Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-830-RC1, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Glacier evolution in high mountain Asia under stratospheric sulfate aerosol injection geoengineering" *by* L. Zhao et al.

Anonymous Referee #1

Received and published: 17 October 2016

General comments

This paper presents the first analysis of the Himalayan glacier response to solar geoengineering, analyzing the response to a number of scenarios of future greenhouse gas emissions and stratospheric aerosol geoengineering. The authors employ a mixed empirical / statistical model of glacier change driven by temperature change alone and use statistical down-scaling of the GeoMIP multi-model ensemble to produce the input data for their model. They find that solar geoengineering could slow the rate of retreat of Himalayan glaciers and that this benefit would be lost were solar geoengineering to be terminated.

This study is a novel contribution to the geoengineering literature and addresses an important issue; however, there are a number of shortcomings in the study that need

to be addressed.

Most importantly, the method employed by the authors excludes precipitation which is the most critical climate difference between scenarios which include solar geoengineering and those which don't. Studies have reported substantial reductions in monsoon precipitation in scenarios which include solar geoengineering and so it seems odd to exclude this from the analysis. The authors note that excluding precipitation will likely mean that their estimate of the efficacy of solar geoengineering is an overestimate but it is not clear the magnitude of this overestimate; is it of order 1%, 10%, 100%? I'd strongly recommend that the authors either reintroduce precipitation back into their methodology (the model described can use precipitation but it was excluded) even if this is only to provide a robust sensitivity estimate that justifies its exclusion, and I'd suggest doing this even if there are serious shortcomings in the down-scaling method for precipitation or other methodological concerns. Failing that, a much more detailed justification for excluding precipitation should be made along with some robust regional estimates of the significance that the changes in precipitation should be expected to have. The authors note that Rupper and Roe (2008) calculated zonal-mean sensitivities of SMB to temperature and precipitation; I suggest using these to estimate the sensitivity. This study should make clear what the research needs are to make a better estimate of the effects of solar geoengineering on Himalayan glaciers and the significance of including precipitation is a critical question that this paper does not address well.

I had a number of questions about the validity of the methodology adopted in this study that either require the authors to change the methodology, present some additional justification for their approach, or provide some sense of the sensitivity of their approach to these issues. I list the most significant ones here but there are other minor concerns listed afterwards.

First, the authors make comparisons between the responses of a changing ensemble of models for different experiments. It is unclear how much of the difference between

these experiment-ensemble means is due to the changing make-up of the ensembles and how much due to robust differences in response. This is particularly concerning in the case of G4 were the models produced very different temperature responses due to their differing treatments of the 5Tg of injected SO2.

Second, the authors don't do enough to validate their approach. A comparison against historical data or a more explicit reference to studies which validate this approach is needed. Does this model predict stable, growing or shrinking glacier mass for pre-industrial climate conditions? Is this model in agreement with other similar models?

Third, the authors need to fix parameters for the thousands of glaciers in their study area but only have 13 glaciers to base these parameters on. As they are making a regional analysis they use only the 3 nearest glaciers to fix parameters in a regional manner. It is not immediately clear how different these 13 glaciers are from the results presented and so it is not clear now sensitive the results are to this sampling. The authors don't discuss whether one could expect the sampled glaciers to be representative of their region or why one would expect robust regional differences between the glaciers' behavior.

Finally, the authors downscale temperature to a high-resolution grid however they don't make any assessment of whether this downscaled temperature data matches observations. I'd expect that there could be quite large differences between the real glacier altitude and the altitude of the nearest high-resolution gridpoint given the complex topography. I was also wondering given that the observed altitude of the glaciers is used to calculate the ELA, why not use it directly to perform this lapse-rate adjustment?

Relatedly, I'd recommend that the authors broaden the uncertainties section into a proper discussion that covers the shortcomings of the methodology employed and discusses the sensitivity of the findings to various assumptions. A key question which should motivate the material in this section is – how would the glacier response in a model with a full surface mass balance treatment driven directly by high-resolution

climate data differ?

I had a few concerns about the structure and the focus of the paper that relate to the regional climate analysis. First, it seems strange to separate the regional climate analysis from the regional glacier analysis. Why not start with the whole-region analysis of climate and glacier response and then conduct a regional climate and glacier analysis afterwards? Second, Figures 2 and 3 seem of very little use as there are no cues on these plots to relate the results presented to the regional glacier analysis in figure 7. Given glacier response is calculated across the region or over sub-regions, I'd recommend doing a sub-regional-mean analysis as in figure 7 instead of including figures 2 and 3. This would also help to make clearer how significant excluding precipitation from the analysis was.

