We thank Anonymous Referee #2 for the helpful suggestions and comments which improved the manuscript. Our point by point answers to the comments are presented below. Referee comments are in bold and our replies in body text.

The broad objective of this paper is exactly the type of research that geoengineering needs. However, there are a number of clarifications needed. Furthermore, the primary motivation for the specific seasonally-dependent scenarios considered is based on tracking the latitude at which the insolation is strongest, but the actual situation is somewhat more complicated and not as well described in the paper as it could be. Because of the stratospheric circulation, the peak aerosol concentrations will not occur at the latitude of injection (other than for the equatorial case). Thus, if the only thing you cared about was being "efficient" in the sense of trying to best align the peak aerosol concentration with the peak of insolation, you'd have to do some complicated estimation of where to inject as a function of time of year, taking account of the seasonally varying Brewer-Dobson Circulation. So what you picked is a reasonable first guess just to see whether the seasonal-variation idea has any merit at all, but should simply be described as an initial step towards coming up with better strategies, acknowledging that much more work would be needed to understand the options. Figure 4a,b should be given somewhat more prominence in the discussion, that is, the aerosols are widely dispersed relative to the insolation even with the seasonal strategies.

We agree with this comment, and have added the following text in the revised manuscript: *"This study should be taken as a first step in evaluating optimal injection strategies in terms of geographically more uniform aerosol fields/radiative forcing/climate impacts without losing the effectiveness of geoengineering compared to continuous equatorial injections. In order to fully optimize the injection strategy, one should try to also account for the effect of stratospheric circulation on aerosol transport, together with existing planetary reflectivity and a detailed analysis of aerosol microphysics. These aspects are out of the scope of this study. "*

We also added a new section (3.1.2) to the manuscript which helps to highlight this issue. New figure 6 shows the zonal mean aerosol optical depth at different times of the year together with the injection area and latitudes with the strongest solar intensity. In addition, we added figure 6f which shows the seasonal atmospheric circulation at the height level of the sulfur field. This section and the figure provide more information about why the seasonally changed injection area leads to a slightly larger global mean radiative forcing and a larger midlatitude forcing than the equatorial injection scenario. It also shows where and when the sulfur field was not optimally located and thus helps in estimating the effects of sulfur injection strategies which are aiming for either a maximum global mean radiative forcing response or an increasing radiative effect in the midlatitudes .

Also, I'm unclear whether the objective is to be more efficient by aligning aerosols with peak of solar radiation, or whether the objective is to do a better job of compensating for the spatial pattern of warming due to CO2 (as described in a few papers using patterns of solar reduction). These are different objectives, and the "right" strategy for each will be different (this is why I raise questions with your use of

terms like “optimal” and “efficacy” below). You mix these objectives in your motivation; the introduction talks more about the latter objective, but the choice of seasonally varying injection is motivated by the former. In principle its ok to say that both of these are issues with the usual equatorial injection and that you’re exploring how alternate strategies affect things, but you should be clear that you simply picked something that was somewhat physically motivated to see how it would affect the climate, and that there’s no attempt to optimally solve either of these two problems.

Seasonally varying injections are motivated both by better compensating for the spatial forcing pattern due to greenhouse gases while simultaneously trying to maximize the cooling effect at the time when solar radiation is at its peak over the subtropics. However as was replied in the previous comment, the aim of the study is not to fully optimize the global radiative forcing or optimal temperature pattern. We will clarify this in the revised manuscript.

Our aim of the study is clarified in the introduction in the following chapter:
“In this study, we have investigated injection scenarios that aim to produce a geographically more even radiative forcing pattern than equatorial sulfur injections, while still maintaining a high global mean forcing. Such scenarios are sought via seasonally varying injection areas in which the target injection area follows the maximum solar intensity with different time lags. These scenarios are compared to more commonly used strategies with fixed injection areas. This study should be taken as a first step in evaluating optimal injection strategies in terms of geographically more uniform aerosol fields/radiative forcing/climate impacts without losing the effectiveness of geoengineering compared to continuous equatorial injections. In order to fully optimize the injection strategy, one should try to account for also the effect of stratospheric circulation on aerosol transport, together with existing planetary reflectivity and a detailed analysis of aerosol microphysics. These aspects are out of the scope of this study.”

