Responses to associate editor and reviewers

Associate Editor

Comments to the author:
Dear authors,

thanks for the revised paper and apologies that it took some time to secure the second review.

I have now received the assessment of both original reviewers, and they are very happy about how you responded to their comments. Reviewer #1 (Richardson) is happy with the content, and makes a large number of editorial and language suggestions. Reviewer #2 makes three important points. Although these points have been addressed in the discussion, the reviewer comments that they contain fundamental caveats. Essentially, you have to pathways to deal with that: (1) either, you address them by improving the numerical scheme and set up, or (2) be up-front with these caveats and reason them. The former would be quite a lot of work, and the latter has been explicitly suggested by the reviewer. If you want to take approach 2, please add some material to the method description of the CFD, early in the paper. Highlight the potential caveats and provide a reasoning as to why you choose your approach (this could be, for example, a statement that a high order approach is numerically inhibitive). In the discussion, you could add a few sentences on potential future improvements.

I have decided on major revisions, as I would like to have the option to send the revised paper to reviewer #2 again.

Please provide a detailed rebuttal to the comments.

Thanks a lot and I am looking forward to seeing your revised manuscript.

With best wishes,
Jens Turowski

Dear Prof. Jens Turowski,
Thanks a lot for the valuable comments and suggestions!

All the remarks of both yours and the reviewers have been addressed and replied in this document. We agree to take the second approach suggested by you. Notations for the potential caveats have been added in Section 2 (e.g., in Lines 195-196). Potential future improvements have also been added to Section 4.5 in the discussion. Besides, the appendices have been moved to the supplement file in order to reduce the length of the manuscript. This may make the paper easier to follow for the readers.

All the best,
Chendi Zhang
On behalf of all co-authors

Report #1

Keith Richardson

This revision is a great improvement on the initial submission. The entire paper is more clearly written, more technically accurate and more authoritative. I would like to congratulate the authors again on this paper. This paper contains some novel and excellent data and some important conclusions and it deserves and needs to be published. I would also like to thank the authors for responding positively and constructively to my comments. However, that said, I still have some concerns over terminology and the description of results – although this aspect shows a distinct improvement on the first draft – and it needs further revision, although much more minor than the first revision. I shall discuss this concern in detail first, and then list general comments.

Thanks again for the detailed comments and suggestions! They contribute greatly to improve the clarity of the manuscript.

The word "regime" is misused at several places. Line 230; the "water surface regime" probably did not change during the experiment (it would always have been the plunging jet regime), so it's not clear what was measured here. Line 235; the jet and the jump are flow structures or components or regions of flow, not flow regimes. Line 284; "Plunging jet" (Wu & Rajaratnam 1998) is a flow regime and refers to a type of flow over a vertical drop, consisting of a jet plunging into a hydraulic jump. This regime contrasts with the surface flow regime over a drop in which the jet travels horizontally and enters an oscillatory jump. However, a jet is not a flow regime so it is incorrect to say that "flow accelerates… before plunging into the pool as the jet regime."

The misuse of the word "regime" has been checked and revised throughout, not limited to the three places listed in this comment.

For line 230, "water surface regime" has been revised to "the boundary between the jet and recirculation cell at the water surface". This boundary was extracted from the pictures taken by the top camera during the experiment and compared with the simulated results. Accordingly, the caption of Fig. S3 has been revised to "…(b) upstream edge of the recirculation cell at the water surface from the top view".

Line 235 has been revised to "…the boundaries between the jet and jump components…"

In line 284, "as the jet regime" has been revised to "as a jet feature".

Lines 284-286. This is a misinterpretation. This description gives the impression that the region of high flow velocity moves from the upper part of the flow to the lower part. In fact, the position of the region of high flow velocity remains unchanged in the upper part of the flow and the step stones protrude upwards into it.

The sentence has been revised to "The highest flow velocity in the vertical profile at the crests of step stones mainly exists near the stone surface (Fig. 4), different from the vertical profile of flow velocity upstream of the step where the flow velocity is higher near the water surface (at 43.6 L/s) or shows relatively uniform distribution (at 49.9 L/s)." Our point is to illustrate the difference in vertical distribution of flow velocity.

