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Review and rebuttal of the paper

Analytical Models for Determining the Propagation of Rectangular Surface Jets for Fishway Attraction Flow

Wiering et al.

Editor handling the paper: Hans Bihs

The reviewers remain anonymous.

Review Round 1:

Point-by-point responses to reviewer A comments

Point 1: The article demonstrates the idea to estimate fish attraction flow for the design of fishways with the help of existent analytical concepts of jet theory. But there is a high confidence in the method of using RANS simulations to verify the concept even though there is missing supporting experimental data for a comprehensive validation of the RANS model itself. This concern of a scientific method may be considered for revision of the paper and elaborated on in the discussion. The fit with the RANS model results might be enough to demonstrate that the approach is valid but needs to be investigated further.

Research gap is properly defined in the introduction:
- finding an efficient way to estimate the propagation length and shape of fish attraction flow with the proposed solution to make use of theory from turbulent surface jets

Research Question(s) and objectives are clearly defined in the Introduction:
Is jet theory applicable to predict fish attraction flow? More precise:

1. are the analytic concepts
   - for rectangular horizontal free jets with W/H>1 and
   - for plane & round free jets

   adaptable to predict the propagation of rectangular surface jets with W/H<1?
2. If yes -> Are these analytical concepts suitable for fishways?

Methods
Comparison of two analytical models

- with 3D RANS modeling for 1/16< W/H < 4
- with results of free and surface jet in the scientific literature

Response 1: Thank you for your valuable suggestions to improve the manuscript. Please find the responses to your comments below.

Point 2: The basis and the paper's focus is the comparison of two analytic concepts to results of RANS simulations. The representation of results for free and surface jets from the scientific literature gets less attention in the author's conclusions.

Response 2: We started the “Conclusion” by naming the result of this study. To further include the results for free and surface jets into the “Conclusion”, we have added the percentage deviation from the analytical models to the literature results that we have taken from chapter 4.3. (p.16 l.16-17 + 21-22)

Point 3: Hence, the validation of the RANS model is an important part to support the paper's findings.
Concerning validation: Generally the RANS model features several constants and parameters to specify. E.g. the turbulence intensity or mixing length. A validation for the chosen setup is made with data gained for W/H=1, where there is no 2D zone. The situation to be evaluated (with W/H<1) is different though. Including validation with other setups that take into account the 2D zone would be desirable. Also because the validation data is included in the results later on. A more comprehensive calibration and validation process was expected. However, the analytical concepts are compared with other data from scientific literature. Therefore the focus is not always in particular on the RANS model and the analytical approaches gain trust together with experimental data as well.
In the **discussion and conclusion** however, the RANS simulation is used as the main argument to evaluate the applicability of the analytical concepts. The agreement with the results of the RANS model are seen as the validation for the analytical approaches. Additional experimental data would make this more powerful. Supporting the model with actual measurements for the setup the jet theory is intended to be adapted for would make the conclusion more reliable.

Response 3: We share the reviewer’s opinion that a more extensive validation with additional data would further increase the confidence in the RANS simulations. However, we are not aware of any publication or study that provides results or experimental data for rectangular surface jets with W/H < 1. Previous studies on surface jets focus on plane orifices (e.g. Giger et al. 1991), round or horizontal orifices (e.g. Rajaratnam & Humphries 1984; Madnia & Bernal 1994).

The validation with W/H = 1 shows that the jet propagation is well represented for the case of two zones of free jets and surface jets. Unfortunately, there is a lack of suitable measurement data for validation for other cases. Therefore, we additionally provided an extensive comparison of longitudinal jet propagation L_x of RANS and both analytical models with values from the literature (chapter 4.3).The agreement of L_x computed from RANS for W/H = 1/8 to 4 and results of available literature increases the confidence of simulation, since L_x may be located in the two-dimensional or axisymmetric zone depending on W/H.

We have addressed the reviewer’s comments accordingly in a new first paragraph of the “Discussion”. (p.15 l.5-11)

Point 4: Moreover it would increase the focus on the adaption of jet theory to attraction flow of vertical slot fishways. This method evaluates the applicability for limited and ideal boundary conditions. This is discussed only very briefly. In the end, the identified question nr. 2 - if the analytical concept is applicable for fishways - is not answered with this approach due to its

limitations.
Improvements could be made by elaborating on the fact that the results of the RANS simulation might not be enough to finally decide that the analytical concepts predict the propagation of fish attraction flow for fishways.

Response 4: In the “Introduction”, we stated more clearly what the boundary conditions are for fishways and described that we are assuming idealized conditions in this study. Accordingly, a direct transfer of these results to fishways is not possible. Additional parameters that consider the complex conditions at fishways were established in Heneka et al. (2021). This study sets the basis and confirms that the analytical models are well suited to estimate the propagation of idealized surface jets. (p.1 l.35-38)

Point 5: Besides this concern about confidence in the results of the simulation, this paper can be considered a decent basis for applications in hands-on engineering which is why the suggested approach might still be published to enhance the development for fishway design. It illustrates the possibilities of this idea. Just like it is mentioned to already be included in a parametric approach for concrete cases in practice (Heneka, P.; Zinkhahn, M.; Schütz, C.; Weichert, R. B. (2021): A Parametric Approach for Determining Fishway Attraction Flow at Hydropower Dams. In Water 13 (743). DOI: 10.3390/w13050743).

