Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/esurf-2022-52-RC2, 2022

Comment on esurf-2022-52

Anonymous Referee #2

Referee comment on "Method to evaluate large wood behavior in terms of convection equation associated with sediment erosion and deposition" by Daisuke Harada and Shinji Egashira, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-52-RC2, 2022

Dear authors,

I have read your ms "Method to evaluate large wood behavior in terms of convection equation associated with sediment erosion and deposition" with great interest. The prediction of wood dynamics (the term "driftwood" should be used for wood elements floating in lakes and oceans only, not in rivers) during large floods is undoubtedly crucial in many regions worldwide, and available models are still few and very often untested against real events.

Having said this, I do not think the model proposed in the ms represents a step forward in our ability to predict wood transport, and thus manage potential wood hazards. Although the effort made by the authors in trying to express mathematically wood transport processes in Eulerian terms is understandable, it is well known on one hand that a model should simplify the reality only up to the point that dominant driving factors are still captured. On the other hand, a model should not be more complex than needed for its purposes. While the authors claim that the model has been successfully validated against the 2017 post-flood surveys, in my opinion this is not the case. The major points of criticism from my side are reported below, and I hope they may help you.

Best wishes

- The 2D model is claimed in the introduction to be able to "describe the behavior of large wood based on the convection equation and the storage equation with sediment erosion and deposition to simulate the behavior of numerous numbers of large wood pieces". If I am not wrong, the present formulation – for both sediment and wood – neglects entirely the bank erosion process. Such process is well known to be the dominant wood supply mechanism in partly-confined and unconfined rivers, and

bedload rates may also be greatly influenced by lateral channel migration. Also in the case of the Akatani river bank erosion/channel widening seems (based on Fig. 4) to have been a massive "player" during the flood;

- The model to compute the "amount of sediment and large wood inflow from the basin at the upstream boundary of the 2-D analysis" is swiftly presented, with too few details and insights about its plausibility/performance. Uncertainties in the prediction of mass movements processes (location, volumes, connectivity with the channel network) coupled with their wood supply are huge, and there is not track of this in how the upstream boundary conditions have been later used for the 2D simulations. Furthermore, the forest stand parameters are said to have been "assumed", but in such a relatively large basin area a constant value for them is highly unlikely to be real. Several different wood input scenarios should have been tested at least, integrated with bedload transport scenarios;
- The validation of the model is absolutely not convincing, being proposed in a highly qualitative and non-systematic way though the domain. In addition, the arguments brought to support that idea that the model has been successful mostly rely on comparing – again too vaguely, in semi-quantitative terms at best – the flow field and on the deposition pattern in the proximity of the bridges. But this is an "easy win", as for sure bridges are areas where wood was trapped (Dirac delta imposed = 1) and thus flow (increase in flow depth and diversion around the bridge) and sediment transport (deposition) were affected. Therefore, I would say that the model validation suffers from both strong equifinality issues due to its large number of unconstrained parameters and from a tautological argumentation without an accurate, statistically-based accuracy analysis, recalling also the uncertainties regarding the input conditions.
- Regarding the practical outcomes of the model proposed, the same conclusions about the role of bridges in the Akatani flood could have been obtained by applying any hydraulic or morphodynamic model with the use of reduced cross-sectional areas at bridges due to expected high wood load. As the authors correctly say, an accurate estimation of the extent to which wood may clog a bridge is crucial for this aim, but this is not incorporated in the proposed model. I was a river manager, I'd certainly use simple, empirically-based rules to include the role of wood and sediment deposition at bridges in robust hydraulic models rather than using a very complex, time-consuming 2D model whose outputs are subject to large epistemic and aleatory uncertainty.