JOURNAL OF COASTAL AND HYDRAULIC STRUCTURES

Review and rebuttal of the paper

CFD modeling of flow and local scour around submerged bridge decks

G. Fleit et al.

Editor handling the paper: Rebekka Kopmann

The reviewers remain anonymous.

1 Round 1 of review

1.1 Authors' responses to Editor

The authors would like to thank the reviewer for considering their manuscript. The authors also thank the reviewers for their thorough work and believe that the constructive criticism (in forms of comments and questions) contributed to the notable improvement of the paper. The manuscript has been revised according to the comments of the reviewers.

The answers/responses for each reviewer questions/comments are given in the following.

1.2 Authors responses to Reviewer A comments

General Reviewer comment:

I have personally found the manuscript content interesting, well-written and suitable for publication in the Journal of Coastal and Hydraulic Structures. It is also relevant for the scientific and engineering community, as a practical application of a not-so-common issue discussed in the literature.

> **Response**: The authors thank the reviewer for the detailed, thorough review and the constructive comments. Author's responses are listed below for all comments/questions individually.

Reviewer comment/question: In Equation 2, how does the pressure term is accounted for in REEF3D? Is it splitted into hydrostatic and dynamic components?

> **Author reply**: REEF3D is a fully-nonhydrostatic 3D model; the pressure term in the governing equation is the total (hydrostatic + dynamic) pressure. The equations of fluid motion are solved on a staggered gird, ensuring tight velocity-pressure coupling. The pressure gradient term in the governing equations is modeled with Chorin's projection method *for incompressible flow.
> *Chorin A. (1968) Numerical solution of the Navier-Stokes equations. Mathematics of Compuitation 22(104):745–762.

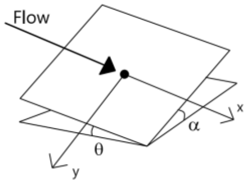
Reviewer comment/question: How does the influence of the bottom roughness is considered into the model?

> **Author reply**: Considering that the boundary layer is not resolved in our RANS approach, wall functions are applied to consider the relevant effects near the solid boundaries – that is where the roughness is considered.

Reviewer comment/question: Section 2.5: more clarification/details are needed to better understand the way in which the bottom slope and/or the sand slide algorithm are incorporated into the model. Are there any parameterization involved?

> **Author reply**: A reference is given to the work of Dey (2013) – presenting an empirical formula for the approximation of the critical shear stress reduction factor (*r*). This is an explicit formulation; no parametrization is involved. Additional information regarding the sand-slide model is also given in the revised version of the manuscript.

(Image credit: *Bihs & Olsen (2011) Numerical modeling of abutment scour with the focus on the incipient motion on sloping beds. Journal of Hydraulic Engineering 137:1287–1292*)



$$r = 0.954\left(1-\frac{\theta}{\varphi}\right)^{0.745}\left(1-\frac{\alpha}{\varphi}\right)^{0.372}$$

Reviewer comment/question: Section 2.5: more clarification/details are needed to better understand the interaction between flow, sediment transport and bed update (coupling period, etc.)

Author reply: Additional details have been added.

Reviewer comment/question: Could you please provide some indicators related with the CPU time, computational power, etc. required to perform the numerical simulations presented in the manuscript?

Author reply: The following sentence has been added to section 3.2: "The steady state (rigid bed) simulations took approximately 0.5 hours, the mobile bed cases around 10 hours of CPU time (Apple M1 Pro) on a personal notebook."

Reviewer comment/question: Figure 3: Could you please provide a detail/zoom of the bridge desk's computation mesh?

Author reply: The model uses the ghost-cell immersed boundary layer method to account for complex geometries in the Cartesian grid. That is, the mesh is not fitted to or refined around the bridge geometry. Regardless, the figure has been updated.

Reviewer comment/question: Is there any explanation of the shock wave/discontinuity from the numerical simulation result in Figure 4b (just above the deck)?

Author reply: That issue is related to the high gradients in fluid properties (air/water) around the surface, more precisely its treatment through the free surface turbulence damping method employed in the paper. Since the original submission, the hydrodynamic simulations have been redone with slightly modified model settings (the software code has also changed, as it is under continuous development). As per the latest results, this issue is resolved. It is also worth mentioning, that the general accuracy of the model did not change (as per the comparison with experimental data), suggesting that the free surface anomaly in question does not affect the simulation results in general, but it is rather a local numerical artifact.

