“Shortwave radiative forcing and feedback to the surface by sulphate geoengineering: Analysis of the Geoengineering Model Intercomparison Project G4 scenario” by Hiroki Kashimura et al.

Response to the Referee #2, Dr. Aaron Donohoe

Dear Dr. Donohoe

We thank you for a carful review and constructive comments. Please find below the authors’ response. In this reply we denote referee's comments and questions using blue; our responses are in black and relevant text in the manuscript in Times font with changes shown in red.

The referee’s comments are kindly repeated in detail after “main points”, so that quotations of changed sentences from the manuscript is written after the comments in “main points”.

This manuscript employs a single column isotropic shortwave radiation model to decompose the changes in the net surface shortwave flux in response to solar radiation management in the geomip model ensemble. The use of the single column model in conjunction with the assumption that changes in clear sky reflection and absorption are due to sulfate aerosol forcing and water vapor feedbacks respectively is very clever (especially putting these changes back into the full sky equations).

However, I do question whether the cloud feedback can be isolated from the effective radiative forcing of aerosols associated with the direct and rapid response of clouds.

=> We recognized that we did not distinguish the rapid response (or adjustment), which does not depend on ΔT, and feedback, which is proportional to ΔT in the previous manuscript. This may be the main reason for many of your comments. What we called “feedback” in the previous manuscript was the sum of the rapid response and feedback. In the revised manuscript, we defined “rapid adjustment” and “feedback” as described above, and we defined the word “total reaction” as the sum of rapid adjustment and feedback, for convenience. We revised the expression related to “feedback” through the text.

We also revised the title of the manuscript as
“Shortwave radiative forcing, rapid adjustment, and feedback to the surface by sulphate geoengineering: Analysis of the Geoengineering Model Intercomparison Project G4 scenario”.

I suggest an improved methodology below. I highly suspect that much of what the Authors interpret as a cloud feedback (i.e. associated with temperature changes) is actually the cloud changes due to the aerosol forcing itself and is better characterized as a forcing.

=>Thank you for the suggestion. We used the methods similar to the Gregory plots and found that the previously called “cloud feedback” (now we call this “total reaction of clouds”) is a rapid adjustment due to the cloud amount change. The referee’s suspicion was correct.

Though the rapid adjustment is characterized as a forcing (i.e., effective radiative forcing; ERF) in the recent studies of climate change, we consider that for the study of geoengineering simulation, it is better to separate the direct forcing and rapid adjustment to explore which processes have a large uncertainty in the sulphate geoengineering simulation, which is not well verified by observations or field experiments in global scale. We added sentences mentioning these points in Introduction.

I also question the use of the surface radiative budget as opposed to the top of atmosphere of tropopause. As such, I think the main conclusions of the manuscript are not supported and the work could be misleading for the field.

=> This study used net shortwave radiation (SW) at the surface, but did not consider the radiative (energy) budget. We consider that the SW at the surface is very important for vegetation and human activities such as agriculture and solar power generation, and they will be strongly affected by the solar radiation management (SRM). Moreover, the recent study of Kleidon et al. (2015) showed that longwave radiation (LW), sensible heat flux, and latent heat flux can be derived from SW changes at the surface.
On the other hand, we agree that many studies on the climate system used the energy budget at the top of the atmosphere (TOA) and many readers in the field of climate science are accustomed to considering at TOA. Thus, we introduced the measures calculated at TOA and compared them with those at the surface in the revised manuscript. We consider that this discussion clarifies the meaning of the conclusions for readers in the climate science.

I do recognize that the analysis pursued could allow the authors to determine the magnitude of forcing and feedbacks associated with each cloud, water vapor and surface albedo changes and, potentially informs which physical processes determine both the robust changes in the ensemble average and the cause of inter-model differences. There is great potential for the work to offer new insights into the response to geoengineering but, as is, the methodology is flawed and conclusions are misleading. I do not recommend publication of the manuscript in its current form; the Author's need to fundamentally modify the methodology and focus of the manuscript.

=>We consider that the first reason why the referee thought, "the methodology is flawed and conclusions are misleading" is our misuse of the word "feedback". The second reason is a lack of explanation of why we use the net SW at the surface. And the third reason is a lack of comparison with the estimation at TOA. We have corrected the word misuse and added the explanation (in Introduction) and discussion (Section 4.1), so that we believe our study becomes valuable for readers.