Finally, the conclusion needs to be revised as it presents a lot of new results and material, some of which are not appropriate for a conclusion.

Specific comments

L18-21 – unclear sentence: 5 under G4 what?

L23-25 – I don't recall this point being made in the paper.

L30-31 – This is a far-reaching general statement extrapolated from a specific scenario. It holds for the specific case but not for the general case.

L55 – delete hence

L55-57 – These are very different types of reasons and I don't see how the second matters.

L57-60 – Irvine et al. 2012 is a semi-empirical study and McCusker et al. discussed ice-sheet implications of their climate model results but did not simulate ice-sheet or sea-level response at all.

L75 – "regions of newly defined regions of" – fix

L86-89 – This sentence is unclear. Why is this simple? What issues could this pose?

L90-95 – This approach needs some justification and citations – why is it appropriate? Is this a widely accepted approach? Etc.

L98-99 – What precisely is done? I believe this is explained in the next section, if so make that link explicit.

Fig 1 + other lat-lon plots – It would help to make sense of the lat-lon plots if there was some frame of reference beyond the lat-lon coordinates. Would it be possible to mark the numbered regions (referred to later) with boxes or outlines in this figure and then overlay this outline in the other plots. This would make these subsequent figures much more useful. Or alternatively lose the lat-lon plots entirely and focus on the area-average means for the different regions shown here and assessed in figure 7.

L115-145 – Is there any validation of this modeling approach? If not this is a major shortcoming. 2 previous papers are cited; if they contain a validation of this approach that should be brought up and explained.

L116-130 – Some sense of the sensitivity of the method to this sampling of glaciers is needed. I did not find the values in the table easy to read so I couldn't tell from looking at this how large the differences were; perhaps a figure would help to illustrate this. Can you be sure that these differences are systematic regional differences rather than the quirks of individual glaciers? Why not take the average over all glaciers rather than just the 3 nearest? These questions ought to be answered here or else citations which address these questions cited. This is another source of uncertainty in the model projections but it is unclear how large it is.

L140-144 – The justification for excluding precipitation here is incomplete (as it is in section 4). What matters is not the fractional precipitation change but rather the ratio of a x dT to b x dP. If you have the beta values it should be straightforward to make a rough calculation of b x dP so that a comparison can be made. This issue is critical to

address properly as your model cannot distinguish GHG from solar forcing (as it uses dT alone) and solar geoengineering has a distinct effect on precipitation.

L146-189 – Is there a validation for this approach, i.e. do the down-scaled, lapse-rate adjusted reanalysis data match observational data?

L148 – "the beginning years" not clear

L150-151 – This sounds like you don't believe them, explain the limits of this approach or rewrite this sentence.

L159 – "and THE INCREASE IN greenhouse gas forcing" rather than the total forcing relative to pre-industrial.

L159-161 – Here and elsewhere need to make it clear that different models had a very different temperature, etc. response to G4's 5Tg.

L162 – No. Injection stops which means the aerosol forcing will decay rather than instantly disappear.

L171 – be more specific, e.g. resolutions of $\sim$2 degrees.

L176 – Does the glacier not have an altitude of its own recorded somewhere? How large could the difference between the nearest grid-box and the glacier's actual altitude be? I suspect this could be quite large. Has this been dealt with by other papers / methods? Is there any way to get a sense of this?

L191-222 – Why is a regional analysis here? The regional analysis of the glacier response is made much later, it would seem sensible to present this material alongside that.

L191-222 – Some warning should be given about reading too much into the small scale "features" in these plots. These are artifacts of the ensemble average of mismatched, coarse model grids that have been downscaled – no individual model has the kind of spatial variation seen in figure 3a for example.

L193 – estimate = calculate.

Figure 2 and throughout – It is not clear whether the differences plotted in this figure and in all other figures are due solely to differences in the response to the experiments as is implicitly assumed or whether the different make-ups of the ensembles used in each experiment has affected the results substantially. Some measure of this effect should be made.

Figure 2 and other map plots – The continuous color scale is hard to read, is it possible to use a banded scale or to add contour lines?

