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Geoscientific Model Development Discussions

Interactive comment on "JRAero: the Japanese Reanalysis for Aerosol v1.0" *by* Keiya Yumimoto et al.

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This is a very comprehensive and well written paper on the JMAero. I don't have too much to add to the other reviews. The task at hand is straightforward: Provide an overview of the model components and provide verification. At least in regard to aerosol optical thickness they have done so admirably. I could follow what they were doing quite well, the figures were well done and they provide comprehensive error stats. One minor thing that needs mentioning, is that the verification really is toward AOT. When NRL composed its reanalysis paper (Lynch et al., 2016), we called it an AOT reanalysis even though it is a full 4 dimensional aerosol simulation because quite frankly AOT was all we could verify against. How these things pan out for surface concentrations is another kettle of fish, and perhaps the authors should note that. Indeed, not only is modeling surface concentrations (or concentrations at any level) difficult,

but finding appropriate verification data is even more difficult. I don't hold the authors accountable to the honest facts, but they should mention it. Similarly, the JMAero does multi-bin particle size distributions (8 bins per specie), but they do not discuss at all how these bins interact, or if at the end of the day it buys them anything as al verification projects onto AOT metrics. This is ok of the paper is about AOT, but that should simply be stated up front in the abstract, introduction and conclusions. Other than this I have a series of equally minor comments that the authors might find helpful. Be well, Jeffrey S. Reid, US Naval Research Laboratory.

Abstract, line 14; Please be clear on an r of 0.96 against the assimilated data. Also, this is just a personal preference, I tend to prefer r2 to r because r2 represents the fraction of variance explained. The authors are of course free to present how they wish, but too often r is used to put a happy face on things. If you want the best of both worlds, you can present bias and rms deviation separately

Please be clear when you refer to "size" if you mean radius or diameter. For example, (Page 6, Line 22) states size ranges from 0.2 to 20 um, but then says the dry radii are from 0.136 to 8.5 (line 23), which implies the original numbers were diameter. Traditionally aerosol science is in cgi units diameter, but it certainly is up to you. Please keep it consistent throughout the paper.

A little more discussion on where secondary OC comes from would help me understand the model better. As a by the way "mk-2 includes production from terpene" is stated on page 8, line 13. In the context of the paragraph in its isolation it is a bit of a non-sequitur. In just a couple of sentences can you please lay out how all primary and secondary POM production is handed with references? Also, no reference is given for the source function of primary POM or BC.

Can you please elaborate a little more on the paragraph starting Page 8, line 28 on how the coupling between AGCM and MASINGAR mk-2? For example what are the timescales of exchange? Are they run at exactly the same resolution? Is data assimila-

tion between meteorology and aerosol particle handed at the same time or are aerosol particles handled after the fact?

On Section 2.2 (Data assimilation). Just a couple of things I am unclear about. First, how does JMAero handle the situation where AOT Obs say there should be a major event, and it is not at all in the model. This happens frequently due to a multitude of mesoscale forcing phenomenon or biomass burning. At NRL we use a climatology, and at GMAO they use the local displacement ensemble.

Section 3.3.2, (Page 13 line 13). The papers describe the AERONET AOPs, but really it is just the AOT that are being used. AOPs implies the inversion products I think.

Page 13, line 29. Listing of vertical levels is ambiguous, Are those the tops of the layers, or the layer thicknesses?

Page 15, line 5: To be fair, the AOT values are not that good either. It is hard to determine who is right when it comes to sea salt. . .

Page 16, line 24 “70.0% of the deviations exceeded 0” I assume you then mean positive deviations? But then you said that overall the model is negatively biased. You might want to double check the language here.

Page 17, lien 21: Again, don’t beat yourself up on Beijing. That and Kanpur have the worst performance in all global models (Sessions et al., 2105). This is a place where 2- and 3 d var is bound to fail. Need EnKF to make it work (http://onlinelibrary.wiley.com/doi/10.1002/2016JD026067/abstract). This I think is different from the overall low bias problem.

Page 18, Line 33: This is because by nature the highest AOT events also have a lot of spatial variability and consequently it gets filtered out in the QA process.

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