We thank Anonymous Referee #1 for the helpful suggestions and comments. Our point by point answers to the comments are presented below. Referee comments are in bold and our replies in body text.

Referee #1 comments:

I have one major point, where I do not understand the results of the model simulations: It is not clear to me, why in the model the peak of the stratospheric sulfate burden is reached only five months after the eruption (see also below). I strongly suggest a more detailed discussion of this point, which without further explanation looks like a model artifact to the reader.
// *It is not clear to me, why in the model the peak of the stratospheric sulfate burden is reached 5 months after the eruption. I can understand how this could be the case for the HIRS observations, where the initial plume has to be diluted somewhat to be properly quantified by measurements. But I do not think that such a behaviour is seen in Lidar observations of the Pinatubo cloud. In the model however there is no substantial source of stratospheric sulphur after the eruption. The only process I can see is the conversion of SO2 to sulfate, but the timescale for this to happen should be much less than five months. I suggest more discussion of this point.//*

As the referee commented, the only process that contributes to stratospheric sulfate burden is the conversion of the SO2 to sulfate, and this will take some time and depend e.g. on the OH concentration at a specific location. Roughly 80% of SO2 is oxidized after three months after the eruption (see Figure A2). The oxidation rate in the NH (i.e. which during the eruption is the summer hemisphere with abundant OH) is fast in the months following the eruption, and but slows down considerably during the winter months (Fig. A2a; months ~4-8). As a result, the NH peak burden is reached at around month 3 and then declines very slowly until around month 6. On the other hand, in the SH (i.e. during the eruption in the winter hemisphere), the peak burden is reached only in late spring (~month 5) when the OH concentration has increased as a response to increased solar radiation. As a result, also the global burden peaks around month 5.

Text is now modified:
"The maximum stratospheric sulfate burden after the volcanic eruption (Volc) is 8.31 Tg(S). 75% of the erupted SO_2 is oxidized in two months after the eruption, mostly in the summer hemisphere (NH). However some of the SO_2 is spread to the winter hemisphere (SH) where OH concentration in the first months following the eruption is low due to lower solar radiation. As the OH abundance in the SH increases towards spring, the peak burden in this hemisphere is reached around month 5 (Fig. A2b). As a result, also the global maximum of sulfate burden is reached 5 months after the eruption (Fig. 2a, black solid line)."

It is stated in the paper that under unperturbed conditions, the atmosphere is "almost clean" of particles. I think this is an overstatement. First, it is unclear under which conditions the stratosphere is really unperturbed, i.e. not influenced by small volcanic eruptions. Second, OCS provides a source of sulphur to the stratosphere. Therefore there will always be a Junge layer in the stratosphere, so that "clean" is misleading.
We admit that "clean" is a bit misleading here. Thus "Clean" is now replaced by "unperturbed".

In any case, there is no reference here for this statement Volcanic ash emissions are not taken into account in the study. The argument is that ash particles are deposited fast. However the citation (Niemeier et al., 2009) used to back up this conclusion is a model study, I recommend considering a study based on observations. For example, the eruption of the Chilean volcano Puyehue-Cordón Caulle in June 2011 emitted a lot of ash that prevailed long enough in the atmosphere to cause interruption of passenger

aircraft activity in Australia. In any case, it is not the question how much sulphur is contained in volcanic ash (close to zero) but how much sulphur is emitted in conjunction with the ash. The sulphur contribution is not the same for each eruption – again there should be observational studies here.
It is true that volcanic ash could have some impact on air traffic. However only fine ash particles reach the stratosphere. Larger particles stay in the troposphere but have an impact on aviation and health. Guo et al 2004a have shown that volcanic ash is sediments within the first days after the Mt.Pinatube eruption. Less the 5% of the erupted ash was detected in the stratosphere after 5 days the resulting ash cloud is typically fairly local and short-lived. Also previous model studies (Niemeier et al. 2009) and our test simulations showed that volcanic ash does not affect conclusions related to emitted sulfur. Because of these reasons, and because we wanted to keep study as simplified as possible, we decide to leave volcanic ash out of this study.

Reference (Guo et al 2004) is now added in the text:
"We do not include volcanic ash emissions as it has been shown that ash is deposited relatively fast in the atmosphere and the surface area affected by the ash cloud is relatively small (Guo et al 2004a). The effect of fine ash on the distribution of the volcanic cloud in the atmosphere is also relatively small (Niemeier et al 2009)."

