Geoscientific Model Development Discussions

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Comment on gmd-2021-313

V. Voller (Referee)

Referee comment on "Modeling of streamflow in a 30□km long reach spanning 5 years using OpenFOAM 5.x" by Yunxiang Chen et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-313-RC1, 2021

The work in this paper uses relevant field data to provide appropriate calibrations and constraints in the development of a 3-D CFD model of the streamflow in a reach at large length and time scales.

I applaud this ‘grand challenge’ effort to establish a significant bench-mark for the accurate long time/length scale prediction of streamflow.

I have a number of comments for the authors to consider. In my view, unless the authors so wish, none of these require changes to the current manuscript.

1. The authors provide a comprehensive survey of the literature of the full range of previous modeling efforts for streamflow. One set of work which may have been overlooked is a series of papers by Kang, Sotiropoulos and others

Kang, S., A. Lightbody, C. Hill, and F. Sotiropoulos (2011), High-resolution numerical simulation of turbulence in natural waterways, Adv. Water Resour., 34(1), 98– 113.

Kang, S., and F.Sotiropoulos (2011), Flow phenomena and mechanisms in a field-scale experimental meandering channel with a pool-riffle sequence: Insights gained via numerical simulation, J. Geophys. Res., 116, F03011, doi:10.1029/2010JF001814.

Kang, S., and F.Sotiropoulos (2011), Assessing the predictive capabilities of isotropic, eddy viscosity Reynolds-averaged turbulence models in a natural-like meandering channel, Water Resources Research, 48, https://doi.org/10.1029/2011WR011375

(full disclosure I was on Kang's PhD committee)

These works compare 3-D free surface/ turbulence (both RANS and LES) model predictions of streamflow with highly resolved measurements in a natural channel. The scale of these calculations is smaller than the current work (10 's m as opposed to 10's km) but the conclusion of the Kang et al work does point out possible accuracy issues in using time averaged turbulence models.

Some questions in this regard, Is a RANS model sufficient for the task at hand? Would LES improve predictive performance? Is LES currently feasible at the scale of the current simulation?

2. The authors provide a nice explanation of how they balanced the modeling efforts between computational efficiency and predictive accuracy. Much of this focuses on how the code was constructed to segment the calculations and reduce the CPU requirement. Of course, in a large modeling study, of the scale reported here, the actual CPU time may only be a part of the overall effort used. Could the authors comment on the resources to set up the model (meshing, calibration etc) and the resources required to validate the model ; Were these of the same order as the CPU?

3. The authors close by correctly pointing out the possible benefit of their approach in assessing impacts of climate change. They could be a little more specific here. In particular, are the space and time domains presented here sufficient for meaningful climate change scenario modeling? If not, what time scales and reach sizes would be meaningful?

4. While outside of the scope of the current paper. In future work it might be worthwhile to compare the performances (CPU/predictions) of the proposed 3D RANS/fee surface calculations with the more widely used 1-D and 2-D streamflow codes noted in the literature review

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