Line 293. The word "wake" is still misused. The region at the toe of the step cannot be described as a wake. See my comments on the use of "wake" in my review on the first draft.

"as a result of wake turbulence" has been deleted.

Lines 308-309. It is not evident from Figure 5 that high velocity regions occur at the low points in the step crest.

The sentence has been revised to “The x0-6 section, which is located at the step crest, shows that the highest flow velocity is located above the No. 2 and 3 step stones (Fig. 3d) which are among the lowest points within the step crest.”

Line 339. Why does flow velocity decrease with an increase in discharge? This is counterintuitive.

The water depth also increased. It may not be appropriate to say that the flow velocity decrease as the section-averaged flow velocity was not calculated. The point is the highest flow velocity decreases. To be more accurate, the sentence has been revised to “The increase of flow rate from 43.6 L/s to 49.9 L/s leads to…”

Line 366 and Figure 8. Surely these are time-averaged turbulent coherent structures, not instantaneous ones? Although the authors state that the structures illustrated in Figure 8 are instantaneous, the description that follows in the text is of time-averaged structures, and certainly it is only the persistent, time-averaged structures that are relevant to the study.

Based on the turbulence mode (k-epsilon RNG) used in this study, the numerical simulation was based on Reynolds-averaged Navier–Stokes equations, which means that the turbulence modeling is based on time-averaged scheme. However, transient solver was used in the simulation, so the time derivative term was solved in the N-S equations. In this case, the flow structures based on $Q_{criterion}$ are instantaneous for advection part of the flow, but time-averaged in terms of turbulence modeling. If time-averaged velocity is used to compute the turbulent structures, many small-scaled vortices due to the rough elements may not be displayed. Thus, the results shown in Fig. 8 are instantaneous ones at one moment rather than time-averaged ones. We did not calculate the time-averaged $Q_{criterion}$ of the exported data from the solver.

Line 367. What does “streamwise coherent structures” mean? This is vague. What type of structure are they?

They refer to the coherent structures following the protruding grains. The point here is to illustrate the general orientation of these coherent structures. The sentence has been revised to “…coherent structures are mainly streamwise and located near the bed, particularly downstream of protruding grains at the flow rate higher than 12.4 L/s.”

Lines 23, 369 and 444. The vortices at the step toe are described as “streaky”. This adjective is applied to structures that are highly elongated in the streamwise direction, which these vortices are not.

Revised to “the discrete vortices extending along the streamwise direction attached to the step toe close to the bed” in Line 369. “streaky structures” in Line 444 is changed to “structures along the streamwise direction”. The “streaky” in Line 23 is replaced by “streamwise”.

Lines 373-375. This description of curvature in the recirculation cell of the hydraulic jump correlating with the positions of step toe vortices is not convincing. If you decide to stick with it, I recommend replacing “upper bends” and “downward bends” with “convex-upwards curvature” and “convex-downwards” curvature respectively.

The reviewer is right that the correlation is not that evident. The sentence has been removed.

Lines 400-404. I suggest that, rather than the shape and maximum height of the step stones influencing the distribution of shear stress, it is the presence of the step toe vortices. The downstream curvature of the high shear stress zone you mention on the upper faces of the step stones matches the boundary of the step toe vortices as shown in Figure 8.

We agree that the downstream curvatures of the high shear stress zones on the step stones match well with the shapes of the step toe vortices. This point has been added in Lines 416-417. However, the shapes of the vortices are also influenced by the shape and maximum height of the step stones, which affect the separation of jet from the step stones. So the description linking the shape and maximum height of the step stones and the distribution of shear stress is kept.

Lines 429-435. Is it necessary to present drag and lift coefficients as well as drag and lift forces? It does not seem to add anything to the interpretation of step failure and stability.

Indeed, the drag and lift forces show similar trend with drag and lift coefficients. The figure for drag and lift coefficients has been moved to Section S2 in the supplement as Fig. S14 and the text has been shortened in Lines 437-440.

