

Interactive comment on “The spatial distribution of mineral dust and its shortwave radiative forcing over North Africa: Modeling sensitivities to dust emissions and aerosol size treatments” by C. Zhao et al.

Anonymous Referee #1

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General comments:

This article tests 2 parameterizations for dust aerosols emissions, 2 dust transport treatments, and 2 dust aerosol size distribution in the regional WRF model against a wide array of measurements in North Africa. The article is well-written, clearly presented, and presents results that are important to the dust modeling community. For example, the authors show that the WRF/Chem model has skill in reproducing observed dust emissions and can thus be used in future studies. Moreover, the dust emission and dust transport schemes are varied in this study, which is useful in as-

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sessing their relative effect.

The article thus attempts to accomplish two separate goals. First, it tests parameterizations for dust aerosol emissions in WRF/Chem, a commonly used regional model. Second, it runs several cases to test the influence of the dust emission scheme, size distribution and modal or bin transport method on the predicted dust concentration, optical depth, etc. The paper clearly meets the first goal, which makes it an important contribution to the literature. However, it falls short of the second goal for reasons I note below. I envision the article will be suitable for publication in ACD after substantial revisions.

Specific comments:

- The article presents results of 4 separate runs, with different emission schemes, emitted dust distributions, and transport (modal vs. sectional) schemes. However, only 2 of these runs (Modal1-G and Sect1-D) are tuned to measurements, whereas the other 2 (Modal2-G and Sect1-G) are not (that is, the total emitted mass is kept the same as in the Modal1-G run, even though the dust size distribution, which greatly affects the dust optical depth and other measures, is varied substantially). The asymmetry in the tuning of the 4 runs makes the comparison between them of reduced value. Although the authors can still draw conclusions about the general effect of the different dust emission, transport, and size distribution parameterizations, they cannot draw firm conclusions about which one compares better with measurements. For example, the correlation coefficients of the 2 untuned runs with AOD measurements (Figure 7) are much lower than for the tuned runs, but it's impossible to say whether this is because of the lack of tuning for the former 2 runs or because the size distribution (or transport scheme) is less realistic. And being able to conclude which size distribution and transport scheme is preferable would be important to optimizing future dust models. I thus recommend that the authors redo runs Sect1-G and (especially) Modal2-G and tune the parameter C in the same manner that they tune it for runs Modal1-G and Sect1-D. The authors will then be able to draw conclusions on which transport scheme and (especially) size dis-

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tribution parameterization compares better with measurements. Because of the broad range of measurements used by the authors, this would be of value to the literature and would greatly improve the article.

- The authors find that the modal approach retains more fine dust particles than the sectional approach. Why is that? And can this be interpreted as a deficiency of the modal approach? That is, if one would perform an idealized simulation (for example by using a very large number of bins in the sectional approach) would the modal approach deviate substantially from it?

- Page 10, lines 7-13: Please briefly summarize the SW dust treatment in the model, since it is critical to interpreting the results of this study.

- Page 12, lines 9-12: The authors keep the geometric standard deviation of the modeled log-normal modes constant. I understand that this is necessary for computational reasons, but the authors should discuss what inaccuracies are introduced into the simulation because of it. Does it explain the differences between the modal and sectional approaches? For example, differential particle removal rates will surely affect both the geometric standard deviation and the volume mean diameter. Also, is the volume mean diameter also held constant or does it decrease in the model as the aerosols age and the larger particles fall out? If so, how is the decrease in the volume mean diameter calculated?

- Page 13, lines 19-20: The simulated total emission over 31 days (if I counted correctly) for North Africa is ~ 200 Tg. That seems rather large compared to global model estimates of ~ 2000 Tg/year (see for example Cakmur et al., JGR, 2006), and is also large compared to the ~ 1800 Tg/year of Ginoux et al. (2001), who developed the main dust emission schemes used in this study. This is especially surprising considering that the tuning parameter C is 35% smaller in this study than in Ginoux et al. (2001). So why is the modeled dust emission seemingly so much larger in this study than in Ginoux et al. (2001)? Was it a particularly dusty period, does the increased resolution

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produce increased emission, or is there another reason?

- Page 14, lines 21-23 and Figure 5: Please show the original data from Osborne et al. (2008) in the figure, not the fitted function.

Technical corrections:

- Abstract, line 20-25: the authors mention maximum heating rates and radiative forcings here. Those are much more noisy and of less use than averages. Please replace maximum rates by domain average rates.

- Page 3, lines 5 and 10: The authors use "Sahara" on line 5 and "Sahel" on line 10. Is this intentional or do the authors mean to refer to the same geographic area?

- Page 3, line 21-22: The recent study by Balkanski, ACP, 2007 would be appropriate to cite here.

- Page 3, line 25-26: Readers might not be familiar with the processes of saltation, creep, and suspension, and these should be defined and an appropriate reference should be cited.

- Page 4, lines 4-5: Readers might not be familiar with the modal and sectional transport approaches. Please define them here, and discuss their benefits and drawbacks, which will also make the rest of the article easier to follow.

- Page 5: lines 18-25: The relation of DABEX to AMMA is unclear to me here. Is one a subset of the other or are they separate field campaigns?

- Page 6, line 5: Please define "nephelometer"

- Organizational comment: I think the article would be better organized if the authors first present their methods (i.e., chapter 3), and then the data they use to test their methods (i.e., chapter 2). That is, I recommend they switch chapters 2 and 3.

- Figure 4: The x-axis here is confusingly labeled. Please include the actual dates and

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mention them in the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 9753, 2010.

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