the region following the eruption (Trenberth and Dai, 2007)

The land model incorporates downward direct and diffuse radiation in calculating what is absorbed in the canopy. Gross primary production increase with a larger diffuse fraction in the model (Thornton and Zimmerman, 2007). This point has been added to paragraph 2 of Section 2.1 of the manuscript.

I am not sure what the main purpose is to examine the averaged responses to volcanic eruptions. What can we learn from the averaged responses that cannot be learned from the response to individual volcano eruptions? For example, as the authors noticed, these eruptions occur at different times of the year, which would impact growing season differently. Therefore, the averaged response in the land carbon cycle is damped compared to the response to individual eruptions. Do we learn anything new here?

To goal of looking at multiple volcanoes is to see if the response to volcanoes is always uniform, or if we expect different responses. Not all volcanoes will be identical to Pinatubo. Thus there is value in looking to see if other volcanoes (which occur at different times and places) appear to impact climate and the carbon cycle differently.

Fig. 1: Legend "While prescribed CO2 is used for radiation in the model, the CO2 in the runs plotted here is fully interactive." If the model calculates atmospheric CO2 interactively, why does the model use prescribed CO2 in the radiation cal-

culation?

The caption for Figure 1 is incorrect; the model uses fully prognostic CO2, and this prognostic CO2 interacts with the radiation without any prescription. The caption was altered to indicate this.

Review, Chris Jones

General Comments

For example, in that study we found the Amazonian and European regions to be areas with a strong signal and focussed some analysis there. In your study you might find other regions require more in depth analysis – for example the region you use over Europe misses an area in SW Europe (specifically over Spain) which looks interesting in your figures – there is a clear T and P response here. Alternatively you might find a zonal mean plot of response useful – especially to replace figure 10 which seems a little redundant given figure 7.

We previously studied several regions around the globe, including a larger swath of Europe, sub-Saharan Africa, zonal bands including the high-latitudes and the tropics, and North America. The response was consistently stronger in the tropics – especially in the Amazon – as can be seen in Figure 9; for this reason, and for comparing with Jones and Cox (2001), we focused primarily on the Amazon in the manuscript. However, we believe the recommended plot of zonal response clarifies this result more clearly than our current Figure 10; the response to both Pinatubo and the mean of several eruptions is much stronger in the Southern hemisphere tropics than elsewhere, and we have replaced Figure 10 with such a plot of the zonal response (Figure 1 at the end of this interactive comment). Furthermore, we have clarified in the text that several regions beyond what is depicted in the figures were studied but found to respond in lesser magnitude than the Amazon.

I also wondered about the statistical significance of your results – especially

your technique for getting the signal from a single run rather than using the control run. This would indeed be a valuable thing to do, but by subtracting the 2 years before the eruption you risk subtracting a period with an exactly opposite ENSO phase. E.g. for any period, even without external forcing, I would expect there to be quite large differences between a 2 year period and the subsequent 2 year period – because this is precisely the period of ENSO variability. You should at least quantify the sensitivity of your results to the choice of period used – e.g. what if you use a 4-year period prior to the eruption to compare the signal?

We studied how the choice of years used to compute seasonal cycle affects the signal, and there were not any significant impacts. A remark was added in Section 2.3.1 to indicate this, although including additional figures to illustrate this does not seem necessary.

More generally, it would be great to see a larger ensemble. I would have some concerns that 3 simulations may not be enough. For example the 1920s CO2 excursion you see is likely just long-term variability, and the fact this is as big as the volcanic signal you see implies you might not have a big enough ensemble to be able to reliably see the signal you look for. Instead of having to perform many 130-year simulations, have you considered an approach, as in Jones and Cox 2001, of many short (10 year) simulations spawned from within the pre-industrial control run. These would rapidly give you a much bigger sample size and have no issues of having to remove the concurrent climate change signal.

A larger ensemble might improve the robustness of the signal in our modeled response to large eruptions, but we feel that the initial ensemble size with 3 pairs of members is still enough to ascertain the response. We had not previously considered using an ensemble of shorter simulations to study the response – our goal in this manuscript was to look for broad consistencies across multiple volcanic eruptions in contrast to much of the literature which focuses on the single eruption of Pinatubo (Jones and Cox, 2001; Frölicher, 2011). Such an ensemble study seems beyond the original scope of this paper.