Line 455. This is a misinterpretation of the surface flow regime. These experiments did not generate the surface flow regime because there was always a hydraulic jump across the entire flume width. However, I agree that the fact that in some longitudinal sections the jet did not impinge on the pool bottom indicates that (1) the Q=49.9 l/s discharge is probably transitional between the impinging jet and surface flow regimes, and (2) this indicates that the flow regime over a step is not necessarily binary (i.e. either impinging jet or surface flow).

The sentence has been revised to "However, the 3D flow structures exhibit that the jet does not impinge the pool bottom in some longitudinal sections (Figs. 4-5)."

We agree with the two interpretations provided by the reviewer and have added them in Lines 463-466.

Lines 486-487. This statement is not apparent from Figure 13. Also, why do the values for the step toe vortex not drop to zero downstream of the end of the vortex?

The greatest difference between the *TKE* values for the recirculation cells near the water surface and at the step toe is less than 4 times (Fig. 12d, Figure 13 in last version is Figure 12 in this version) downstream of the contraction section. So there is no difference of order between the *TKE* for the recirculation cells at different locations in the pool.

The streamwise distance shown in Fig. 12 does not cover the whole pool length. The recirculation cells at the step toe and the water surface merge on the negative slope where the algorithm used to separate them does not work anymore. The merge of the recirculation cells already appears in some vertical lines in the cross sections near the pool bottom. So the calculation was only conducted for the streamwise distance where the recirculation cells are clearly separated by the jet. As a result, the calculation stopped a bit upstream of the pool bottom. This is why the decrease of *TKE* for the step toe vertex was not shown in Fig. 12. This information of the calculation method has been added in Lines 281-283 in Section 2.4.

I shall now list general comments.

Line 20. Replace "by" with "from".

Done.

Line 24-25. Replace "with comparable capacity" with "of comparable magnitude".

Done.

Line 26. Replace “increase” with “expansion”.
Done.

Line 27. Delete “as”. Place “grain clusters” in parentheses.
Done.

Lines 34 and 37. “Benefits” and “advantages” are subjective terms.
Line 34 has been revised to “This bed structure has been reported to contribute to providing…”
“With these advantages” in line 37 has been removed.

Line 45. What type of products?
Revised to “topographic reconstructions”.

Line 56 Semi-colon rather than comma required after “morphology”.
Revised.

Line 65. Delete “(the lowest area in the pool)”.
Done.

Line 89. Replace “experimental” with “physical”.
Done.

Line 107. Is the superscript “2” required on “px 2 ”?
Yes. Camera resolution needs a unit of area.

Line 111. It would be helpful to report D100 and D84 as well.
D_{100} (D_{max}) and D_{84} were 140 mm and 50 mm, respectively. Added in Line 112.

Line 112. What discharge is the Froude scaling based on, and why was Froude scaling (rather than some other scaling method) chosen?
The peak discharge for the prototype is 12.6 m^3/s (Zhang et al., 2020), between the peak discharges of 10.4 m^3/s on 14 September 1994 in Rio Cordon, Italy (Lenzi, 2001) and 14.6 m^3/s in the Erlenbach on 20 June 2007 in Switzerland (Turowski et al., 2009), respectively.
Froude scaling based on gravity similarity was selected because gravity played the leading role in high-gradient mountain streams rather than other forces (e.g., viscous force, pressure or surface tension).

Line 117. Change “the step” to “the model step”.
Done.

Figure 2. “L/s” should be “l/s”.
Liter can be written in both “L” and “l” (e.g., in the Guide for SI unit system of the National Institute of Standards and Technology, US). Furthermore, “L” is preferred sometimes to avoid the risk of confusion between “l” and “1”.

Line 125. What are T runs?
“CIFR T runs” are the names used in Zhang et al. (2020). The reference has been added here. “T” is short for topography.

Line 133. Insert “for discharges < 56.1 l/s” after “CIFR T2”.
Done.

Lines 152-153. Insert “all” after “characterize”. Replace “e.g.” with “including for example”.
Done.

Lines 184-185. “The gravity model was activated…” Is this sentence necessary?
Yes. Gravity module is an independent physical module in the software.

