Review of Blichner et al, Implementing a sectional scheme for early aerosol growth from new particle formation in the Norwegian Earth System Model v2: comparison to observations and climate impacts

This paper documents a new sectional sub-model for representing the evolution of nucleated particles in NorESM. It is a good paper: interesting, well-written and well within the scope of GMD. I have two major comments and a few minor suggestions I would like the authors to address before the paper is published.

Major comments:
Often, new aerosol microphysics schemes have been tested in box modeling frameworks before implementing into a global model, but such tests are not reported here. In my opinion box modeling would address my two major comments most effectively and might also shed light on other behavior of the model.

(Line 270) Have you considered the effects of numerical diffusion (Jacobson 2005, section 13.5.3)? To minimize these wouldn't a moving-center structure be better, as done in the sectional microphysics models TOMAS (Adams et al, JGR 2002), GLOMAP-bin (Spracklen et al, ACP 2005) though not some others (e.g. APM, Yu et al ACP 2009, I think)? Is it possible to show that numerical diffusion doesn't matter?

What is the timestep and is the same timestep used for advection and for microphysics? Is it half an hour for both as implied on line 106? Are the results sensitive to the microphysics timestep (or the model timestep, if it's the same)? (Maybe add timestep in section 2.1 introduction, since it is needed to understand 2.1.2). My understanding is that ~30 minutes is quite a long timestep for nucleation-mode aerosol microphysical processes unless sulfuric acid is in pseudo-steady state (see Pierce & Adams, Aerosol Science and Technology 2008), though I assume it is fine for models that don't resolve the nucleation mode like OsloAero_def. For example, in GLOMAP where sulfuric acid is not in pseudo-steady-state, the timestep is 3 minutes for condensation, nucleation and coagulation (Mann et al GMD 2010), while in APM I think the timestep is variable but also less than 30 minutes. So I would expect your results might change in some situations if (say) a 5 or 10 minute timestep were used. I note there is a helpful discussion of some timing issues at lines 455-460, but sensitivity studies are called for here.

Minor comments:

The detailed comparison at surface sites is very nice but of course it only presents a two-dimensional picture. Entrainment of the nucleated particles you are interested in from the upper troposphere is likely a bigger source of cloud-level CCN than boundary-layer new particle formation (e.g. Merikanto et al, ACP 2009). So it would be really nice to see a comparison of Ntot to aircraft measurements e.g. from ATom (e.g. Williamson et al, Nature 2019; Ranjithkumar et al, ACPD 2020), but I realise this is a lot to ask. Perhaps something to think about for a follow-up paper?

Line 117: Would be nice to be consistent in referring only to diameters, not radii, except where necessary. Your scheme accounts for 5-39.6nm diameter particles; the smallest mode apparently has an initial diameter of 47.2nm. Or is 23.6nm actually the diameter, as stated in the caption of Figure 1? If not, what happens between 39.6nm and 47.2nm?

Line 199: would be helpful to clarify that this is the original unperturbed version of the model

Section 2.2.1 Would be worth commenting here that the Riccobono et al parameterization of nucleation is still very uncertain, as it lacks a temperature dependence and was not based on ELVOC measurements, but rather on pinanediol ELVOC precursors, and it doesn't take account of the role of ammonia in nucleation.

Section 2.3 Just a comment: I imagine this is a big improvement, and a good catch that could be implemented in the default NorESM for all users, even those not prepared to accept the extra cost of your nucleation-mode microphysics scheme, so the OsloAeroImp simulation is useful.

Section 3: Is 1.9x2.5 the resolution of the whole model, or just the SST/sea ice input file? If the latter, what is the model resolution?

Line 310 "from 2007 to 2014 inclusive" perhaps?

Line 395: what is the "tail one NPF-particle mode"? Also please don't start a new paragraph with "This" - it makes the text hard to follow. I don't really understand the content of this paragraph. Is there no way to transfer particles from one mode to another if the mode gets too large or small? This seems inconsistent with previous descriptions, somewhere you also suggest particles in the lowest mode could shrink?

Line 405 "the sectional scheme overestimated the number of particles for this"
Can you specify "the number of particles at low diameters"
Line 407 "the surface distribution"-> "the distribution of particle surface areas"

Line 410: I don't think you represent nitrate or ammonium aerosol, could this, or errors in biogenic or anthropogenic SOA, be responsible for an underestimated condensation sink?

Line 415: should probably specify the size range over which you reduce concentrations.

Line 430: maybe comment that the large change in the upper troposphere is expected because the default model versions simply don't represent small particles.

Figure 6: It seems a shame to cut off the y axes at about 8000 – why not 12000 for example?

Figure 11: I think e and f are effective radius, not droplet concentration.

You might consider reducing the total number of figures; they are many for a paper whose text is relatively short. Figure 3, for example, does not seem to convey very different information to Figure 4, and Figure 5 has much the same information as Figure 2, given that the changes in 100-500nm particle concentrations between simulations are negligible. On the other hand, I found Figure S6 illuminating even though it is similar to Figure 6; Figure S6 might be worth promoting to the main text.