The authors thank Dr. Chris Jones (Referee#1) for the positive and constructive feedbacks toward this study. In addition, Referee#1 have raised some important points with regards to quantifying the change in climate sensitivity and carbon cycle feedback associated with the volcanic forcing, which are now included in the paper. We think that the revised manuscript is now substantially improved compared to its initial version. Below are the detailed responses to each of Referee1's comment:

Ref1: The paper presents novel concepts of general interest to the ESD audience, but my main concern about the manuscript is over the motivation – is this really motivated by wanting to know the response of the carbon cycle to a repeated volcanic eruption throughout the 21st century? (in which case why?) This seems a very hypothetical and unlikely case – why would we want to study it? More likely, your motivation is to use the volcanic forcing as a proxy for geoengineering via stratospheric aerosol injection.

The paper initially was developed to study the impact of volcanic forcing on the future climate and carbon cycle projection using a coupled climate and carbon cycle model. Our analysis focuses on identifying any substantial long-term feedbacks introduced to the system when the volcanic forcing is included. This is important, as no study have looked into potential volcanic feedbacks on the future global carbon cycle system. In the revised manuscript, we have extended the introduction to include more detailed discussions on why this is important and why it can be valuable to the scientific community. In addition to assessing the carbon cycle feedback, since volcanic forcing is a good proxy for geoengineering via stratospheric impact, we decided to have additional discussions on how our study could be relevant for the geoengineering topic. However, since this is not the main focus of the paper, we have toned down the geoengineering discussions in the revised manuscript.
As the referee correctly stated, some of the volcanic scenarios used in this study are unlikely to occur. We therefore decided to add a new experiment into the study, which includes a more plausible eruption-scenario (i.e., Pinatubo-like eruptions with 25-year frequency). With the addition of this experiment, we have further analyzed the role of different frequency and magnitude of volcanic forcing on the global carbon cycle. The paper has now been revised to include this additional experiment.

Ref1: This is fine I think – there is a general requirement to better understand the climate and carbon cycle implications of such schemes. But if this is your motivation then be clear about it up front rather than adding it at the end of the discussion. Secondly, if this is your motivation then why look at periodic forcing such as a 5-yearly Pinatubo eruption? Why not also consider a constant (or smoothly increasing?) level of forcing at a level which would offset incoming SW by about the same amount as GHG radiative forcing. (As an aside, there is a coordinated set of experiments called GEOMIP which might interest you to look at exactly these issues across a multi-model framework).

As stated above, we decided to steer the manuscript more toward the carbon cycle climate sensitivity than the geoengineering. Applying a constant forcing to better study the effect of geoengineering does make sense, and this is something that we will consider in the future if we decide to pursue the geoengineering topic in more details.

Ref1: 1. title. As described above, be clear on your motivation – if this paper is about volcanic forcing as a proxy for geoengineering then why not have a title like "role of stratospheric aerosol injection on future climate/carbon cycle"...

The title is kept as it is (see explanation above).

Ref1: 2. regarding the experimental design of periodic eruptions – are there any dangers of a 5-year repeating forcing accidentally hitting a resonance (e.g. with ENSO)? Have you considered a similar approach but with more randomly timed eruptions?

While such an experiment design is indeed useful and doable, it would require several sets of ensemble simulations to clearly understand the role of the pulse-like volcanic forcing at different times. It is also better to perform such experiment under constant (e.g., preindustrial) forcing, thus it's not contaminated or mixed with the ongoing anthropogenic climate change. At the moment, it is computationally too expensive to do such simulation with a fully coupled Earth system model as in this study. In addition, a study by Frölicher et al. (in preparation) will discuss the role of volcanic forcings on the climate variability such as ENSO when applied at different times of the ENSO cycle.

Ref1: 3. in intro and discussion you could refer to some studies on the climate/carbon consequences of geoengineering by Andy Jones: Jones et al., 2009, JGR, 114; Jones et al, 2010, ACP, 10; Jones et al., 2011, ASL.

We will include the above-mentioned studies in our geoengineering introduction and discussions.

Ref1: 4. just to clarify your experimental design – do you specify a perturbation to stratospheric AOD that subsequently affects SW radiation? Or do you have an interactive aerosol scheme and actually inject aerosols?