I'm not sure I understand the rationale/agree with the premise that the net shortwave flux at the surface is a useful metric for understanding inter-model differences in the response to solar radiation management (SRM). Why favor this metric over the forcing, or the net (longwave plus shortwave) radiative change either at the surface or (preferably) the tropopause?

=>As described above, SW flux at the surface is important to consider the influence of SRM to vegetation and human activities; in addition, LW radiation, sensible heat flux, and latent heat flux can be derived from SW changes at the surface as reported by Kleidon et al. (2015). These are the reason for using surface SW radiation in this study.

Is there an a priori physical reason to expect the correlation between net surface shortwave and temperature response? I could not find one in the manuscript.

=> We simply consider that it is natural to expect the correlation between changes in net SW radiation at the surface and that in surface air temperature, because these two are in the relation of “forcing and response”. Though detailed analyses of full energy balance are required for accurate prediction of ΔT, it is useful and important to show a rough and easy relation for ΔT. The strong correlation between ΔT and ΔF^{net}_{SURF} at least for the range of $-1.1 < \Delta T < 0.2$ is a part of findings in this study as shown in Fig. 3.

In particular, the shortwave water vapor feedback differs in both sign and magnitude when considering the surface fluxes versus the tropopause or TOA and it’s hard to justify the interpretation of this feedback defined at the surface (as pursued in the current manuscript); in a warmer planet, the moister atmosphere directly absorbs more solar radiation which has a heating impact on the climate system but this reduces the downwelling shortwave flux to the surface which the Authors would interpret as a cooling feedback in the framework used within the manuscript. This feedback is found in the current manuscript to have a magnitude of order one half the net surface shortwave change and likely confuses the results and interpretation of the manuscript.

=>Your comment is correct. The effect of water vapour differs in both sign and magnitude when considering at the surface and at TOA. Amounts of water vapour in G4 is less than that in RCP4.5, and SW absorption rate of the atmosphere in G4 is less than that in RCP4.5. This means more incoming solar radiation reaches the surface (i.e., sense of heating). On the other hand, at TOA, less absorption rate results in increase of outgoing SW radiation (i.e., sense of cooling). At TOA, the upwelling SW radiation that is affected by the absorption rate experiences a reflection at the surface. Therefore the magnitude at TOA is much less than that at the surface. This interpretation is consistent with our results at the surface and newly added results at TOA. These results and discussion are added in the new Section 4.1, and we consider this section clarifies the meaning of the water vapour reaction.
We also revised the expression “cooling/heating” for rapid responses and feedbacks at the surface to simply “decrease/increase of net SW at the surface”, because decrease/increase of the SW at the surface does not necessary result in cooling/heating in total (including effects of LW).

I’m not sure that the correlation found between the temperature response and net shortwave flux at the surface is anything more than a statistical coincidence (given the number of independent data points available when accounting for expected correlations between ensemble members of the same model).

=> As described above, we consider that it is natural to expect the correlation between changes in net SW radiation at the surface and that in surface air temperature, because these two are in the relation of “forcing and response”.
Six data points (one from each model) are used to obtain the correlation coefficient of 0.88. Ensemble mean is used for the models that have ensemble runs to avoid overweighting the models that have many ensemble runs. We consider that the number of data points is enough to state the correlation.
At least for the range of ΔT from –1.1 to 0.2 K as shown by Fig. 3, it is a statistical fact that ΔT and ΔF_{SURF}^{net} has a good correlation.
We added some words to clarify as follows.

Page 8, line 22–28, Section 3.1
For CanESM2, HadGEM2-ES, and MIROC-ESM-CHEM, the filled symbols indicate the ensemble mean whilst the unfilled symbols indicate individual ensemble members; for the other models, the filled symbols indicate the results of a single run. This figure shows a strong correlation between the mean ΔT and ΔF_{SURF}^{net} ; the correlation coefficient for the six filled symbols is 0.88. This strong correlation allows ΔF_{SURF}^{net} to be used as a measure of the SRM effects at least for $-1.1 < \Delta T < -0.2$ K, although the surface air temperature depends on the energy balance among SW, LW, and sensible and latent heat fluxes at the surface.

I believe that looking at the same diagnostics (including LW changes) from the perspective of the TOA radiation alongside the surface would help to illuminate the underlying physical mechanisms responsible for the inter-model differences in the response to SRM.

=>As suggested, we added a discussion comparing the results at the surface and at TOA. We cannot treat LW radiation in the same manner as SW radiation, because we need to consider LW emission from atmosphere, surface, and clouds. Hence, we simply analysed the LW rapid adjustment in the clear-sky condition, which should represent effect of LW absorption by stratospheric sulphate aerosols. These discussions are added as Sections 4.1 and 4.2.