The dynamical feedback from the increased stratospheric sulphur load was taken into account. I think this is an interesting point of the study. Could the authors present some discussion on this point?
Dynamical feedback is taken account, but it is very difficult to identify how this has affected the our results without making an extensive additional analysis, which is out of the scope of this study.
We have added:

"*However global aerosol model studies of the Pinatubo eruption (Timmreck et al 1999; Aquial et al 2013) showed that the dynamic response to local aerosol heating has an important influence on the initial dispersal of the volcanic cloud. Performing non-interactive and interactive Pinatubo simulations these studies revealed that an interactive coupling of the aerosol with the radiation scheme is necessary to adequately describe the observed transport characteristics over the first months after the eruption. Only the interactive model simulations where the volcanic aerosol is seen by the radiation scheme are able to simulate the observed initial southward cross-equatorial transport of the cloud as well as the aerosol lifting to higher altitudes. A further improvement of the interactive simulation is a reduced northward transport and an enhanced meridional transport towards the south, which is consistent with satellite observations."*

There is some discussion on the differences between MPI-ESM and the SALSA aerosol treatment and the consequences for the radiative effects. Is there a reference to a study, where these differences have been investigated? Can "somewhat different radiative forcing" be quantified? How well does the scaling to other wavelengths work? Would the scattering radius be a better quantity to use than the effective radius? I suggest further discussion of these issues.
Unfortunately there is no reference study for our treatment of aerosol radiative properties. We admit that scaling to the other wavelengths is not unproblematic, as this leads to some overestimation on both SW and LW radiative forcings. The overestimation is larger for LW radiation which in our case leads to about 0.65 W/m2 smaller global mean aerosol radiative forcing in ESM for SRM scenario. However, this does not impact the conclusions of our study. Even though there is some difference between the models in their calculated radiative effect of stratospheric aerosol, there are many uncertainties in the model which cause much larger uncertainty in aerosol radiative effects. Adding to this, the aerosol forcing is affected also by other things that impact radiation in atmosphere such as surface albedo or cloud properties which could be quite different between a model which includes a coupled ocean model and a model that uses fixed sea surface temperature. Most relevantly, we do not directly compare the results from ECHAM-HAM and ESM. All scenarios are compared against scenarios from

the same model. Furthermore, the general conclusions which were made for aerosol radiative effects from the ECHAM-HAM simulations, also hold for simulations with MPI-ESM.

Text in the section 3.3 is rewritten:
"This in turn leads to an overestimation of the longwave radiative forcing (0.7 W/m^2 for SRM) while the shortwave forcing is less affected (-0.2 W/m^2 for SRM)."

HIRS does not directly observe the stratospheric sulphur burden (p. 21864). I think a bit more information on the satellite measurements should be given here. How do the HIRS values agree with other observations?
We are not aware of a proper comparison between HIRS and the other measurements. Also the lack of measurements of SO4 burden makes comparison difficult. HIRS instrument was originally designed for vertical sounding of humidity and temperature profiles and was not ideally for stratospheric sulfate measurements.

Some studies (Guo et al 2004b and Bluth et al 1992) have estimated that the initial SO2 mass was 18-20 Mt (9-10 Tg(S)) after the eruption. As mentioned earlier, this was oxidized to sulfate in a few months after the eruption. Based on the HIRS measurements, maximum sulfate burden was lower than 6 Tg(S) which implicates much smaller initial SO2 mass or that large proportion of sulfur mass was removed from the atmosphere first few months after eruption.

Please not that after our manuscript was submitted, we found out that there is a mistake in the figure caption of Baran and Foot (1994). Whereas the caption states that figure shows burden of H2SO4, in reality the figure shows shows mixture of (75% H2SO4 and 25% H2O). This decreases our values from HIRS data by 25%"

How is the sulphur lifetime in the stratosphere defined? Burden over loss rate? This quantity could also be a function of time. I suggest further discussion.
For SRM lifetime is calculated as Burden/Injected sulfur. For volcanic eruptions we do not quantify lifetime, because, as the referee pointed out, it would be a function of time and thus difficult to define.
The text now reads: "The total sulfur amount (SO_2 and sulfate) in the stratosphere is 8.8 Tg(S) which indicates the average sulfur lifetime (*sulfur burden divided by the amount of the injected sulfur*).."

There is some discussion of the oxidation of SO2 in the model (p. 21860). If there is something problematic in the model here, it will have to do with the OH concentrations in the model – correct? Is there any information on the quality of OH in the model from previous studies? OH is a pretty important component in atmospheric chemistry.

