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Interactive comment on "Global dust cycle and uncertainty in CMIP5 models" *by* Chenglai Wu et al.

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Global dust cycle and uncertainty in CMIP5 models Chenglai Wu, Zhaohui Lin and Xiaohong Liu

Presented in this study is an evaluation of the global dust cycle simulated by 15 models participating in the Coupled Model Intercomparison Project (CMIP5). The models are compared with each other, aerosol reanalysis data and station observations of dust deposition and concentration. Differences between model simulated dust emission, load, deposition and other aspects are discussed. I believe this is a very valuable study which allows us to better understand the state of the art of dust modelling and better understand the areas where research is needed.

It is probably not surprising that very large differences exist between the model simulated features of the dust cycle, as we already know for some time. It remains a challenge for the models to converge to the truth. This study is a valuable reminder of the challenges ahead and contribution to better quantifying the error bars of the aerosol radiative forcing estimated by climate models.

The paper is well written and logically structured, although a more concise description would be my preference.

There are a number of issues, which I suggest the authors to consider:

Abstract appears to be long.

L11: address their strengths . . .

L28-29: deposition is a flux, not a sink

Model data: a description of the dust schemes examined in this study is given in this section. These schemes differ in a number of aspects. It would be helpful if some statements were given here, how it is ensured that the comparison is fair. For instance, all models have the same spatial resolution? Do they use the same land surface data?

In Section 4.1, I suggest to write explicitly the equation for the global dust budget, and state how the individual terms are computed, so that we can easily understand how the quantities examined are related and why they are chosen. For example, while residence time is important for dust deposition, surface shear stress is important for dust emission, so why is residence time compared here, but not surface shear stress?

L222: may be useful to state, whether we are talking about the same size range. If it is not the same size range, then it is not meaningful to emphasis the range of 735-8196 Tg /a, and a size range correction is necessary. I am not sure whether I missed something, but it is not clear to me whether this is the total emission for the particle size range 0 – 20 microns for all models, or the emission for some models using size range 0 – 20 microns and some 0 – 63 microns.

L245: I recall that in earlier studies dry and wet depositions are about the same order of magnitude, the finding that wet deposition makes only 12-39% of the total deposition is somewhat surprising.

Section 4.2: some dust emission schemes are already adjusted to satellite observed dust load (so much emission is allowed such that the dust load matches the satellite observed global dust load). I think it would be useful to point out which these models are.

Section 5, Discussion and Conclusion: Experience shows that differences in land surface schemes can have a major effect on dust emission estimates, in particular the simulation of soil moisture. It may be useful to say something about it.

L587-589: Again, is the size issue considered in the comparison? Because mass is proportional to size cubed, a small difference in size range can result in huge differences in the dust budget terms. If size correction is not done, then what we can learn from such an assessment study is limited.

I suggest, separate the discussion with conclusion. As it is very a long section.

Uno et al. (2006 JGR), Textor et al (2006; 2007 ACP) have done model comparisons. These papers may be interesting to this study.

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