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Interactive comment on "Global aerosol simulations using NICAM.16 on a 14-km grid spacing for a climate study: Improved and remaining issues relative to a lower-resolution model" *by* Daisuke Goto et al.

Anonymous Referee #2

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Review of "Global aerosol simulations using NICAM.16 on a 14-km grid spacing for a climate study: Improved and remaining issues relative to a lower-resolution model" by Goto et al. for publication in Global Model Development

The paper presents a fairly comprehensive overview of a pair of simulations performed with the NICAM.16 model with a coupled aerosol component based on SPRINTARS. Comparisons are made between a high-spatial resolution simulation performed at a horizontal14 km resolution and a lower resolution simulation at 56 km. In addition to presentation of such a high resolution simulation the main novel aspect of the paper

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is that the simulation was run for several years, which is significant in terms of the comprehensiveness.

The paper is well written and fairly exhaustive in terms of comparisons made, but I am somewhat unsatisfied with the attribution aspects and suggest some needed modifications for publication. I do want to call out that I thought the presentation of the internal variabilities of the different resolutions was quite interesting and makes a case for bearing the costs of the higher resolution simulation, but it seems the conclusion of the paper is this is not necessary at the moment given the relative performances. Actually, I'm a little puzzled at what the overall conclusion is. Is it that the model performs well enough at the lower resolution to not justify the added cost? I wonder about specific case studies. Could the simulations be initialized to look at, say, a dust storm episode and dig more into the variability of such a case. For the overall conclusion, as stated what is recommended is that the tuning parameters associated with the aerosols in the LRM are applicable to those in the HRM. This is maybe true enough for dust and sea salt emissions, which are heavily tuned in most models. I wonder though if this is undermined by the apparent differences in the wet removal between the two runs. Further, I'm surprised the computational cost is only a factor of ten since the implied resolution differences suggest a factor of 16 more grid boxes in the high resolution run.

The discussion of the aerosol budget needs to be looked at more closely and given more discussion. In particular, I'm confused about what is shown in Table 2 especially with respect to black carbon components and for that matter the POM and sulfate. Differences in especially the lifetime of WIBC from nearly 9 to 6 days between the HRM and LRM runs are not explained by the budget given. Both runs have the same emissions, and the reported depositions for both runs are identical. So why is the lifetime different? Are there underlying mass conservation issues in the model that are not explored? WSBC has the same issue but the difference is less dramatic. For POM the lifetime also does not seem consistent with the loss and emissions numbers give. For sulfate I'm curious about the partitioning of aqueous production versus gas production, which is not spelled out. My take on the paper is that most of what is different is due to wet processes, but the budget numbers don't clearly bear that out.

Most of my other comments are more minor or for clarification:

Could you please make explicit: are the aerosols radiatively coupled to the AGCM? Are they fully interactive with the cloud scheme?

Page 4, line 19: The NASA GEOS forecasting system is actually run at higher resolution in its operational forecasts with aerosols, and that system has been running for several years, although it is a quasi-operational system and so is not a single, consistent model experiment.

Page 7, line 24: 10 bins for dust is kind of a lot for this kind of model. You do not break down system costs, but how much compute could be saved by running half as many dust bins?

Page 8, line 2: "one modal" -> "monomodal"

Page 13, line 17-18: It is incorrectly stated that HRM is closer to data than LRM; the opposite appears to be true, or only at equator is HRM so close to data for COT. This is also stated on page 14 lines 9-10. I'm missing something here. Related, given the apparent discrepancies in the cloud fraction and COT I don't understand how the radiation parameter in 3E and 3F looks so good, and similarly for Figure 4.

Page 15, line 5: Please clarify use of word "global" here to refer to sum of diffuse+direct (i.e., could write: global (sum of diffuse+direct)). Later you refer to biasing of global averages (line 10) by the BSRN site locations. Where is the global averaged compared to BSRN even presented? I don't understand what you are trying to make a point of here.

Page 15, line 13 and Figure 5: The masking used here is curious since the simulations are AMIP runs untethered to actual events. Please explain the nature of the masking (presumably snow covered surfaces, although not sure about in Brazil). Another point that bears some discussion about how the comparison is approached here: MODIS attempts to do a clear-sky aerosol retrieval, while presumably the model AOD is the all-sky AOD. In CTM-type runs where real events are simulated (and so, real clouds) it is found that by masking the model results with the MODIS cloud masks the AOD comparisons make more sense. You cannot do that here, although you could play games with excluding high cloud fraction grid cells from the comparisons. Or are you comparing a clear-sky calculated AOD (and how)? I suspect this is also relevant to the high AOD bias in the model over the southern ocean.

Page 15, line 21: over land AOD is “most uncertain” in MODIS products

page 17, line 15: strike “the most”

Page 18, lines 21-22: Here and elsewhere (like page 22, line 22) you implicate grid resolution as an explanation for differences but don’t go far enough to say why. What process is different that you can point to?

Figure 15: What is going here with “macro”? Is this a separate model run? This isn’t clear at all.

Page 23, line 22: the reference should be figure 15.

Page 32, line 13: The statement that the clouds are not underestimated with respect to MODIS is completely belied by Figure 3b and 3c. What am I not understanding here?

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