Text is also modified in relevant parts and we avoid using word “optimal scenarios” when talking about scenarios studied here.

1. L11-13, the actual issues here are a bit more subtle. Equatorial injection is often picked because the aerosols will disperse globally, so to know that the radiative forcing from equatorial injection is highest at the equator, one needs to also know that the aerosol concentrations from equatorial injection are at best uniformly distributed spatially (and in fact they’ll be concentrated equatorially, as shown in your Figure 4). Not sure how to convey this concisely, but the second sentence isn’t quite right.

These lines now read:
“In geoengineering studies, these injections are commonly targeted to the Equator, where the yearly mean intensity of the solar radiation is highest and from where the aerosols disperse globally due to the Brewer Dobson Circulation. However, compensating the greenhouse gas induced zonal warming by reducing the solar radiation would require a relatively larger radiative forcing to the mid and high latitudes and a lower forcing to the low latitudes than what is achieved by continuous equatorial injections."

2. L12, “optimal” in what sense?

We now avoid using the word ‘optimal’. See also reply to the earlier referee comment.

3. L15, what do you mean by “efficacy”?
Efficacy was changed to “*the mean radiative forcing of stratospheric sulfur* “

4. L23, I would be careful using the word “significant” unless you mean it in the narrow sense of “statistically significant” (which will of course depend on the magnitude of forcing). More to the point, the last sentence of the abstract does seem like a rather important result, and quite “significant” in the non-narrow sense of the word.
“Very significant” changed to “*as large*”

5. L31, not sure what “efficiently” means in this context. Ditto page 2 line 2.
Word “efficiently” was removed from the sentence. The second sentence in Page 2 was changed to: *“Because of the stability of the stratosphere and the lack of efficient removal mechanisms which are prevalent in the troposphere, the stratospheric lifetime of...“*

6. P2, L17, I’ve cited that paper; I think the year is 2013 not 2012. (The same authors also have a more recent study from 2016 in ESD that would be appropriate to also cite.)
This is correct, and the year was changed to 2013. We also added Kravitz et al. 2016 to the reference list and cited it here and section 3.1.2

7. P2, L24, “this kind” means which kind? Specifically studies looking at how injection at different latitudes affects the climate differently? (Didn’t Tilmes do a study on that in the last few years too?)
“This study” was rewritten as follows: “*where other than equatorial injection was studied by global aerosol-climate* model"

Tilmes has done several good geoengineering studies, but none of those (to our knowledge) study injections to various latitudes.

8. P2, L30, what do you mean by “target area”?
“Target” changed to *“the injection”*

9. P2, last two sentences, I know why you use two models, but you might want to say that explicitly. (And be explicit about what you’re giving up by not having a single model that includes everything.)
Text “*(MPI-ESM) does not include a prognostic calculation of aerosol processes in its current configuration*” was added to the end of the line

In section 2.2 Model description now reads:
“The two-step approach was selected because the currently available middle atmosphere configuration of MPI-ESM does not include a prognostic calculation of aerosol properties. In addition, modelling aerosol microphysics is computationally heavy. Thus it was feasible to

simulate aerosol microphysics only for a relatively short period (few years) and use the ECHAM-HAMMOZ simulated aerosol fields as prescribed fields in the longer simulations in MPI-ESM. Simulations with ECHAM-HAMMOZ were carried out using a free running setup to include the dynamical feedback resulting from the additional heating due to absorption radiation by the injected aerosols. However, stratospheric circulation could also be altered by changes in the atmospheric GHG concentration (in our case following the RCP4.5 scenario) and its impacts on the tropospheric climate; however, these impacts were not taken into account when the aerosol fields were calculated in ECHAM-HAMMOZ. . .”