Overall, you conclude a smaller response in your model. One unique aspect you have is coupled nitrogen cycle. How do you think this might affect your results? On climate change timescales we know this reduces the sensitivity of land carbon uptake to climate change (by liberating N and stimulating growth). Could the same be true on shorter timescales where volcanic cooling REDUCES nitrogen mineralisation and offsets some of the GPP change you might get otherwise? Have you got a no-nitrogen version of these runs? That would be a really nice (and new) result if the N-cycle affects your sensitivity to volcanic forcing.

Unfortunately, we do not have a set of runs controlling for the nitrogen cycle coupled to the land model, so we cannot explicitly study how the N-cycle affects our sensitivity to volcanic forcing. It has previously been shown that the inclusion of C-N interaction in this model produces a smaller positive feedback, and includes the possibility of a negative one (Thornton et al, 2009). This helps to explain the smaller response observed in our model as compared to the one seen in the CSM1.4-carbon (Frölicher, 2011) and the HadCM3 (Jones and Cox, 2001).

Since the carbon-climate feedbacks from volcanoes (1-2 years) and from climate change (longer time scales) are similar, this suggests that the different time scales don't matter so much for carbon-climate feedbacks in this model, which is rather interesting. We add some text to make this point more clear in the conclusions.

- your use of the phrase "control run" confused me at first as this is often reserved to mean "a zero-forcing run" where everything is (hopefully) constant in time. Whereas your use here means for a transient simulation of the 20th century but without volcanic eruptions. Although this isn't a wrong use of the phrase I wonder if it would be less ambiguous to specifically call this "NOVOLC" or something rather than control

The nomenclature "control" here reflects runs with zero volcanic forcing, since this

it is the response to this forcing which we seek to evaluate. We are hesitant to rename these runs "NOVOLC" in order to avoid creating confusion over our "no-control" anomaly analysis, which seeks to estimate the volcanic signal when there are no paired runs available which control for volcanic forcing.

- P282, line 13, I don't think CO2 DECREASED after Pinatubo, but the growth rate dropped (but not to -ve)

The reviewer is correct – the reference indicates that the CO2 growth rate only decreased slightly; atmospheric CO2 levels did not decrease after Pinatubo. This sentence was re-written to reflect this.

Can you be more clear describing how you apply the aerosol forcing? You say the model has prognostic aerosols, but then you also describe that the forcing dataset includes a prescribed lifetime and removal rate. I don't see how the two go together, so can you be more specific if you prescribe the volcanic aerosol amounts, or if you input them as emissions and have a prognostic scheme?

In our simulations, we specifically prescribe volcanic aerosols – including their lifetime and removal rate. All other aerosols are prognostic. Paragraph 2 of Section 2.1, which discusses the volcanic aerosol forcing dataset, was amended to clarify this point.

You very brieïňĆy mention diffuse light as a possible driver of land-carbon changes, but you don't say if this effect is included in your model. You could cite papers by Mercado et al and Angert et al on this topic which could be a significant part of the signal.

The review by an anonymous referee raises a similar point, to which we previously responded in these comments. Section 2.1 of the manuscript was amended to indicate that the diffuse-ration effect is included in our land model.

I found some of the text descriptions confusing – at first it appears contradictory in places. Specifically: page 293 line 13 mentions a "net uptake by land", line 24

mentions “decrease in ïňĆux into the land”. Which is it? Again on p.294, line 3, you mention “small increase in NEP”. So I was left wondering exactly which way the land responds!

The decrease in the flux of carbon into the land mentioned in page 293 isn’t enough to reverse the sign of the response. A source of confusion here is the manner in which we discuss the results in these sentences; the increase in flux of carbon to the atmosphere we mention is only from changes in carbon loss due to fire in the model. Overall, our modeled response is consistent – a decrease in the uptake of carbon by the land – and we have attempted to clarify this point.

As others have found the land signal is much bigger than the ocean. But I wonder if you have looked at the longer term response of the ocean carbon uptake? A recent paper Gregory et al (I think – can’t find ref right now I’m afraid – contact me if you want me to look again) showed volcanic eruptions have a lasting impact on ocean heat content (even over a century timescale) and so I wondered if the same might be true of ocean carbon storage. If you compare the cumulative ocean uptake for your simulations with/without eruptions you might see a non-negligible cumulative effect of volcanoes even if the immediate response is much smaller than on land.

