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Discussions

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

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Review of Rothenberg et al., "Volcano impacts on climate and biogeochemistry in a coupled carbon-climate model"

This manuscript deals with simulations of the CCSM3 coupled climate-carbon cycle simulations of the 20th century with specific focus on the impact of major volcanic eruptions on the carbon cycle system. The manuscript is presented as an evaluation of the model rather than an exploration of the processes behind observed carbon cycle sensitivity to volcanic forcing, but does also offer some insight into this observed response. I found the manuscript clearly written and easy to follow.

Overall I would recommend publication after a few (mainly minor) revisions. I hope

these comments help improve the manuscript.

Chris Jones

A few general comments: The study clearly uses a paper of mine, Jones and Cox 2001, as a template for some of the aspects to evaluate. Whilst this is flattering, I'd recommend that you are not a slave to the precise diagnostics presented in that paper. 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.

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?

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.

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.

More specific comments: - 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.

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

- 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?

- You very briefly 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.

- 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 flux 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!

- 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.

- 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...

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

- 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.

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

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

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

- 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?

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

Interactive comment on Earth Syst. Dynam. Discuss., 3, 279, 2012.