# Response to the reviewers

We thank the reviewers for their critical assessment of our work. In the following we address their concerns point by point.

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# Reviewer 1

## General comments

**Reviewer Point P 1.1** — Moseid et al. compare surface downwelling shortwave radiation from CMIP6 models and from ground stations. They show the discrepancy between modeled and observed SDSR is partly caused by erroneous aerosol and aerosol precursor emission inventories, thus providing important information for the evaluation of ESM. While the research topic is essential, the methodology can be improved to clarify the impacts of clouds and cloud-aerosol interaction. Instead of using all-sky SDSR, I would suggest the authors compare the sunny-day SDSR from CMIP6 and from ground stations throughout the whole text.

**Reply**: We agree that the manuscript should include a description of the impact of clouds and cloud-aerosol interactions. A new part was added in line 27-36 in the revised manuscript:
*Aerosol particles cause changes in the amount of sunlight reaching the surface together with changes in insolation , cloud cover, water vapor and other radiatively active gases (Wild et al., 2018). Insolation at the top of the atmosphere changes on millennial timescales when the Earth's orbital parameters change, and the famous 11-year cycle does not create multidecadal trends in surface radiation. Water vapor amount have not changed in a sufficient magnitude in recent decades to have an effect on decadal fluctuations in incoming sunlight at the surface (Wild (2009), Wang and Yang (2014), Yang et al. (2019), Hoyt and Schatten (1993), Ramanathan and Vogelmann (1997), Solomon et al. (2010)), and radiatively active gases dominate in the longwave spectrum (Ramanathan et al. (1989)). The relative roles of clouds, aerosols and their interactions in the variations of historically are still disputed, but previous studies have found that aerosol effects dominate on multidecadal timescales, while cloud effects are relevant on shorter timescales (Wild (2016),Romanou et al. (2007)).*

And Section 3.4 "Clear sky and cloud cover in China" has been improved throughout, including changing Figure 3 into a table that is easier to read and analyze.
In The discussion section we have added the lines 326-330:
*Analyzing the RFMIP simulations in light of Folini and Wild (2015) points to aerosol effects dominating over cloud cover for causing multi-year SDSR changes in China. As mentioned in the introduction, the relative roles of clouds, aerosols and their interactions in dimming and brightening are still disputed. Table 2 showed inconclusive connections between modelled and observed cloud cover, but clear connections between clear sky SDSR and all sky SDSR.*
Unfortunately neither GEBA nor CMIP6 models provide sunny day SDSR. Previous studies such as Allen et al. (2013) have used the GEBA data set to create a clear sky proxy for a selection of stations to compare with the clear sky flux variable of CMIP models. However, this is beyond the scope of our study.

**Reviewer Point P 1.2** — To be more accurate, I would also suggest the authors compare the SDSR conditions on the atmospheric relative humidity, which is associated with the scattering from water vapor.
Note that the clear-sky SDSR from climate models is usually used for calculating cloud radiative forcing and is not the same as sunny-day SDSR.

**Reply**: We are looking at longtime fluctuations in SDSR. Water vapor has not changed in a sufficient magnitude in recent decades to have an effect on decadal fluctuations in SDSR (Wang and Yang (2014), Yang et al. (2019), Wild (2009), Hoyt and Schatten (1993), Ramanathan and Vogelmann (1997), Solomon et al. (2010)). This was added in the new version of the manuscript in line 26-33 as cited above. We therefore assume in this study that the SDSR effects of water vapor scattering is negligible.

### Minor comments

**Reviewer Point P 1.3** — The title: I would not use the "1961-2014" in the title. It provides little information.

**Reply**: Fixed.

**Reviewer Point P 1.4** — The title: compare to -> compare with.

**Reply**: Fixed.

**Reviewer Point P 1.5** — The title: maybe the authors should include "aerosol", which is the theme of the paper

**Reply**: Fixed.

**Reviewer Point P 1.6** — Figure 3: Please double check the cloud fraction and the calculation of anomaly. If the trend is reversed, it explains everything.

**Reply**: This is double checked and the presented Figure was correct. In the new version of the manuscript this Figure has been made into a table. (Table 2 in the revised version)

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## Reviewer 2

### General comments

**Reviewer Point P 2.1** — It would improve the paper if more background information in the introduction section was provided on the key drivers of SDSR i.e. clouds and greenhouse gases can also influence SDSR in addition to aerosol effects.

**Reply**: We thank the reviewer for the comment and agree more background information should be provided regarding SDSR. We have added a more detailed description of what can influence SDSR in line 27-36 in the introduction, as cited in our reply to reviewer point P1.1.

**Reviewer Point P 2.2** — Throughout the paper there are numerous mentions to the fact that aerosols play a key role in the dimming signal of SDSR observed and simulated across all regions. However, the same cannot be said for the observed brightening signal in more recent years. A key question seems to be why are aerosols a key driver in the dimming but not brightening?
If the emission inventories and aerosols were in error throughout the whole period of study then surely the models would not be able to simulate the temporal evolution of both phenomenon across all regions?