We did not see any signal to indicate any sort of long term ocean carbon storage in our ocean tracers over the time period of our model runs; in light of this, we do not believe that the cumulative ocean storage (on the time scale we are studying) is significant.

Your existing ocean analysis mentions the role of changed dust – can you say exactly how it changes (and why)? more/less dust? Is this because winds change? Or soil moisture? Or what...

We did not look into the source of the change in dust in our results; this is beyond the scope of our study, primarily because the monthly output we used for our analyses is insufficient to analyze the source of these changes. We would need to re-run the

ensemble with at least daily output to identify this.

- related: any impact of dust on land uptake (Fe fertilisation of land plants?)

In our model (and as far as we know in the real world) there is no impact of Fe fertilization onto land plants. Dust can provide phosphorus to tropical forests, which on the 1000 years time scale could be important (e.g. Chadwick et al., 1999).

a few times you say that the observed precip signal is not consistent with the Jones and Cox study. Remember that the Pinatubo eruption coincided with an El Nino event which would have had a precip reduction effect over Amazonia which we would not expect to be in the simulations (as we can’t force the model to have the same timing of El Nino). So although a valid comment, not necessarily a like-for-like comparison. You would need to look at the sub-set of simulations which had a comparable ENSO phase to the observations

The first comparison of this result is on page 292 of the Discussion paper on line 18. We added a clarifying sentence, indicating the caveat that the sign of the precipitation response could be influenced by the phase of ENSO in the model, as well as in the conclusions.

Figure 1 – can you use heavier or different colour lines to mark the 5 eruptions you focus on

A triangle glyph on the x-axis to indicate the members of the 5-eruption subset was added to the figure.

- Figure 1 – can you clarify the sign of ocean/land ïňĆuxes? In the figure as it stands ïňĆuxes TO both land and ocean decrease after eruptions. Is this right? If so then atmospheric CO2 would go UP

The caption on Figure 1 now clarifies that in this particular figure, a negative surface flux of CO2 indicates a flux from the atmosphere to the land/ocean, which is opposite the sign convention used in subsequent plots.

Figure 3 – strange choice of scale on your axes (why choose 0.14, 0.41, 0.68...?)

Figure 3 is included to facilitate comparison to Shindell et al, 2004 and the observations presented in that paper; the bounds on the colorscale are identical to those used on all the plots of warming/cooling surface air temperature patterns following volcanic eruptions.

- Figure 7 – when you show a standard deviation I wasn't clear whether this is (a) the standard deviation over the ensemble of the 3 mean values for this region, or (b) the standard deviation on a gridpoint-by-gridpoint level within that region. Can you clarify?

The standard deviation was computed on a gridpoint-by-gridpoint level within that region; this was clarified in the figure caption.

- Figure 8 – can you make units consistent (some have g/m2, some are totals in GtC)

The labeling on the y-axis for the "Sfc Flux CO2 – Land/Ocean" sub-figures had incorrect units. The analysis was already in GtC and merely labeled incorrectly.

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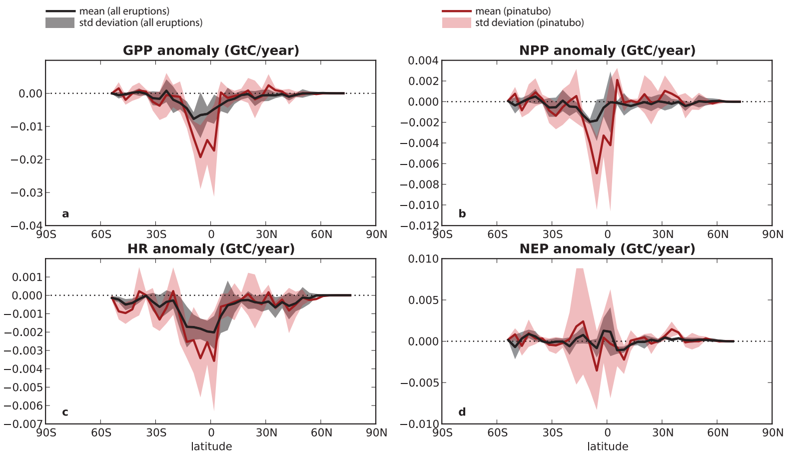

Fig. 1. Zonal mean anomalies in factors contributing to the uptake of CO2 from the atmosphere, computed by suing the volcano-control anomaly method for Pinatubo (red) and the mean of several eruptions (black)