

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

Anonymous Referee #2

Received and published: 17 June 2010

General Comments:-

This paper presents a new off-line dust model which links existing models of dynamic vegetation (Lund-Potsdam-Jena), dust production (Tegen et al.) and tracer transport (TOMCAT), allowing reanalysis or GCM data to be used to drive vegetation and hence dust source areas as well as dust emissions. The process of model tuning is described: the technique of Latin Hypercube Sampling is used to select 20 sets of values of the model's 7 tunable parameters, which are used together with three variants of the wet deposition scheme in a set of 60 model simulations, from which the best is chosen from comparison with observational deposition data from DIRTMAP and Ginoux and concentration data from the University of Miami. The combination of a dynamic vegetation model with a dust emission model to allow variation in source areas to be

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included in the calculation of dust emission is a useful step forward for off-line models. Such a combined model should allow more realistic dust simulations in past and future climates to be performed off-line than has possible with models relying on fixed vegetation. The approach to tuning is interesting, and it is good to see a description of a systematic approach to a part of dust model development which is too often glossed over. However it would have been useful to have some consideration of the physical meaning of the tunable parameters. This omission raises the question of whether the most appropriate forms of the parameters have been chosen and hence limits the validity of the results. It might be hoped that a good choice of tunable parameters would show in the results, with each having a clear range of values for which skill scores were high (i.e. an area in parameter space where the model was near to reality). The decision to have different values of the global tuning parameter (T) for different observational datasets means that the tuning process does not finally provide a single set of parameters for use in future experiments. Further experimental exploration of the reasons for the large differences between the values of T , and the resolution of the problem to provide a single value for use in the model would enhance the paper, as would a review of each of the tuned terms.

Specific comments:-

1) Comments on the tuning method

Seven parameters and the deposition scheme are included in the tuning process. Some, but not all of these terms have a clear physical meaning. Looking at each in turn:

The 3 versions of the removal scheme are different representations of the process of wet deposition and as such have clear physical meaning. The results summarised in Table 2 show clear differences in model performance between the schemes, and this is obviously a case where this tuning approach has worked well.

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mfpar_lim is used in the calculation of source areas in shrub and grass regions. It is not obvious why mfpar_lim should be the same for both grass and shrub, indeed the reader might naively expect the values to be different, as the processes involved are different for each plant types and when $\text{mfpar} > \text{mfpar_lim}$ $A_{\text{grass}} = 1 - (\text{mfpar} / \text{mfpar_lim})$, but $A_{\text{shrub}} = 1 - \text{mfpar_max}$. The results do not seem to show that any particular range of values of mfpar_lim is better than any other, suggesting that this may not be a useful parameter for the model. Would separate grass and shrub limits work better?

sm_lim, the upper limit of soil moisture for which dust emissions is permitted, has a clearly defined meaning, but the physical realism of this is not discussed. There is much evidence to show that the effect of soil moisture on emissions is dependent on soil type (see e.g. results summarised in Fecan et al., Ann. Geophys., 1999), so the use of a single global value needs discussion at the very least. Once again, the lack of any indication from the results that one value of this parameter is better than any other raises doubts about its usefulness.

sd_lim is used in the calculation of source area due to snow cover. Again, there is no discussion of the appropriateness of a parametrisation of the form used, and no encouragement from the results to think that this is a suitable parameter to use in this form.

eta is used in a variant of the well-known horizontal flux equation of Marticorena and Bergametti. It seems to have been introduced in this work, but there is no explanation of its physical meaning, or of the reason for deviating from the more usual form of the equation. It is used as a multiplier of both (U^*_t / U^*) and of $(U^*_t / U^*)^2$ which seems curious, as one might expect a term related to U^*_t to vary with it (e.g. multiplying the $U^*_t^2$ term by eta^2 in eqn 10). Explanations of the meaning, form and need for eta would be most useful.

The final 3 terms are global multipliers on emissions, used in the comparison with different observational datasets: T_dirtmap, T_ginoux and T_miami. Global multipli-

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ers are so widely used in dust models to make up for inevitable shortcomings of the parametrisation that no justification is absolutely required. It is usual to use a single value for all cases, though. The use of different values of T for different observations may allow apparently good agreement with data to be shown at this stage, but does not achieve the objective of tuning, which must be to find a single set of parameters for use in the model. It could be very enlightening to see the equivalent of Table 2 with skill scores based on each dataset separately.

There is some discussion of the reasons for different values of T in the Conclusion, however the implications of these reasons tends to undermine the rationale of the method of calculating skill scores, and therefore the results presented.

If, as is suggested, the discrepancy is due to uncertainties in the observations then these data should not be used for model validation and the ranking of the experiments should be based only on NRMSE_Miami.

If the discrepancy is due to interannual variability or trends in the data not captured in the short experiments, as is also suggested, then that must raise questions as to the usefulness of tuning based on such short runs alone. In this case, at the very least one long run needs to be performed to assess model interannual variability and trends compared with data and also to assess whether the results of the short tuning runs are representative of longer simulations. Unless the model performed well in such a test, a new suite of longer tuning runs would need to be performed.

2) Other comments

Sec2.1.1: Monthly soil moisture over a 50 cm layer will have much less variability than that seen on the timescale of dust events in the surface layer from which deflation occurs. It would be good to have some comment on the likely effect on simulations, and also the effect of using monthly mean snow cover.

Sec 2.3.1: It would be useful to have numbers quoted for the distance particles of each

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size will fall in a timestep versus model level depths, to back up the assumption that particles don't fall through more than 1 layer. Even the 24 μ m particles will have fall speeds of the order of ten cm s⁻¹, i.e. hundreds of m hr⁻¹, so this assumption is quite surprising

Sec 2.8 & 3.0: "Results" seem to contain a lot of information on the method of calculating skill scores, rather than actual results - which are mostly found in "Conclusions". Some reorganisation of these sections would be helpful.

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Technical Corrections:-

Sec 2.5: (after eqn 16) should G be the _horizontal_ flux?

Interactive comment on Geosci. Model Dev. Discuss., 3, 473, 2010.