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Interactive comment on “A new dust cycle model with dynamic vegetation: LPJ-dust version 1.0” *by* S. Shannon and D. J. Lunt

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We thank the reviewer for their helpful comments on this paper. We respond to the comments below:

This approach is really questionable because: the physics may be more precisely and explicitly described. The best way is not always to use simple schemes, with many parameters and to adapt these parameters. In the case of friction velocity threshold, explicit schemes exist and are already implemented into dust emissions and transport models (see Iversen and Shao for example). The tuning may be efficient for some regions or some parameters. But this is not really robust and mainly depends on the multi equilibrium of the physics in the model. This is completely model-dependent and

not universal.

We have used the scheme of Iversen and White (1982) to calculate the threshold friction velocity (u_t) , which is a function of particle size. We tune this by tuning a scale factor which serves to increases or decrease u_t by a constant factor, but retaining the same form of size dependence. We will make this clearer in the revised version of the paper. We could have used an even more physically-based scheme to calculate u_t. For example, the scheme of Fecan et al. (1999) which includes the influence of the soil moisture on u_t. However in remote regions, uncertainty in the CRU precipitation, caused by the lack of observational data, means that the soil moisture may not be known well. In this case, tuning the dust fluxes may be preferable to improving the physics. We will comment on this in the revised paper.

We agree with the comment that the tuning is completely model-dependent and not universal. The purpose of the tuning is to improve the performance of this particular model. We recognise that the tuned parameters are specific to this model and that the tuning is limited by the availability of observational data, both spatially and temporally. Again, we will comment on this in the revised paper.

The ’best’ parameters are valid for specific locations and periods, preferentially when the physics is quasi-linear and relatively ’smooth’. This is not the case of dust emissions fluxes, by definition a sporadic process. It seems difficult to apply ensemble methods on a process mainly based on extreme responses. If ensemble modelling showed improvements in the forecasted results for temperature (for example, a process with a diurnal cycle and rarely extreme values), this approach is difficult to adapt to physical phenomenon based on multi threshold values.

Using an ensemble approach to tune non-linear processes is very standard in the climate literature. For example, model parameters associated with cloud processes are often tuned in a similar way (see for example Jones et al, 2004; Rougier et al, 2009). Indeed, the very fact that gusts are sub-gridscale (and sub-temporal scale) in the ERA reanalyses (and climate models) means that tuning is almost essential

More specifically: The whole sections 2.3.1 and 2.3.2, about the dry and wet depositions are completely already written in [Seinfeld and Pandis, 1998] without any new material. The authors should removed these sections and just cite the book. Idem for 2.6 about sub-cloud scavenging.

Unfortunately, this book is not freely available and is expensive. This section is relatively short and included for completeness. We think the benefit of retaining this section outweighs the disadvantages of a longer model description, especially as the journal is specifically aimed at complete and transparent model descriptions.

The terminology "new dust cycle model" is completely overestimated. There is nothing "physically" new in this paper. Just statistical adjustement of well-known (and sometimes'old') schemes.

We disagree with this comment. The inclusion of dynamic vegetation is "physically" new in the model. Previous offline dust models have used equilibrium vegetation models to simulate vegetation cover, or satellite-based representations of modern vegetation which cannot be applied to future or past climate studies. Including dynamic vegetation makes it possible to calculate seasonal

and inter-annual variability in dust source areas. A subsequent paper will investigate the importance of this process.

The dust model is constituted of the scheme of [Marticorena et al., 1995]. With this scheme, the vertical flux is simply diagnosed using an alpha factor linking the vertical to the horizontal flux. This approach is robust, widely used, but a lot a schemes were developed in the last ten years. For example, the schemes of [Shao et al., 2000] and [Alfaro and Gomes, 2001] are much more physical. By using this type of schemes, the authors would have more realistic and "state-of-the-art" results.

We agree with this comment. Although a robust approach, the alpha factor is treated in a relatively simplistic way in this scheme. There is potential to improve the treatment of alpha in a subsequent version of the model. We will comment on this in the discussion section of the revised paper.

The "conclusions" section includes all results. This is shortly presented and a lot of questions remain open. For example: what about the relative uncertainties of the dust emissions model and the chemistry-transport model? What about the meteorology uncertainties?

We also agree with this comment. In the revised version we will extend the conclusions to discuss the relative uncertainties in the meteorology, the chemical-transport model and the dust emissions.

The scores are built using 'monthly' dust fluxes and the 'primary' fluxes are calculated using 6-hours ERAinterim meteorological data. This seems now relatively crude when a large part of all dust models used in the world are using, at least, hourly meteorological data, knowing that the dust emissions are sporadic and may occur during very short periods of one to two hour.

We have used 6 hourly metrological fields so the model can be forced by GCM or reanalysis data on a global scale. One reason for this is because we want to use the model to simulate the dust cycle under past and future climate forcings, where detailed 1-hourly meteorological fields are not available. We agree with the comment that dust emissions are sporadic, and model could be improved in the future by including a gustiness parameterisation. We will comment on this in the discussion section of the revised paper.

Finally: to tune a model containing many parameters and by using results averaged over several months (results are presented as annual mean surface concentrations) in Fig.9 is not a step forward for dust emissions modelers and chemistry-transport model applications

The step forward in this paper is not the tuning of the model (although we believe this is the first ever model to be systematically tuned in this way), but the inclusion of the dynamic vegetation in the dust cycle.

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