**Reply**: We respond to this point in two parts - first the role of aerosols in brightening:
We would like to point the reviewer to the studies by Allen et al. (2013), Chiacchio et al. (2015) and Wild (2012), which show that indeed aerosols are a key driver to the observed brightening in recent years. The reduction of anthropogenic aerosol emission leads to brightening. We would also like to thank the reviewer for mentioning this point that we did not explicitly make in the original manuscript. The following text has now been added in line 45-48:
*In some areas a positive trend in SDSR follows the dimming, and this SDSR increase has been termed "brightening" (Wild et al., 2005). Brightening is connected to the reduction in anthropogenic aerosol emission (Nabat et al., 2014). Less particles suspended in the air allows for more sunlight to reach the surface and thereby an increase in the measured SDSR.*

Then the final question on emission inventories: Correct, this is why we are proposing errors in emission inventories as a possible reason for discrepancies in the regions where the models are not able to simulate the temporal evolution of dimming (and brightening).

**Reviewer Point P 2.3** — The paper states that the CMIP6 models are able to represent the observed SDSR signal over Europe relatively well. However, I think there are a few interesting discrepancies which should be discussed further. Prior to 1980 the observations do not show much of a dimming signal (in fact the observed anomaly is slightly positive at times) but the CMIP6 models do show a consistent dimming signal. Is there a specific reason for the absence of a dimming in the observations, when we know there were large concentrations of aerosols over Europe at this time? Contrary to what was mentioned in point 2 above Europe is the only region where there is a simulated brightening signal in both the model and observations, implying that models are able to reproduce brightening signal over certain regions. It would be good to know if there a reason for this over Europe and does it occur over other regions like for example North America.

**Reply**: The referee is right that Figure 1b does show some interesting discrepancies in the beginning of the time period in the study that was not mentioned in the original manuscript. The observational data set used in this study starts in 1961, and the anomaly shown in the figure is made as a difference from the the mean value of SDSR from 1961-1966. Since the European dimming started before 1960 (Wild, 2012) the "true" European SDSR anomaly might not be achieved using this data set, as is also seen by the weak European dimming in Storelvmo et al. (2018) using the same data set.
In the new version of the manuscript we have added in line 197-2000:
*The dimming in Europe is believed to have started before the start time (1961) of the observational*

*data set used here (Wild, 2009), which partly explains why the dimming in Figure 2 (b) is weak. GEBA shows a short-term positive anomaly between 1970 and 1980, which is not caught by the models. This peak is currently unexplained, but a short assessment of its possible association to changes in cloud cover is found in Section A2 the Appendix.*
The observed and simulated brightening in Europe are quite comparable and we therefore propose that the emission inventories of aerosols in Europe are estimated well.
North America has not been shown in Figure 1, but is included here in the reply as supplementary Figure S1. See Leibensperger et al. (2012b) and Leibensperger et al. (2012a) for a closer look at the climatic effects in North America due to anthropogenic aerosol emissions. We chose Europe and Asia as areas of focus to give the readers a clean impression of one example region where the models perform well and one example region where they do not perform well.

**Reviewer Point P 2.4** — For the analysis over China the paper suggests that the error between the models and observations of SDSR are due to the errors in emission inventories that translate into errors in the calculation of atmospheric burden of aerosols. (1)Are we certain that the errors in the emission inventories are that large to account for the discrepancy in model and observed SDSR? Is there an estimate of the uncertainty for the CMIP6 emission inventory and how does CMIP6 compare to other global and regional emission inventories? (2) Can these differences explain some of the inconsistencies of models with observations? I am not convinced that the observed trend reversal in SDSR over China in 1990 can be explained by errors in the emission inventories alone. (3) Are we anticipating a slowing down of $SO_2$ emissions in China from the 1990s onwards? As far as I understand, anthropogenic emissions of aerosols and their precursors (particularly $SO_2$) have largely been increasing over China up until 2010 when air pollutant control measures were then implemented to reduce emissions. Therefore, if aerosols were driving the temporal change in SDSR over China a dimming signal should have been observed up until this point, but it isn't. This is present in the observed and simulated change in SDSR over India but not China. (4) How do this discrepancy match with the conclusions of the paper and what else could be driving the SDSR trend over China throughout this period? I think this needs to be explored further in the paper as the assumed underlying trend in emissions (and therefore aerosols) and SDSR do not seem to match over China and from what I can tell it cannot be reconciled by errors in the emission inventories alone.

**Reply**: To make it easier for the reader we have marked up numbers to the questions in the reviewer point. (1) We are not certain that the errors in the emission inventories are large enough to account for the discrepancy in model and observed SDSR alone, but we suggest that this error plays an important role. Unfortunately there is no estimate of uncertainty in the CMIP6 emission inventories, but this is planned to be included in the next generation of CMIP emission inventories (see Hoesly et al. (2018)). Due to the lacking estimation of uncertainty we do not have evidence to say that errors in emission inventories are too small to cause a discrepancy between model and observation.
(2) The CMIP6 emission inventory is made using CEDS that makes datasets based on EDGAR, as is described in more detail in Hoesly et al. (2018). There are probably some differences between the CMIP6 data set and other regional emission data sets, but this study does not look further into finding such differences. We propose at least some of the discrepancy between model and observed SDSR is caused by errors in emission inventories, but we do not have enough evidence to claim that all discrepancies are emission driven. We recognize that the original manuscript may have given the wrong impression to the reader that errors in emission inventories *alone* cause all discrepancies, and this has now been