In this study, we specify a boundary condition perturbation to the model simulation, which the AOD is changed according to the different scenarios of the volcanic eruptions. In the paper, Figure 1 illustrates the modification in the global aerosol loading applied in each volcanic scenarios, shown as monthly optical depths at 0.55 microns, in the middle of the visible spectrum. This, in turn, alters the simulated SW and LW radiation. We now state this clearer in the "Model description" section of the revised paper.

Ref1: 5. p.40, line 18. How do you know the reduced precip is due to cooler climate? Could it also be due to reduced surface evaporation due to reduced SW (from the perturbed AOD)?

The referee is correct in that there is also a global decrease in the simulated surface evaporation following the volcanic forcing. In addition, the simulated water vapor content also yields a similar pattern as shown in the figures below. The initial statement, which suggested that 'the simulated temperature reduction is responsible for the reduced precipitation', has now been revised accordingly. The revised manuscript now states that the reduction in incoming SW leads to reductions in global water vapor content and evaporation, which in turn alter the global precipitation.

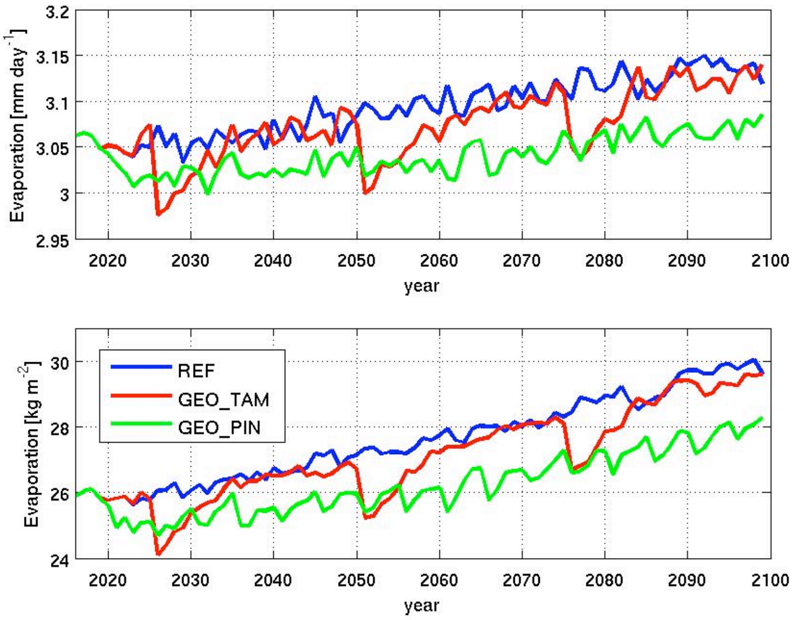

Figure R1. Time series of global mean (top) evaporation [mm/day] and (bottom) water vapor content [kg/m^2] simulated between the different volcanic scenarios.

Ref1: 6. p.141, line 20 "qualitatively" - why not do this quantitatively?

In the revised manuscript we have remove the word "qualitatively", and in the new subsection "Climate sensitivity and carbon feedback" we quantitatively analyze the global carbon feedback and climate change associated with the different aerosol loading as suggested by Referee#1 below.

Ref1: 7. sec. 3.1 – can you discuss the role of diffuse light? Most models don't include this effect yet and given you don't mention it I assume yours doesn't either, but it could be important. See, e.g. Mercado et al, Nature, 2009 or an earlier paper by Anger t et al. This could also explain an increase in NPP following an eruption.

It is correct that the BCM-C model doesn't include the diffuse light as a parameter in the terrestrial carbon cycle. We have therefore included a sentence at the end of the manuscript stating this as one of the model caveats and cited the above references.

Ref1: 8. p.142, line 25-27. How do you know the reduced SW doesn't have an effect? How have you separated this out from the other changes (T, precip, CO2 etc)?

In the revised manuscript, we have removed this sentence. The model simulates the reduction in the incoming SW as a result of increasing aerosol loadings. Hence the SW would certainly affect the terrestrial carbon cycle. However, we think that in order to correctly analyze the role of changing SW alone (without taking into consideration changes in T, precip, etc.), separate set of model simulations would be required.

