Review for “Present and future aerosol impacts on Arctic climate change in the GISS-E2.1 Earth system model”

By Ulas Im et al.

Summary

The manuscript reports on an extensive set of simulations with the GISS-E2.1 Earth system model spanning both near past and future, which investigate the impact of changing anthropogenic aerosol emissions on Arctic climate. The study is interesting and in principle suitable for ACP, but in places I find the text quite hard to follow to the point that I am not sure whether the results support the conclusions. However, this is mainly due to the presentation of the results (both in text and figures), not the results themselves and I would be happy to review a revised version of the manuscript again.

General comments

The terms used in the radiative forcing discussion are somewhat outdated. In AR5, the IPCC recommends moving from direct and indirect effects to using radiative forcing due to aerosol-radiation interactions (RF_{ARI}) and due to aerosol-cloud interactions (RF_{ACI}), together with their resulting rapid adjustments and the final effective radiative forcing (see chapter 7 of AR5). In order to keep up with this development, I strongly recommend rewriting the text with respect to this.

Many parts of the results section contain long listings of changes of different quantities in the different scenarios for several time periods and are quite hard to follow. I'm wondering whether it would be more beneficial to organise these results in tables and rather concentrate on systematic or principle differences between the different simulations. For instance, if there is a systematic decrease in sulfate emissions in the ssp simulations, how does this translate into Arctic sulfate burdens, radiative forcings and temperatures and how are the results of the Eclipse simulations different from that?

In the Discussion section I am missing a discussion on how the biases that have been found in the model evaluation section may affect the modelled climate impacts in the future and if and how much that adds to the uncertainty of the results.

Specific comments

Abstract

- lines 30—32: add “In the simulations” to “Surface aerosol levels ... have been significantly underestimated”
- line 32: “The nudged simulations” have not been defined at this point. I recommend changing this to “...when winds were nudged to reanalysis data”
- line 34: A change from “fully coupled simulations” to “simulations where atmosphere and ocean where coupled’ or something similar might be better at this point.

- lines 37—48: None of the simulation names have been introduced at this point (naturally) and it might therefore be hard for the potential reader to grasp the general message of the abstract. I therefore recommend to re-write this paragraph. In my opinion a “maximum vs minimum effect”-type of discussion would be easier to digest at this point.
- line 46: remove “both”
- line 46—47: Change “In 2050” to “By 2050”?
- line 52: “while scenarios no or little…” – add “with”
- line 53: “lead” --> “leads”

Introduction

- line 71: “This contribution ... puts” or “These contributions ... put” ?
- lines 80 --- 85: “BC” and “SO_4^{2-}” have already been defined.
- line 90: I’m not sure myself: Is BC depositing on snow and ice or is BC being deposited, e.g. can you use the active form here?
- line 93: While you talk about the lifetime and vertical extent effects here, if I understand the model description correctly, these effects are not included in your simulation, or are they?
- lines 111—112: Is that global emissions?

Materials and Methods

- line 169—174: Can you elaborate on how that works? If Everything except dust and sea salt is externally mixed, does that mean that the model assumes separate sulphate, nitrate, BC and OC particles? How do you then treat the sulphate and nitrate coating of the dust particles?
- Even though SOA production in the model is described in Tsigaridis and Kanakidou, maybe you could describe it briefly here as well. In particular, what are the assumptions of how SOA formation affects OC concentrations. This is important, as you attribute higher OC concentrations to higher SOA formation, but it is not clear, how that is modelled. Do you have separate SOA tracers or does VOC oxidation lead directly to OC production in the atmosphere? In the former case, how do you convert SOA into OC. Am I right in assuming that OC from the emission inventories is emitted as particulate matter?
- line 178—180: How does that work? If the model treats the first indirect (i.e. aerosol concentrations affecting CDNC and (I guess) cloud droplet size), how do you stop the model from changing LWP and precipitation rates?
- line 186: I guess this is also just the first indirect effect?
- Section 2.2.3: Do I understand this correctly: Eclipse emissions have been complemented in some sectors by using CEDS emissions, while CEDS emissions are entirely “original”, or did you also have to complement CEDS emissions in some sectors?
- Lines 284 – 288: You have been quite thorough in explaining the differences between the ECLIPSE scenarios, but the differences between the different CEDS scenarios is quite compact. What, for instance, does “lowNTCF” mean?
- Section 2.2.3: How do the emissions and concentrations of Greenhouse gases evolve in the simulations? Are they kept fixed to capture the aerosol effect, or do they change? In the latter case, please elaborate on how you separate the aerosol effects from the Greenhouse gas effects.

- Section 2.2.3: If emissions are provided at 0.5x0.5° resolution, but the model operates at 2x2.5° resolution, I'm guessing you re-grid the emissions somehow?
- lines 317—326: As a side note, it has become more and more common to co-locate modelling data and observations in time to reduce the effects of observational "data sparseness" mentioned here. I understand that this is probably out of scope of this study, but worth considering in the future.