addressed and made clearer throughout the text.
(3) During the review process we found more information regarding the observed trend reversal in the GEBA data in China. According to the CMIP6 data set of sulfate emission we do not expect a slow down of emitted $SO_2$ from 1990, but rather from around 2005. The observed trend reversal in SDSR does therefore not fit with CMIP6 emitted sulfate. However, previous studies have found that the trend reversal in SDSR is to a considerable extent caused by the fact that the measurement devices at the Chinese radiation network stations were replaced with new ones between 1991 and 1993, which caused a spurious upward jump in the records (Wang and Wild (2016), Wang and Yang (2014), Yang et al. (2019)). With this new information we have added a Section 4.1 "The trend reversal in China" under Section 4 "Discussion" where we compare our results to that of Yang et al. (2018) where the "jump" has been removed by homogenization. The main point from this section can be summarized as in line 359-360:
*Models do not accurately represent the strength of dimming, or the evolutionary pattern of SDSR observed in China with or without the early 1990s brightening* (the "jump").
(4) The conclusions of the paper propose that errors in anthropogenic aerosol emission inventories play a role in the discrepancy seen between simulated and observed SDSR. Even if the trend reversal in the observed SDSR in China was to be an artifact, the models would still largely underestimate the magnitude of dimming. With regards to the trend reversal, the assumed underlying trend of increasing sulfate emission until 2010 as proposed by the reviewer (and CMIP6) is being questioned in this manuscript, as even though Wang and Wild (2016) suggests most of the "jump" is an artifact, they still estimate that 20% is real. We thank the reviewer for this comment and hope the new Section 4.1 is satisfactory.

**Reviewer Point P 2.5** — Only limited discussion within the paper is provided on clouds and aerosol-cloud interactions, which needs to be improved throughout the paper. Within section 3.3 a link is made between cloud cover change and SDSR but how much of an influence do clouds have on all-sky SDSR? How reliable are the observed and simulated cloud cover changes and can some uncertainty bounds be placed on them? Is a regional cloud cover change of 1-2% significant in terms of SDSR? In figure 3 the temporal change in observed cloud cover is similar to that in observed SDSR so even if clouds can't explain the magnitude and all of the observed change then surely they must be exerting some influence on SDSR? Is it possible to compare a clear-sky derived observed SDSR to that from model simulations to eliminate any influence of clouds on the signal?

**Reply**: We thank the reviewer for this comment, and would refer to our reply to reviewer 1 point 1 (P1.1) where we cite lines from our new manuscript regarding clouds' role in all sky SDSR. In the appendix of the new version of the manuscript (Section A2) we have added an idealized estimation for how much a theoretical 1 % cloud cover increase in China would affect SDSR. Line 435-436:
*... in China, the theoretical effect of 1% increase in cloud cover on all sky SDSR is between -1 and -3.5 W/m$^2$, using the idealized computation described above.*
Previous studies have found that the link between cloud cover and SDSR trends depends on what region you are looking into. In Europe cloud cover has most of an effect on SDSR on the shorter time scales, and the dimming and following brightening observed in Europe is dominantly caused by changes in anthropogenic aerosol emission and thereby the aerosol absorption and scattering (Norris and Wild, 2007). In China cloud cover made a negligible contribution to all sky SDSR trend in GEBA until 1989. After 1980 the heavily discussed trend reversal is observed in China, and Norris and Wild (2009) suggests half of the observed brightening between 1990 and 2002 is caused by a reduction in cloud cover. Please note that this paper was published before the proposal of the trend reversal being an artifact of a change

in instrumentation (Wang and Wild, 2016). This complicates the story and is the reason Norris and Wild (2009) is not discussed in the new version of our manuscript.
In North America cloud cover is found to have played an important role in the observed brightening (Long et al., 2018). Other studies have made clear-sky derived observed SDSR (Norris and Wild (2007), Norris and Wild (2009)) when assessing the cloud signal for Europe and China (mentioned above in this reply), but this goes beyond the scope of our study.

**Reviewer Point P 2.6** — The previous comparison of CMIP5 models to observed SDSR by Storelvmo et al., (2018) is mentioned throughout this study, with similar results presented here for CMIP6 models. A key question is therefore why has there been no improvement in simulating observed SDSR between CMIP5 and CMIP6 models? This is despite some changes to individual aerosol schemes within models and also different historical aerosol precursor emission datasets being used. Some discussion is needed on what is continually missing from the models and what are the model developments to focus on to improve the future simulation of SDSR.

**Reply**: To answer this question we must first find out whether the source of the error is within the model's codes or within the emission inventories, or a combination. Storelvmo et al. (2018) argues that the discrepancy between observed and modelled SDSR may be attributed to errors in the treatment of processes that translate aerosol emissions into clear-sky and all-sky radiative forcings. Here, we show that simulated SDSR develops similarly in time, but opposite in sign, to simulated atmospheric burden of SO2. By doing this we narrow down the potential source of error by suggesting that the atmospheric burden in the models are at fault, and that the processes translating burden into clear-sky and all-sky radiative forcings are behaving as expected. The final answer of what is at fault is still not found, but we suggest to have found a piece of the puzzle in the emission inventories.
It is important to note that Storelvmo et al. (2018) included all CMIP5 models, and we "only" include eight models.
We thank the reviewer for this comment and have updated the end of the conclusion in the new manuscript. Lines 409-414 is added:
*As the observed climate change is the result of warming from greenhouse gases and simultaneous cooling from aerosol radiative effects, getting aerosol emissions correct is an important part in earth system models' ability to simulate the past for the right reasons.*
*Since the SDSR measurements are not only sensitive to aerosol effects, further studies could include other observations and proxies for aerosol effects in the historical era, such as long-term satellite retrieved aerosol optical depth, deposition of anthropogenic sulphur, organic carbon and nitrate in ice cores, as well as daily temperature range records.*

**Reviewer Point P 2.7** — Further details are required, either in Table 1 or a new table, on each of the CMIP6 models used in this study. In particular, it would be useful to know horizontal resolution and some information on the individual chemistry and aerosol schemes in each model. This could provide useful information to the reader of the potential causes of discrepancies between models. In addition, it would be good to have a record somewhere of the actual output used from the ESGF (e.g. temporal period, variant ID, CMIP table ID etc). Furthermore, if there is data now available for additional CMIP6 models then it would be useful to include it, as long as it further informs the current study.

