Geosci. Model Dev. Discuss., 3, C124–C128, 2010
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Geoscientific
Model Development
Discussions

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

C. S. Zender (Referee)

zender@uci.edu

Received and published: 18 June 2010

General Comments

The manuscript describes LPJ-dust, an off-line dust module. The strength of the manuscript is that it conveniently offers, in one place, a rather complete description of a current dust model. This suits the mandate of the journal to enhance the reproducibility of geoscientific models. The use of Latin Hypercube Sampling to fit free parameters makes good sense. The manuscript would be much stronger if it explored more deeply the science for which it is well-poised, such as vegetation/dust response to precipitation anomalies (described below).

Specific Comments

An advance of LPJ-dust relative to previous research is said, both in the manuscript and in responses to reviewers, to be the incorporation of *dynamic* vegetation as a constraint. The novelty and significance of this seem questionable, and the revisions should be more clear about this claim: Most current dust models are agnostic about where their vegetative cover constraint originates, because the dust physics are kept independent of the biogeochemistry and only “know” about the end result, e.g., vegetative cover. Do no other publications describe dust model results driven by dynamic vegetation?

Moreover, LPJ-dust is evaluated after the vegetation has reached an equilibrium with the current climate, so the readers do not gain a sense of the significance of the dynamic vegetation: How well does the model simulate observed dust/vegetation response to interannual precipitation variability? Does LPJ-dust predict reasonable pre-industrial to present changes in dust/vegetation when driven by 1870–present meteorology?

p. 474, line 20: Evans *et al.* (2006) appear to make the same connections among vegetation, rainfall, and dust emissions in the Sahel as Zender and Kwon, 2005. In the latter we note that strong precipitation is associated with *increased* dust emissions in many regions such as the Saudi peninsula (our Table 2). In this context the revisions might note some of the emission processes that would be the next step beyond dynamic vegetation-driven dust.

The use of Latin Hypercube Sampling to fit free parameters is generally well done. However, the reasons for choosing the particular handful of parameters that are tuned (instead of others) could be clarified. For example, why not tune the size distribution as did Cakmur *et al.* (2006)?

p. 475, line 2: Be clear that inability to simulate the fast observed response of vegeta-

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tion to precipitation in the Sahel is a limitation of vegetation models, not of dust cycle models. And note that Yoshioka *et al.* (2007) examined whether the feedback went the other direction (dust driving vegetation).

p. 484, equation 14: If the smooth roughness length $z_{0s} = 0.001$ cm and non-smooth surface roughness $z_0 = 0.01$ cm, always, then this equation reduces to a scalar constant in dust-emitting regions. This should be acknowledged and the value of the constant given. Why are not soil texture or satellite estimates used to infer locally varying roughness lengths over emitting surfaces?

sm_{lim} is the soil moisture for complete dust suppression and is discussed in units of mm. Using an absolute scale (mm) for soil moisture applies to a certain thickness of soil, presumably the thickness of the LPJ top soil level. A normalized scale (e.g., volume of water per volume soil, or mass of water per mass of soil, etc.) is depth-independent and would facilitate reproducibility and inter-model comparisons. Please change the units to something like volumetric water content, or gravimetric water content.

The terminology for the evaulation datasets is awkward (e.g., "Ginoux data"). I suggest "DIRTMAP deposition", "modern deposition" or "Ginoux deposition", and "surface concentration", respectively.

The U. Miami dataset supplied by Mahowald should be attributed to Prospero and collaborators, e.g., Prospero *et al.* (1996)

Why were not satellite-estimated Aerosol Optical Depth (AOD) or Absorbing Aerosol Index (AAI) (Stowe et al., 1997; Cakmur et al., 2001; Torres et al., 2002) in dusty regions used to constrain the model? The density self-consistency of such observations far exceeds that of the three datasets employed.

Technical Corrections

p. 488, equation 23: Exponential is missing negative sign

p.494 line 3 and elsewhere: The units of the surface concentration dataset are , or equivalent, not, as variously presented in the manuscript, μm^{-3}, or μgm^{-3}.

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