10. P3, L4, note that keeping the height constant while varying latitude might matter because the tropopause height varies with latitude, at higher latitudes you’re putting material higher into the upper branch of the circulation.
This is true and will definitely have an impact if the injection area varies over a wider area and reaches higher latitudes. Most of the scenarios studied here included injection between 30 N and 30 S latitudes, where the tropopause height varies only a little. However in the p2w scenario, it might have some effect. We will mention this in the revised manuscript.

“*In addition, the tropopause height varies with latitude and when injecting sulfur to higher latitudes in p2w scenario, some of the sulfur is injected into the upper branch of the stratospheric circulation*.”

11. Section 2.2.1, I haven’t read all of the references, but is there any validation against, for example Pinatubo observations, to suggest that the aerosol processes are correctly captured? Might want to mention that explicitly. Did aerosol simulations involved stratospheric chemistry also? (Interactions with ozone concentrations could matter.) How does your aerosol spatial distribution and amplitude compare with previous simulations for the equatorial case?
Our model has been evaluated against the observations of the Mt Pinatubo eruption in Laakso et al 2016.
We added the following text “*The model has been shown to simulate the stratospheric aerosol loads and radiative properties consistently compared to the observations of Mt Pinatubo eruption as well as other models (Laakso et al., 2016).*” to section 2.2.1.

Our simulations used prescribed ozone fields, and therefore the injected aerosol did not impact the ozone concentrations. This is now explicitly stated in the manuscript.

“The hydroxyl radical (OH) and ozone concentrations are accounted for through prescribed monthly mean fields. Thus, the effect of sulfur injections on the ozone layer is not simulated in our model”

We added the following lines to the manuscript to point out that our results are consistent with earlier studies:

“However, outside the tropics the forcing declines fast (EQ)) as seen also in the case of equatorial injection in Niemeier et al. (2011).”

*"In scenario **EQ**, sulfate is concentrated near the injection area in Equator and near the 50° N and 50° S latitudes as shown in earlier studies (English et al., 2012; Jones et al., 2016; Niemeier et al., 2011)"*

"In the case of equatorial injections, AOD is clearly larger close to the Equator and in high latitudes than in mid latitudes. This is consistent with earlier studies (English et al., 2012; Jones et al., 2016; Niemeier et al., 2011)."

12. Page 6, what do you mean by saying they were based on GeoMIP G4?
"Were based" changed to "*our setup of scenarios was similar to* ".
Here we mean that the baseline scenario (RCP45) as well as the start and end years of SRM were chosen based on GeoMIP G4. In addition, SRM was started (and suspended) at full force similar to G4. However the amount of injected sulfur was double compared to GeoMIP G4.

13. Section 3.1 has lots of good insights and observations, but one thing missing is any discussion of aerosol size distribution – is it the same for the different injection scenarios? This could have a big impact on the radiative forcing.

We added two more figures to the manuscript and a new section 3.1.2 with discussion on this topic. Figure 3 shows the zonal mean effective radius in the most of the studied scenarios in different seasons. Figure 6 shows the time dependent zonal mean of AOD for 533nm.

In addition to the completely new section 3.1.2, where we discuss resulted zonal AOD at the different time of the year, the following texts were added to section 3.1.1 :
"*Thus the particle effective radius is clearly smaller in scenario NHSH than in scenario NH, especially in the northern hemisphere (Fig 3).* "

"As a result, the amount of smaller particles increases. Figure 3 shows that the particle effective radius is on average smaller in scenario p2 than in EQ."

