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Earth Surface Dynamics Discussions

Interactive comment on "Opportunities from low-resolution modelling of river morphology in remote parts of the world" *by* M. Nones et al.

Anonymous Referee #2

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General comments

I recently had the pleasure of reading the discussion paper "Opportunities from low-resolution modelling of river morphology in remote parts of the world". The aim of the work, which is clearly stated in the paper, mainly lies in assessing the capabilities of a simplified approach for overcoming the problem of data lack in long-term morphodynamic modelling of large rivers. The choice of the case studies appears particularly appreciable, for it allows to investigate two problems of major interest at the considered scale, namely the impact of human interventions throughout a river reach and the effect of climate change.

The paper reads easily and clearly; nevertheless a broader description of the theoretical framework in Section 3 would significantly improve the overall readability. True it is

that provided references give a complete account of the model formulation, but some more details within this section would probably enhance the "stand-alone" usability of the paper.

The discussion is mostly focused on picking up some of the points raised in the previous sections of the paper and expanding them with an eye on the results found. I would suggest to put more emphasis in discussing model limitations, with particular attention to the implications of some assumptions and results (uncertainties related to geometric and hydrodynamic forcings, underestimate of large values of the equivalent discharge, etc.), eventually recalling some of the arguments used in the interactive comment published on October 31. Coming to possible model applicability, I suggest hereby a couple of points that may also be worth discussing.

I think that a brief account of the implications and requirements for extending this approach to smaller rivers might be of practical interest for a number of readers. For instance, satellite data resolution is presented in the paper as a bottleneck for the description of river cross-section, substantially restricting to wide rivers the use of this kind of information. On the other hand, some smaller rivers in more densely populated areas may sometimes rely on a richer database for hydrology and morphology: in these cases, would the present approach be appropriate for predicting long-term morphodynamics?

More generally, this modelling approach has in my opinion a good potential for estimating sediment supply to coastal zones at the mesoscale, eventually in climate change scenarios, addressing one of the key issues in long-term coastal morphodynamics. What is the feeling of the authors about this point? What are possible advantages and limitations to this kind of application?

My overall evaluation of the paper is definitely positive, and I look forward to read the Authors' responses to my comments. Hereinafter I list some specific points. Please note that English is not my mother tongue, hence some language concerns may have

not been completely addressed by my review.

Specific points

408/1-3: as this paper shows, the study of river morphodynamics does not strictly require considering all the processes at the different scales: therefore I suggest to substitute, in the first part of the abstract, “The study of rivers morphodynamics requires modelling of...” with “River morphodynamics is the result of...”.

408/9: “code” sounds implementation-specific. For the sake of generality I would suggest something like “approach” instead.

408/14: I would avoid semicolumn and rather start a new sentence.

408/23: free water surface.

409/24: I suggest to replace “depending” with “dependence” .

410/20: I suggest to replace “regards” with “concerns”.

411/29: Are there any studies, in which the role of extreme events is explored, that the authors consider worth mentioning in the framework of hydrological variability?

412/2: I suggest replacing “lower” with “smaller”.

412/4: alluvial plains.

412/11-12: considering the slope of alluvial plains (which is actually presumably driven also by fluvial sediment transport across the hydrological network) as a constraint for river dynamics is equivalent to uncoupling the investigated dynamics from geological processes (say, from the LGM to present days). If this is the case, I would suggest to mention this point in the assessment of the limitations to model applicability.

415/1: fulfill.

415/10: I suggest to clarify how the effective discharge is defined and computed.

415/16: please add a brief explanation about the Schaffernak approach or cite some references.

416/8-9: d was also used for representing sediment diameter. Although the change in notation is clearly stated here, I would suggest to use a different symbol for event frequency (is this an occurrence frequency? exceedence frequency? Please clarify).

417/14: modify.

417/17-18: I guess that a water flow reduction due to damming is actually quite expected to rapidly propagate downwards, I would therefore suggest the authors to explicitly point out the implications and the relevance of this result.

417/21: How is this transition expressed in the model results? I guess that in a 1D approach this can be stated based on some Leopold-and-Wolman -like approach, is this the case? Please clarify.

417/24-25: Is vegetation encroachment included in the simplified cross-section morphology expression?

418/9-13: In principle, the morphology of the Delta is the result of an interplay between riverine sediment supply and oceanographic processes at coastal and regional scale. In absence of a dedicated model for the long-term description of transitional environments morphodynamics, please discuss the relative importance of these two processes (a first-guess proxy could possibly be given by the ratio between solid discharge from the river and long-shore drift) in the framework of the results obtained.

418/15: I would suggest to replace “were boundary” with “were prescribed as boundary”.

419/9-10: Please discuss more in detail this point. What are possible causes for the underestimations of largest discharges? What are the implications in terms of sediment transport and sediment budget?

421/12: remote parts.

428/Fig.1 (A): As an alternative caption I would suggest considering: "Main African rivers' basins, focus on the Zambezi river".

431/Fig.4 (B): Satellite image of the Zambezi Delta.

Manuscript evaluation

Scientific Significance: 2

Scientific Quality: 2

Presentation Quality: 2

1. Does the paper address relevant scientific questions within the scope of ESurf? Yes.

2. Does the paper present novel concepts, ideas, tools, or data? Yes.

3. Are substantial conclusions reached? Yes.

4. Are the scientific methods and assumptions valid and clearly outlined? They are, although I would suggest to recall with some more detail the key concepts of the theoretical framework.

5. Are the results sufficient to support the interpretations and conclusions? Yes.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes.

8. Does the title clearly reflect the contents of the paper? Yes.

9. Does the abstract provide a concise and complete summary? Yes.

10. Is the overall presentation well structured and clear? Yes.

11. Is the language fluent and precise? In my opinion it is in most cases, but English is not my mother tongue.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes, although I suggest a slight modification in a specific point.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Only in a few points, which I list in the comments.

14. Are the number and quality of references appropriate? Yes.

15. Is the amount and quality of supplementary material appropriate? Yes.

Interactive comment on Earth Surf. Dynam. Discuss., 1, 407, 2013.