**Reply**: We thank the reviewer for this comment and we have added Table A2 in the Appendix of the paper listing information such as variant ID, variables, references to model documentation, horizontal resolution and aerosol scheme. More data has been published since the first submission of this paper, and we have therefore decided to include more models in this study. Three models have been added (GISS-E2-1-G, IPSL-CM6A-LR, MRI-ESM2-0) to the analysis as more data was released.

**Reviewer Point P 2.8** — The methods section (2.3) appears to lack important details of what model data is being used (see point 7) and how the gridded model data has actually been compared to the observations which are at point locations. In calculating the regional means at observation locations, do the number of sites used change over time period and does this have any impact on the results? Furthermore, in the results section the clear-sky SDSR is discussed but is not mentioned in the methods section. I also think that it is important to use multiple ensemble for meaning purposes when using coupled experiments members from models so that the internal variability in each model can be shown (this would give a range of variability important to show on some of the Figures for certain variables).

**Reply**: We thank the reviewer for this comment and agree that the methods section was indeed lacking both clarity and details. In Section 2.3 "Methods" in the new manuscript we have added line 138-140:
*All model output and CRU results have been co-located to GEBA station locations using the nearest neighbour method. This entails that if two GEBA stations are within one grid box of a model, data from that grid box will be retrieved twice by nearest neighbour interpolation, as every station has been weighted equally.*
We tackle the question regarding number of sites used in time in both Section 2.1 " Observations" with line 79-81 added in the new manuscript:
*This allows for all 1487 stations to have data on each time step, so that all regions have a complete record and the same amount of stations throughout the entire time period in question.*
And in Section 2.3 "Methods" in line 135-136:
*The number of stations per region remains constant throughout the time period.*
In the new version of the manuscript we have added three ensemble members per model for the historical simulation. Both inter-annual variability and inter-ensemble variability is shown in Figure 1 of the new manuscript, that is presented in the new section in results called Section 3.1 " Model variability". We have changed Figure 1 in the old manuscript into Figure 2 of the new manuscript, where we present ensemble means per model, and show shading for the standard deviations of the total 24 ensemble members.

**Reviewer Point P 2.9** — A General comment on the figures is that they could be improved to make them easier to read by using better colours (I found the light green very bright), tick marks on the axis and line types that are easier to distinguish between different model experiments. Also, if it is possible to include a measure of observational and model uncertainty on any of the figures then this would improve the comparisons. When values from figures are continually referred to in the text it would help the reader if there is reference table containing some of the key numbers included (like the supplementary table).

**Reply**: We thank the reviewer and have chosen a different color chart for the figures, more tick marks, and different line types to better differ the graphs. Variability and uncertainty is shown in the new Figure 1 and Figure 2 as explained in the previous reply (P2.8). We are currently not referring to specific values until Section 3.4 "Clear sky SDSR and cloud cover in China" where we have changed the previous Figure 3 into a table to make the point more clear and the discussion easier to follow.

## Minor comments

**Reviewer Point P 2.10** — Page 1, line 9 – Reword this sentence as mentioning SO2 emissions, which are not aerosols, and then other aerosols relevant to SDSR. Be more precise in this statement.

**Reply**: Changed to line 11: *The emissions of $SO_2$ used in the models show no pattern that could explain the observed SDSR evolution over China, and neither do emission of aerosols relevant to SDSR, such as black carbon (BC).*

**Reviewer Point P 2.11** — Page 1, line 13 – Can you say how much error is associated with aerosols and emission inventories that might contribute to error in SDSR?

**Reply**: Unfortunately the emission inventory data set for CMIP6 does not have estimates of uncertainty, which is why we chose the word "partly" in line 13 as we have no evidence telling us how much of the discrepancy can be attributed to emission estimates.

**Reviewer Point P 2.12** — Page 2, Line 30 – Is this statement true across all regions? What about for Europe?

**Reply**: This statement is only true globally based on previous studies. Added the word global in line 48: *Previous studies show that historical simulations from ESMs do not reproduce the global transient development of SDSR as observed (Storelvmo et al. (2018), Wild (2009)).*

**Reviewer Point P 2.13** — Page 2, line 35 – For the introduction it would be good to include a bit more detail on what the GEBA observations on their own show before introducing any comparisons to models.

**Reply**: We thank the reviewer for pointing this out, as the introduction to global dimming mentioned several citations that all used GEBA to identify dimming (and regional brightening), which was not explicitly mentioned. This has now been clarified in the text in line 53-56: *this study we use gap-filled data based the GEBA dataset. The GEBA dataset is the observational dataset as used in the citations in the previous paragraph, together with several recent CMIP6 historical model experiments from eight ESMs to investigate the aerosol effect in the time period 1961-2014, globally and regionally.*

**Reviewer Point P 2.14** — Page 2, line 46 – here the study says that two observational datasets are used but only one has been mentioned in the previous paragraph. Please include details of what is the second dataset used in this study.