Main points:

Separation of cloud feedbacks from direct aerosol forcing of clouds

Clouds respond directly to forcing agents (e.g. aerosol, carbon dioxide, etc) and to changes in surface temperature. The IPCC (and field as a whole) includes the rapid cloud response to forcing agents in the “effective” radiative forcing whereas the cloud radiative changes due to surface temperature changes are generally classified as a radiative feedback. The present manuscript associates all the cloud changes with the feedback (equation 11) and I suspect much of what is called a cloud feedback is actually inter-model differences in the effective cloud forcing. This suspicion is based on two lines of evidence:

1. The cloud radiative changes in figure 4 seem to coincide with the nearly step function changes in aerosol as opposed to the surface temperature changes. Panels E and C are the best examples. The cloud radiative changes ramp up almost immediately at 2020, before the surface temperature has decreased and return to near their unperturbed value almost immediately when the SRM stops at year 2070 even though the surface temperature takes longer to recover.

2. The published cloud feedbacks differ in sign and magnitude from those found elsewhere in the literature for the same models. More fundamentally, the Authors conclude that cloud changes damp the response to geo-engineering whereas the models included in the study have been found to have positive net cloud feedbacks in response to CO2 (see Table 1 of Andrews et al. 2012 – Forcing. Feedbacks and climate sensitivity in the CMIP5 coupled atmosphere-ocean climate models) The comparison I’m making is unfair to Authors since I am comparing net cloud radiative impacts at the TOA to the surface SW impact. However, figure 3 of the above manuscript suggests a sign difference for at least the hadGEM3-ES model. Either way, the ensemble average negative cloud feedback suggested by the Authors seems at odds with the literature, is likely confused with the effective forcing and should be further analyzed (remove forcing, look at net

radiative impact, compare TOA and surface) since this result contradicts and confuses the existing literature.

A fairly straightforward solution to the above objections would be to compute the same fields outlined in equations 10-12 for each year of the simulation where the SRM is approximately constant (2025-2070 ish) and plot the radiative changes of each term versus the surface temperature change for all. As suggested by Gregory, the feedback is the slope of the linear best fit line and the effective forcing of each term is the y-intercept. This would also allow the Authors to calculate the impact of the aerosols on the shortwave absorption within the atmosphere which is alluded to in the discussion. I think this would appropriately isolate the effective forcing of clouds and the Authors might find the very interesting result that the inter-model differences in climate response to SRM is well correlated with effective forcing where the latter includes both the direct forcing of the aerosols and the rapid impact of the aerosols on the cloud radiative effect.

=>Thank you for the detailed explanation and suggestion. First of all, we misused the word “feedback” in the previous manuscript. We had used “feedback” for the sum of rapid response and feedback (in the meaning in the field of climate science). We have recognized we need to try to separate the rapid response and feedback. In the revised manuscript, we made plots similar to the Gregory plot as suggested by the reviewer. As the reviewer suspected, most part of E_C is “rapid response (or adjustment)”, which do not depend on ΔT, and the feedback part is not dominant.
Because the rapid adjustment of the cloud is caused by various processes (e.g., changes in atmospheric stability and water vapour distribution), its sign and amount can be different (or inconsistent) between CO_2 increased simulations, such as Andrews et al., and SRM simulations. In fact, Kravitz et al., (2013, JGR-Atmos, Vol. 118, pp.13087–13102) analysed GeoMIP-G1 experiment and showed a positive (sense of heating) SW cloud rapid adjustment of about 5.5 W m^{-2}, which is consistent with our results. We consider more detailed studies on cloud processes in SRM is needed. However, it is out of scope of this study.

We added description on the method at the end of Section 2, its result in the new Section 3.4, and some remarks on the difference between our results and Andrews et al. in Section 5 as follows:

Page 8, line 1–7, Section 2
To decompose the total reactions (E_{WV}, E_C, and E_{SA}) into rapid adjustments and feedbacks, a method similar to the Gregory plot (Gregory et al., 2004) is used. That is, the globally and annually averaged data of total reactions are plotted against that of ΔT ($\equiv T_{G4} - T_{RCP}$), and linear regression lines in the following forms are obtained by the least squares method.