Response 5: This is exactly the purpose of this paper. The analytical models provide the basis for the application by Heneka et al. (2021).

Point 6: ([note on p.4 l.8-9]) "Shorter and longer axes of the rectangular orifice are denoted as minor and major axis, respectively, so that denotations of W and H reverse at W/H = 1."
The denotations don‘t reverse in Figure 2. The Figure states the „velocity profile along the minor/major axis“ for the same axis for W/H<1 and W/H>1. This would get more clear if adapted in graph 2b). Moreover, the differences could be pointed out, since there are already 2 graphs and it is about adapting the concept which is originally for W/H>1 to W/H<1.

Response 6: We revised figure 2a and 2b completely and also – to be consistent – figure 1. The erroneous denotation is corrected. Velocity profiles are revised to stress the differences between vertical and horizontal orifices in the two-dimensional zone. (p.4 l.8-10 + p.5 l.1-2)

Point 7: ([note on p.4 l.8-9]) The inner diffusion angle is not mentioned here but will play a role in the upcoming concept. It could be shortly mentioned here together with the explanation of the Figure to draw some attention to it.

Response 7: The comment shows that we have not consistently separated the inner diffusion angle in “2.1 General Remarks” and did not introduce it first with Kraatz’s concept (IDAC). To be consistent, we have deleted the reference to the inner diffusion angle in the ”Introduction” as well as in the caption in Fig. 1. In order to illustrate the inner diffusion angle in the description of “2.3

Inner Diffusion Angle Concept (IDAC)" we have left its designation in Fig. 2 and added a note in the caption as suggested. (p.3 l.5+10 + p.5 l.4)

Point 8: ([note on p.6 PSC]) PSC: for W/H>1, so there is no adaption of the analytical concept to W/H<1, it is basically directly used to predict also jets for W/H<1. If so, this could be clarified here already. The impression is created before that these concepts are modified to be adaptable to this investigated case.

Response 8: We clarified that we rewrote the existing equations using symmetry considerations and an alternative definition of h = H/2 for our application of surface jets. (p.6 l.6-9+13-14)

Point 9: ([note on p.7 IDAC]) IDAC: Figure 3: The bold and dashed lines could be clarified in the description of the Figure to make the graph more easily accessible for the reader.

Response 9: We added this comment in the caption of figure 3. (p.7 l.16-17)

Point-by-point responses to reviewer B comments

Point 1: The paper presents a comparison of different models to predict a surface water jet to measured data from literature. The models are presented in a comprehensive way. The comparison is well done and described in an understandable way. The figures are of good quality as well as language and structure. I found the paper worth publishing after the athours have adressed the following two comments.

Response 1: Thank you for your valuable suggestions to improve the manuscript. Please find the responses to your comments below.

Point 2: What brings you to the assumption that the analytical models do not work in the first place. I would have assumed that they work and it seems that you did not present any novelty here. If it is stated somewhere in the text please make sure to emphasis it more to make the reader understand what the novelty of this paper is.

Response 2: We have two major points concerning novelty that motivated us to propose this paper:

1. To our knowledge, the application of analytical models for surface jets especially with W/H<1 has not been validated. E.g. Demissie (1980) derived equations for deeply submerged jets with horizontal orifices (W/H>1) and validated his approach with measurements for orifices with W/H=4 and 13, respectively and minimum submergences H0/H=19. Hence, neither surface jets nor vertical orifices have been regarded although the equations are theoretically not restricted to the

cases used for validation. To reduce uncertainty in the application of these equations for surface jets, we rewrote the analytical models and validated their suitability with RANS simulations.

2. Recent research for horizontal surface jet (e.g. Gholamreza-Kashi et al. (2007)) does not consider analytical models which led us to the assumption that these models are largely unknown (both have been published as PhD theses) in the field of jet propagation. Furthermore, we compare both models and discuss application for fishway attraction flow.

In our understanding, the present contribution exceeds the character of a pure review of analytical models and adds novel information specified above. To be more precise for the reader, we highlighted the knowledge gaps more concretely in the last paragraph of the "Introduction". (p.3 l.21-27)

Point 3: The comparison show that the analytical models predict the jet characteristics in a similar way than the more advanced CFD model does. Please comment on how you want to use the analytical models in a practical case where the upstream boundary condition is not symmetric. I guess the jet will behave differently than predicted by the analytical models. Consequently a more advanced model has to be used anyway to confirm the flow characteristics needed to attract fish. So bottom line, what can the analytical models as they are presented herein, be used for?

Response 3: We agree about the effect of boundary conditions on jet propagation. Our current understanding is that this effect may not be captured by analytical models because boundary conditions are too manifold to be generalized in an applicable model. As an alternative, for example, the influence of upstream turbulence on jet propagation was investigated numerically (Mahl et al 2021) and correction factors that may be applied in connection with the present models were derived (Heneka et al. 2021). We clarified this point more precisely in the "Introduction" and "Conclusion". (p.1 l.35-38 + p.16 l.24-28)