**Reply**: The second observational data set has been added in line 58-59: *We also use observational cloud cover data to briefly assess the role of cloud cover in the historical development of SDSR.*

**Reviewer Point P 2.15** — Page 2, line 47 – please reword sentence "An explanation of the methods used to obtain and analyse the data complete Section 2."

**Reply**: Changed to line 66-67: *The methods used to obtain and analyse the data finalize Section 2.*

**Reviewer Point P 2.16** — Page 3, line 57 – it would be good to include the error in the observations on all figures to show the uncertainty in the observations.

**Reply**: Unfortunately sources of error in observation differs from station to station and we only have a general estimation of error from the instruments used. In addition to the instrumental error presented in line 76 we have chosen to include a light grey line with the highly variable yearly observational data in the background of Figure 2 in the new version of the manuscript.

**Reviewer Point P 2.17** — Page 3, line 60 – Please clarify if this temporal gap filling technique allows for all 1487 stations to have a complete record of observations over the entire 1961-2014 and how this technique impacts the observations. If the number of stations used changes over the entire time period then it could be important for the analysis.

**Reply**: This has been clarified and is cited in reply to P2.8.

**Reviewer Point P 2.18** — Page 3, line 74 – insert 'is' between "these the"

**Reply**: Fixed.

**Reviewer Point P 2.19** — Page 4, line 93 – replace 'stales' with "stalls"

**Reply**: Fixed.

**Reviewer Point P 2.20** — Page 4, line 94-95 – "So these experiments will show to what extent the removal of cloud cover change from global warming has an effect on SDSR." – I am sure that this is the case as there will be still be variability in the cloud fields simulated by climate models in these experiments. In addition, as the aerosol fields are changing in these experiments, they will also impact the simulated clouds in the models. Therefore, to make this statement further evidence would be required from each model that the cloud fields are being properly constrained to isolate their impacts on SDSR.

**Reply**: We are not stating that all cloud cover change is removed, only the cloud cover change that is induced by global warming - as global warming essentially is removed in these experiments. Cloud cover will change due to aerosol emissions and thereby impact SDSR - but not due to global warming. Changed the wording to line 115: *So these piClim-experiments will show to what extent the removal of cloud cover change caused by global warming has an effect on SDSR.*

**Reviewer Point P 2.21** — Page 4, line 107 – It would be good to show on a figure the spatial distribution of the GEBA observations within each defined region.

**Reply**: Storelvmo et al. (2018)s Figure 1 is an excellent figure showing the spatial distribution of the stations used in both this and her study in addition to the trends of the stations in colours. I have added a reference to that figure in line 136.

**Reviewer Point P 2.22** — Page 4, line 110-112 – Please clarify exactly how anomalies have been calculated. Are anomalies calculated for each individual observation site within a region first before then calculating a regional mean value?

**Reply**: Clarified. New line 142-146: *When a result is shown as an anomaly, as opposed to an absolute value, the general formula has been to subtract the mean of the first five years of the investigated time* *period (1961-2014) from the timeseries in question. To clarify - first an average value per year per region is calculated, and then a new mean is created from the first five years of this timeseries. This 5-year-mean is then subtracted from each year in the timeseries for the region in question and presented as an anomaly.*

**Reviewer Point P 2.23** — Page 4, line 112 - Supplementary table number is not shown

**Reply**: Fixed.

**Reviewer Point P 2.24** — Page 4, line 113 – Provide more information on exactly what model data has been obtained from the ESGF (perhaps in a separate table) e.g. CMIP table ID, variant label etc. (see general comment 8)

**Reply**: We thank the reviewer for this request and a table has been added as cited in the reply to P2.7

**Reviewer Point P 2.25** — Page 4, line 115 – I think it would be more prudent to use more ensemble members for coupled experiments and with this an idea of the internal variability for each model could be obtained for variables such as cloud cover and SDSR.

**Reply**: Three ensemble member have been used in the historical experiment, see reply to P2.8 for citation.

**Reviewer Point P 2.26** — Page 4, line 116 – It is not clear if the 10-year running mean is used for the model data, observation data or both?

**Reply**: Running means have been exchanged for 6-year-intervals means in most figures in the new manuscript. the only exception if Figure 4 which shows $SO_4$ burdens form models as a 10-year running mean, while the observation is shown as yearly data. This is clarified in the figure caption.

**Reviewer Point P 2.27** — Page 5, line 121 – it is hard to see from Figure 1 a) as to whether the global SDSR representation in the models is similar to the observations at all. There is clearly a difference in magnitude but there does not appear to be a strong dimming signal in many of the models. Is this just the scale on the figure or is there not much change in the model at all? Can the Figure be improved in any way to make this easier to see?

**Reply**: Figure 1a) corresponds to Figure 2a) in the new manuscript. The models generally do not represent the *global* change in SDSR as observed. We have included gray shading for the ensemble standard deviations and changed the method from a running mean to 6-year-interval-means to show clearly the weak signal in the models in Figure 2 of the new manuscript.

**Reviewer Point P 2.28** — Page 5, line 122 – Change “these discrepancy originate” to “this discrepancy originates”

**Reply**: Fixed.

**Reviewer Point P 2.29** — Page 5, line 125 – More discussion on European model observational differences (see general comments point 3)

**Reply**: This discussion has been added and is cited in reply to P2.3.

**Reviewer Point P 2.30** — Page 5, line 135 – I think that this is only true for certain models as others seem to have opposite temporal changes compared to observations e.g. NorESM2.