Since the issue is not present anymore, it is not discussed in the revised version of the manuscript.

Reviewer comment/question: Are the results shown in Figure 6 instantaneous or averaged in time?

Author reply: Results in Figure 6 are averaged over time. The figure has been replotted based on the latest results and the caption has also been updated.

Reviewer comment/question: For Guo's experiences, indicate the time required to reach the morphological equilibrium. Which is the mobility condition (\tau_critical) for this case?

Author reply: The sentence before Figure 8 has been modified to contain the experimental equilibrium time. The critical shear stress is 1.20 N/m^2

Reviewer comment/question: Figure 8a: could you comment on the "phase shift" observed between the numerical result and laboratory observations?

Author reply: A possible cause of the phase shift (related to the temporal inaccuracy of the scour development) is now discussed in the Discussion section.

Reviewer comment/question: The influence of turbulence/flow structure on the scouring processes presented in paragraph page 11, L12-15 must be explained/described in more detail.

Author reply: An additional figure and more description is added to the revised version of the manuscript.

Reviewer comment/question: In addition to Figures 9-10, shouldn't it be interesting to present the results of the pressure distribution (hydrostatic + dynamic), as well as shear stress distribution for the study case? (in an appendix for example).

Author reply: The authors partially agree. The longitudinal variation of the bed shear stress before and after the morphodynamic simulations is presented in a new figure (Fig. 11). The paragraph related to the variation of near-bed TKE (and the corresponding bed shear stress) has been complemented. In terms of the pressure distributions, while there are no practical obstacles to do that, the authors do not think that would have notable added value to the manuscript.

Nonetheless, if the reviewer insists, or there is editorial request, the authors will be happy to add such plots to an appendix.

Reviewer comment/question: Add reference + shorter explanation at the end of the phrase "...for more advanced free surface treatment" (page 13, L22).

Author reply: The sentence has been rephrased and addition reference has been added.

Reviewer comment/question: In page 14, paragraph L17-19, the range of values of the Shields parameter is function of the density of the sediment, the density of the fluid, and the particle diameter of the sediment.

Author reply: Correct. The parameter in question is the critical Shields parameter. Corrected in the revised version of the manuscript.

Reviewer comment/question: In my opinion, the Conclusion must be shortened and more to the point, based on the different findings presented in the manuscript. The Authors would also provide advice on possible improvements and future work based on the manuscript content.

Author reply: Agreed. The Conclusions section has been significantly shortened and modified.

Minor remarks

- page 2, L21: …bridge deck ginder…
  Corrected.
- page 2, L30: I would remove "the surprising"
  Corrected.
- page 3, L20: define LSM
  Definition is given two paragraphs before.
- page 3, L20: I'd say instead of "...and mobile sediment bed tracking…" -> free surface, sediment transport and bed evolution processes
  Corrected.
- Define \sigma_{\omega} in Eq. 5s
  Added.
- page 5, L7: is the non-dimensional Shields number…
  Corrected.
- Section 3: I'd call it 'Numerical scenarios"
  Corrected.
- page 5, L33: avoid word "cheap"
  Word replaced.
- Caption Figure 2: direction is FROM left…
  Corrected.
- page 6, L22: define \sigma_g
  Definition added.
- page 6, L22: particle diameter $d = d_{50} = …$
  Corrected.
- page 8, L28: define n. In the text write mi and si in italics.
  Corrected.
- page 9, L18: typo "belove"
  Corrected.
- page 10, L13: (Figure 8B).
  Corrected.
- page 10, L17-18: improve the writing -> ex. The effects of the submerged ratio are investigated…
  Corrected.
- page 10, L18: …are rather similar FOR BOTH CASES; …
  Corrected.
- page 10, L19: In Figure 9, H1 and …
  Corrected.
- page 11, L15: BSS?

BSS is defined in section 2.5, but the shortening is actually not necessary – a corrected.

- page 12, L25-26 almost repeated just below on L34-35.
  True. p12 L25–26 has been removed.
- page 13, L5: typo "an"
  Corrected.
- page 13, L37: avoid "...to the next level".
  The sentence has been rephrased.
- page 14, L16: .,
  Corrected.