"*Figure 3 shows that in scenario p2w particles are consistently smaller than in p2. However, due to the atmospheric circulation, in p2w particles are removed more quickly from the atmosphere because sulfur is injected at a larger distance from the equator. Thus there is no difference in stratospheric sulfur burden between p2 and p2w scenarios (Table1).* "

14. P10 (P9)**, L17&18, what is the standard error in the temperature changes due to natural variability? (Are the differences between scenarios statistically significant?) Ditto for the rates of warming on L28, and for precipitation changes on next page.**

Lines 17-18 now read: *"Compared to RCP45, the global mean temperature is -1.27 (± 0.18), -1.13 (± 0.13), -1.21(± 0.19), -1.34 (± 0.14) and -1.29 (± 0.15) K cooler in scenarios EQ, NH, NHSH, p2, and p2w (not shown in the figure 5) between 2060-2070"*

L28:
"Between the years from 2030 to 2070 the warming rate is 1.95 (± 0.68) K / 100 yr in RCP45 scenario, but in scenario EQ the warming rate is reduced to 1.25 (± 0.55) K / 100 yr"

For precipitation changes:
"Compared to the years 2010 - 2020 the global mean precipitation has been changed by +0.044 (± 0.013), -0.051 (± 0.013), -0.036 (± 0.013), -0.043 (± 0.015), and -0.054 (± 0.011) and -0.05 (± 0.014) mm/day in RCP45, EQ, NH, NHSH, and p2 and p2w"

15. P10, L32, I'm not sure what you mean...I assume you mean that nonlinearities in climate feedbacks could change the rate of warming (since if the feedbacks were linear, there would be no effect beyond the dynamic one you already mentioned regarding ocean equilibration timescales). It is certainly true that the ice albedo feedback will have some nonlinearity in it, but I would expect that to behave with opposite sign – that is, in the warmer world, there is less sea ice left to be melted, less change in sea ice per unit increase in warming, and thus that positive feedback that amplifies warming would start to saturate. Re first line of page 10, why do you say that ice area is "clearly higher"? What figure shows this? It isn't obvious to me why it should be higher (aside from global temperatures being slightly lower, but since I know that at least with EQ you overcool tropics more than the poles, the poles are probably warmer in 2070 compared to 2010, so ice area could easily be lower, not higher).

The Arctic temperature did not change linearly with global mean temperature. We have included some numerical values of the Arctic temperature and sea ice extent to the manuscript as well as some discussion concerning cooling in the Arctic.

The text now reads:
"Over the latitudes higher than 70 N, the mean temperature is on the average still 0.6 (+- 0.5) K cooler in SRM scenarios during the years 2060-2070 compared to the years 2010-2020 even though the global mean temperature was roughly compensated. Simultaneously, the sea ice cover is 7 % larger. The cooling of the Arctic has not seen in previous studies in which solar radiation management has been investigated (Schmidt et al., 2012; Niemeier et al., 2013; Jones et al., 2016). The reason behind our simulation result is not totally clear. Section 3.1.2 showed that the AOD was relatively large at high latitudes which would have an impact on the radiation in summer months. On the other hand, the total received the energy in the arctic area depends also on energy transferred by the oceans and the atmosphere. Figures 6 a and b show that there is warming in the subpolar North Atlantic. In this area, the sea surface temperature (SST) increases by 2-4 K in scenario EQ. On the other hand, there is a 1-2 K cooling in the SSTs in the Arctic Ocean. This indicates that there are changes in the ocean circulation. Since these patterns are seen also in scenario RCP45, they likely originate from in our reference years 2010-2020. The pattern of SST regions is similar to what is seen in CMIP5 RCP scenarios, where there was an amplified SST increase in the Nordic seas while in sub-Polar North Atlantic the warming rate was subdued compared to the global average trend (Sgubin et al. 2017). However, investigating the changes in the ocean circulation is out of scope of this study. Overall, different warming rates in SRM and RCP45 scenarios might also be affected by the asymmetric climate system response to the increase or decrease of forcings (Schaller et al., 2014). It has been shown that there is a slow decrease in the temperature still decades after a decrease in shortwave radiation (Schaller et al., 2014). Kashimura et al. (2017) studied the GeoMIP G4

scenario in several models. Their study showed that the difference in global mean temperature between the RCP 4.5 and SRM scenarios increased for 10-25 years after solar radiation management was started. Here the amount of injected sulfur was twice as large as in G4 and Kashimura et al. (2017) which can explain why here the temperature difference increased until SRM was suspended."