**Reply**: We agree and this line has been removed.

**Reviewer Point P 2.31** — Page 5, line 138 – It is hard to say without tick marks on the figures as to whether the end points in models are similar to the observations. For example, is a -10 Wm-2 anomaly in 2014 from GEBA considered to be similar to a -6 Wm-2 from NorESM2?

**Reply**: We agree that this statement was questionable in the old manuscript. By adding more models to the analysis the remark of similar end points between model and observations became blatantly wrong and we have removed all statements regarding this.

**Reviewer Point P 2.32** — Page 5, line 140 – please explain what "temporal forcing evolution" means in this context.

**Reply**: This line has been removed due to the added discussion of the trend reversal in China in observations.

**Reviewer Point P 2.33** — Page 6, line 156-157 – does this imply that the greenhouse gases impact on SDSR over China throughout this period?

**Reply**: When adding more models to the study this implication became untrue, and the statement has been removed in the new manuscript.

**Reviewer Point P 2.34** — Page 6, line 157-158 – I am not sure this is true for all models. The temporal evolution of SDSR from CanESM5 seems quite different in the historical and piClim-histall but perhaps not so much in MIROC6.

**Reply**: With new models added to the study the entire RFMIP paragraph has been updated to line 239-244: *Recall that the experiments of RFMIP utilize pre-industial SST's, meaning essentially there is no global warming in these experiments. In the RFMIP experiments shown in Figure 3(c) both piClim-histaer and piClim-histall contain anthropogenic aerosol emissions, and all simulations show a continuous dimming throughout the period. There is no clear divide between experiments containing both GHG emissions in addition to anthropogenic aerosol emissions (solid lines, piClim-histall) and the experiments only containing anthropogenic aerosol emissions (stipled lines, piClim-histaer). This implies that greenhouse gases without their global warming effect do not largely affect all sky SDSR throughout the period.*

**Reviewer Point P 2.35** — Page 6, line 167 – Aerosols have a key role in dimming but not it appears brightening – why not? (see general comment 2)

**Reply**: We thank the reviewer for this comment and refer til our reply in P2.2

**Reviewer Point P 2.36** — Page 6, lines 168-169 – similar to point above in that there are differences between these simulations which don't appear to be the temporal driver of SDSR but perhaps can influence it? It would be good to show the actual differences between models in these simulations and what influence other factors (like clouds and greenhouse gases) can have on SDSR.

**Reply**: Clouds and greenhouse gases can influence SDSR, but are, as mentioned in the introduction, not a dominant driver of multidecadal SDSR changes. It is therefore expected to see small differences between these simulations. The overall picture of models showing dimming with anthropogenic aerosol emissions, and no dimming without it remains whether or not you include greenhouse gases or SST changes. This has been clarified in line 245-249: *Overall there is a clear difference in SDSR between experiments that include anthropogenic aerosol emissions and experiments that do not. Dimming is apparent in every simulation containing anthropogenic aerosol emissions, but absent in the simulations containing pre-industrial aerosols only. This points to anthropogenic aerosol emissions playing a key role in global dimming. Whether the sea surface temperature is pre-industrial, prescribed historical, or decided by a coupled ocean model seems to be unimportant for the SDSR in most models.*

**Reviewer Point P 2.37** — Page 6, line 173 – how has all-sky SDSR been decomposed into clear-sky?

**Reply**: This is a diagnostic that is output from the models. The general idea is that clear-sky SDSR from models represents the amount of sunlight reaching the surface if all shortwave effects from clouds were removed. Clear-sky SDSR is not to be confused with sunny day SDSR which is from actual cloud free days.

**Reviewer Point P 2.38** — Page 6, line 179-180 – Can the clear-sky and all-sky changes be shown on the same figure to compare differences?

**Reply**: This figure has been replaced by Table 2 in the new manuscript showing changes in cloud cover, all sky SDSR and clear sky SDSR for three different time periods.

**Reviewer Point P 2.39** — Page 6, line 182-189 –How have the changes in model cloud cover been calculated? This needs to be in the methods section. Also line 183-184 states that cloud cover changes mask the clear-sky SDSR signal. This implies that the clear-sky decrease would have been even larger without changes to clouds indicating that clouds do have an important influence on SDSR in models. I think this needs to be explained more - see general comment section 5 for more details.

**Reply**: Cloud cover is a standard output from climate models and has not been calculated by the authors, and the source of the data has been added in the Appendix of the new manuscript. The effect of clouds in SDSR has been added and is cited in our reply to P2.5.

**Reviewer Point P 2.40** — Page 7, line 193 – "session" should be "section"

**Reply**: Fixed.

**Reviewer Point P 2.41** — Page 7, line 194 – "In this session we found the clear-sky SDSR to be stronger than all-sky SDSR, indicating the simulated dimming is primarily caused by aerosol-radiation interactions." But also that clouds have an influence on SDSR too.

**Reply**: This sentence has been removed in the renewal of the manuscript. But in general clouds have an influence on SDSR which is clarified and cited in our reply to P2.5. but our findings are that aerosols effects are the dominating cause of dimming.

**Reviewer Point P 2.42** — Page 7, line 205 – "SO2 burden" is mentioned but should this not be SO4 burden.

**Reply**: Fixed.

**Reviewer Point P 2.43** — Page 7, line 205-206 – Given that all models have the same SO2 emissions, do we know why the changes in SO4 burden are so different between NorESM2 and CESM2? Could this indicate some of the potential problems in translating emissions into atmospheric burden or aerosols, which lead to errors in SDSR?