Figure 2 and other map plots – I'd suggest removing the stippling. First, I'm skeptical whether the measure plotted is useful, i.e. does it really give a good sense of the robustness of the regional results? Second, looking at these plots the stippling is applied at a scale much finer than the resolution of the models that generated the results. Third, it's entirely absent from some figures suggesting that the stippling code was not implemented appropriately.

Figure 2 and 3 – Are these annual-mean or JJA-mean results?

L197 and throughout – Are mean +/- standard deviation (which I presume these are) measures appropriate when the ensemble consists of only 4 models? Perhaps mean (min, max) may be more appropriate and informative.

Table 3 – I'd recommend adding much more information here and converting this into a figure which covers the full range of the model responses for each experiment across the different glacier regions. This would help to address my concern about the absence of precipitation from the simulations and the poor justification for why this was left out. Reporting the changes in precipitation in percentage change may help or else this could be combined with an estimate of the beta parameter for glacier sensitivity to precipitation (perhaps expressed as a fraction of the temperature response) to give a quantified estimate of how significant the precipitation response is. Without such

information and given that precipitation is excluded from the glacier model I don't see why devoting a page to the precipitation response was necessary.

Figure 3 – These precipitation results seem to have been down-scaled, how was this done?

L218 – "Precipitation change ratios"? not clear what this means.

L225 – "projections ensembles"?

L233-241 – How does these results compare to previously published results? Perhaps cite the work that made the initial analyses of G3 and G4.

L233 – Should "and the highest rate" read "at the highest rate"?

L238 – 1.7C over what period?

L239-241 – Why does this happen? Here and elsewhere more care should be taken to make clear that G4 produced very different forcing and hence temperature responses in the different models. Some evaluation of how effective G4 was at cooling the climate in different models should be made or reference should be made to a study which makes this analysis.

Figure 4 – There is a suspicious degree of agreement between the models in panel d. These models presumably have quite different TCRs, surely there would be greater differences than this at the regional scale. Please cite some other work or present some evidence that this is not a processing error on your behalf.

Figure 5 – Here and elsewhere the comparison is not simply between different experiments but between different ensembles of models. Some assessment of the significance of this difference ought to be made. A similar line plot like this would be a good place to do so.

Figure 5 – Rather than going into the spatial pattern of the precip response in figure 3 (which is not used) why not add 2 panels on the precip response to this line plot?

Alternatively, do the regional-average analysis I suggested earlier.

L249 – “averaged over the grids” what does this mean?

L263-264 – Explain why this happens. I presume it’s because the most sensitive glaciers have already retreated / disappeared but this isn’t mentioned anywhere. I’m wondering idly whether an overall sensitivity could be calculated.

L269 – explain why G3 > G4.

L270-280 – There is too much reporting of values and not enough explanation in this paragraph.

L283-284 – Is this area-volume relation surprising? Aren’t they explicitly linked in the model? Figure 7 – This regional break-down is great, however it is a shame that none of the previous results are presented on the same basis. This regional (and sub-regional) analysis is much more useful than the spatial maps shown elsewhere, could temperature and precipitation responses be plotted for these regions?

L288-301 – Why isn’t the regional climate analysis here with the regional glacier analysis.

L303-359 – It is unclear what this section is for. I’d suggest reframing this as a regular discussion section and broadening its scope to cover all the shortcomings of this study.

L304-306 – This sentence is unclear.

L315-320 – why is this included? Your simulations excluded precipitation effects.

L328-333 – This paragraph is unclear, it is hard to follow the references.

L334-338 – This is also unclear.

L339-342 – Does it? You have not shown the relative significance of precipitation to temperature in this study and table 3 only reports the area-average results which differ from the regional responses.

L339 – if A1B shows a significant trend then surely RCP 8.5 would too given their similarity?

L344-346 – This sentence is unclear and it’s not clear which observational data is referred to here.

L350-352 – I’m not sure how useful this measure is given the shortcomings in the approach and the systematic over-estimation due to the exclusion of precipitation. Also, extending the simulations out to 2150 could give rise to the problem that your simple approach to uncertainty bounds would include negative glacier mass.

L361-end – Much of the material presented in this conclusion is new, why does this not appear earlier?

L372 – why is G3 extreme? There are more extreme possibilities. This is an arbitrary scenario.

L383-385 – This is flatly wrong as written here. This result is scenario dependent. If a greater cooling were exerted by solar geoengineering more glaciers would be saved.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-830, 2016.