Line 223. 2 Hz and 30 s are a low sampling frequency and period respectively. This sampling strategy cannot capture high frequency velocity fluctuations. What are the implications of this for the results?
The turbulence model (RNG k-ε) used in this study cannot capture the velocity fluctuations of high frequency. Thus, the velocity fluctuation was not reflected in the results. However, the spatial distribution rather than the temporal characteristics of hydraulics in a step-pool unit was the focus of this study. So both the low sampling frequency and short sampling period were not problems for capturing the time-averaged characteristics of hydraulics.

Line 244. Replace “in three directions” with “and the subscripts denote the respective coordinate axis”.
Done.

Line 246. Delete “in three directions”.
Done.

Lines 247-248. “Q-criterion” notation should be consistent. Replace “calculate and visualize” with “identify”. How was the threshold value of 1200 selected?
The first “Q-criterion” refers to the visualization method while the rest refer to the variable. So they are written in different forms. “identify” has been used. The choice of threshold value was determined by trial and error to clearly present the coherent structures for the recirculation cells both at the water surface and the step toe.

Line 253. Replace Ps with Pd.
Done.

Line 267 and 270. Replace “taken” with “occupied”.
Done.

Lines 281. Insert “mean” before “flow velocity” in title.
Done.

Line 289-290. Change “the” to “a” in “the recirculation cell”. Insert “associated with a hydraulic jump” after

“water surface”. Insert “associated with an attached vortex” after “step toe”. Delete “sliding”.
Done.

Line 296. Replace “reduction” with “contraction”.
Done.

Line 308. Replace “locate” with “are located”.
Done.

Lines 319-321. Confusing sentence.
Revised to “As the discharge increases from 43.6 L/s to 49.9 L/s, water depth increases in all the five cross sections while the areas occupied by the flows >1.8 m/s decreases in the sections x0-6 and x0+2. The vortices formed at the toe of the step expand their areas in the sections x0+2 and x0+15 at 49.9 L/s than 43.6 L/s.”

Line 329. Replace “with” with “as those in”.
Done.

Line 330. Replace “are” with “is”. Replace “at a much lower level if compared with” with “lower than in”.
Done.

Line 331. I suggest replacing this sentence with “These areas of low TKE coincide with areas of high mean flow velocity”. Indeed, would it be simpler to state that Mean flow velocity and TKE are inversely correlated in general?
Good idea. Both are accepted.

Line 332. Insert “Like mean flow velocity,” at the start of this sentence.
Done.

Line 333. Replace “above the bed surface” with “within the attached vortices”.
Done.

Line 334-335. Move “both” to after “recirculation cells”. Insert “at the” after “water surface and”.
Done.

Line 335-336. Replace “and much higher TKE is contained…” with “although that in the recirculation cell above the jet is much higher”.
Done.

Line 346, 353, 368. Replace “at the downstream area of the” with “downstream of”.
Done.

Line 366. Replace “In the upstream area of” with “upstream of”.
Done.

Line 368. Replace “as a combination of” with “in both the”.
Done.

Line 369. Insert “the” after “water surface and”.
Done.

Line 372-373. Replace “A near bed vortex starts” with “The vortices at the step toe start”. Replace “its” with “their”. Delete “to the” and “direction”. Replace “vortex vanishes” with “vortices vanish”.
Done.

Line 376-377. Replace “do not show streaky features as they do” with “are not elongated in the streamwise direction as they are”.
Done.

Line 387. Replace “flow” with “jet”.
Done.

Line 399. Delete “The step stones bear the highest level of shear stress in the step-pool unit”. This sentence is basically repeated in the next sentence.
Done.

Lines 403-404. Replace “The edges of the” with “There is also a”. Replace “in the back sides” with “on the downstream faces”. Replace “show clear” with “with a”.
Done.

Line 405. Replace “flow” with “jet”.
Done.

Line 445. Replace “different” with “as distinct”.
Done.

Line 448. Replace “will be followed by the” with “generate”.
Done.

Line 452. Delete “eventually”.
Done.

Line 465. Insert “of the profile” after “middle”.
Done.

Line 467. Replace “of the flow for” with “within”.
Done.

Line 470. Move “both” to after “distributions of”.

Done.

Line 474. “Appealingly” is not appropriate.
Revised to “strongly”.