**Reply**: Burdens are a result of emission, aerosol formation, transport and deposition. The emissions in both models are the same but the other processes dependent on many different parameterisations within each model. The atmospheric circulation in CESM2 and NoreSM2 differs, among other things, so for example a sulfate particle may be brought higher up in the atmosphere in NorESM2 - giving sulfate a longer lifetime and thereby making NorESM2 have a higher sulfate burden. In addition to this these burdens are calculated using co-location to point locations as described in the methods section, and this is where transportation plays a role.

**Reviewer Point P 2.44** — Page 7, line 210 – can a more scientific term be used than "real story".

**Reply**: Definitely. Added line 307: *Assuming GEBA data provide a reasonable representation of the historical development of SDSR and implicitly sulphur burdens in China, the problem in $SO_4$ burden must come from either the emissions, aerosol formation, transport or the removal processes of $SO_4$.*

**Reviewer Point P 2.45** — Page 7, line 210-211 – This sentence makes the assumption that aerosols are the sole driving force in SDSR and that it is only the emissions and removal processes that could be in error. Other potential causes could be mentioned like the model translation of emissions to burden which leads to the larger differences in simulated SO4 burdens between models. Also see major comments above.

**Reply**: As cited in the previous answer we have added "aerosol formation, transport" in this sentence. The model burden is translated from emission, transportation and removal processes. We are assuming the model translation from emission to burden is behaving as expected in the two regions of special interest, and an explanation for this is found in the Dicussion in line 366-377:
*..we can see an anti-correlation between simulated $SO_4$ burdens from Figure 4 (a) and (b), and simulated SDSR from Figure 2 (b) and (f), respectively. Therefore we suggest that the simulated SDSR is dominantly a result of simulated $SO_4$ burdens. Since simulated SDSR agree relatively well with observed SDSR in Europe (Fig 2(b)), simulated $SO_4$ burden anti-correlates relatively well with observed SDSR in Europe as well (Fig 4 (a)). This means that the model code translating burdens into forcing in Europe is behaving as expected. Cloud cover affects forcing, and if models translate burden into forcing correctly in Europe, does not mean they translate burden correctly in other regions. We suggest the code translating burdens into forcing in China is also behaving as expected, due to the above statement*

*saying aerosols are the main cause of dimming in China (Wild, 2009; Yunfeng et al., 2001; Kaiser and Qian, 2002), and the lack of consistency found in simulated cloud cover and SDSR anomalies in China (Table 2). By suggesting the translation process from burden to forcing is behaving as expected in both regions, the potential source of error causing discrepancies between observed and simulated SDSR can be traced to the causes of the simulated atmospheric burdens in the first place.* If there is an error in burden than the error is sourced in either emissions, transportation or removal processes.

**Reviewer Point P 2.46** — Page 7, line 212 – "the precursor of SO2", should this not be SO4?

**Reply**: Fixed.

**Reviewer Point P 2.47** — Page 7, line 215-218 – Should we be expecting a trend reversal in SO2 emissions over China between 1980 and 1990? At this point in time emissions would have been increasing over China and emissions have only begun to reduce recently (since 2010). See general comment point 4

**Reply**: A section discussing the trend reversal was added and is cited in our reply to P2.4.

**Reviewer Point P 2.48** — Page 8, line 235 – Is it possible to include the clear-sky proxy from GEBA here and compare to that from models on Figure 3 to show how well models simulate the aerosol radiation interactions?

**Reply**: Unfortunately that is not easily done and is beyond the scope of this study. There is currently an NSF project working on creating clear sky proxies at ETH Zurich lead by Dr Martin Wild.

**Reviewer Point P 2.49** — Page 8, line 238 – change "shown in Figure displayed" to "(Fig. 2) show"

**Reply**: Fixed.

**Reviewer Point P 2.50** — Page 8, line 242 – But the magnitude of the dimming was not sufficient to reproduce that observed (same as Allen?) and implies emissions are not high enough historically?

**Reply**: Correct.

**Reviewer Point P 2.51** — Page 8, line 247 - change "burden of SO2" to "burden of SO4".

**Reply**: Fixed.

**Reviewer Point P 2.52** — Page 8, Lines 246-249 - The study only shows change in SDSR is opposite to SO4 burden over Europe and not the case over China so can we really say that the process of translating burden to forcings are ok? What about over other regions? Might not just be due to errors in atmospheric burdens, but other factors combining?

**Reply**: A new section answering this question has been added in the Dicussion and has been cited in our reply to P2.45.

**Reviewer Point P 2.53** — Page 8, Line 250 – "The models of this study ..." changed to "The models used in this study ..."

**Reply**: Fixed.

**Reviewer Point P 2.54** — Page 9, line 254-255 – Should we expect a reversal of emissions across China over this period?

**Reply**: A section discussing the trend reversal was added and is cited in our reply to P2.4.

**Reviewer Point P 2.55** — Page 9, line 256 – Is this referring to Figure 3 in Hoesly et al., (2018)? Make clearer.

**Reply**: Fixed.

**Reviewer Point P 2.56** — Page 9, line 258 – should we expect BC and OC to influences SDSR much? Need to mention these aerosol components earlier in the manuscript if going to mention now as no introduction to them at all. All discussion previously has been made about SO4 so why suddenly bring them in now?

**Reply**: This sentence has been removed from the manuscript. With future studies a line have been added in line 393-394: *In addition model experiments looking into the effects of black carbon versus sulfate on all sky SDSR would greatly benefit our understanding of global and regional dimming and brightening.*

**Reviewer Point P 2.57** — Page 9, line 259-261 – Do these studies give an uncertainty in emission inventories and can this be used to see if it can account for the differences between model and observed SDSR.