Ref1: 9. sec 3.2. as with diffuse light for the terrestrial carbon cycle, there are other processes which you lack in the ocean. e.g. what is the role of iron deposition from an eruption? Or other micro-nutrients to the ocean. Are these potentially important? Should they be included?

The referee is correct and that several previous studies have suggested an impact of volcanic dust deposition on the marine biological production. However due to the difficulty and uncertainty in modeling such processes (see below), we think that a proper treatment of the role of dust on marine carbon cycle is beyond the scope of this study. We have included the following text in the revised manuscript:

"Some studies show that the volcanic ash deposition could potentially lead to micro-nutrient fertilization (particularly iron) and enhance marine biological production locally following the short period after the volcanic event (Frogner et al. 2001; Jones and Gislason 2008; Duggen et al. 2010; Watson, 1997). However, the review paper by Duggen et al. (2010) indicates that the scientific community at present lacks a comprehensive understanding of the role of volcanic ash on the marine phytoplankton growth. In addition, difficulties regarding how to correctly model the volcanic dust impact on the marine production also arise from the diversity of particle dust compositions for the different volcanic sources and from the effects of volcanic dust on different phytoplankton species. Also, there are large uncertainties associated with the regional distribution of aerial volcanic dust deposition to the ocean, since this is highly dependent on the location of the eruption event. Finally, potential effects of other toxic trace metals associated with volcanic dust on various phytoplankton species remain poorly understood. Therefore, the analysis shown here simply represents the change in biological production that predominantly take place due to changes in the physical variability."

Ref1: 10. sec3.2. Your discussion that increases in uptake due to lower temperature in GEO_PIN balance decreased uptake due to reduced atmospheric CO2 make sense. This seems reasonable. But it would be interesting if you could estimate the approximate size of these two competing terms – are they big? e.g. if CO2 is roughly 10% lower then this might drive approx 10% less uptake – which is in the region 30-40 GtC by 2100. This would make these two effects relatively large (comparable to the changes on land) – it just happens that they cancel. But if this cancellation is model-specific or scenario-specific then this may still be a feature we need to know about.

This is an interesting and important point. In the revised manuscript, we have included a new subsection quantifying the climate sensitivity and carbon feedback (section 3.1). There, we use the sensitivity of oceanic and terrestrial carbon uptake to changes in atmospheric temperature and CO2 concentration. As discussed in the paper, while the oceanic carbon uptake appears to be unperturbed, the change in atmospheric CO2 and temperature alone is not sufficient to explain the change in carbon uptake. Similarly, we also find the similar characteristics with respect to the terrestrial carbon cycle. In the revised paper, we show that these changes in the terrestrial and oceanic carbon uptake appear to be proportionally related to the different quantity of accumulated aerosol loading added into each experiment.).

Ref1: 11. p.144 line 18 (and figs 6/7). Why does the Arctic behave differently here? Is this due to a decrease in sea-ice maybe and hence the effective ocean area in this region increase over time? Whereas the other basins are of fixed size...

In addition to changes in sea ice, the solubility of CO_2 in seawater depends strongly on the surface temperature and salinity. In this case, the Arctic region experience the largest short-term change associated to the different volcanic forcing. Relatively larger variability is also simulated in the polar Southern Ocean region.

Ref1: 12. end p.144. Why discuss the biological pump so briefly? I'm not clear if it is important or not here. If you think it is important then discuss in more detail why it behaves differently under GEO_PIN. If it isn't important then maybe don't need to mention it at all?

Future reduction in the net and export production (biological pump) is well studied and mostly attributed by the reduced nutrient delivery to the surface ocean linked to enhanced stratification (Steinacher et al., 2010). However we agree that while we quantitatively show the change in the annual export production between different volcanic scenarios, the paper did not discuss the reason behind it. In the revised paper, we show the regional differences in the annual export production between the different volcanic scenarios with the REF over the 2020-2099 periods. We continue with the discussions on where the marine export production is most affected by the volcanic forcing and why.

Ref1: 13. p.145. similarly for ocean pH – if this is an important aspect of your study then it deserves more than 3 lines. Otherwise don't mention it. Questions I can think of why is it the same for GEO_PIN?

While future change in pH is important, we did not find any significant different in the simulated changes in pH between the different experiments. Expected global change in pH under the A2 scenario is generally well known. Thus, we decided to remove the brief discussion on ocean acidification in the revised manuscript.

