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Interactive comment on "Assessing atmospheric dust modelling performance of WRF-Chem over the semi-arid and arid regions around the Mediterranean" *by* Emmanouil Flaounas et al.

Anonymous Referee #2

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Review of "Assessing atmosphere dust modeling performance of WRF-Chem over the semi-arid and arid regions around the Mediterranean" by Flaounas et al. for publication in Atmospheric Chemistry and Physics

The paper presents simulations of the dust loading over three regions of interest (northern Africa, the Mediterranean, and the Middle East) for the six month period of spring and summer 2011. Simulations are made with the WRF-Chem model nudged by meteorology from the ERA-Interim reanalysis. The dust component of the chemistry module was run, and dust was carried as a passive tracer (i.e., no radiative or cloud interactions). Three different dust emissions schemes were considered in order to assess their relative performance at simulating observed dust distributions. Detailed compar-

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isons were made to MODIS and AERONET AOD and aircraft lidar extinction profiles. Each of the three dust emission schemes were run with different scaling factors in order to investigate bias relative to observations. The general conclusion is that no one of the emission schemes tried can be optimally configured to provide the best performance over the three regions of interest. There is a secondary conclusion that suggests that simulated surface mass concentrations for the default configurations are excessively large, which can be somewhat ameliorated by including smaller sized dust particles in the simulation (which have lower mass but greater extinction efficiency).

My recommendation is that the paper needs major revision. I basically have two major criticisms to the work presented.

The first is that the analysis is far too simplistic as presented. Because you are using fixed meteorology and there is no feedback between the dust load and the atmosphere then there is really no point in performing 4 simulations for each of the emission schemes. Of course they have the same correlation coefficient. You are only running dust and no fancy microphysics and the processes simulated are linear (aren't they? I think they are in most models). The rescaling to different tuning coefficients could have been done a posteriori and essentially the same results derived. The analysis could be increased in complexity one notch by doing a regression to find the optical tuning coefficient for each of the source schemes. This of course would not resolve the regional disparities. So maybe a next level of complexity would be to run the simulation turning on and off individual source regions (say, separate eastern from western North Africa an separate from Middle Eastern sources) and then you might find an optimal set of scaling coefficients for each region and each emission scheme (actually, this would require I think only 3 runs for each scheme. . .all sources, eastern Africa sources, and Middle East sources only, with the western African sources, for example, recovered by differencing the other simulations). This works because no feedback and processes are linear. This would yield some better estimate of the emissions needed in your model to make things work. (As an aside I see that the G01 and MB95 schemes yield very similar results, and they differ from the S04. You don't really get into why this is the case, and that is perhaps interesting.)

The second is that the entire discussion of the dust particle size distribution Section 4.2 is too simplistic. Wouldn't the Fennec observations have some bearing on this discussion? Aren't there are in situ observations of dust mass to compare directly to? The problem I have with this entire line of analysis is that the authors never say how the dust particle size distribution is emitted at the source. For example, the Ginoux et al. 2001 prescription provides one example of an initial particle size distribution. Is that what is being used here? How about MB95, which does not provide explicitly the simulated particle size distribution at emission? And for S04? So some assumption is made about the initial particle size distribution, and again, couldn't the Fennec observations be used to evaluate that? What is not clear is why simply switching from 5 to 8 size bins reduces the simulated dust mass. Obviously you also changed the initial particle size distribution. How? Couldn't you have changed the distribution using the 5 bins to achieve a similar result? All that is described here is what the bin effective radius is, not what the size distribution actually looks like. There's nothing mysterious here about this: if you change the size distribution you change the mass extinction efficiency. Actually, that would be a useful thing to show, how that is different among the simulations.

Minor points:

Page 1: Line 25: Should read "however fails to capture…" Line 38: Should read "…the Arabian Peninsula has annual dust emission about one fifth as large as the emissions from North Africa…"

Page 2: Line 22: Instead of "resides to" use "relies on"

Page 4: Line 22: Why do you call the first scheme G01? You are not using the emission equation from Ginoux et al. 2001, but rather the equation from Gillette & Passi 1988. You are using the Ginoux source map, but you are using that after all for all the simulations. As an aside, could you clarify what resolution of the Ginoux map you are using? Ginoux et al. 2001 describes a 1 degree source map, but he has separately provided a version of that map at 0.25 degree.

Page 5: The entire first paragraph is confusing to me. I just don't understand the distinction you are making here between S04 and the G01 and MB95 sources. Is S04 using some other source of sub-grid scale information prior to using the Ginoux map? Otherwise, why isn't this exactly the same as the other two? Fundamentally, don't you use the grid cell resolved winds with some flux equation, and then scale the resulting flux by the efficiency of the grid cell for emissions? Is there some difference in thinking of the Ginoux map as being an efficiency factor versus the fraction of the grid cell available for emissions? Unless there is some other source of sub-grid scale information I don't see what the difference is. Line 31: The OMI data is available nearly time-coincident with the Aqua overpass, but offset in time from the Terra overpass. How do you cope with that in driving this comparison of the model to MODIS data?

Page 7: Line 7: I don't see how G01-0.75 overestimates the Arabian peninsula AOD in Figure 3d. Line 9: How do you conclude that G01-0.5 produces the most realistic AOD over the Arabian peninsula when it looks more like G01-0.75 does (more white space)? Line 18: You refer to a second scaling. See my comment above about S04 scheme. I guess I'm confused about how the Ginoux map is being applied.

Page 9: Line 36: I find this conclusion about the background AOD confusing. I could argue that the simulated AOD is less than the background from AERONET at Crete, but it is not clearly the case for Lampedusa. But AERONET shows total column AOD, and you are only simulating the dust component. So I don't buy anything you are saying about the background AOD level. Am I missing something here? Do you have some reason to think that the AERONET AOD shown is due entirely to dust?

Page 10: Line 23: There is no analysis presented which supports the assertion that the higher altitude part of the dust AOD in the simulation results from long-range transport

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as opposed emission processes. My experience, admittedly with global models, is that a deep boundary layer develops over Saharan Africa, so that dust may be mixed quite. So what is the PBL height in these simulations and along these profiles?

Figure 10: Use a consistent date labeling for each of sub figures (see title at top of each figure).

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