**Reply**: Unfortunately none of these studies presents number for uncertainty in emission inventories, but Aas et al. (2019) show annual average trend in sulfate in aerosols from 2000-2015 and found that the standard deviation was larger than the actual trend for East Asia, and none of the locations used in that study was located in China Aas et al. (2019)[Tab. 1].

**Reviewer Point P 2.58** — Page 9, line 270 – change "CMIP6 experiment models" to "experiments, CMIP6 models"

**Reply**: Fixed.

**Reviewer Point P 2.59** — Page 9, line 273 – mention that the dimming is underestimated by the models.

**Reply**: Fixed.

**Reviewer Point P 2.60** — Page 9, line 276-279 – Would we not have anticipated the SO4 burden to have increased across China over this period as SO2 emissions are anticipated to have also increased? Are the errors in SO4 burden and SO2 emissions really that large to account for the observed discrepancy in SDSR? More work to back up this statement and other factors should be included in conclusions. Uncertainty in emission inventories probably do contribute to this but the trend changes in SDSR and anticipated emission changes don't match for China, so this cannot be the sole reason and needs to be expanded on. see general comment point 4.

**Reply**: As we do not know the estimation uncertainty for emission we do not have evidence to rule out that the emission inventories can have large errors. The observed trend reversal in China have a new discussion section which is cited in our reply to P2.4.

**Reviewer Point P 2.61** — Page 10, line 285-287 – how would these future investigations improve our understanding of SDSR temporal evolution?

**Reply**: A comparison between different observational datasets such as GEBA and ice cores will give a unique insight in aerosol presence before the satellite era, especially if the emission inventories are wrong. Satellite products can be used to compare to CRU cloud cover data and give a full picture. Model experiments with different aerosol emission as input can disentangle the role of different aerosols on SDSR assuming their translation from emission to SDSR is correct. Have added in the manuscript "Since the SDSR measurements are not only sensitive to aerosol effects, further studies could include... "

**Reviewer Point P 2.62** — Fig. 2b – why is CanESMS so different in Hist-Nat and does show that other drivers influence the SDSR trend?

**Reply**: We do not know why CanESM5 differs from the others in it's hist-nat experiment, but this single experiment is unfortunately not enough evidence to say that other drivers influence the SDSR trend - except for in CanESM5 only.

**Reviewer Point P 2.63** — Fig. 3b – Can the uncertainty in cloud cover from observations and models be shown?

**Reply**: The section explaining the CRU dataset has been improved in line 78-84:
*CRU covers the period 1901-2017 (Harris et al., 2020) and consists of a climatology made from measurements at meteorological stations around the globe, interpolated to a 0.5$^o$ latitude/longitude resolution grid covering continental areas. Information on interpolation methods and procedures used to create the gridded data set are given in Harris et al. (2020) and references therein. In short, CRU has its foundation in station data, but is interpolated to a grid using angular-distance weighting. The cloud cover variable is largely derived as a secondary variable, based on measurements of other parameters such as sunshine hours and diurnal temperature range.*
As cloud cover is a secondary observed variable we have added line 274 in the Results section regarding clear sky and cloud cover data in China:
*It is important to note that the robustness of observed cloud cover changes must be verified by satellite observations, which goes beyond the scope of this study.*
Uncertainty in cloud cover from models is hard to quantify with only three ensemble members, but Table A1 shows the different baseline values for cloud cover in each model, which can be seen with a spread of 50%-64%.

**Reviewer Point P 2.64** — Fig. 4 – CESM2 seems to show a small change, can you confirm that this model has interact aerosols included? If not then why such a small change compared to others?

**Reply**: Aerosols interact with the climate in CESM2. We have added a Table in the appendix (Table A2) showing which models that have interactive aerosols and which that don't.

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# 1 Supplementary Material

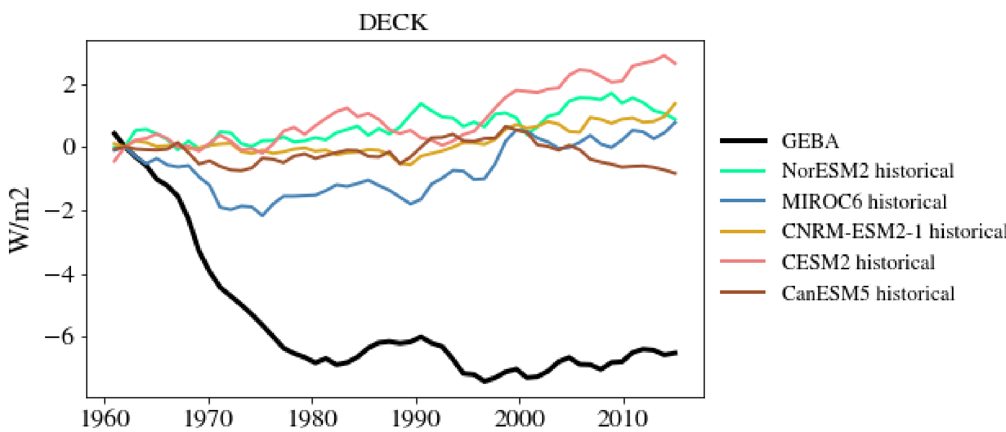


Figure S1: SDSR anomaly North America, model results are co-located to GEBA stations following the methodology as described in Moseid et al 2020 in prep