1.3 Authors responses to Reviewer B comments

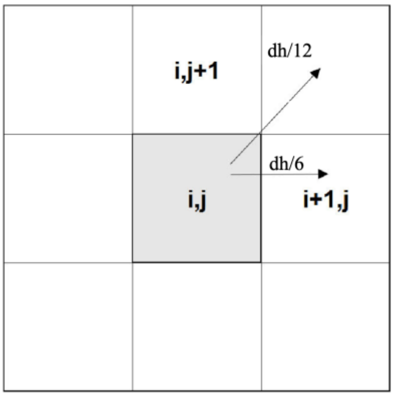
The reviewer submitted his/her comments and questions by working to the manuscript file (.docx). The authors have corrected the minor stylistic and grammatical errors pointed out there. Comments and questions are copied and answered here, for the sake of clarity.

Reviewer comment/question: As the sediment transport is strongly dependent on the near bed kinetic energy, it would be interesting to know if you used a wall-approach in the turbulence model or rather a very fine mesh with y+ values in the range of one to get the values of k in the near-bed region. Or is this not necessary in your case?

> **Author reply**: Considering that the boundary layer is not resolved by our RANS approach, wall functions are applied to consider the relevant effects near the solid boundaries – that is where the roughness is considered. The authors acknowledge that this is a notable simplification of the flow problem near the bed, however, a goal of the paper was to present the practical applicability of a computationally affordable RANS-based model for the prediction of the local scour.

Reviewer comment/question: Does the redistribution also go over the corners not only over the faces? Or don't you have cubic cells? I would expect 6 neighboring cells for a cube.

> **Author reply**: On point remark. Whenever the bed slope exceeds the angle of repose of the bed material, the slope of the concerned bed cell is readjusted by redistributing the volume of sediment in all eight neighboring cells (horizontal neighbors, as presented in the figure below, which is a top view). During the redistribution, the heights of the direct neighbor cells are increased/decreased by $dh/6$, where dh is the height difference between the adjacent cells, while the height of the concerned is decreased/increased by the same amount. Same approach is applied for the diagonal cells, only the value of alteration is now $dh/12$. The process is continued iteratively until the bed slope becomes equal to the angle of repose of the bed material. This is the case for truly 3D cases. However, in case of the 2DV simulations used in this study, there are only two horizontal neighboring cells, that is the redistribution is only performed for those ones using the same factors ($dh/6$). The related section has been rephrased to clarify this issue.



Reviewer comment/question: How often do they (hydrodynamic and sediment transport algorithm) exchange “results”? How does the coupling between the hydrodynamic part and the morphodynamic part of the solution work?

Author reply: In terms of time treatment, the hydrodynamic and the morphodynamic model are decoupled. The solver determines the time step size for the hydrodynamic model based on the CFL criterion using the flow velocities. At the same time, another CFL criterion is used for the morphodynamic model as well, where the time derivative of local bed level changes is used as an input. This gives the time step for the sediment transport model. Sediment transport (i.e., bed change calculation) is evaluated after every fifth hydrodynamic time step. This allows the hydrodynamic model to always adapt the prevailing bed changes.

The issue is clarified in the revised version of the manuscript.

Reviewer comment/question: How did you define that steady state is reached after 42 h? In the first testcase (Fig 8b) steady state was reached much faster than in the second (Fig 11b).

Author reply: In the numerical simulations (S1–S3), the scour profile is basically unchanged after ~5 hours, however, the simulations were run for a total of 42 hours. The number 42 comes from the experiments of Guo (2011), who reported quasi-equilibrium morphological conditions after 42 hours. In the comparison of Fig. 8b, the measured data of Guo is plotted against our numerical results to show the differences between the real and the simulated temporal evolutions. The same timespan was used later in Fig. 11 to avoid reader confusion.

The reason of the 42 hours sediment simulation time is given in the revised version of the manuscript.

Reviewer comment/question: Please define U0. Is there a reason you used a capital letter for U0 whereas you used a lower case bold “u” in your result plots.

Author reply: U_0 is a relatively conventional notation of the depth and time averaged flow velocity (usually calculated from the continuity equation). U_0 is now defined in the Introduction section and also written out in Section 3.1. Capital letter is used to note that it is an averaged value, basically a scalar (contrary to lower case bold $\boldsymbol{u}$) which is the velocity vector.

Reviewer comment/question: In Fig 4a and 4b the difference between the numerical and the photograph seems much larger than in your line plot in Fig 5a. What is the reason?

Author reply: The hydrodynamic verification cases have been redone with slight adjustments made in the parametrization of turbulent free surface dampening. The new results now show better agreement and a more realistic free surface profile as well. It is also noted that the photograph is not orthogonal to lateral axis of the channel, which can make the qualitative comparison slightly misleading.