16. P11, L17-19, slightly confusingly written (being generous; it is unequivocally false as written). A uniform reduction in SW does not lead to warming in high latitudes, indeed in every GeoMIP model, the high latitudes cool *more* than low latitudes in response to solar reduction, this is due to the spatial pattern of climate feedbacks. However, the polar amplification is even stronger for CO2 warming, so that the net effect is that the solar reduction overcools the tropics and undercools high latitudes relative to CO2.

These sentences now read:
"If the global mean temperature change due to the increased GHG concentration is compensated by a relatively uniform reduction in the SW radiation (reduction in the solar constant), it has been shown to lead to warming in the high latitudes and cooling in the low latitudes compared to the temperature before the increase in GHG concentration and SRM (Kravitz et al., 2013c; Schmidt et al., 2012)."

17. And following from that, it is quite surprising that your equatorial case cools the Arctic more than the mid-latitudes; if I look at GeoMIP G1, there is not a single model that does that, and I would expect equatorial SO2 injection to have an even stronger ropical cooling relative to arctic cooling than G1. The sentences on P11, L20,21, does not really explain why this model should behave differently from the models in G1 (including MPI). Regarding the one ensemble member that is significantly warmer in 2010-2020 in this region, you can look at what pattern you get if you exclude that member, and then state whether or not that explains the result, rather than simply commenting that it might explain the result; this isn't hard to test.
Even though the mentioned temperature patterns of the one ensemble member differ from those of the other two, excluding this one member did not lead to a different sign in arctic area. This issue is now discussed more in section 3.2.1 as was replied to comment 15.

Sentences on P11 are now rewritten as:
"However there is also cooling at the Arctic (Fig 6b) which was discussed in section 3.2.1. Overall the size of the area of this arctic cooling region is small compared to the regions in the midlatitudes which have warmed after the year 2020."

18. Fig 6e is repeated twice and 6f is missing.
Figure 8 (originally 6) is now fixed. In addition, the map projections were changed so that the projections do not stretch the latitudes and the hatching shows areas which are not statistically significant (instead of significant differences)

19. So NH case has slightly lower SO4 burden, slightly lower globally averaged radia-

tive forcing, but preferentially loaded in the North. Not surprising that it is more effective at cooling the Arctic than EQ in the summer, nor that EQ is more effective in boreal winter, but the idea that it is actually LESS effective at cooling high northern latitudes than equatorial injection when averaged over the year does seem remarkable; this is also inconsistent with other model results that I have been shown but that have not yet been published.

From Fig 7, the NH does indeed cool the Arctic more in the boreal summer than EQ does, as expected, and supports the idea that if the only thing you cared about was Arctic ice cover (in September), then the NH case ought to be better, in contrast to your unsupported claim. If you are going to make a claim, even for just this model, that EQ prevents melting of arctic ice better than NH, you should show a plot of it, because Fig 7 doesn't actually support that claim and looks contradictory.

Unfortunately our original text was misleading. In the polar region north of Eurasia, the ice cover is larger in the case of equatorial injections, also in the boreal summer months. However, if we take into account the total sea ice cover in the northern hemisphere (also outside the Arctic Circle) there is not a large difference in the yearly total ice cover between the scenarios. This is now discussed in more detail in the manuscript and we also added one figure (9) related to the issue.