Line 478. Insert “section” before “-averaged”.
Done.

Line 486. Insert “section” before “integral”.
Done.

Line 489. Insert “cell” after “recirculation”.
Done.

Line 490. Replace “one” with “vortex”. Insert “at 43.6 l/s” after “near the water surface”.
Done.

Line 491. Replace “below” with “of”.
Done.

Line 492. Replace “intensification of” with “increase in”.
Done.

Line 494. Replace “is enlarged” with “increases”. Insert “at high discharges” after “pool development”.
Done.

Line 499. Replace “concentrates” with “occurs”.
Done.

Line 501. Replace “local” with “pool”.
Done.

Line 504. Replace “a drop as well as the pool at the downstream” with “an artificial 2D drop”
Done.

Line 513-514. Replace “in the pool” with “of the step”.
Done.

Line 520-522. Is this noteworthy?
“It is noteworthy that” has been deleted.

Line 526. “The grain clusters at the pool bottom…”. This sentence is a tautology.
The sentence has been revised into “The grain clusters at the pool bottom are mainly located where the bed is impinged by the jet, rather in the areas occupied by the recirculation cells connected to the step toe (Fig.

10 and Fig. S14 in the supplement). These grain clusters have very limited…" The point here is that the grain clusters are not distributed in the entire channel width at the pool bottom. The alternation of jet and vortices attached to step toe affects the formation of grain clusters to a large degree at the pool bottom.

Line 531. How is the distribution of micro-bedforms at the pool bottom affected by the jet?
See our reply above for line 526.

Line 548. The variation in the direction of the lift force is unlikely to be "sudden".
"sudden" has been removed.

Report #2

Dear authors,

Thanks for the detailed responses to my comments and observations. The article is better than the first version, especially in terms of writing and clarifications. I enjoyed reading it and analyzing the results.
Thanks for reviewing our manuscript again and the constructive comments!

I still think some aspects of the CFD implementation and description in the article are still missing or need more clarification in the text (some are well explained in the responses). Also, some comments were not addressed, but the response said they were.
For example,
line 78: "In the flume experiment of Zhang et al" In that article there is more than one, so using "the" is incorrect here.
Removed.
but (new) line 96 says: "in the flume experiment of Zhang et al. (2020)"
This is just an example, but this happens in other parts too.

The line 96 in the revision indeed has the same problem with the line 78 in the first submission and 'the' should be removed. However, line 78 was in the last paragraph of the Introduction Section while (new) line 96 is in the first paragraph of the Methods Section. They do not correspond to each other. As we replied, 'the' in line 78 was removed according to this suggestion. But the entire sentence was further removed according to the comment of the other reviewer.

My primary concerns are related to the CFD implementation and the impacts that it may have on the results, especially in the magnitudes of the variables. The following three points summarize this:
1) The complete paper is constructed around the results of highly diffusive numerical schemes. They are first order in all cases and impact the magnitude of every single variable, especially those related to forces and turbulence. The authors tried to justify this in line 578 saying: "The RNG k-ε turbulence model and first-order momentum advection were applied in the CFD simulation. Such settings ensured the computational stability for the flow over the highly complex bed surface of a step-pool unit but could only provide time-averaged results"
While it is true that the configuration will be more stable, the results are impacted by this setup. This should be acknowledged in the paper. As it is now, it seems to be an advantage rather than a loss in accuracy. For

CFD studies, we want second order accuracy in any simulation.
The problem with first-order accuracy is that we don't know if the magnitudes are under or over-estimated (most likely underestimated because velocity fluctuations almost disappear).
The loss of accuracy due to the first-order momentum advection has been added to the line mentioned by the reviewer. The future improvement has also been added to this point of limitation as "The performance of a higher order approach for step-pool features remains an open research question for future research." in Lines 594-595.
The limitation of using first-order accuracy has also been stated in Lines 195-196 as "First-order momentum advection was applied also to ensure numerical stability under the complex bed surface conditions in this study but inevitably led to the loss in accuracy."

