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Interactive comment on "The impact of aerosol optical depth assimilation on aerosol forecasts and radiative effects during a wild fire event over the United States" *by* D. Chen et al.

Anonymous Referee #2

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This reviewer does not find this paper very interesting at this point, although it has large potential to be a very interesting study. Unfortunately, the authors start with a bad model forecast (almost no AOD in there, probably because no fire emissions were used). That makes it very easy to show improvement. Large changes in AOD make it quite expected to see differences in the meteorological forecasts. Does this represent an improvement? Is there any evaluation of this? Even without the evaluation this could still be a very interesting paper with a few more diagnostics on the differences. I recommend publishing with major revisions. I do not expect a major rewrite (even though my sentences above may sound like this and I am somewhat disappointed by what is in the paper). The following are my most important comments at this point:

Pg. 3854, line 10 “covered most of the . . .” What does that mean? Either a figure or an explanation is needed

Line 13/14: The use of “were implemented” makes it sound like you did this work. Is this the case? If not, does a reference exist?

Line 15: Why did you choose this shortwave scheme? Was RRTMG not available for your version? That would allow you to also look at LW impacts.

Line 18: Fast et al. 2006 must be cited in addition to Barnard et al. Fast et al were the first to look at the direct effect and to implement the interaction with radiation.

Fast JD, WI Gustafson Jr., RC Easter, RA Zaveri, JC Barnard, EG Chapman, and GA Grell. 2006. Evolution of ozone, particulates, and aerosol direct forcing in an urban area using a new fully-coupled meteorology, chemistry, and aerosol model. J. Geophys. Res., 111:D21305, doi:10.1029/2005JD006721.

Line 27: “parameterized within the GOCART model”? This sentence is not clear. It sounds like you used a different model to calculate the fluxes? Do you mean to say “Within WRF-Chem . . ... are parameterized using the GOCART dust and sea-salt modules” ?

Pg. 3855, line 6/7: Wouldn’t it be better to have this figure in the paper instead of putting in this obscure web address? It would also be useful to see a comparison figure from your simulations. What does your integrated PM2.5 look like if plotted in similar fashion? What is the data source for the MODIS data? What level of quality control? Are they real-time or near real-time?

Line 12: Only 6 hours for spin up? AOD over the western US is probably only available at afternoon local time (maybe 18Z and 00Z?). Why do you use a domain that covers almost all of the northern hemisphere? What happened over the US for times when you do not have AOD data available? Do you use the same AOD data every 6 hours? I assume you are using a time window?

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Section 3, pg 3856: Do you have any sulfate comparison available? Why is OC so much more affected than BC?

Section 4: pg 3857: what is "accumulated day-2 forecasts"? What do the vertical profiles of bc, oc, and sulf look like compared to Fig. 4? What are the impacts on parameterized versus resolved precipitation? Would a horizontal difference plots for the precipitation fields look different? FDR is a large area. Precipitation differences in areas where AOD is largest may be different than in areas where AOD is low.

Section 5, pg 3859, line 13: Sloppy writing. "Enhanced aerosol (varied from. . .)" What are you talking about?

Line 22: "However, in our trials,. . . . . ." This sentence does not belong here. Is this comment supposed to be related to the paper by Grell et al ? I do not think that WF_ABBA data were available for Alaska. Are you saying the Grell et al paper was only seeing a noticeable difference since the fire emissions were overestimated? Even if that were so, you don't show anything in your paper to prove that suspicion.

Interactive comment on Geosci. Model Dev. Discuss., 7, 3851, 2014.