Review: manuscript ”Determinants of modelling choices of 1-D free-surface flow and erosion issues in hydrology: a review”

General comment

The paper presents a review of modelling approaches to free-surface flow and morphology. It aims to characterise the modelling choices which are gathered from published studies, as function of the dimensional and dimensionless characteristics of the physical system to be modelled. The final goal is to establish a list of guidelines and best practices helping modellers in choosing the right model.

Four different flow models (namely the Navier Stokes Equations, the Reynolds-Averaged Navier Stokes Equations, the Saint Venant equations, and Approximations to the Saint Venant equations) and related morphological models are considered. After introducing the equations, the paper proceeds by reviewing the use of these models in published studies, to build up a classification of model usage as function of various dimensional and dimensionless criteria. In detail, initially the correlation between model choice and spatial and temporal scales of the study domain are analysed. Then, a classification of flow typologies is introduced, based on characteristic depths and slopes, and modelling choices are matched to flow typologies using the above schematisaton. Finally, the usage of models as function of characteristic values of dimensionless numbers is analysed.

The paper raises a hot topic, since the increasing availability of computational tools based on different and competing mathematical models requires modellers to be increasingly aware of the range of validity and best usage of models. In fact, quoting Escauriaza et al. [2015] in his very recent review of morphodynamic models for gravel-bed rivers,

At present there is no systematic, reliable method to define the model category appropriate for a specific phenomenon in nature.

Thus, this paper aims to provide a number of criteria to address this issue.

Having said this, I have a number of major concerns. I think that the proposed classification criteria do not fully address the matter, and therefore the manuscript does not fully reach the objective (as stated in the abstract) *to help each modeller positioning his (her) choices with respect to the most frequent practices, within a generic, normative procedure.* In this respect, I think some different view could be incorporated, or at least discussed. Furthermore, I find the presentation of the morphodynamic part of models a bit generic, unnecessarily stretched over different subsections, and incomplete of some recent development. These concerns are discussed in detail below.

In addition to these, I also have a number of minor concerns and suggestions. Among these, I would suggest simplifying the language (especially in the introduction) for improving understanding. Minor issues are listed in detail below.

This is why I recommend a severe major revision.

Major issues

- I think that the present discussion on determinants of modelling choices misses a fundamental point: how modelling choices are determined by the objective of the study, i.e. by the natural phenomenon to be modelled. By this, I mean that the same flow and erosion event/process can be described by different models, depending on the required detail in the modelling description. One of the possible examples of this issue is the dynamics of a dune-dominated fluvial bed, which, depending on the modeller's focus, can be readily studied by the SV approximation with appropriate continuity models for sediment [e.g., Ribberink, 1987, Blom, 2008] or instead by much more refined flow and sediment transport models, which fully take into account the vertical coordinate and the distribution of forces on each sediment particle [e.g., Nabi et al., 2012, 2013b,a]. In my view, to select the most appropriate model for their applications, modellers shall compare the smallest spatial/temporal scale at which the selected model operates, and the scale of the phenomenon to be analysed. Therefore it is not an intrinsic scale of the physical system/domain, but instead the scale of the object of interest within that system, which dictates the modelling choice. The multiplicity of modelling approaches allowed for the same case may help to explain the overlap of approaches across scales which is observed in this manuscript.

- Another issue originates from the same observation. When comparing modelling choices for the same natural phenomenon, a more detailed description of the system, by the use of a more complex model, does not necessarily represent a refinement, but instead a fully different view on the same physical system. In other words, using a more complicated model does not necessarily allow to replicate what a simpler model would do, plus adding more information, but instead can produce a completely different outcome. An example is given by Sloff and Mosselman [2012], who compared the results of two different continuity models (with and without mixed sediment) for the same river bifurcation. I would therefore like this issue to be discussed with reference to the modelling approaches analysed in this manuscript.