2) As expressed in the first review, the distance between the inlet and the first step is (based on the figures) 10 to 20 cm. Boundary conditions are critical in a CFD simulation. A short distance with a uniform velocity profile does not represent the inlet of a step-pool unit. The authors justify this by mentioning the work of Wohl and Thompson (2000), but they had developed turbulence when working in the field. Also, adding 2 to 5 cm is still not enough. I mentioned this because I have experience simulating step-pool sequences using LES and noticed that the flow variables in the first unit are different than the 2nd and 3rd. Actually, the first unit may not be used to calculate average properties because it is the one that helps in developing the flow structure in the subsequent units. Then the authors said that "This is supported by the fact that the streaky coherent structures already formed at the downstream of protruding grains upstream of the step in this study". This is not an accurate statement because it is a result of the model. You will always have some flow structure, but you can only determine if it is valid if you have measurements.
We admit the relatively short distance from the inlet to the step-pool unit as a limitation to this study and has stated it in Section 4 as point 3. Mentioning the work of Wohl and Thompson (2000) was to indicate that flow turbulence would significantly decrease as the flow velocity increases over the step stones. The flow turbulence might be underestimated upstream of the step-pool unit, but such mechanism would restrict the influence of inlet setting to the hydraulics over a step-pool unit.
Thanks for sharing the experience on LES simulations for step-pools. It is important and we would pay attention to it if we get the chance to compare the performance of LES and RNG on step-pool hydraulics, especially for step-pool sequence in future.
In order to further highlight this limitation, we added "It is noteworthy that the limited distance from the inlet to the step available in the DSMs might lead to underestimated turbulence upstream of the step (see detailed discussion in Section 4.5)." in Lines 203-205. Also, a sentence "Long enough distance from the inlet to a step-pool unit is suggested for future research to better reconstruct the incoming flow turbulence for the step-pool unit." has been added in Lines 589-590.

3) There is only one step-pool unit in the experiment. This is not representative of reality because they are sequences most of the time.
It is true that most step-pool features appear as sequences in nature and that it is more physically-solid if step-pool sequences are reconstructed to study step-pool units in flume experiments. Using step-pool sequences in flume experiment would lead to great difficulty in controlling variables, especially for the replicate flume experiments that needs the same initial conditions. As a result, only one step-pool unit was inspected in the experiments of Zhang et al. (2020) in which the initial geometric conditions of the step (locations and orientations of all the step stones) needed to keep as same as possible in all the tested runs. We admit this

setting to be a limitation and has added it to Section 4.5 in the discussion Section. The flume experiment for step-pool sequence with detailed hydraulic and morphological measurements is our plan for future research when the techniques such as 3D printing to make the experiment repeatable are ready in our lab.

So, when considering the cumulative effects of the different experimental configurations, 1st order + boundary conditions + single step-pool unit, I don't know if the results are a good representation of what was happening in the actual experiment.

I believe all these three points must be acknowledged and explained earlier in the article and not leave them for a small discussion at the end of the text. I would place them in section 2. This is a good study and will certainly be a reference for future studies, so these simplifications and decisions must be highlighted. Subsequent studies can identify these gaps and improve upon them. There is no problem by saying that simplifications have been done, actually that would be an advantage because they can be clearly identified.

These points have been highlighted in Section 2 and sentences on potential future improvements corresponding to the simplifications has been added in Section 4.5.

Finally, some responses are very useful but were only included in the line-by-line responses and not in the actual article. For example, the comment about convergence criteria, boundary conditions for k and epsilon, etc. Make sure that the answers are included in the text too. My comments are intended for the general audience.

These points were not presented in the first revision to avoid increasing the length of the manuscript which was already long. As suggested by the reviewer, some responses (including all the points mentioned by the reviewer) in first revision have been incorporated into the manuscript, especially in Section 2.2. For instance, the GMRES method has been added in Lines 197-198 and the setting of k and epsilon at the inlet boundary has been presented in Lines 215-217.

References

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Turowski, J.M., Yager, E.M., Badoux, A., Rickenmann, D., Molnar, P., 2009. The impact of exceptional events on erosion, bedload transport and channel stability in a step-pool channel. *Earth Surface Processes and Landforms, 34*(12), 1661–1673.

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