$E_{WV} = Q_{WV} - P_{WV}\Delta T$, (15)

$E_{C} = Q_{C} - P_{C}\Delta T$, (16)

$E_{SA} = Q_{SA} - P_{SA}\Delta T$. (17)

Here, Q_X (X =WV, C, SA) denotes the rapid adjustment, $-P_X$ is the feedback parameter, and the overline denotes the global and annual average. This method is similar to the Gregory plot, but note that ΔT is the surface temperature difference between the G4 experiment and the RCP4.5 scenario experiment, in which the anthropogenic radiative forcing depends on time and the simulated climate does not reach an statistically equilibrium state.

Page 11, line 17–34, Section 3.4

3.4 Decomposition of total reaction into rapid adjustment and feedback

The total reactions due to changes in water vapour amounts, cloud amounts, and surface albedo discussed in the previous two subsections are the sum of the rapid adjustment, which are independent of ΔT, and the feedback, which depends linearly ΔT. In this subsection, we attempt to decompose the rapid adjustment and the feedback using a so-called Gregory plot (Gregory et al., 2004). Figure 7 shows globally and annually averaged E_{WV}, E_C, and E_{SA} as a function of averaged ΔT for each model. Now, we consider that a slope and a y-intercept show a feedback parameter and an amount of rapid adjustment, respectively, as shown by Eqs. (15)–(17); these values and correlation coefficients are shown in Table 2. The multi-model mean values are also shown.

There are no qualitative inter-model differences and each model has the following properties. E_{WV} (orange ⋄) shows high negative correlation with ΔT, and the rapid adjustment and the feedback are clearly separated. In the multi-model mean, the rapid adjustment is –0.30 Wm–2 and the feedback parameter is $-0.91\ Wm^{-2}K^{-1}$.

Unlike E_{WV}, E_C (blue +) is not well-correlated with ΔT. In addition, the spread of the blue plots is large. This means that the amount of rapid adjustment due to cloud changes varies largely, depending on the simulated state of ESM. The feedback of SW cloud radiative effect is not dominant in G4 experiment.

The y-intercept of E_{SA} (green x) is almost zero, so that the rapid adjustment from the surface albedo change is negligible. The feedback parameter is $0.38\ Wm^{-2}K^{-1}$ in the multi-model mean, and the strength (absolute value) of the feedback is less than a half of that of E_{WV}.

Page 16, line 10–17, Section 5

The decomposition analysis has revealed that about 37 % (multi-model mean) of Ewv is explained by the rapid adjustment and the rest is the feedback. On the other hand, almost all amount of E_C consists of the rapid adjustment, and a linear relationship between E_C and ΔT for the global and annual mean was not obtained for any models. The cloud rapid adjustment in G4 deduced in this study is similar as found for G1 by Kravitz et al. (2013c) but disagree with that in the 4xCO2 experiment shown by Andrews et al. (2012). Because the rapid adjustment due to changes in clouds can be caused by various processes (e.g., changes in atmospheric stability), it is possible that the cloud rapid adjustment differs between SRM and global warming. More detailed studies on effect of clouds in SRM are required for the reduction of the uncertainty and for a better assessment of impact of the sulphate geoengineering on climate and human activities.

Use of the surface radiation budget

The surface energy budget is not closed with respect to the radiation and it is widely recognized that changes in surface radiation are balanced by turbulent energy fluxes with only small temperature adjustments. Generally, the radiative changes are viewed at a level where the system is closed with respect to radiation – either the tropopause or TOA. It is fair to challenge this paradigm and the surface radiative budget may be useful for geo-engineering but that point should be discussed and analyzed, not taken for granted as it is in the current manuscript.

=>We agree with the reviewer that the system is closed with respect to radiation at TOA and the energy budget or balance is generally viewed at TOA. However, this study intends to estimate forcing and reactions to the surface SW radiation, which is important to consider the influence of SRM, especially for vegetation and human activities. Exploring full energy budget or balance is out of scope of this study. (We do not consider that it is much meaningful to struggle with the energy balance in G4 experiment; because, the baseline experiment RCP4.5 is a scenario experiment and does not reach statistically equilibrium state.) As the reviewer pointed out, the description for "why this study analyses surface SW radiation" in the previous manuscript was too short. In the revised manuscript we explained our motivation and purpose of this study at the end of Introduction and repeated at the end of Section 3.1 as follows:

Page 3, line 30–Page 4, line 13, Section 1
A simple procedure is used for quantifying the contributions of different types of SW rapid adjustments and feedbacks to the climate model behaviour to geoengineering with stratospheric sulphate aerosols. Here, a rapid adjustment is defined as a reaction to the SRM forcing without changes in globally averaged surface air temperature, whereas a feedback is defined as a reaction due to surface air temperature changes in the global mean induced by the SRM forcing (e.g., Sherwood et al., 2015). (Hereafter, the term "total reaction" refers to the sum of a rapid adjustment and a feedback.) In the recent studies of the climate change, rapid adjustments are included in forcing agents and the concept of effective radiative forcing is widely used. However, for the study of the sulphate geoengineering simulation, which is not well verified by observations and thus is expected to have many uncertainties, the separation of the direct forcing and total reactions is important to improve the simulation and to enhance the degree of understanding of the sulphate geoengineering by refining individual related processes. Many studies on climate energy balance have analysed changes in the net radiation flux at TOA, where the energy budget is closed by SW and longwave radiation (LW). However, in the geoengineering study, the radiative changes at the surface are also important, because vegetation, agriculture, and solar power generation for example will be strongly affected by radiative changes at the surface as well as surface temperature changes. Though the surface energy budget is balanced among SW, LW, sensible heat flux, and latent heat flux, Kleidon et al. (2015) showed that the latter three are mainly determined by the air and/or surface temperature. Hence, this study focuses on changes in surface air temperature and SW. The direct SW forcing to the surface are evaluated by considering the total reactions due to changes in water vapour amounts, cloud amounts, and surface albedo. Also, these total reactions are decomposed into adjustments and feedbacks, which indicate the rapid change just after injection of SO2 and the change with globally averaged surface air temperature change by SRM, respectively. We provide results for both global and local effects, focusing on cross-model commonalities and differences.

Page 8, line 25–Page 9, line 3, Section 3.1
This figure shows a strong correlation between the mean ΔT and ΔF_{SURF}^{net}; the correlation coefficient for the six filled symbols is 0.88. This strong correlation allows ΔF_{SURF}^{net} to be used as a measure of the SRM effects at least for $-1.1 < \Delta T < -0.2$ K, although the surface air temperature depends on the energy balance among SW, LW, and sensible and latent heat fluxes at the surface. Moreover, as described at the end of Section 1, it is important to explore the SW flux at the surface to estimate the effect of SRM on vegetation and human activities such as agriculture and solar power generation. Therefore, this study mainly focuses on SW at the surface and estimates the SRM forcing and the total reaction of SW due to changes in the water vapour amount, cloud amount, and surface albedo.

In particular, one place the surface radiative changes are less than useful is the interpretation of atmospheric solar absorption on the surface energy budget. As the atmosphere warms and moistens it absorbs more shortwave radiation that would have otherwise mostly (since the majority of the Earth's surface is dark) been absorbed at the surface. As a result, less shortwave is fluxed to the surface, which would be seen as a cooling influence on the surface. Yet, in the column average, slightly more shortwave is absorbed. Since most of this additional shortwave absorption occurs in the lower troposphere, where water vapor is abundant, it is tightly coupled to the surface energy budget and will warm the surface even if the surface shortwave flux is reduced as a result. Radiative kernels estimate this feedback to result in +1.0 W m^-2 K^-1 more absorption in the atmospheric column and +0.3 W m^-2 K^-1 as measured at the TOA (Donohoe et al. 2014, Shortwave and longwave contributions to global warming under increasing CO2, PNAS). Therefore, the surface feedback would be deduced to be -0.7 W m^-2 K^-1 with the wrong sign and more than twice the magnitude of the changes at the TOA. In the very least, the manuscript should include similar diagnostics at the TOA to resolve this sign paradox and a discussion of these points to support the assertion that surface shortwave changes are a useful metric.

=>As we described above, we consider that it is important to explore surface SW radiation under SRM. We agree with reviewer's comment that the increase of the water vapour gives a positive feedback in total (i.e., sum of SW and LW effects), and in the case of geoengineering, the less water vapour may give cooling effect in total. We recognized that the use of word "heating" for the water vapour and cloud effects was misleading, because we only consider changes in SW at the surface. We changed the expression in the manuscript to describe that changes in water vapour and cloud amounts increase the SW radiation at the surface.
We also include the similar analysis at TOA and discuss the difference between the surface and TOA in the new Section 4.1. Especially, difference in the water vapour effect is notable and well explained. The explanation is consistent with the reviewer's above comment.