- Furthermore, the manuscript misses the analysis of a critical point in modelling, which is cost-effectiveness and feasibility. By cost-effectiveness, I mean the possibility of reproducing the observed behaviours by including the minimum amount of processes [e.g., Escauriaza et al., 2015] and trying to minimize computational cost, which, although less of a limiting factor than in the past [e.g., Mosselman, 2012], still is a critical point in making modelling practically feasible. An effort towards simplicity could indeed help reducing parametrisation, data requirement, and thus minimizing well-known model shortcomings such as equifinality and other modelling mistakes [see, e.g., Mosselman and Le, 2016]. The principle of minimizing modelling effort could then help modellers in better placing their modelling efforts within the classification diagrams proposed in this manuscript, when working in regions characterised by significant overlap of different modelling choices. This concept is somehow buried inside the discussion (lines 960-964) but would in my view require some expansion and references to available studies on the matter.

- I have some concerns about the structure and content of Section 2, regarding erosion models. I am fine with the presentation of flow models from the most complete (NS) to the most simplified (ASV). However, the presence of individual subsections devoted to erosion model associated to each flow model looks unnecessary, because the difference between the erosion models in each subsection is unclear. Furthermore, the relevant equations are not presented, which does not help in understanding. Finally, some recent developments such as the direct numerical simulation of turbulent flows based on the NS equations and particle-based morphodynamic models [Kidanemariam and Uhlmann, 2014, Colombini, 2014] could be incorporated.

Minor issues

- In the title and throughout the paper: I consider "erosion issues" as an excessively limiting and possibly misleading term, as the modelling here considered both includes erosional and depositional processes. I would therefore change it into "morphodynamics".
- Line 34: "help each modeller positioning his (her) choices" → "help modellers in positioning their choices".
- Line 80: "... through successive flow aggregations over various bed topographies" not clear.
- Line 83: "main rivers".
- Lines 89-90: "which is the open normative procedure designed to allow comparisons between studies and to be fed to the community" not clear.
- Line 92: "genericity" → "generality".
- Line 104: "Earth's surface" → "earth surface".
- Lines 109-111: "under the angle of connecting ... or debating the merits ...": not clear.
- Line 113: It is not clear what "This" refers to. In the same line, I have some trouble with the definition of "contextual" and "strategic". I would also ask for rewriting of the rest of the sentence, which is not clear to me.
- Line 137: It is misleading here to mention "reduced complexity models" as a synonym of models in river morphodynamics based on the approximation of the sediment discharge being equal to the transport capacity calculated locally or including a transport distance. In fact, these assumptions are very widely applied in 1D and 2D models usually regarded as "physics based" (e.g., BASEMENT [Vetsch et al., 2014], and Delft3D [Sloff et al., 2001]). Conversely, the models usually regarded as "reduced complexity" [e.g., Murray and Paola, 1994] may apply even cruder estimates of sediment discharges.
- Line 141: "erosion science".
- Line 145: "conceptual element", "contextual element". The meaning of these items in my view is neither obvious nor standard in the literature. A definition would be needed.
- Sections 2.1, 2.1.1, 2.1.2: The content of these very short subsection is essentially an introduction to the more detailed description of models in the following subsections. I therefore advise to simply remove these headers and make it just an introduction to the following content.
- Section 2.2.1: Authors may insert a sentence to clarify that the list of models here considered does not enclose all the possible modelling choices in the literature.
- Line 157: "from the richness of their physical basis" → something like "depending on the degree of refinement in their physical description".
- Line 160: please introduce the acronym "ASV".
- Line 161: "Diffusion Wave" → "Diffusion Wave Equation", "Kinematic Wave" → "Kinematic Wave Equation".

- Line 169: "... the examination of erosion issues from the angle of decreasing refinement ... as a whole" not clear.

- Line 171: "disconnection" → "discrepancy" or "inconsistency".

- Equation (1) is not the full set of the 2D x-z NS equations. The vertical momentum equation and the continuity equation are missing. Authors are in general advised to present mathematical models more thoroughly.