Results

- Figure 3: It would be quite beneficial to add the station names to the figure. Especially because some of the stations are discussed in the text.
- lines 395-398: Could these high bias outliers be a problem with the representativeness of the observations (e.g. too few data points, or quickly changing orography)? Trapper Creek, for instance, is right next to another, blue, point.
- lines 433—436: Later in the article (line 774) you state that a higher cloud fraction may lead to higher in-cloud SO4 production – please add this statement also here.
- Tables 3 and 4: Please consider breaking up these tables into two parts and displaying them in portrait mode. At least in electronic form it would make the manuscript easier to read.
- Why do the AMIP runs have such a high bias in SST, if SST is prescribed?
- lines 461—462: Is that due to model resolution? After all, SIC is prescribed, right?
- lines 470—471: Do you mean the climatology of the cloud fraction for the entire year here?
- Figure 5a: I think here it would be worth mentioning that the seasonal trends in observed and modelled cloud fraction trends are reversed. Looking at panels b and c, it almost looks like the model produces to few water or mixed-phase clouds during the winter months, did I get this right?
- lines 474 – 478: This sentence is very hard to grasp: Less overestimation due to an underestimation? Do you mean to say that you trust AVHRR CLARA-A2 less than CALIPSO, because CALIPSO does a better job at separating bright surfaces from clouds? Also, you could add in line 466 that there you compare to AVHRR data.
- Figure 6 and Section 6.2: I take it that by Arctic burden you mean the integral over all grid boxes between 60 and 90° north and over all vertical levels, but then using monthly averages? Why do you use the unit kTon in the text, but Tg in the figures?
- line 533: What do you mean by "better resolved"?
- lines 541-542: If you term it "reduction", I guess the number should be positive...
- line 549 and following: How has statistical significance been tested?
- line 554—555: See my comment in the Materials and Methods section. If OC is a separate tracer, you should explain somewhere, how a larger SOA production leads to larger OC concentrations. If it is what I think (i.e. you talk the sum of OC and SOA species), I suggest calling it something else. Maybe organic aerosol (OA) or organic matter (OM) would be suitable?
- Figure S1: This links directly to the comment above. Without any explanation, it is not really understandable what you are showing here.
- line 580: ...because CLE levels off earlier (no further legislation after this point?). The calculated trend cannot really be 2015—2050.
- Figure 7: What are you actually plotting here? From the explanation in the text (double call to the radiation code with and without aerosols) it sounds like you are

showing the radiative effect due to aerosol-radiation interaction (RE_{ARI}) (see Chapter 7.3.4.1 of the IPCC AR5), formerly termed the “direct radiative effect”. A radiative forcing due to aerosol-radiation interaction (RF_{ARI}) would be the change in RE_{ARI} relative to some reference point, e.g. preindustrial levels. Please elaborate.

- line 595 – 602: Why do you only talk about Eclipse here?
- 595 – 597: Why is that? This is quite a substantial difference – can this be explained by differences in aerosol burdens alone?
- line 601—602: What is the meaning of the third value here?
- Figure 7: Why do the AMIP runs differ so much from the other simulations (2000—2015)? Also, there is visible difference between the black and brown lines (NINT_Cpl and CMIP6_Cpl_Hist?) in the anthropogenic aerosol radiative forcings, byt the same difference is not visible for the total aerosol radiative forcing – what is compensating for the difference here?
- If SOA can contribute to OC and if SOA can originate from both natural and anthropogenic sources, how can you separate the anthropogenic contribution of OC to the radiative forcing?
- Figure 8: how are the speciated forcings calculated?
- lines 640—651: This appears to be exactly the same text as lines 604—615.
- line 650: What is higher to what here?
- line 656: “sinnulations” --> “simulations”
- line 657: You use the term “anomaly” the first time here – how is this calculated and what do you mean by “aerosol forcing anomaly”?
- Figure 9: In the figure you show only the surface temperatures between 2020 and 2050, but you talk a lot about temperature trends in earlier times – is there a reason for this? Also, it would be much easier to follow the discussion, if the observed trends would be added to the figure.
- Lines 665 – 673 I can’t really believe the numbers you give in this paragraph. A 10° C/decade increase in surface air temperature is huge, even for the Arctic. As a reference, in the Figure 9 you show the surface temperatures between 2020 and 2050, which change by about 1-2°C in three decades. Please check your calculations or provide a figure, if the numbers are correct.
- 698: ”warnings” --> ”warmings”?
- Figure 10: How statistically robust are these spatial distributions? Looking only at the SSP results (panels c, d and e), it looks like the changes are not very systematic in many regions, which makes me wonder how noisy the results are.
- line 702: Figure 9 does not show SST.
- line 712: Do you mean “Greenland sea”?
- line 736: Here and in some other places where you compare the means of two time periods, you could consider replacing “... is projected to decrease by ... compared to ...” with “... is projected to be ... lower than ...”
- Figure 11: Even though I the discussion is generally about the entire Arctic region, in this figure I’m wondering if it would be better to “zoom in” to where the changes are actually happening.
- line 748: “Figure S1” --> “Figure S3”
- line 751: “Figure S2” shows SST
- line 754: “Figures S3—S7” --> “Figures S4—S7”

Summary and Conclusions

- line 773: Like in the abstract, I would try to avoid using the names of the individual simulations in the conclusions.
- line 808: add “future”
- lines 815 – 818. There appears to be one “Eclipse” too much.
- line 826 – 829: Could one interpret this as the melting of sea ice acting buffering the changes in surface air temperatures?