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Interactive comment on "The SPRINTARS version 3.80/4D-Var data assimilation system: development and inversion experiments based on the observing system simulation experiment framework" *by* K. Yumimoto and T. Takemura

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I must apologize upfront for having such a short review-I have been in the field for most of the month. I did give the paper a good read. All in all I concur with the first anonymous reviewers on recommendations. As for what I have to add, I temper my comments with the understanding that for this journal, the paper is really a report on SPRINTARS and their methods for improving computational cost relative to an inline model version for future reference. They then apply their system to the problem of inverse modeling. The paper is all in all well written and clear for which the authors

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should be commended. Theory is reasonably well laid out, although their intro has a distinct European and Asian focus. The only major concern I have is in section 5, regarding the use of fine and coarse mode aot from MODIS (see below). But all in all I recommend accepting with moderate revisions. I don't need to see the paper again.

Section 2 & 3: I do wonder about the practicality of having a 4D var system applied to a nearly 3 degree horizontal grid spacing-at that spatial resolution and that the correlation length for aerosol particles is about 150 km, I bet 2D var would work just as well with 98% cost savings (indeed, they should just try it as a control). But, as I noted above, this is largely a descriptive journal so I have no problem with the underlying premise.

For section 4, I suggest that not so much time be spent on the angstrom exponent, rather they compare fine and coarse mode AOT to aeronet- a much more straightforward and robust metric. For Table 1, the use of a regression as a metric is ambiguous. I would recommend plot of rmse as a function of AOT. Also in the table text define the acronyms in the table (that goes for all tables actually). Table 2 and 3 can also be combined to save space.

Another interesting thing to consider that while indeed there are small differences between the inline and offline model (say 6-8%), it would help if it was framed into the context of some aeronet data. How does a 6% change compare to the rmse for a few key stations instead of looking at bulk? In fact, a map of the aeronet sites used would be very helpful. The earlier sections are a bit terse on this issue. For section 5, what is not clear to me is if they are using fine and coarse mode AOT from MODIS for over land in their analysis. If so, it needs to be retracted in the next draft. The fine/coarse aot over land has no skill as even admitted by the algorithms. Section 5 is an area where the authors need to be more careful . Again, as the point of the paper is to demonstrate the model, not invert source functions I am not too concerned. But, satellite error characterization is $\frac{3}{4}$ of the problem, and there is precious little information on this point. So, they authors should spend more time discussion how their simulated observation relates to real data and associated error. Also, clearly (from figure 6) the da system is

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doing its job. But the background difference between CR and NR is really quite large in Asia and in places where the AOTs are quite large. Thus, while on average spatially and temporally the two models are very similar, as depicted in figure 6 b the difference for over a week could be as much as a factor of 2. This needs a bit more work and description than is in there than what is simply presented in Figure 8. Are there certain meteorological conditions that lead to the skewness of figure 8? Is one 10 day run for the inversion experiment sufficient? It might be one way for one period, another later or earlier.

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