Text in the manuscript reads now:
"If the stratospheric sulfur injections were concentrated to the Northern Hemisphere (NH), it would lead to a significant cooling in the northern midlatitudes compared to the injections to the Equator (EQ). However, the polar region north of Eurasia in NH is not cooler compared to the scenario EQ. The Arctic area is warmer than in EQ especially in the boreal winter, when the cooling effect of the particles from the NH injections is weak (Fig 7a). On the other hand, in the EQ scenario the mean global climate will be cooler, which can affect the Arctic temperatures through oceanic and atmospheric circulation. Figure 10 shows the difference in the Arctic sea ice cover between the EQ and NH scenarios in the boreal summer and winter. Scenario NH leads to a larger ice cover north of the North America and East Siberian Sea in the boreal summer and over the Atlantic and Pacific in the boreal winter. However over the Barents and Kara seas there is more sea ice cover in the EQ scenario. This area is affected by the warm Gulf Stream and the Norwegian current. In the EQ scenario, the Atlantic SST is cooler than in scenario NH and sea ice cover north of Eurasia is larger in scenario EQ also in the boreal summer months, when sulfate from NH scenario reflects radiation most efficiently. Thus, based on these results, the injections only to the Northern Hemisphere do not increase the yearly arctic sea ice cover compared to the injections to Equator. A more detailed analysis would be required to generalize these findings; however, it is out of the scope of this study. Furthermore, it would be beneficial to repeat these scenarios also with other climate models to see whether the simulated response is robust across models."

20. P12 L19, chapter should be section
"Chapter" changed to "section"

21. P13, L19, I don't recall seeing any optimization in this paper what do you mean by optimize?

This sentence now reads: *"We estimated how different emission areas of stratospheric sulfur could be used to prevent the overcooling of the tropics and undercooling of the midlatitudes and the Arctic without a decrease in the global mean radiative forcing of the stratospheric sulfur injections."*

22. Bottom of P13, can you be more consistent? You use one set of metrics to compare EQ and p2w, and a different set of metrics to compare p2
This now reads:
"Thus the radiative forcing was relatively larger in the summer hemisphere (17% in the Northern and 14% in the Southern) and relatively weaker in the winter hemispheres (14% in the Northern and 16% in the Southern) compared to EQ."

23. P14, L6, what do you mean be efficiency here?
This is now rewritten as: *"Our simulations indicate that the global mean radiative forcing of the aerosol was not significantly increased in any of our simulations compared to the equatorial injection scenario EQ."*

We also added:
"However, the scenarios studied here are only a first step towards more optimal injection scenarios. A full optimization would require a more detailed analysis of incoming and reflected solar radiation, atmospheric circulation and how it is affected by sulfur fields as well as aerosol microphysics and chemistry. Overall, however, results of this study already show the potential of time-varying injection scenarios."

24. P14, L11-13, meant to comment on this earlier, but was this effect seen in previous G4 simulations? If it was, not really something to highlight in conclusions here, since it is rather tangential to the purpose of this paper. If it wasn't, why not? (Obviously wouldn't show up in models without a real ocean, but at least some of those did?)
This was not seen in G4 simulations (at least to our knowledge) and thus it was mentioned in the conclusions. In Kashimura et al 2017 a similar behaviour as here is seen, but only for few decades. However, here the amount of injected sulfur was twice as large as in G4.

This is now discussed in section 3.2.1:
"Kashimura et al. (2017) studied the G4 scenario with several models. Their study showed that the difference in the global mean temperature between RCP 4.5 and SRM scenarios increased for 10-25 years after solar radiation management was started. In the current study the amount of injected sulfur was twice as large as in G4 and Kashimura et al. (2017), which can explain why here the temperature difference increased until SRM was suspended."

However as this was not main interest of this study, we removed these lines from the conclusions.

25. P14, L20, just to reiterate, you haven't shown this. (It may be true in your simulations, but you haven't shown any simulation results to back that up.
These lines were removed. Making final conclusions on this would require a more detailed study on this topic and simulations with other models.