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Interactive comment on "Assessing the large-scale impacts of environmental change using a coupled hydrology and soil erosion model" *by* Joris P. C. Eekhout et al.

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The paper presents an innovative combination of the existing SPHY hydrological and MMF erosion models, with intended use at the large catchment and decadal scales. Despite this foreseen use at large spatial and temporal scales, the combined model is presented as a physically-based model. I agree with this assessment: most relevant processes that determine catchment water and sediment yield are included, and well presented in the paper. My main concern in this part of the paper is that there is insufficient attention for the large number of parameters that these extra formulae introduce. I recommend that the reader be provided with more information about all parameters,

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especially whether they are 1) measurable or not, 2) available from literature, and if so for which kind of environments, or 3) most be obtained through calibration. Ideally, this would go together with a detailed global sensitivity analysis, that could give insight in how robust model simulations are under the weight of all these parameters. A good example for how to do this is https://www.geosci-model-dev-discuss.net/gmd-2017-236/ , or (I apologize) my own paper in Computers and Geosciences about the Lorica model a few years back. If the authors find that sensitivity analysis is a bridge too far in this already long paper, then that lends more weight to my first suggestion (provide details - even if it happens in supplementary material).

The model is then calibrated and validated to simulate water and sediment yield for a large catchment in Spain, and two scenarios for the future state of the catchment are used to demonstrate the models' capability. The quality of calibration and validation results is not concerning at first sight, but should be placed in the context of other models' results - even though these are bound to be in other areas, for other timescales, etc. Only reporting such results leaves the reader with questions. During my review, I also missed reporting on results from the first (conservation agriculture) scenario - a climate change scenario appeared to get all the attention.

Figures in the paper are good as it stands - although a bit more detail in them and their captions would make them more useful. I would appreciate another table with information on parameters (see my comment above). Detailed annotations and suggestions are available in the attached scan.

All in all, I consider this an important contribution to literature presenting a model that usefully supplements the range of available models. I especially appreciate the authors' detailed and deliberate explanation of model equations. In light of the modest suggestions for improvement, I suggest minor revisions.

Arnaud Temme

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2018-25/esurf-2018-25-RC1-
supplement.pdf

Interactive comment on Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2018-25, 2018.