Reviewer comment/question: Figure 6: The legend of the velocity plot and the plot seem to have different number of colour steps. Please adapt the legend similar to the k-legend. Please increase the colour legend size so that the font size corresponds to the paper font size.

Author reply: The very same coloring/color legend is used throughout the whole manuscript. For velocities: linear scaling, with range 0–1.0 m/s with 20 steps; for *k*: logarithmic scaling, with range 10^{-4}–10^{-1} using 9 steps. These settings ensured that all relevant features are visible, and the figures are aesthetic in general. In case of Fig. 6, however, most of the values are between 0.3– 0.8 m/s, resulting in less color steps actually present in the figure.

Reviewer comment/question: How do you prove the adequacy? (Regarding the selected mesh resolution for the morphodynamic simulations)

Author reply: A grid convergence study was performed for the hydrodynamic case based on measured and simulated water surface profiles. The results showed that the model converges around a relative grid resolution of $H_0/\Delta x = 56$ with a MAPE somewhere below 6% and gains only minor improvement (MAPE slightly above 5%) with a notable higher resolution ($H_0/\Delta x = 89.6$).

Considering that the flow problem is very similar in the morphodynamic case study, and the fact that there is no hydrodynamic control data available, an assumption is made that an adequate grid resolution can be derived based on the aforementioned grid convergence study. With that in mind, a relative grid resolution of $H_0/\Delta x = 62.5$ is selected, which corresponds to an absolute grid resolution of $\Delta x = 0.004$ m.
The issue is described more in the revised version of the manuscript.

Reviewer comment/question: "The authors note that such slight inaccuracies could be reduced by the additional tuning of the related parameters and algorithms (e.g., Shields parameter, critical shear stress reduction formula, or the sand slide algorithm).": How do you know? This is an assumption, right? Or did you do tests? If yes, please describe your tests.

Author reply: The reviewer is right. Although it is a basic assumption, that a model (especially a sediment transport model) could always be further tuned and refined, it is indeed unprofessional to express this without actual, case-specific proofs. The sentence has been removed.

Reviewer comment/question: Do you know the reason for the different intensities of the scouring processes?

Author reply: The issue is addressed in the Discussion section.

Reviewer comment/question: Figure 9. Please enlarge the velocity legend

Author reply: The velocity legend has been enlarged.

Reviewer comment/question: “Despite the fact that the equilibrium scour depths (3–4 cm) are comparable to the upstream water depths (14–19%)”: How are the scour depths comparable with the upstream water depths? This is not clear to me.

Author reply: The phrasing was inadequate, we agree. The sentence has been completely rephrased to better deliver the authors’ thoughts.

Reviewer comment/question: “Notable variations are observed between the TKE fields for the fix bed simulations.” This sentence makes a different statement to the first sentence of the next section: Notable variations are observed between the TKE fields for the fix bed simulations …Vs. Similar near-bed TKE patterns are observed for the fixed bed variants …

Author reply: Agreed. Unlucky phrasing again. While the first sentence refers to the overall patterns of TKE and the following text mostly discusses its distribution around the deck itself, the second one refers to the near-bed distribution of TKE, which is indeed similar. This suggests, difference suggests, that even though the flow around the deck does change notably with the level of submergence, this does not affect the near-bed regions (mostly determining local scouring).

Nonetheless, the sentences in question have been refined to avoid reader confusion.

Reviewer comment/question: Figure 11: Isn’t it S2 in Figure 11b? (I would read from Figure 11b: S1 and S2 similar – S3 different)

Author reply: Unlucky phrasing, agreed. The paragraph in question has been rephrased.

Reviewer comment/question: “A level set method-based CFD model (REEF3D) was used to simulate the complex hydro- and morphodynamic processes around submerged bridge decks.”: Is there special knowledge necessary to simulate the complex hydrodynamic conditions?

Author reply: No special knowledge, but rather a special modeling environment with high-order discretization and advanced free-surface tracking (i.e., LSM) is necessary to accurately simulate the prevailing flow conditions.

2 Round 2 of review

Reviewer A:

Thank you for the revision. I can recommend to publish the revised version of the paper as proposed by the authors.

Reviewer B:

I've carefully read the updated version of manuscript, as well as the (complete and very clear) responses provided by the Authors to the reviewers. I accept the submission in its final form.