Ref1:If export production is higher then wouldn't this lead to lower surface ocean pCO_2 with more carbon transported to depth? So would we expect pH to be higher here? Or how has the ocean circulation changed in these runs? The surface T is notably different in GEO_PIN so has there also been a different response of, say, THC strength? How does this affect carbon transport to depth and hence surface ph?

As mention above with the export production, there will also be some regional variability in the change of surface pH, partly due to the fact that the volcanic forcing affects the ocean surface properties (e.g., SST, mixed layer depth) differently. The relatively similar change in the global mean surface pH is mainly because the global ocean surface is still in a transient state as it continues to reduce the gradient between atmospheric and surface ocean pCO_2. Therefore, reductions in ocean carbon uptake in one region essentially lead to more uptakes in other regions. Hence, globally, the mean change is relatively small.

The AMOC strengths simulated between the different experiments generally show consistent decreasing trend (Figure 4 of the revised manuscript). The role of AMOC in transporting carbon to depth has been discussed in Tjiputra et al. (2010, Ocean Science).

Ref1: 14. p.147. Final sentence. I'd avoid policy prescriptive phrases like this! Keep the paper scientifically objective. This isn't meant to be a policy discussion forum.

We agree with the referee, and as suggested, we have accordingly removed this policy related statement from the paper.

Ref1: 15. figure 1. can you quote or show the time-mean level of aerosol loading in these two scenarios? It's hard to tell how they compare. By eye it looks like the GEO_PIN scenario has more forcing? (5*0.15 vs 1*0.35 perturbations every 25 years). In which case why did you choose different levels? Why not compare two scenarios of equal mean forcing? This way it is hard to tell if the reduced climate impact of GEO_TAM is due to the magnitude or frequency of the forcing.

In the revised version, we have now included time-integrated change in the aerosol loading for each experiment (see the new Table 1). In addition, we have also included a new scenario where the frequency of the forcing is the same as the GEO_TAM (25-year), but with different magnitude as suggested. Because of this addition, we have expanded the discussions in the paper addressing the role of different frequency and magnitude. We are in the early preparation stage of designing future scenarios with different combination of frequencies and magnitudes of volcanic forcings, but with equal mean forcing as stated by Referee#1. This study will likely be focused more on understanding the potential change on the climate variability.

Ref1: 16. figure 3. you attribute the cooling in GEO_PIN by 2100 to the volcanic forcing. But there will be some reduced GHG forcing due to lower CO2. Can you quantify this? 50 ppm less would give maybe 0.2-0.3 degrees cooling?

This is also a very important point that can be quantitatively estimated in this study. In the revised paper, the additional new section (3.1) discusses this point and concludes that based on the model climate sensitivity, the reduction in the atmospheric cooling simulated in each experiment can not be explained from the change in atmospheric CO2 alone. While aerosol loading induced additional cooling to the atmosphere, the analysis shows that the additional cooling appear to be linearly related to the amount of aerosol loading added in the different experiments. However, over long period, the additional cooling on the less frequent volcanic scenarios is substantially small.

Ref1: 17. for figure 4 I think you plot T and precip over land – is that right? I think that's the best thing to do, but can you say so specifically in the caption?

Yes, the temperature and precipitation here only represent land point grid. We have revised the figure caption accordingly.

Ref1: 18. figure 4 – can you add a 5th panel to show NET carbon balance (NPP-RH)? It can be hard to see by eye how to add panels (c) and (d).

The net ecosystem production (NEP=NPP-RH-Fire flux) is now included in the revised figure as well as in the manuscript.

Ref1: 19. figure 4. I assume the signals you show here are statistically significant? How big is the standard deviation compared with the signal? I wouldn't want you to try to plot +-sigma on these plots – that would make them cluttered. But you should at least check the significance and mention in the text where it is/isn't a robust signal.

We have performed a student t-test to test the significance of these signals. However, we found it not very informative (or illustrative) to plot (as a function of latitude) only the point where the signals are significant. We have now instead, as suggested by the reviewer, include the +/- standard deviation of the year-to-year variability in the mean latitudinal of temperature, precipitation, terrestrial NPP, Rh, and NEP, as suggested.