- Lines 194-200: I do not understand why the discussion over turbulence models, until the development of the RANS equations, is is placed here, since the RANS are then introduced in another section.

- Section 2.2.2: Here "erosion issues" are presented without mentioning any continuity model for sediment. A presentation of the the continuity framework could be useful, as the resulting equations are themselves part or the hydro-morphodynamic mathematical model.

- Line 219: "debating the case of turbulence damping ..." please rephrase for clarity.

- Line 221: "the matter is not free from doubt today" please rephrase for clarity.

- equation (2): same comment as for equation (1).

- Line 248: "judiciously".

- Line 255: "private hunting grounds".

- Lines 272-274: The hypothesis of shallow water ($H << L$), which limits the admissible free-surface slope and implies quasi-hydrostatic pressure distribution over the vertical, shall be mentioned when introducing the Saint Venant equations.

- equation (3): same comment as for (1) and (2).

- equations (5) and (6) represent a generalisation of (3). My advice is just to present (5) and (6) instead of (3) for brevity.

- Line 309: "erosion-hydrology": definitely "morphodynamics" or "hydro-morphodynamics".

- Line 310: the Exner equation is presented here, but never properly introduced in the text.

- Line 312: there has been quite more recent work on the diffusive character of the Exner equation. See for instance Furbish et al. [2012] and related papers.

- Line 312: I do not agree with the Authors on the point that "most studies ... take particle velocity equal to water velocity", as I could not think of a single study in which this assumption is done. Generally speaking, the Exner equation requires the evaluation of solid discharges to be fed to the continuity framework, not of particle velocities. Although some empirical formulae for particle velocity based on the hydrodynamic variables exist, these are not of immediate use for evaluating bedload fluxes. In fact, even if the particle velocity was known, bedload discharges would come from the product of particle velocities and the thickness of the transport layer, once again not precisely known. Therefore assumptions on particle velocities are not generally made. Theoretical studies though exist [Furbish et al., 2012, Ancey and Heyman, 2014, Ballio et al., 2014] in which the complex linkage between particle velocity distribution and sediment discharge in the continuity framework is addressed.

- Line 361: "shear stresses are generally calculated from near bed laminar or near laminar profiles". Could Authors provide reference to this statement? I disagree with it, because the sediment transport formulae mentioned in line 365 have all been derived for turbulent natural flows.

- Line 479: "This dispersion contains a lot of information". I would delete this sentence as unnecessary.

- Section 3.1.1: In Figure 2 I cannot easily gather a proper trend from the figure in terms of model choice as function of the L/T system speed. Instead, a trend is detectable if we consider the L scale only (e.g., the Navier-Stokes equations seem to have been applied to smaller domains than the ASV equations, which makes sense). Could the Authors discuss on this point within the section?

- Figure 5: I have some concerns over the use of a dimensional vertical coordinate such as H for discriminating between flow typologies. In my view, depth does not provide a unique criterion unless it is compared with the size of roughness. This is somehow obviated within the figure by plotting a non-dimensional threshold ($\Lambda_z = 100$), but it is then inconsistent with the axis.

- Section 3.3.1: Please remark that dimensionless numbers arise in the non-dimensionalisation of systems of governing equations, i.e., they are an inherent feature of the model in use to describe the physical system.

- Lines 806-810: "accelerated by pressure effects" does not necessarily correlate with supercritical flows. Furthermore, the discussion over the direction of propagation of waves is sound only if the bed is fixed. With movable bed, more characteristics come into play, and the identification of a sub- and super-critical regime is not entirely possible [Lyn, 1987, Lyn and Altinakar, 2002].

- Lines 811-813: I have a concern over the use of the bed slope S. Actually, the free-surface slope appears to be much a stronger control over the flow characteristics.

- Line 843: "angle" → "point of view".

- Line 930: "each modeller ... his or her" → "modellers ... their".

- Line 933: "comprehensive view" → "comprehensive set" or "database".

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