

## ***Interactive comment on “Modelling water availability, sediment export and reservoir sedimentation in drylands with the WASA-SED Model” by E. N. Mueller et al.***

### **Anonymous Referee #2**

Received and published: 27 March 2009

This paper presents the WASA-SED catchment scale hydrology, erosion and sediment transport model. It outlines the numerical descriptions used for the erosion and sediment transport processes on the hillslopes, in the river channel and in the reservoir modules. While this model is an impressive effort in terms of the both the scope and the scale, and worth publishing in order to provide a comprehensive model description for potential users, in its current form lacks some necessary detail and information and is incomplete. I have the following specific comments:

1) In its current form the paper reads like a manual of the model with little science content. It needs more discussion in terms of the rationale and justification for the

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choice of equations and the overall approach. The paper also needs to explain what it is about this model that makes it specific to drylands. I'm assuming that it is mainly because it is driven by surface-runoff hydrology but without the hydrological description it is difficult to assess. Is there anything in the erosion/sediment transport approach that makes it more specific to drylands?

2) Although description of the hydrological component may be already presented elsewhere, if this paper is intended as a detailed and full description of WASA-SED then for the sake of completeness I would also include the hydrological module.

3) The introduction and discussion of the WASA-SED model in relation to existing erosion models is quite thin and really does not convey the full spectrum of approaches that exist in the literature. I think a summary of current approaches and their limitations would be useful to set the WASA-SED model into a better context. At this point, a clearer outline of the niche of the model would be helpful.

4) Leading on from point 3) there are also some contradictions in the paper. In the introduction (p. 287, lines 23-25) the authors state that "the erosion component in these [other] models is usually based on modifications of the USLE or MUSLE approach..." and imply that this is a negative aspect (understandably!). However, WASA-SED also uses the USLE and derivatives. So they really need to clarify what they are implying here.

5) The model is referred to as 'process-based' but the hillslope erosion component is based on the USLE which is empirical and, to a large extent, inappropriate for use in a model like this as it is based on small-scale plot data and contains many semi-quantitative parameters. This approach needs justifying within the context of this model and given the claims of being physically-based.

6) In its current form the paper does not provide any evidence of model evaluation. I believe that this requirement is outlined in the GMD journal white paper. It is impossible to evaluate the merit of the model without some form of evaluation. The authors

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say that they have already published model applications. So the authors need to decide whether there is enough detail on the WASA-SED model already published (and therefore this paper is unnecessary) or there is a need for a stand-alone and comprehensive description of the model (in which case this paper is merited but needs to add a selection of model applications). I think this paper would be much more useful as a complete entity with some selected examples of model testing and maybe even a 'lessons learnt' section in which the limits of applicability of the model are evaluated. This would be much more valuable to someone wanting to use the model. In the current version of the paper we have to take the authors' word that the model yields good results when compared to field data. There is a definite need for a 'Model Application and Evaluation' section in this paper in order to address this gap. Currently, section 3 of the paper (p.298) is very weak and is mainly a qualitative discussion of some of the applications but there is little quantitative information on model performance and model uncertainties. Discussion of uncertainties, in particular, seems to be more of an afterthought and there is little substance in statements like "uncertainties towards process descriptions existed in regard to processes that occur in the interstorm period such as the soil moisture dynamics..." (p. 300, lines 25-28). In a revised version that should include model evaluation, I would recommend including a quantification of the uncertainties for a range of application settings in a more robust manner.

7) Without the inclusion of any evaluation or application, the title of the paper should have just been 'Description of the WASA-SED model for catchment-scale sediment export and reservoir sedimentation' as there are actually no modelling results included.

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Interactive comment on Geosci. Model Dev. Discuss., 1, 285, 2008.

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