We are not aware of a proper validation of quality of OH in the model. In the model OH based on monthly mean values with artificial diurnal cycle. Pietikäinen et al 2012 (Atmos. Chem. Phys., 14, 11711-11729, 2014 www.atmos-chem-phys.net/14/11711/2014/doi:10.5194/acp-14-11711-2014) have studied using a statistical proxy based on Mikkonen et al 2011 (Atmos. Chem. Phys., 11, 11319–11334, doi:10.5194/acp-11-11319-2011, 2011.) This lead to a better agreement in boundary layer with measurements. However, the proxy is a function of radiation and is thus linked to clouds which do not have a significant role on the OH-concentration in the stratosphere.

Since the earlier formulation indicated that there was something problematic with the oxidation of SO2 (which was not our purpose) the text is now rewritten,.:

"One possible explanation to the larger burden and effective radius in the model could be that the amount of erupted sulfur is overestimated in the model compared to the real Pinatubo eruption. Recent global stratospheric aerosol studies indicate a much better agreement with observations if they assume a smaller amount of the volcanic SO2 emission of 5 to 7 Tg S(Dohmse et al. 2014; Sheng et al., 2015). Another possible explanation is that a larger proportion of sulfur was removed from the stratosphere during first months after the eruption due the cross tropopause transport out of stratosphere or the enhanced removal with ash and ice cloud (Dhomse et al 2014), Unfortunately, there is only limited amount of observations after the eruption of Pinatubo which makes comparison between model results and observations difficult. However, our results here are similar to the previous model studies (Niemeier et al 2009, English et al 2012, Dhomse et al 2014, Sheng et al 2015).

The statement that "in July polar vortices are weaker" needs to be corrected. In July there is no polar vortex in the Arctic, solely a solid body (anticyclonic) circulation. The polar vortex in the Arctic (and Antarctic, where of course the seasons are shifted) is only present in the winter/spring period. The transport barrier at the edge of the vortex is indeed most strongly pronounced in winter. Important for the arguments here might also be the seasonal variability of the transport barrier between the subtropics and tropics (there are a number of recent studies on this issue), which should be discussed here.

Text are now modified and argument about subtropical barrier is now added to the text:

"In contrast, in July the atmospheric flow is towards north at the northern high latitudes (Fig. B1) and the sulfur stays in the Arctic. At the same time, the seasonality of subtropical barrier affects how sulfate is transported to the tropics. As figure B1 shows, winds in the northern border of the tropics are towards south only between April and July and sulfur is transported to the tropic only during this time period. There is clearly more sulfate at the northern border of the tropics during these months after the Arctic eruption in January while most of the sulfate is already removed from the atmosphere if volcano was erupted in July. Thus.."

All minor comments below have been fixed if not otherwise said.

throughout the paper: replace 'volcano eruption' by 'volcanic eruption'
p 21839, l 9: aerosol aerosol particles

p 21839, l 25: decades or centuries?
We would like to think that decades. There could be many opinions about this, but if SRM is going to be used for centuries, we would argue that it is used for wrong purposes (as an alternative to GHG reduction)

p 21840, l 1: papers by Robock 2000 'and' Timmreck . . .
p 21841, l 21: a number the number
p 21841, l 22: a number concentration one number concentration
p 21842, l 14: a sea sea
p. 21845, l 14: citation for 8.5 Tg

p. 21850, l 14: quantify 'faster'
'Faster' cannot be easily quantified, since it is so much dependent on aerosol size and atmospheric circulation.

p. 21851, l 4: restarted
p. 21851, l 21: quantify 'quite large'
" (±0.67 K compared the mean of the ensemble)"-added
p. 21852, l 2: Isn't 'cooling' a radiative effect' – I think I know what you mean but the point could be made a bit clearer here.
Text is now rewritten as follows:

“Similar to Volc simulation, the global mean temperature is lower compared to the pre-eruption level even radiative forcing has leveled off.”

p 21853, l 3: to low at low
p 21855, l 17: reduction compared to what?
p 21859 l 24: poleward transport of what?

Throughout the paper there are little issues in the text where a "the" is missing or should be replaced by "a". Please correct.
We have done our best to correct this issue; however, none of the authors are native speakers of English. The final text will be polished off by the ACP copy-editing team.

In acknowledgements: Julich Jülich; also Center ‘of’ Climate . . . (l. 7, p. 21864)