Page 13, line 23–Page 14, line 23 Section 4.1

4.1 Difference between the surface and TOA

This study has focused on the surface net SW because of its importance to human activities. However, the situation at TOA is also of interest. Now, we discuss how the measures used in this study differ when TOA is used for the analysis. The net SW at TOA can be written as

[Equation 18]

so that the direct forcing of SRM and the total reactions measured at TOA (F_{SRM}^{TOA}, E_{WV}^{TOA}, E_C^{TOA}, and E_{SA}^{TOA}) can be calculated in the same manner described in Section 2. Figure 10 shows their globally and temporally averaged values' dependencies on ΔT. The difference of F_{TOA}^{net} is also plotted.

The qualitative features of the measures other than E_{WV}^{TOA} are same as the analysis at the surface shown in Fig. 6. The quantitative difference in the SRM forcing ($F_{SRM}^{TOA} - F_{SRM}$) is as small as –0.047 Wm^{-2} (1.8 %) for the multi-model mean. In contrast, $|E_{SA}^{TOA}|$ is less than that of $|E_{SA}|$ by about 35 %. This is because the upward shortwave radiation that was reflected at the surface must pass the atmosphere being decreased by the absorption and reflection before reaching TOA. The difference of $E_C^{TOA} - E_C$ is 0.12 Wm^{-2} (16.5 %) for the multi-model mean. Remember that the effect of the cloud amount change includes both changes in reflection rate (R^{cl}) and absorption rate (A^{cl}). The effect of a change in R^{cl} should appear almost equally at the surface and TOA, as the case for the SRM forcing, because both R^{cl} and R^{cs} appear in the Eqs. (7) and (18) in the same way. Therefore, most of $E_C^{TOA} - E_C$ should be caused by the difference in how the change of the absorption rate affects the net SW at surface and that at TOA. This is discussed below.

The total reaction at TOA due to the change in water vapour amount shows a negative sign at TOA, which is opposite to that at the surface. This disagreement is attributed as follows: Surface cooling reduces the amount of water vapour in the atmosphere and the SW absorption rate decreases. Then, more incoming solar radiation reaches the surface, so that the decrease in water vapour amount brings increase of SW flux at the surface. On the other hand, when the SW absorption rate decreases, the more upwelling SW that was reflected at the surface pass through the atmosphere and reaches TOA. This leads to a cooling effect. Because the effect of decrease in the SW absorption rate is carried to TOA by the upwelling SW that was reflected at the surface by the rate of α, $|E_{SA}^{TOA}|$ it is much less than $|E_{SA}|$. This does not mean that the change in water vapour is negligible for the energy budget at TOA, because we have not explored LW in this study. An analysis of LW rapid adjustment of clear-sky is discussed in the next subsection, but that of clouds and LW feedback is left as our future work.

From the above discussion, we have found that the effect of changes in atmospheric SW absorption rate appears differently between at the surface and at TOA (in its sign and amount), but that in reflection rate appears almost equally. The effect of change in the surface albedo is weaker at TOA than at the surface. We will bear these properties in our mind, when we discuss the influence of SRM on the energy budget of the climate system, which is usually considered at TOA, and human activities, which are mainly performed at the surface.

To play devil's advocate, it seems like most of correlation between the temperature response and net surface shortwave comes from the forcing. Is the use of net shortwave at the surface a better predictor of the temperature (statistically distinguishable) from that of forcing alone (surface or TOA)? The latter certainly would result in a stronger regression – and one more consistent with climate sensitivity—than using surface shortwave even if the correlation is slightly worse. More generally, what would the correlation be if one used forcing alongside published estimates of the model's climate sensitivity in response to CO2? It looks like the outlier from the strong relationship between forcing and response is the MIROC-CHEM-AMP which has a pronounced cloud feedback. As suggested above, I believe that cloud feedback is misidentified and is really an effective forcing associated with rapid cloud changes due to the direct impact of the aerosols. I think that calculating the effective forcing may offer a better correlation with the climate response than the net surface shortwave metric used in the manuscript.

=>We calculated ERF and found that ERF has a slightly better correlation than ΔF^{net}_{SURF}, as the reviewer expected. However, finding the best predictor of ΔT is not the aim of this study. Although the ERF would be the better predictor of ΔT, ERF is a sum of forcing due to the SW reflection by injected sulphate aerosols and the rapid responses of many other modelled physical processes in the ESMs. Therefore, it is difficult to explore, estimate, and compare contributions of each process to change in SW at the surface, by using ERF. Similarly, using the climate sensitivity to CO_2 increase estimated in the published papers will not give information about the contribution of each modelled process. We considered the description about the aim of this study was not enough, so that we added more description in Introduction as we showed above.
(The reviewer is correct in the point that the "cloud feedback" which we previously called was not a feedback but a rapid adjustment.)